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# **Unemployment Benefit Exhaustion: Incentive Effects on Job Finding Rates: A Systematic Review**

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# Executive Summary/Abstract

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## BACKGROUND

In order to reduce unemployment levels, policymakers may wish to reduce the generosity of the unemployment system. While it may be politically intractable to lower the amount of unemployment benefits, the length of the unemployment benefit eligibility period is often used as a political instrument to create work incentives for the unemployed. If the prospect of exhaustion of unemployment benefits results in a significantly increased incentive for finding work, shortening the benefit eligibility period may reduce the share of long and unproductive job searches and thereby decrease the overall unemployment level.

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## OBJECTIVES

The primary objective of this systematic review was to study the impact of exhaustion of unemployment benefits. The primary outcome was unemployed individuals' exit rate out of unemployment and into employment prior to benefit exhaustion or shortly thereafter. To determine if benefit expiration was associated with poor job matches, the secondary outcome of exit rate from the re-employment job was also explored.

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## SEARCH STRATEGY

Relevant studies were identified through electronic searches of bibliographic databases, government policy databanks, Internet search engines, and hand searching of core journals. We searched to identify both published and unpublished literature. The searches were international in scope. Overall, 23,991 references were screened, 454 full text reports were retrieved, and 47 studies were finally included. In addition to the general search, the reviewers have searched citations and reviews of related subjects.

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## SELECTION CRITERIA

All study designs that used a well-defined control group were eligible for inclusion in this review. Studies that utilised qualitative approaches were not included in the review due to the absence of adequate control group conditions.

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## DATA COLLECTION AND ANALYSIS

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The total number of potential relevant studies constituted 23,991 hits. A total of 47 studies, consisting of 65 papers, met the inclusion criteria and were vetted by the review authors. The final group of 47 studies were from 19 different countries. Only 21 studies provided data that permitted the calculation of an effect size for the primary outcome. Of these 21 studies, 4 studies could not be used in the data synthesis due to too high risk of bias, and a further 5 studies could not be used in the data synthesis due to overlap of data samples. Only 12 studies were therefore included in the data synthesis. Only 4 studies provided data that permitted the calculation of an effect size for the secondary outcome. Of these, 1 study could not be used in the data synthesis due to overlap of data samples.

Random effects models were used to pool data across the studies. We used the point estimate of the hazard ratio. Pooled estimates were weighted with inverse variance methods, and 95% confidence intervals were used. Subgroup analysis was used to examine the impact of gender. Sensitivity analysis was used to evaluate whether the pooled effect sizes were robust across components of methodological quality and in relation to the quality of data. Funnel plots were used to assess the possibility of publication bias.

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## RESULTS

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A statistically significant exhaustion effect in the month/week of benefit exhaustion was found. The effect estimate translates into an increase of approximately 80% in the exit rate from unemployment into employment. The increase in the exit rate starts even earlier: two months before benefits expire. The analysis revealed a statistically significant exhaustion effect one and two months before benefit exhaustion, though these effects were smaller than the effect in the month/week of exhaustion. The effect estimate one month before benefit exhaustion translates into a 30% increase in the exit rate from unemployment into employment. The effect estimate two months before benefit exhaustion translates into a 10% increase in the exit rate from unemployment into employment. No significant effects were found more than two months before exhaustion and no significant effects were found after benefits had expired. Thus, available evidence supports the hypothesis that there is an incentive effect of approaching benefit exhaustion but only shortly prior to exhaustion and at the time of exhaustion. The incentive effect is stronger at the time of exhaustion than one and two months before expiration. However, in all time periods, the hazard rate into employment increases from a low level. There was insufficient evidence to address whether the prospect of benefit exhaustion has an impact on the exit rate from the re-employment job.

The results are robust in the sense that sensitivity analyses of the exhaustion effect evidenced no appreciable changes in the results. We found no strong indication of the presence of publication bias.

We found no evidence to support the hypothesis that the exhaustion effect differs by gender. It was not possible to examine if the exhaustion effect differs for particular age or educational groups, or if factors such as good/bad labour market conditions, high/low initial maximum entitlement, availability of alternative benefits, and whether compulsory activation is part of the institutional system have an impact on the exhaustion effect.

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## **AUTHORS' CONCLUSIONS**

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In this review we have found clear evidence that the prospect of exhaustion of benefits results in a significantly increased incentive for finding work but only shortly (one and two months) prior to exhaustion and at the time of exhaustion. A significant benefit exhaustion effect is the result of a meta-analysis where we pooled measures from seven different European countries, the US, and Canada. Thus, the theoretical suggestion that the prospect of exhaustion of benefits results in a significantly increased incentive for finding work has been confirmed empirically by measures from a variety of countries. Hence, shortening the benefit eligibility period may reduce the share of long and unproductive job searches.

Whether the increased job finding rate close to benefit expiration implies a significant decrease in the overall unemployment level depends on how quickly those who found a job return to unemployment. We found studies from three different countries, which provided data for re-employment exit rates. Based on this low number of studies, the evidence is inconclusive with respect to the hypothesis that the prospect of benefit exhaustion has an impact on the quality of the job measured as the exit rate of re-employment. Thus, whether the unemployed workers who are affected may actually be worse off than policy-makers intend them to be, in the sense that they accept “bad” jobs, has not yet been fully investigated. While additional research is needed, the findings of the current review support the hypothesis of an increased incentive for finding work as unemployment benefit exhaustion approaches.

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# 1 Background

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## 1.1 DESCRIPTION OF THE CONDITION

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In 1970, the unemployment rate in the US was 5%, while the unemployment rate in the European Union was 3% (Solow, 2000). Since the first oil crisis in 1973, the unemployment rates in Europe and the US have diverged. While it remained relatively steady in the US, there was an upward trend in Europe. By the end of the century, the unemployment rates in most European countries did not seem to go back to the low levels that were commonplace 30 years ago, when the average unemployment rate in the European Union was around 10%.<sup>1</sup> This steady contrast has posed the inevitable question: what explains the difference between the levels of unemployment in Europe and the US? Many hypotheses have been put forward to explain this difference, but no single factor has been identified. In labour market research, the conventional understanding is that the difference rests on differences in labour market “institutions.” The variables considered are, among others, the unemployment benefit system, trade union power, taxes, employment protection, barriers to labour mobility, and wage inflexibility (Layard, Nickell, & Jackman, 2005; Nickell, Nunziata, & Ochel, 2005). Among these variables, the benefit system is shown to be one of the key factors (Layard et al., 2005).<sup>2</sup> The main aspect of the benefit system that influences unemployment is the generosity of the system either in amount or in duration of benefits. In the US, replacement rates<sup>3</sup> are low and duration is short compared to most European countries. According to the Organisation for Economic Co-operation and Development (OECD, 2007), the maximum duration<sup>4</sup> in 2005 was shortest in the US at 6 months,<sup>5</sup> and longest in Denmark, Norway, Portugal, the Netherlands, France, Finland and Spain where it varied between 23 and 48 months. The gross initial replacement rate was around 50% in the US, and in the before mentioned European countries, it varied between 62% and 90%. The natural consequence is that higher levels of active searches and a greater willingness to accept inferior jobs by unemployed workers are seen in the US than in Europe.

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<sup>1</sup> By the end of the previous decade, however, the financial crisis turned things upside down. The US unemployment rate was 10% in 2010, whereas the unemployment rates in major European countries as Italy, the UK, and Germany was lower than in the US. The average unemployment rate in the European Union was, however, at the same level as in the US.

<sup>2</sup> Other key factors are the co-ordination level of the wage bargaining and employment adjustment costs.

<sup>3</sup> The replacement rate is the ratio of the unemployment benefit and previous earnings.

<sup>4</sup> For a 40-year-old single worker without children and with a 22-year employment record.

<sup>5</sup> The maximum duration was also around six months in the Czech Republic, the Slovak Republic, and the United Kingdom.

From a societal point of view, the optimal benefit system is determined as a trade-off between protection and distortion. Benefit programmes protect individuals against loss of income and provide unemployed individuals the possibility of finding a better match between their qualifications and job vacancies. In fact, this positive aspect of inducing risk-averse workers to achieve better job matches has been shown to increase economic efficiency (Acemoglu & Shimer, 1999; Marimon & Zilibotti, 1999). However, the same benefit can also distort incentives through job searches that are long and unproductive. Therefore, unemployment benefits should aim for a balance between protection and distortion (Feldstein, 2005; Mortensen, 1987).

In order to reduce the high unemployment level, European policy-makers may wish to reduce the generosity of the unemployment system. While it may be politically intractable to lower the replacement rate (indeed, examples of reductions of benefit rates and amounts are rare), the length of the unemployment benefit eligibility period is often used as a political instrument to create work incentives for the unemployed. For example, the benefit period was altered in Spain in 1992, in Slovenia in 1998, in Norway in 1997, in the UK in 1996, in Denmark in 1996, 1998 and 1999, and, more recently, in the Czech Republic in 2004, in Hungary and Portugal in 2006, and in Denmark in 2010.<sup>6</sup>

This review focuses on the effect of exhaustion of unemployment benefits and looks at the unemployed workers' exit rate into employment *prior* to exhaustion of unemployment benefits or shortly thereafter. The effect occurring *prior* to benefit exhaustion or shortly thereafter, which we denote the "incentive effect," is relevant because several studies, empirical as well as theoretical, suggest that the prospect of exhaustion of benefits results in a significantly increased incentive for finding work (Card, Chetty, & Weber, 2007; Caliendo, Tatsiramos, & Uhlenhoff, 2009; Feldstein, 2005; Katz & Meyer, 1990; Meyer, 1990; Mortensen, 1987). Hence, shortening the benefit eligibility period may reduce the share of long and unproductive job searches and thereby decrease the overall unemployment level.

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## 1.2 DESCRIPTION OF THE INTERVENTION

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The intervention that is the topic of this systematic review is the exhaustion of any kind of unemployment benefit with a known expiration date. The review focuses on the incentive effect, i.e., the exit rate out of unemployment into employment *prior* to exhaustion of unemployment benefits or shortly thereafter, which can be attributed solely to the prospect of benefits exhaustion.

The benefits may be unemployment insurance (UI) benefits, or they may be unemployment assistance (UA)/social assistance (SA) benefits as long as they have a known expiration date.

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<sup>6</sup> However, the benefit period was extended by six months in March 2012, just prior to the expiration of benefits for those who were first unemployed under the reduced period.

In the majority of OECD countries, the UI benefit has a time-limit. In fact, only Belgium has an unlimited UI period. In other countries, the maximum duration varies between 6 months (as for example in the UK and the US) and 36 months (in Iceland).

In most OECD countries, a secondary benefit is available for those who have exhausted regular UI benefits. This is known as SA benefits. Unlike UI benefits, SA benefits are generally means-tested without any necessary connection to past employment, pay a lower level of benefit, and are indefinite. We know of only one example of a SA benefit with a time limit: the Temporary Assistance to Needy Families (TANF) available in the US. The federal government requires states to impose between 2- or 5-year limits (Gustafson & Levine, 1997). In a minority of OECD countries, UA benefits are paid after exhaustion of UI benefits. Like SA benefits, they are generally means-tested, pay a lower level of benefits, and, excepting Hungary, Portugal, and Sweden, they are generally indefinite.

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### **1.3 HOW THE INTERVENTION MIGHT WORK**

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Search theory offers an explanation as to why we might expect to find an effect for this intervention. According to search theory, one can derive a relationship between the job-finding rate and the time to benefit exhaustion when the maximum benefit duration is fixed and predictable (Mortensen, 1977). This relationship is driven by adjustments in search effort and reservation wages. The reservation wage is the minimum wage at which the unemployed are willing to accept a job. The benefit exhaustion gives the unemployed individual a strong incentive to gain employment to avoid the drop in income after the exhaustion date. How strong the incentive is depends on the magnitude of the income drop. If no secondary benefit is available for those who have exhausted their current benefit, the incentive to gain employment will be stronger. As the unemployed worker approaches benefit expiration, the search intensity goes up and the reservation wage goes down, thus increasing the job finding rate. If an increased job finding rate is mainly driven by lowering the reservation wage, a lower job match quality is to be expected, for example, in the form of lower wages and/or lower re-employment duration.

A number of factors may have an impact on the magnitude of the expected increase in the job finding rate when approaching benefit exhaustion. In general, the overall labour market conditions, i.e. the vacancy rate<sup>7</sup> and, in particular, the unemployment rate, have an impact on the availability of and competition for jobs. If the vacancy rate is high, i.e. the number of vacancies is high in relation to job seekers, we would expect a bigger effect than if the vacancy rate is low. We would further expect a lower effect if the unemployment rate is high, regardless of the vacancy rate. If the vacancy rate is low, competition for available jobs is likely to be high. If the vacancy rate is high coincident with a high unemployment rate, it suggests mismatch in the labour market, i.e., the process by which vacant jobs and job seekers meet is not efficient (Filges & Larsen, 2000; Pissarides, 2000).

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<sup>7</sup> The number of unfilled jobs expressed as a proportion of the labour force.

The maximum benefit duration is also expected to have an impact on the size of the exhaustion effect. The longer the initial benefit eligibility period, more sorting may be expected to occur and, hence, a smaller benefit exhaustion effect would be expected. Sorting refers to a dynamic selection mechanism based on a relationship between individual heterogeneity (i.e., heterogeneity in the individual characteristics of the unemployed) and the hazard of leaving unemployment. Heterogeneity is related to job performance; those perceived to be most productive and more desirable to employers are hired first (Jackman & Layard, 1991; Salant, 1977). Several studies find sorting effects. For example, Lancaster (1979), Narendranathan and Stewart (1993), and, more recently, the analysis in Kalwij (2010), identify significant sorting effects. They show that both observed (to the researcher) heterogeneity (e.g., age and education) and unobserved (to the researcher) heterogeneity (e.g., motivation and 'drive') are important determinants of the unemployment hazard.

The extent to which those left unemployed by the end of the benefit eligibility period are considered unproductive and not desirable to employers has an effect on their unemployment hazard and, therefore, an impact on the exhaustion effect (i.e., it may be impossible to find an employer willing to hire the unemployed regardless of the search intensity or reservation wage).

Whether compulsory participation in active labour market programmes is part of the unemployment system may result in additional sorting. The compulsory aspect may provide an incentive for unemployed individuals to look for and return to work prior to programme participation (Geerdsen, Bjørn, Filges, & Jensen, 2011). Further, participation in active labour market programmes may improve some of the participants' qualifications, thus helping them to find a job. Hence, those left unemployed by the end of the benefit eligibility period may be considered even more unproductive if participation in active labor market programmes did not improve their qualifications or lead to a job. Alternatively, active labor market programmes may have negative stigmatisation and signaling effects to employers. Programmes associated with participants having poor employment prospect may carry a stigma. Because of asymmetric information, employers do not know the productivity of new workers, some of whom they might hire from the pool of the unemployed. Prospective employers might then perceive participants in such programmes as low productivity workers or workers with tenuous labour market attachment (Kluve, Lehmann, & Schmidt, 1999; Kluve et al., 2007).

Finally, the type of unemployment benefit may have an impact on the job finding rate close to exhaustion. As mentioned above, some countries employ two systems to provide benefits to unemployed individuals: an unemployment insurance system for individuals who typically have a strong labour market attachment (UI benefits) and a social welfare system for individuals who often have other problems in addition to unemployment (SA or UA benefits). The effect size in social welfare systems offering unemployment benefits with a known expiration date is expected to be less than the effect size in unemployment insurance systems with a known expiration date.

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## 1.4 WHY IT IS IMPORTANT TO DO THIS REVIEW

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There are many empirical papers on the effect of benefit exhaustion on unemployed individuals (Caliendo et al., 2009; Card et al., 2007; Katz & Meyer, 1990; Lalive, van Ours, & Zweimüller, 2006; Meyer, 1990), but the empirical research has not been summarised in a systematic review to obtain a clearer picture of the available evidence on the employment effect of benefit exhaustion. One paper provides a review of the recent literature on how incentives in unemployment insurance can be improved (Fredriksson & Holmlund, 2006). However, it is not a systematic review, and, furthermore, the authors do not make the important distinction between exits to employment and exits to other destinations such as secondary unemployment benefits. Distinguishing between destinations is vital. As shown in Card et al. (2007), the exit rate from registered unemployment increases over 10 times more than the rate of re-employment at the expiration of benefits. The difference between the two measures arises because many individuals leave the unemployment register immediately after their benefits expire without returning to work.

There is a great deal of political interest in optimising the unemployment benefit system, so it balances the protection and distortion dimensions. The political interest is to reduce the unemployment level, to prevent exploitation of the unemployment benefit system and at the same time protect the unemployed individuals with real difficulties in finding a job. It is therefore of great importance to examine what effect unemployment benefit exhaustion has on employment probabilities.

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## 2 Objectives

The primary objective of this systematic review is to study the impact of exhaustion of unemployment benefits on the job finding rates of unemployed individuals. The primary outcome is unemployed individuals' exit rate out of unemployment and into employment prior to benefit exhaustion or shortly thereafter. Due to the fact that a higher exit rate from re-employment jobs may indicate that benefit expiration forces unemployed individuals into less optimal jobs, the review will also examine the exit rate from the re-employment job as a secondary outcome.

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## 3 Methods

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### 3.1 TITLE REGISTRATION AND REVIEW PROTOCOL

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The title for this systematic review was registered on 21 May 2010. The systematic review protocol was approved on 14 October 2011. Both the title registration and the protocol are available in the Campbell Library at: <http://campbellcollaboration.org/lib/project/171/>. Note that the published titles on the title registration and protocol have changed.

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### 3.2 CRITERIA FOR CONSIDERING STUDIES FOR THIS REVIEW

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#### 3.2.1 Types of Studies

The study designs eligible for inclusion in this review were:

- Controlled trials (all parts of the study are prospective, i.e., identification of participants, assessment of baseline, allocation to intervention, assessment of outcomes, and generation of hypotheses (Higgins & Green, 2008)):
  - RCT - randomised controlled trial
  - QRCT - quasi-randomised controlled trial (i.e., participants are allocated by means such as alternate allocation, person's birth date, the date of the week or month, case number, or alphabetical order)
  - NRCT - non-randomised controlled trial (i.e., participants are allocated by other actions controlled by the researcher)
- Non-randomised studies (includes truly observational studies where the use of an intervention has occurred in the course of usual treatment decisions or peoples' choices)
  - NRS - the allocation is not controlled by the researcher, and there is a comparison of two or more groups of participants. Participants are allocated by means such as time differences, location differences, decision makers, policy rules, or participant preferences.

No controlled trials were identified. We have only included study designs that used a well-defined control group, i.e., unemployed persons whose benefit expiration was not immediate. Studies that utilised qualitative approaches were not included in the review due to the absence of adequate control group conditions.

### 3.2.2 Types of Participants

The participants were required to be unemployed individuals who received some sort of time-limited benefit during their unemployment spell. We included participants receiving all types of unemployment benefits with a known exhaustion date. The only restriction was that the benefits needed to be related to being unemployed. Therefore, studies examining individuals receiving other types of benefits not related to being unemployed were not eligible. We did not restrict our attention to certain types of participants, since the main focus of this review was on the incentive effect to find a job when benefits expire. Therefore, we included all unemployed participants regardless of age, gender, etc., who received some sort of time limited benefit during their unemployment spell.

### 3.2.3 Types of Interventions

The intervention of interest is the exhaustion of any kind of unemployment benefit with a known expiration date. The review focuses on the incentive effect, i.e., the exit rate out of unemployment into employment *prior* to exhaustion of unemployment benefits or shortly thereafter (one month after). The benefits were allowed to be unemployment insurance (UI) benefits or unemployment assistance (UA)/social assistance (SA). The only requirement was that the benefit must have had a known expiration date. The UI benefit usually has a known time-limit, whereas UA and SA usually are indefinite. Unemployment benefits with an indefinite time limit or non-financial benefits were not included in this review.

### 3.2.4 Types of Comparison Conditions

Studies of the effect of benefit exhaustion typically use data that describe individuals over time, making it possible to see when people move between the different states on the labour market, e.g., from unemployment to employment. This type of data facilitates the use of hazard ratios<sup>8</sup> to express the effect of benefit exhaustion. The hazard ratio measures the proportional change in hazard rates between unemployed persons approaching exhaustion (i.e., unemployed persons whose benefit expiration is immediate) and unemployed persons not approaching exhaustion (i.e., unemployed persons whose benefit expiration is not immediate).

A hazard is the rate at which an event happens (in the present context, finding a job) in a short time interval conditional on survival (staying unemployed) until that time or later (see Section 3.4.4 for a more thorough description of hazard rates).

The central problem in studies of benefit exhaustion is the identification of the incentive effect. Often the variable describing time to benefit exhaustion is a function of variables which all have a direct effect on an individual's duration of unemployment. But identification of the incentive effect requires that at least one of the variables be omitted from the

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<sup>8</sup> According to Duerden (2009), "Hazard ratios are increasingly used to express effects in studies comparing treatments when statistics which describe time-to-event or survival analyses are used" (p. 2).

modelling of the hazard rate (the exclusion restriction).<sup>9</sup> Examples of exclusion restrictions used in the primary studies are differences in benefits entitlement between individuals due to age or work experience (Card et al., 2007; Jenkins & Garcia-Serrano, 2004; Portugal & Addison, 2008).<sup>10</sup> In order to use the variation in entitlement to disentangle the incentive effect from other time varying effects, one has to assume that the entitlement does not, on its own, have an effect on individuals' hazard rate.

Sources of individual variation in entitlement (age and work experience) are often, however, correlated with personal characteristics, which may themselves have an impact on the exit rate. For example, in Portugal older individuals are entitled to longer benefit durations. Therefore, if individuals entitled to longer benefit durations find jobs at a slower rate, it can be attributed not only to the entitlement of longer benefit duration, but also to their age. To disentangle these two effects, variation in entitlement across individuals uncorrelated with work experience or age is needed. Legislative changes of the maximum entitlement provide such variation. Identification driven by legislative changes of the maximum entitlement period is used, for example, in van Ours and Vopodevic (2004), Vodopivec (1995), and Schmitz and Steiner (2007). Legislative changes make it possible to compare individual's labour market behaviour just before and after the change was implemented.

The incentive effect in the primary studies is given by the ratio of hazard rates *prior to*, or within one month of, benefit exhaustion for unemployed persons who approach exhaustion to the ratio for unemployed persons who do not approach exhaustion. All included studies examine the exhaustion effect of unemployment insurance benefits, i.e., the treated persons are unemployed receivers of unemployment insurance benefits whose benefit expiration is immediate. The majority of included studies use unemployed receivers of unemployment insurance benefits whose expiration is not immediate as the comparison, using individual variation in benefit entitlement (due to age or work experience) and/or legislative changes as mentioned. One included study (Addison & Portugal, 2004) used unemployed non-receivers of unemployment insurance benefits (whose expiration is not immediate, as they do not receive unemployment insurance benefits) as the comparison condition. Some studies estimate the incentive effect using indicator variables for the number of months or weeks until exhaustion (Portugal & Addison, 2008; Schmitz & Steiner, 2007; van Ours & Vopodevic, 2004), whereas others use a spline function describing the same time period (Card et al., 2007; Jenkins & Garcia-Serrano, 2004; Vodopivec, 1995).

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<sup>9</sup> Alternatively identification can be achieved by assuming that the duration dependence follows a specific functional form. For more information, see Geerdsen (2002).

<sup>10</sup> If, for example (as in Portugal in 1992-1997), an unemployed person of age 24 is entitled to 10 months of benefits, and an unemployed person of age 39 is entitled to 18 months of benefits, and both survive (stay unemployed) 9 months of unemployment, the 24 year old unemployed person's benefit expiration is immediate after 9 months of unemployment whereas the 39 year old persons benefit expiration is not immediate after 9 months of unemployment.

### 3.2.5 Types of Outcomes

The objective of the review is to determine whether the prospect of unemployment benefit exhaustion motivates unemployed individuals to find a job. Distinguishing between destinations is therefore vital. The primary outcome is exit to employment. Studies only looking at exits to other destinations, such as other types of social benefits or non-employment, were not included in this review. Studies that do not distinguish between destinations were excluded from this review.

In addition to the primary outcome measure, we planned to include the following secondary outcomes: duration of re-employment and re-employment wage. None of the included studies provided data that enabled the calculation of effect sizes for the re-employment wage. A few studies, however, provided data on the exit rate from the re-employment job, though none measured it directly as mean duration. We included the measure of exit rate from the re-employment job in the analysis of secondary outcomes. A higher exit rate from the re-employment job may indicate that the exhaustion of benefits forces unemployed individuals to find jobs that do not match their qualifications and, therefore, return to unemployment quickly.

#### Primary outcomes

- a) Exit rate from unemployment to employment

#### Secondary outcomes

- a) Exit rate from the re-employment job

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## 3.3 SEARCH METHODS FOR IDENTIFICATION OF STUDIES

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The search was performed by one review author (AKJ) and one member of the review team (PVH).<sup>11</sup>

### 3.3.1 Electronic Searches

Relevant studies were identified through electronic searches of bibliographic databases, government policy databanks, and Internet search engines. No language or date restrictions were applied to the searches. The searches were conducted between November 2010 and March 2011. Copies of relevant documents were downloaded, recording the exact URL and date of access.

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<sup>11</sup> Members of the review team at SFI Campbell are: the research assistants Pia Vang Hansen, Simon Helth Filges, and Stine Lian Olsen.

### 3.3.2 Search Terms

An example of the search strategy for Business Source Elite and modifications of the search are listed in Appendix 10.1. As this review includes non-randomised study designs, trial filters were not used.

The following databases have been searched:

1. Business Source Elite (1985 - December 2010)
2. EconLit (1993 - December 2010)
3. PsycInfo (1800 - December 2010)
4. SocIndex (1895 - December 2010)
5. Social Science Citation Index (1956 - December 2010)
6. The Cochrane Library
7. International Bibliography of the Social Sciences (1951 - December 2010)
8. IDEAS/Economist Online/Social Care Online<sup>12</sup>
9. Dissertation Abstracts International
10. Theses Canada

### 3.3.3 Searching Other Resources

#### ***Hand-searching***

Reference lists of included studies and reference lists of relevant reviews have been searched. *The Journal of Labor Economics* and *Labour Economics* have been hand-searched for the year 2010 and the available issues of 2011.

#### ***Grey Literature***

Google was used to search the web to identify potential unpublished studies. Advanced search options were used to refine the grey search strategy. OpenGrey was used to search for European grey literature (<http://www.opengrey.eu/>).

Unpublished theses and dissertations were located through the databases: Theses and Dissertations and Theses Canada.

The websites for the following private independent research institutes and economic networks were examined for potentially eligible studies:

1. IZA – Institute of the Study of Labor ([www.iza.org](http://www.iza.org))
2. CEPR – Centre for Economic Policy Research ([www.cepr.org](http://www.cepr.org))
3. NBER – National Bureau of Economic Research ([www.nber.org](http://www.nber.org))
4. MDRC – the Manpower Demonstration Research Corporation – ([www.mdrc.org](http://www.mdrc.org))

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<sup>12</sup> The search strategy has been modified to a great extent to search these databases. The search interfaces do not allow complex searching. Even though these databases contain similar references, we have searched both trying to perform as thorough a search as possible.

5. CESifo – the cooperation between CES (Center for Economic Studies) and IFO (Institute for Economic Research) – ([www.cesifo-group.de/portal/page/portal/ifoHome](http://www.cesifo-group.de/portal/page/portal/ifoHome)) are all covered via IDEAS.

In addition, we searched the following websites:

1. Danish Economic Councils ([www.dors.dk](http://www.dors.dk))
2. OECD - the Organisation for Economic Co-operation and Development ([www.oecd.org](http://www.oecd.org))
3. IMF - The International Monetary Fund ([www.imf.org](http://www.imf.org))
4. AIECE - Association of European Conjuncture Institutes ([www.aiece.org](http://www.aiece.org))
5. ESRC - Economic Social Research Council ([www.esrc.ac.uk](http://www.esrc.ac.uk))
6. Copenhagen Economics ([www.copenhageneconomics.com](http://www.copenhageneconomics.com))
7. The Social Science Research Network ([www.ssrn.com](http://www.ssrn.com)) was also searched to uncover potential preprint discussion papers.

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## 3.4 DATA COLLECTION AND ANALYSIS

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### 3.4.1 Selection of Studies

One review author (ADK) and two members of the review team (SHF, SLO) independently read titles and available abstracts of reports and articles identified in the search to exclude reports that were clearly irrelevant. Citations considered relevant by at least one reviewer were retrieved in full text versions. If there was not enough information in the title and abstract to judge relevance, the full text was retrieved.

Two reviewers (ADK, TF) and one member of the review team (SHF) read the full text versions to ascertain eligibility based on the selection criteria. Any disagreements were resolved by discussion. A screening guide (see Appendix 10.3) was used to determine inclusion or exclusion and was provided in the protocol (Filges et al., 2011).

### 3.4.2 Coding and Numeric Data Extraction

One review author (ADK) and one member of the review team (SHF) independently coded the included studies (see Appendix 10.4). A coding sheet was piloted on several studies (Filges et al., 2011). Disagreements were resolved by consulting a third review author (TF). Information was extracted on: characteristics of participants, intervention characteristics and control conditions, research design, sample size, and censoring. Numeric data extraction (outcome data) was performed by one review author (TF) and was checked by a second review author (ADK). Extracted data were stored electronically. Analysis was conducted in RevMan5.

### 3.4.3 Assessment of Risk of Bias in Included Studies

Two review authors (TF & ADK) independently assessed the risk of bias for each included study. There were only minor disagreements, and they were resolved by discussion. We assessed the methodological quality of studies using a risk of bias model developed by Prof. Barnaby Reeves in association with the Cochrane Non-Randomised Studies Methods Group.<sup>13</sup> This model, an extension of the Cochrane Collaboration's risk of bias tool, covers risk of bias for RCTs as well as risk of bias for non-randomised studies that have well-defined control groups.

The point of departure for the risk of bias model is the Cochrane Handbook for Systematic Reviews of Interventions (Higgins & Green, 2008). The existing Cochrane risk of bias tool needs elaboration when assessing non-randomised studies because particular attention must be given in these studies to selection bias/risk of confounding. It is also important to try to discriminate between non-randomised studies with varying risk of bias, so the model requires assessment on a 5-point scale for some items.

#### **Risk of Bias Judgement Items**

The risk of bias model is based on 9 items (see Appendix 10.5). Some items are judged by High/Low/Uncertain and some by a 5-point scale. Using the 5-point scale, 1 corresponds to Low risk of bias and 5 correspond to High risk of bias. Five corresponds to a risk of bias so high that the findings will not be considered in the data synthesis (because they are more likely to mislead than inform).

The 9 items concern **sequence generation** (relevant for selection bias), **allocation concealment** (relevant for selection bias), **confounders** (relevant for selection bias; only for non-randomised studies, i.e. NRCT and NRS), **blinding** (relevant for performance, detection, and attrition bias), **incomplete outcome data** (relevant for attrition bias), **selective outcome reporting** (relevant for reporting bias), **other potential threats to validity** (relevant for performance, detection, and other sources of bias), and **a priori protocol and a priori analysis plan** (relevant for reporting bias).

#### **Selection Bias and Confounding**

An important part of the risk of bias assessment of non-randomised studies (NRCT and NRS) is how the studies deal with confounding factors (Filges et al., 2011). Selection bias is understood as systematic baseline differences between groups and can therefore compromise comparability between groups. Baseline differences can be observable (e.g., age and gender) and unobservable (to the researcher; e.g., motivation and 'ability'). There is no single non-randomised study design that always solves the selection problem. Different

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<sup>13</sup> This risk of bias model was introduced by Prof. Reeves at a workshop on risk of bias in non-randomised studies at SFI Campbell, February 2011. The model is a further development of work carried out in the Cochrane Non-Randomised Studies Method Group (NRSMG).

designs attempt to solve the selection problem under different assumptions and require different types of data. Most important, there is variation in how different designs deal with selection on unobservables. The “right” method depends on the model generating participation, i.e., assumptions about the nature of the process by which participants are selected into a programme. For examples of identification strategies used in the primary studies included in this review, see Section 3.2.4.

As there is no universal correct way to construct counterfactuals for non-randomised designs, we looked for evidence that identification was achieved, and that the authors of the primary studies justified their choice of method in a convincing manner by discussing the assumption(s) leading to identification (the assumption(s) that make it possible to identify the counterfactual). Preferably the authors should make an effort to justify their choice of method and convince the reader that the only difference between an individual approaching time to exhaustion and an individual not approaching time to exhaustion is exactly the time to exhaustion and that the source of difference between their time to exhaustion is not endogenous to the individuals’ exit rate to employment. The judgment is reflected in the assessment of the confounder “unobservables” in the list of confounders considered important at the outset and defined in the protocol for this review.

In addition to unobservables, for this review, we identified the following observable confounding factors to be the most relevant: age, gender, education, ethnicity, labour market conditions, censoring, and unemployment duration. In each study, we assessed whether these confounding factors had been considered, and in addition we assessed other confounding factors considered in the individual studies. The motivation for focusing on age, gender, education, and ethnicity was that they are major determinants of the risk of being unemployed (Layard et al., 2005).

Concerning unemployment duration, most studies find that the genuine duration dependence is negative, i.e., the longer the unemployment spell, the smaller is the chance of finding a job (see Serneels (2002) for an overview).<sup>14</sup> If the study does not disentangle the effect of the benefit exhaustion from the negative duration dependence the effect of benefit exhaustion will be biased.

Another potential source of bias is differences in labour market conditions. If the study, for example, explores changes in the maximum benefit entitlement over time or space as the source of variation, it is very important to control for changes in labour market conditions over time (as a consequence of the business cycle, for example) or over space as the exit rate to employment most certainly will depend on this factor.

Censoring may also introduce bias. The effect of benefit exhaustion is often measured with survival data. Participants who do not leave the unemployment system before the end of the

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<sup>14</sup> The reason for this is that unemployment implies a loss of skills or that long periods of unemployment lead to a loss of self-confidence. This “genuine” duration dependence should not be confused with sorting which is another mechanism.

study are censored from the outcome data. If not adequately accounted for, such censoring has the potential for introducing bias. Therefore, censoring of participants is a potential threat, both in relation to the level of censoring and in relation to whether censoring is taken into account.

#### 3.4.4 Measures of Treatment Effect

Our main interest was to include studies in a meta-analysis where hazard ratios and variances were either reported or were calculable from the available data. All the effect sizes used in the data synthesis were measured as log hazard ratios. We performed the meta-analyses on the individual included studies using the log hazard ratio and variance. We report the 95% confidence intervals. The secondary outcome, exit rate from the re-employment job, was also measured as hazard ratios and the effect sizes were measured as log hazard ratios. We report the 95% confidence intervals.

The hazard ratio measures the proportional change in hazard rates between unemployed persons approaching exhaustion and unemployed persons not approaching exhaustion. The hazard rate is defined as the event rate (in the present context, the event is finding a job) at time  $t$  conditional on survival (staying unemployed) until time  $t$  or later. A hazard rate is constructed as follows:<sup>15</sup>

The length of an unemployment spell for an unemployed individual (in the present context the length of stay in the unemployment system until finding a job) is a realization of a continuous random variable  $T$ . In continuous time the hazard rate,  $\theta(t)$ , is defined as:

$$\theta(t) = \lim_{\Delta t \downarrow 0} \frac{\Pr(t \leq T < t + \Delta t | T \geq t)}{\Delta t} = \frac{f(t)}{S(t)} = \frac{f(t)}{1 - F(t)},$$

where the cumulative distribution function of  $T$  is:

$$F(t) = \Pr(T < t)$$

and the probability density function is:

$$f(t) = \lim_{\Delta t \downarrow 0} \frac{\Pr(t \leq T < t + \Delta t)}{\Delta t} = \frac{dF(t)}{dt}.$$

$F(t)$  is also known in the survival analysis literature as the failure function and in the present context failure means finding a job.  $S(t)$  is the survivor function:

$$S(t) \equiv \Pr(T \geq t) = 1 - F(t);$$

$t$  is the elapsed time since entry to the state (since the individual entered the unemployment system).

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<sup>15</sup> The following description of hazard rates is based on Jenkins (2005) and van den Berg (2001).

Introducing covariates the hazard rate becomes:

$$\theta(t|x(t, s)) = \lim_{\Delta t \downarrow 0} \frac{\Pr(t \leq T < t + \Delta t | T \geq t, x(t, s))}{\Delta t},$$

where  $x(t, s)$  is a vector of personal characteristics that may vary with unemployment duration ( $t$ ) or with calendar time ( $s$ ).

The incentive effect is the difference in hazard rates *prior* to benefit exhaustion or shortly thereafter between persons who approach exhaustion and persons who do not approach exhaustion. In all studies included in the meta-analyses the incentive effect was given as a proportional change in hazard rates. A proportional hazard rate is given by:

$$\theta(t|x) = \theta_0(t) * \exp(x'\beta),$$

where  $\theta_0(t)$  is the baseline hazard,  $\exp(x'\beta)$  is a scale function of the vector  $x$  of personal characteristics and  $\beta$  is a vector of estimated parameters.

The vector  $x$  of personal characteristics includes the individuals' remaining time to exhaustion. Other personal characteristics typically included in the studies used in the meta-analyses are age, gender, education, ethnicity, labor market conditions, individual labor market history, family and disability.

In the description of the hazard rate it is, so far, implicitly assumed that all relevant differences between individuals can be summarized by observed explanatory variables. But if there are unobservable differences, e.g. motivation and 'ability' (in the literature termed unobserved heterogeneity) and these differences are ignored, the estimated parameters will be biased towards zero. It is therefore common to control for both observed factors given by the vector  $x$  as well as unobserved factors, i.e. unobserved heterogeneity. The hazard rate, including unobserved heterogeneity, is now given by:

$$\theta(t|x, v) = \theta_0(t) * \exp(x'\beta)v,$$

where  $v$  represents factors unobserved to the researcher and independent of  $x$ . It is necessary to assume the distribution of  $v$  has a shape where the right-hand tail of the distribution is not too fat and whose functional form is summarized in terms of only a few key parameters, in order to estimate those parameters with the data available.

In order to control for unobserved heterogeneity, it is of enormous importance for applied duration analysis that multiple spell data are available. Multiple spell data provide durations of multiple spells in a given state for a given individual (in the present context, more than one unemployment spell for a given individual). If two observations are available for each  $v$ , then the estimation no longer requires an untestable assumption on the tail of the unobserved heterogeneity distribution as with single spell data, and need not be

independent anymore.<sup>16</sup> Overall, eight studies used in the data synthesis controlled for unobserved heterogeneity; of these, two used multiple spell data to control for unobserved heterogeneity.

In the assessment of the third item in the risk of bias table (i.e., confounding) it was not considered vital whether unobserved heterogeneity was controlled for in the manner described above. Instead, the assessment paid particular attention to whether the authors of the primary studies had justified their choice of method in a convincing manner, by discussing the assumption(s) leading to identification (the assumption(s) that make it possible to identify the counterfactual, see Section 3.4.3).

Moreover, we assessed whether the observable confounding factors, defined in the protocol, had been considered. Other confounding factors considered in the primary studies, as mentioned above typically individual labour market history, family background, and disability have been assessed. The judgment is reflected in the risk of bias score.

### **3.4.5 Unit of Analysis Issues**

To account for possible statistical dependencies, we examined a number of issues: whether individuals were randomised in groups (i.e. cluster randomised trials), whether individuals had undergone multiple interventions, whether there were multiple treatment groups, and whether several studies were based on the same data source.

#### **Multiple Intervention Groups**

There were no studies with multiple intervention or control groups (with different individuals).

#### **Multiple Interventions per Individual**

There were no studies with multiple interventions per individual.

#### **Multiple Studies using the Same Sample of Data**

Several studies used the same or overlapping sample of data, i.e., the studies used administrative register data from the same country covering the same time period or overlapping time periods. For example, in the case of Slovenia, the administrative registers provide complete coverage;<sup>17</sup> that is, all registered unemployed in the selected period are included in the administrative registers. We identified two primary studies analyzing a 6% random sample from these administrative registers in Slovenia covering the years 1997-1999 and one primary study analyzing a random sample covering the years 1997-2001. The data used in these primary studies were thus representative of the same population of

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<sup>16</sup> Only some fairly innocuous regularity assumptions and normalisations are needed (of course, in addition to proportionality assumptions on the hazard function).

<sup>17</sup> Complete coverage of administrative registers applies to other countries as well.

unemployed at the same time (or there was overlap), and the effect estimates from these studies were not independent. We reviewed all such studies, but in the meta-analysis we only included one estimate of the benefit exhaustion effect from each sample of data. The choice of which estimate to include was based on our quality assessment of the studies. We chose the estimate from the study that we judged to have the lowest risk of bias, and the judgment paid particular attention to the confounding item. In case of equal scoring on the confounding item, we based the choice on the incomplete data item.

### **Multiple Time Points**

It was possible to group the time points as follows: the week or month of exhaustion,<sup>18</sup> one month before exhaustion, two months before exhaustion, 2-4 months before exhaustion, and one month after exhaustion. If a study provided multiple estimates within a time period, we calculated and used a synthetic (average) effect size to avoid dependence problems. This method provides an unbiased estimate of the mean effect size parameter but overestimates the standard error (Hedges, 2007). Each time point was analysed in a separate meta-analysis.

### **Cluster Randomisation**

No studies using cluster randomisation were found.

### **Other Sources of Dependency**

Where studies reported separate effect estimates, for example separated by gender, a synthetic (average) effect size was calculated and used to avoid dependence problems. This method provides an unbiased estimate of the mean effect size parameter but overestimates the standard error. Also, tests of heterogeneity when synthetic effect sizes are included are rejected less often than nominal (Hedges, 2007).

### **3.4.6 Dealing with Missing Data and Incomplete Data**

Missing data and censoring were assessed in the included studies. For studies using questionnaire data, a sensitivity analysis was performed to assess potential bias. For studies in which the censoring level was high (more than 25%) or the level was not reported, a sensitivity analysis was performed to assess potential bias in the analysis. The extent to which the results might be biased by a high censoring level was included in the sensitivity analysis (see Section 4.4.4).

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<sup>18</sup> Some studies report the time of exhaustion on a weekly basis, and some studies report the time of exhaustion on a monthly basis.

### **3.4.7 Assessment of Heterogeneity**

Statistically significant heterogeneity among primary studies was assessed with a Chi-squared (Q) test and I-squared (Higgins, Thompson, Deeks, & Altman, 2003). A significant Q ( $p < .05$ ) and I-squared of at least 50% were considered to indicate statistical heterogeneity.

### **3.4.8 Assessment of Publication Bias**

We used funnel plots to identify possible publication bias.

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## **3.5 DATA SYNTHESIS**

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Studies that were coded with a very high risk of bias (i.e., 5 on the risk of bias scale) were not included in the data synthesis.

In the majority of studies, results were measured at multiple time points. The outcome at each time point was analysed in a separate meta-analysis with other comparable studies taking measures at a similar time point. As outlined in Section 3.4.5, it was possible to group outcomes as follows: 2-4 months before exhaustion, 2 months before exhaustion, 1 month before exhaustion, the month or week of exhaustion, and 1 month after exhaustion.

We carried out our meta-analyses using the point estimate of the hazard ratio. All analyses were inverse variance weighted using random effects statistical models that incorporate both the sampling variance and between study variance components into the study level weights. Random effects weighted mean effect sizes were calculated using 95% confidence intervals.

### **3.5.1 Subgroup and Moderator Analysis and Investigation of Heterogeneity**

We performed single factor subgroup analysis. The assessment of any difference between subgroups was based on 95% confidence intervals. No conclusions from subgroup analyses were drawn, and interpretation of relationships was cautious, as they were based on subdivision of studies and indirect comparisons.

### **3.5.2 Sensitivity Analysis**

Sensitivity analysis was used to evaluate whether the pooled effect sizes were robust across components of methodological quality. For methodological quality, we performed sensitivity analysis for the confounding, incomplete data, and selective reporting items of the risk of bias checklists, respectively. Sensitivity analysis was further used to examine the robustness of conclusions in relation to the quality of data (outcome measures based on weekly or monthly data and whether data were based on questionnaires or administrative registers).

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## 4 Results

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### 4.1 RESULTS OF THE SEARCH

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We ran the searches between the end of 2010 and during the first months of 2011. Two additional databases, Dissertations and Thesis and Social Care Online, were added and searched in June 2011.

The total number of potential relevant studies from “white literature,” “grey literature,” and the hand-search constituted 23,991 hits (white: 22,328; grey: 1,476; hand-search: 178). Hand-searching was done in two journals (see Section 3.3.3 for journals and dates). The results were screened by two individual screeners based on title and abstract.

In total, 454 hits (including results from the hand search) were retrieved for full text screening. The results were screened by two individual screeners. Of these 454 hits, 384 did not fulfill the screening requirements. One article was excluded because of a language barrier,<sup>19</sup> and four articles were unobtainable. No papers from hand-searching or the grey literature were included. See Section 4.2.2 for further details regarding excluded and unobtainable studies.

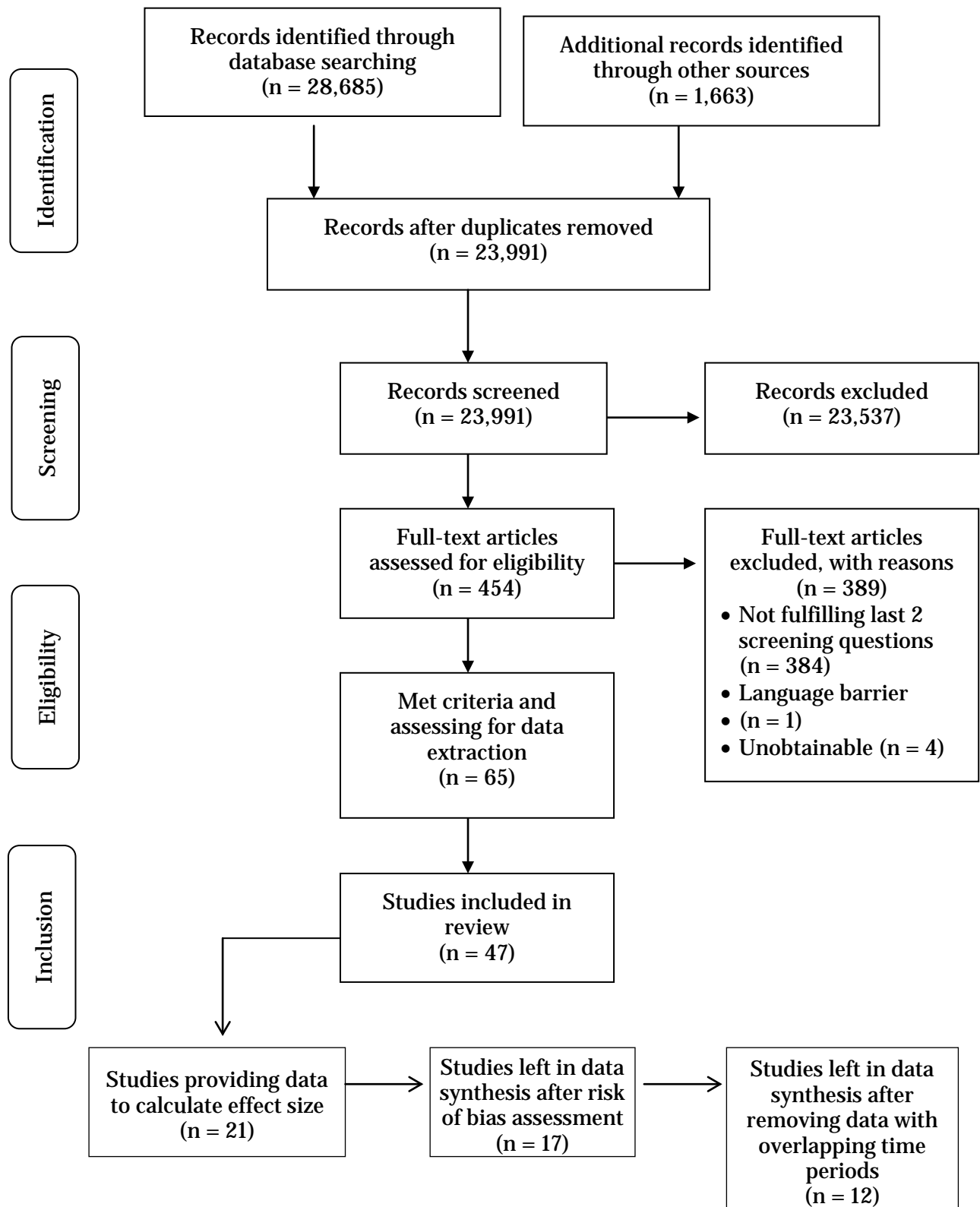
A total of 47 studies, consisting of 65 papers, met the inclusion criteria and were vetted by the review authors.

Figure 4.1 illustrates the flow chart for the literature search and screening. Furthermore, Figure 4.1 shows the division of studies used in the data synthesis and studies that could not be included in the data synthesis.

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<sup>19</sup> The abstract was written in English but the article was written in Chinese. None of the review authors or members of the review team understand Chinese.

**FIGURE 4.1 FLOW CHART FOR THE LITERATURE SEARCH**



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## 4.2 DESCRIPTION OF THE STUDIES

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### 4.2.1 Studies Included in the Systematic Review

The search resulted in a final selection of 47 studies that met the inclusion criteria for this review. Of the 47 studies that met the inclusion criteria, 26 did not provide data that permitted the calculation of an effect size. Of the remaining 21 studies, 4 studies were coded with a very high risk of bias (5 on the risk of bias scale) and were therefore not used in the data synthesis. An additional five studies could not be used in the data synthesis due to overlapping data samples (i.e., the studies used administrative register data from the same country covering the same time period or overlapping time periods; see Section 3.4.5 for this methodological issue). These studies analysed benefit exhaustion in Spain, Slovenia, Germany, and the US. After these exclusions, 12 studies remained and were included in the data synthesis.

In Table 4.1, we show the total number of studies that met the inclusion criteria for this review. The first column shows the total number of studies grouped by country of origin. The second column shows the number of these studies that provided enough data to calculate an effect estimate, in total 21 studies. The third column gives the number of studies that were coded with very high risk of bias. The fourth column gives the number of studies that were excluded from the data synthesis due to overlapping samples. The last column gives the total number of studies used in the data synthesis, in total 12.

The choice of which study to use in the data synthesis among the studies with overlapping data samples was based on our quality assessment of the studies. The citations for the 21 studies that provided effect size estimates can be found in Section 8.1. In cases of overlap, the study that we judged to have the least risk of bias was chosen.<sup>20</sup> In the case of Germany and the US, there were differences in our judgments of confounding, so the studies with the least risk of bias were chosen. In the case of Spain, the two studies were judged to have the same risk of bias due to confounding, so the study we judged to have the least risk of bias due to incomplete outcome data was chosen. In the case of Slovenia, the three studies were judged to have the same risk of bias in both confounding and incomplete outcome data. Two of the studies provided only an estimate of the exhaustion effect in the month of exhaustion, so we chose the one study that provided an estimate of the exhaustion effect in the month of exhaustion as well as one month prior to exhaustion and one month after exhaustion. In total, we were left with effect estimates for 12 unique populations. These are listed in Section 9.1.

The characteristics of the 12 studies that were used in the data synthesis are shown in Table 4.2. A description of the individual studies is provided as a supplementary document.

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<sup>20</sup> The judgment paid particular attention to the confounding judgment, and in case of equal scoring, the choice was based on the incomplete data scoring.

**TABLE 4.1**      **NUMBER OF INCLUDED STUDIES**

Country	Total	Reduction due to			
		Provide effect estimate	Too high risk of bias	Overlap of data samples <sup>1</sup>	Used in data synthesis
Spain	5	3	0	1	2
Netherlands	1	0	0	0	0
UK	2	0	0	0	0
Canada	3	2	1	0	1
Slovenia	5	4	0	2	2
Poland	3	2	1	0	1
Finland	1	0	0	0	0
Hungary	2	0	0	0	0
Estonia	1	0	0	0	0
Germany	4	3	1	1	1
Norway	2	0	0	0	0
Sweden	2	1	1	0	0
Slovakia	1	0	0	0	0
Switzerland	1	0	0	0	0
US	10	3	0	1	2
Austria	1	1	0	0	1
Czech Republic	1	1	0	0	1
Portugal	1	1	0	0	1
Belgium	1	0	0	0	0
<b>Total</b>	<b>47</b>	<b>21</b>	<b>4</b>	<b>5</b>	<b>12</b>

*Note: The reduction due to too high risk of bias preceded the reduction due to overlap of data sample.*

*<sup>1</sup>The data samples used are representative for the same population at a given time (see section 3.4.5 for this methodological issue).*

The studies were mainly from European countries. Two studies were from the US and one study was from Canada. Nine studies analyzed data from the 1990s. One of these nine studies used data from 1981 to 2001. Two studies used data from the 1970s and 1980s, and one study analyzed the period 2000-2007. The outcome measures were based on weekly data in five studies, and the remaining seven were based on monthly data. Data were drawn mainly from administrative registers. Some primary studies report sample size by number of unemployment spells rather than number of individuals. The number of unemployment spells will be different from the number of individuals only if data providing multiple spells is used. Two studies used in the meta-analyses used multiple unemployment spells to control for unobserved heterogeneity. The sample sizes were generally large; all but two studies had sample sizes of more than 2,500 unemployment spells.

All studies analysed the exhaustion of unemployment insurance benefits. None of the studies reported whether compulsory labour market activation was part of the unemployment system. Most studies did not report on the availability of alternative benefits; those that did only reported that means tested unemployment assistance was available. Only one study restricted the analysis to a specific age group (18-25 years), and none restricted the analysis to a specific educational level. Three studies included only males in their analysis, and five studies provided separate effect estimates by gender.

The majority of studies did not report the labour market conditions – only four studies reported the unemployment level. All studies reported the maximum entitlement but in almost all studies there was a high degree of variation in *individual* entitlement, as individual entitlement in most countries depends on work experience and/or age. All but one study used this individual variation in entitlement as part of the identification strategy.

The relevant variation in the individual entitlement for the countries and time periods of the studies included in the data synthesis are shown in Table 4.3, and a more comprehensive description of the existing rules applicable in the respective countries and time periods is given in Appendix 10.2. The individual entitlement investigated within a country varies considerably. In the case of Slovenia, it also varies considerably between time periods.

#### **4.2.2 Excluded Studies**

In addition to the 47 studies that met the inclusion criteria for this review, several studies at first sight appeared relevant for the review but did not end up meeting our criteria. Four studies examined a mix of destinations after unemployment, which could not be separated, and two further studies examined an intervention that was a mix of active labour market programmes and exhaustion of benefits. Two studies analysed the effect on the overall unemployment duration and not the effect for time of exhaustion. Furthermore, one study examined an active labour market programme instead of the exhaustion of benefits, and one study examined temporary benefit exhaustion. None of these studies fulfilled our inclusion criteria and were therefore not included in the final review. One study was excluded due to language barrier. These studies are listed in Appendix 8.2.

#### **4.2.3 Studies Awaiting Classification**

Due to long delivery time and unknown information, two references were not obtained in full text despite repeated attempts to locate them. The known information for these two studies is listed in Section 9.3.

**TABLE 4.2 CHARACTERISTICS OF STUDIES USED IN THE DATA SYNTHESIS**

Study characteristics			
Country		Sample size	
European countries	9	Studies reporting number of individuals	Range: 756-30,337 Average: 7403
USA and Canada	3	Studies reporting number of spells	Range: 7,348-329,347 Average: 113,377
Analysis period		Type of data	
1970s and 1980s	2	Administrative registers	8
1990s	9	Questionnaire	3
2000s	1	Combination	1
Time interval the outcome measure is based on		Entitlement	
Weekly	5	Maximum entitlement	Range: 6-32 months Average: 16.4 months
Monthly	7	Range of individual variation within a study	Range: 1-26 months Average: 14 months
Type of unemployment benefit		Compulsory activation a part of the system	
Unemployment insurance benefits	12	Not reported	12
Availability of alternative benefits		Considered specific age group or education level	
Reported means tested social assistance	9	Specific age group	1
Not reported	3	Specific education level	0
Considered specific gender or separated by gender		Labor market conditions	
Considered only males	3	Reported unemployment percent	4
Separated estimates by gender	5		

*Note: One study used data from 1981 to 2001. This study is counted up in the 1990s category.*

**TABLE 4.3 RANGE OF INDIVIDUAL ENTITLEMENT IN THE COUNTRIES AND TIME PERIODS OF THE STUDIES USED IN THE DATA SYNTHESIS**

Country	Individual entitlement <sup>a</sup>	Eligibility	Period
Spain	3-24 months	Depends on work experience	1987-1992
Spain	4-24 months	Depends on work experience	2000-2007
Austria	20-30 weeks	Depends on work experience	1981-2001
Portugal	10-30 months	Depends on age	1992-1997
Czech Republic	6 months	Not available	1992-1994
Canada	1-50 weeks	Depends on work experience	1976-1984, reform in 1977
Slovenia	3-9 months	Depends on work experience	1997-1999, reform in 1998
Slovenia	3-24 months	Depends on work experience	1990-1992, reform in 1991
Poland	12 months	Not available	1990-1993, reform in 1992
Germany	6-32 months	Depends on work experience and age	1995-2003, reform in 1997
USA <sup>b</sup>	1-30 weeks	Depends on work experience	1996-1998
USA <sup>b</sup>	1-45 weeks	Depends on work experience	1979-1981, extension in 1980

*a: Individual entitlement of the unemployed included in the analysis of the primary study*

*b: No lower level of entitlement is stated*

### 4.3 RISK OF BIAS IN INCLUDED STUDIES

The risk of bias coding for each of the 21 studies from which we could extract an effect estimate is shown in a supplementary document. Because all included studies used non-randomised designs, they were all judged to have a high risk of bias on the sequence generation item and the allocation concealment item. We did not judge the studies on the blinding item. This review focuses on the incentive effect of benefit exhaustion, i.e., the exit rate out of unemployment into employment *prior* to exhaustion of unemployment benefits. The treated group has to know they are treated in order to react to it; therefore, it is not relevant to consider blinding of the participants. Furthermore, the nature of the outcome, exit into employment, is objective and obtained from administrative registers or questionnaires, which were not collected with the aim of analysing unemployment benefit exhaustion.

The central problem associated with risk of bias in the primary studies included in this review is identification of the incentive effect; this judgement is reflected in the score on the confounding item of our risk of bias instrument. Using sources of individual variation in

entitlement such as age and work experience carries the risk of being correlated with personal characteristics, which may have an impact on the exit rate. Variation in entitlement across individuals uncorrelated with work experience or age may be achieved through legislative changes of the maximum entitlement. Studies using legislative changes are generally better off with respect to risk of bias in terms of confounding.

Ten studies used both legislative changes and individual variation in entitlement as their source of identification, six studies used only individual variation in entitlement, one study used unemployed non-receivers of unemployment insurance benefits as comparison, and four studies were given a score of 5 on the confounding item. None of the studies had an *a priori* protocol or an *a priori* analysis plan.

A summary of the risk of bias associated with confounding, incomplete data, and selective reporting for the 21 studies from which it was possible to extract an effect estimate is shown in Table 4.4. Four studies were given a score of 5 on the confounding item, corresponding to a risk of bias so high that the findings should not be considered in the data synthesis. For these four studies, we did not find it relevant to judge on the selective reporting item because of their already high risk of bias. None of the other studies were given a score of 5 on the incomplete data and selective reporting items.

#### 4.4 EFFECTS OF THE INTERVENTION

In order to carry out a meta-analysis, every study must have a comparable effect size. All 12 studies used in the meta-analyses reported hazard ratios. The approach shared by the majority (9) of the 12 studies was to use indicator variables for the number of months or weeks until benefit exhaustion; the remaining studies (3) used a linear spline function. The comparison condition was benefit exhaustion that was not immediate.

**TABLE 4.4 RISK OF BIAS - DISTRIBUTION OF THE 21 STUDIES REPORTING AN EFFECT SIZE**

Judgment	Risk of Bias Item		
	Confounding	Incomplete data	Selective reporting
1	1	0	7
2	7	7	1
3	7	7	7
4	2	7	2
5	4	0	0
Total number of studies	21	21	17

*Notes: The judgment is based on a 5-point scale, where 1 indicates low risk of bias and 5 indicates high risk of bias. Four studies scored 5 on the confounding item and were thereby not included in the data synthesis. Therefore, it was not relevant to judge on the selective reporting item for these four studies.*

In the majority of studies, the results were measured at multiple time points. The effect estimates at different time points were independent, as the effect sizes were measured as the proportional impact on the hazard rate which is the event rate (in the present context, the event is finding a job) at time  $t$  *conditional* on survival (staying unemployed) until time  $t$  or later.

The outcome at each time point was analysed in a separate meta-analysis with other comparable studies taking measures at similar time points. Two studies reported separate effect measures based on number of weeks or fortnights. For these two studies, the average effect size was calculated and used to avoid dependence problems.

Five studies reported separate effect measures for men and women. Of these, two studies further reported separate effect measures for two different regions, and one study reported separate effect measures for recall and exit to new job. For these five studies, the average effect size was also calculated and used to avoid dependence problems.

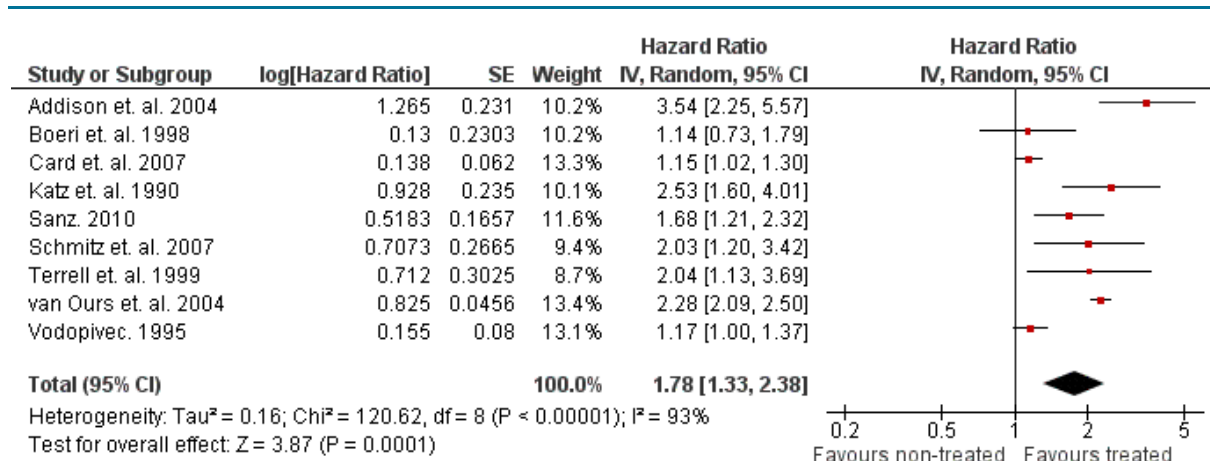
We rely on results of random effects models. We carried out our meta-analyses using the point estimate of the hazard ratio. A hazard ratio greater than 1 favours the treated group, which means that the conditional exit rate from unemployment into employment is higher for persons who approach exhaustion than for persons who do not approach exhaustion.

#### 4.4.1 Primary Outcome Results

##### **The Month or Week of Exhaustion**

Nine studies provided effect estimates in the month or week of exhaustion. All nine studies reported results that indicated a positive exhaustion effect; only two of the study-level effects were statistically nonsignificant. Pooled results showed a significant exhaustion effect. The random effects weighted mean hazard ratio was 1.78 (95% CI 1.33 to 2.38,  $p < .0001$ ); however, there was significant heterogeneity of effects among studies ( $\tau^2 = 0.16$ ,  $Q = 120.62$ ,  $df = 8$ ,  $p < .00001$ ). The forest plot is displayed in Figure 4.2.

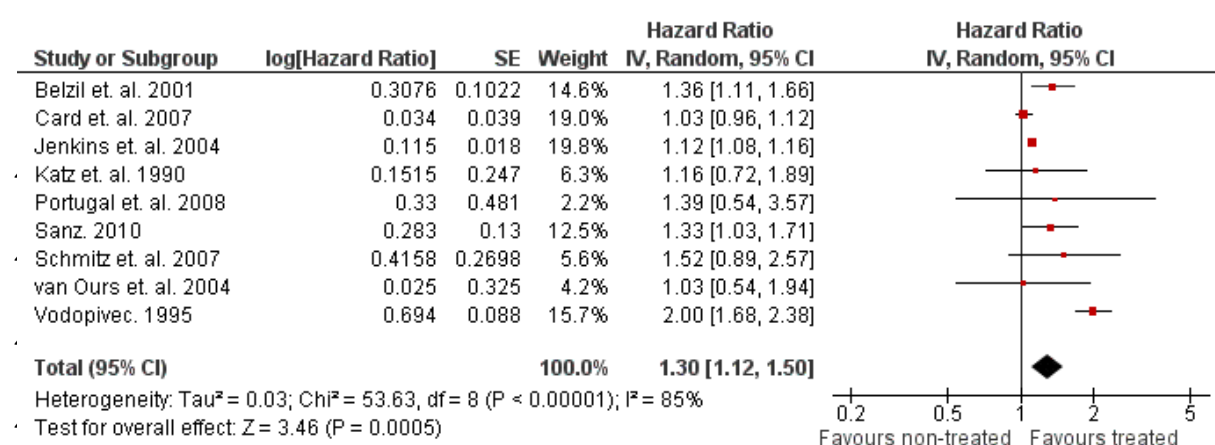
**FIGURE 4.2 FOREST PLOT, THE WEEK/MONTH OF EXHAUSTION**



## One Month Before Exhaustion

Nine studies provided effect estimates one month before benefit exhaustion. All nine studies reported results that indicated a positive exhaustion effect. Five of the study-level effects were statistically nonsignificant, but pooled results showed a significant exhaustion effect. The random effects weighted mean hazard ratio was 1.30 (95% CI 1.12 to 1.50,  $p < .0001$ ); however, there was significant heterogeneity of effects among studies ( $\tau^2 = 0.03$ ,  $Q = 53.63$ ,  $df = 8$ ,  $p < .00001$ ). The forest plot is displayed in Figure 4.3.

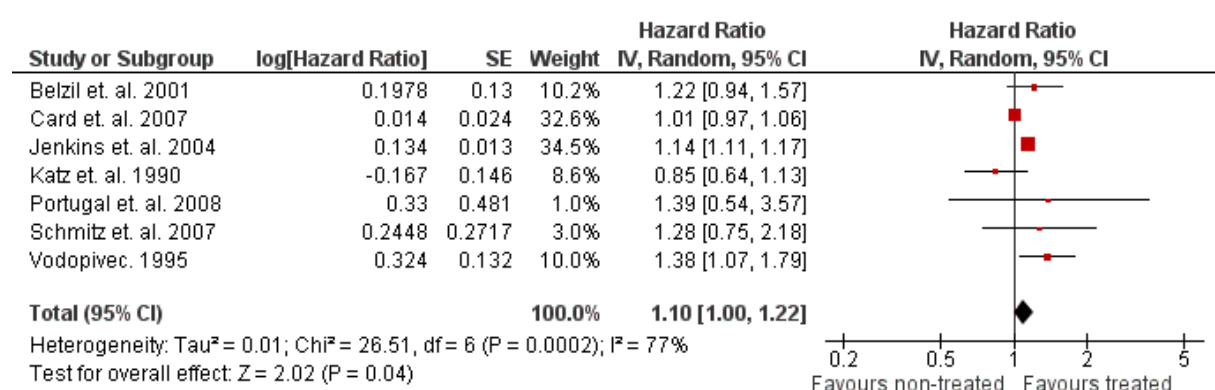
**FIGURE 4.3 FOREST PLOT, 1 MONTH BEFORE EXHAUSTION**



## Two Months Before Exhaustion

Seven studies provided effect estimates two months before benefit exhaustion. A nonsignificant negative threat effect was found in one study, while six studies reported results that indicated a positive exhaustion effect. Only one of the individual study-level effects was statistically significant, and in the positive direction. Pooled results showed a significant exhaustion effect. The random effects weighted mean hazard ratio was 1.10 (95%CI 1.00 to 1.22,  $p = .04$ ). There was significant heterogeneity of effects among studies ( $\tau^2 = 0.01$ ,  $Q = 26.51$ ,  $df = 6$ ,  $p = .0002$ ). The forest plot is displayed in Figure 4.4.

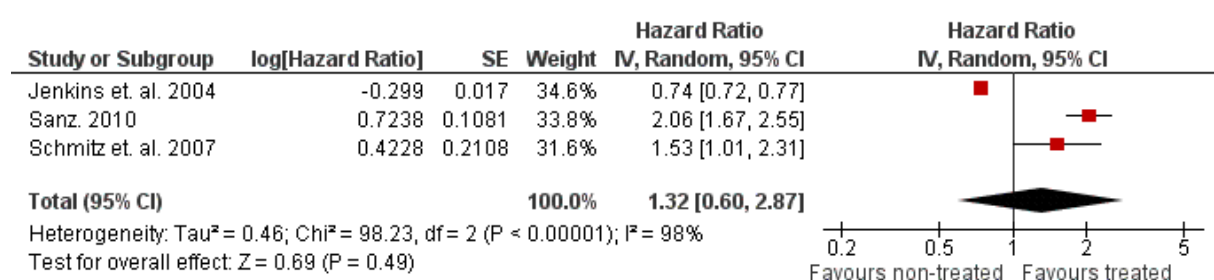
**FIGURE 4.4 FOREST PLOT, 2 MONTHS BEFORE EXHAUSTION**



### Between Two and Four Months Before Exhaustion

Three studies provided effect estimates between two and four months before benefit exhaustion. A statistically significant negative effect was found in Jenkins and Garcia-Serrano (2004), while two studies reported results that indicated a statistically significant positive exhaustion effect. Pooled results did not show a significant exhaustion effect. The random effects weighted mean hazard ratio was 1.32 (95%CI 0.60 to 2.87,  $p=.49$ ). There was significant heterogeneity of effects among studies ( $\tau^2=0.46$ ,  $Q=98.23$ ,  $df=2$ ,  $p<.00001$ ). The forest plot is displayed in Figure 4.5.

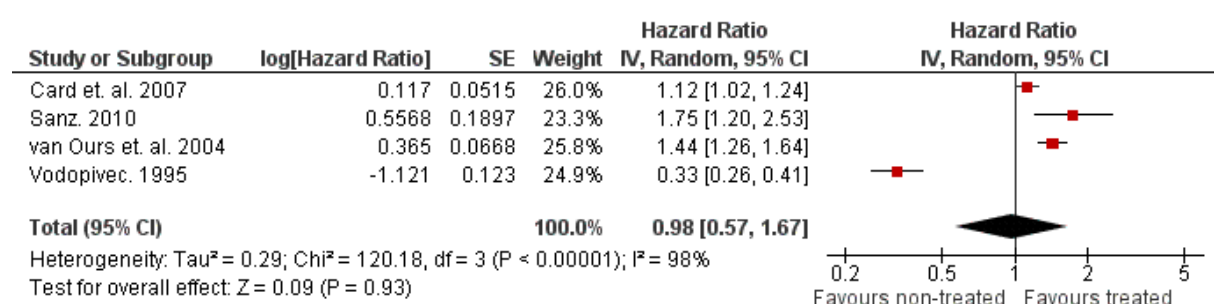
**FIGURE 4.5 FOREST PLOT, BETWEEN 2 AND 4 MONTHS BEFORE EXHAUSTION**



### One Month After Exhaustion

Four studies provided effect estimates one month after benefit exhaustion. A statistically significant negative effect was found in one study, while three studies reported results that indicated a statistically significant positive exhaustion effect. Pooled results did not show a significant exhaustion effect. The random effects weighted mean hazard ratio was 0.98 (95%CI 0.57 to 1.67,  $p=.93$ ). There was significant heterogeneity of effects among studies ( $\tau^2=0.29$ ,  $Q=120.18$ ,  $df=3$ ,  $p<.00001$ ). The forest plot is displayed in Figure 4.6.

**FIGURE 4.6 FOREST PLOT, 1 MONTH AFTER EXHAUSTION**



## **Summary of Primary Outcome Results**

The data synthesis for the primary outcome, the impact of exhaustion of unemployment benefit, revealed a significant exhaustion effect in the month/week of benefit exhaustion, one month before exhaustion and two months before exhaustion. No significant effects were found more than two months before exhaustion or one month after benefits have expired.

### **4.4.2 Secondary Outcome Results**

In addition to the primary outcome, exit rate from unemployment into employment, we planned to consider secondary outcomes in terms of the impact of exhaustion of benefits on the exit rate of the re-employment job and on the re-employment wage. No studies provided data on the re-employment wage. However, estimates of the relative exit rate from the re-employment job, i.e., the re-employment hazard ratio, were provided. We included this measure in the analysis of secondary outcomes. A higher exit rate from the re-employment job indicates that the exhaustion of benefits forces unemployed individuals to find jobs that do not match their qualifications and, therefore, they may return to unemployment quickly. One study, Gaure, Røed, and Westlie (2008), analysed both re-employment hazards and monthly earnings, but the study did not provide data that permitted the calculation of an effect size. Four studies provided an effect size in the form of re-employment hazard ratios, of which two were from Slovenia analysing the same time period. The choice of which study to use in the data synthesis could not be based on our quality assessment. The two studies were almost identical, using the same data, method of estimation, and both used legislative changes and individual variation in entitlement due to labour market history to identify the effect. The only difference was that in van Ours and Vodopivec (2006), work experience was included (among other variables) as a confounding factor in the analysis, and this factor was not controlled for in Boone and van Ours (2009). We therefore chose to use van Ours and Vodopivec (2006) in the data synthesis. The effect sizes from the two studies do not differ much, and a sensitivity analysis shows that the pooled effect size from using the one or the other study does not differ. Only the lower limit of the 95% confidence interval of the pooled effect size differs marginally (by 0.01).

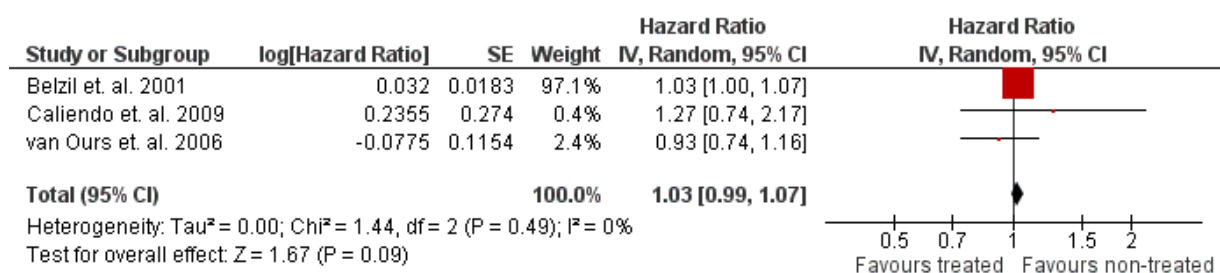
Of the three studies used in the meta-analysis, two studies reported hazard ratios using indicator variables for the number of months or weeks until benefit exhaustion, and one study used a linear spline function. It was not possible to analyse the exit rate of jobs found at different time points before benefit exhaustion. Two studies reported separate effect sizes for men and women, and one study reported separate effect sizes for permanent and temporary jobs. For all studies, a synthetic (the average) effect size was calculated and used to avoid dependence problems.

We carried out our meta-analysis using the point estimate of the hazard ratio. A hazard ratio of less than 1 indicates that treatment groups are favoured. That is, the conditional exit rate from the re-employment job into unemployment is lower for persons who found the job

when they approached benefit exhaustion than for persons who found the job when not approaching benefit exhaustion.

All three studies reported individual non-significant exhaustion effects. Pooled results were also non-significant. The random effects weighted mean hazard ratio was 1.03 (95%CI 0.99 to 1.07,  $p=.09$ ). There was no evidence of significant heterogeneity of effects among studies ( $\tau^2=0.00$ ,  $Q= 1.44$ ,  $df=2$ ,  $p=.49$ ). The forest plot is displayed in Figure 4.7.

**FIGURE 4.7 FOREST PLOT, EXIT FROM RE-EMPLOYMENT JOB**



#### 4.4.3 Moderator Analysis and Investigation of Heterogeneity

The included studies differed in terms of their sample characteristics, comparison conditions, and methodology. With between three and nine studies in a single meta-analysis, the statistical power to detect heterogeneity of effects was quite low; nevertheless, evidence of statistical heterogeneity was found.

With the aim of explaining observed heterogeneity, we planned to investigate the following factors: study-level summaries of participant characteristics (e.g. specific age group, gender or educational level), labour market conditions (good/bad), type of unemployment benefit (UI or SA/UA), maximum entitlement (less than one year, between one and two years, more than two years), whether alternative benefits were available, and if compulsory activation was part of the system.

Among the studies used in the data synthesis, only one study restricted its analysis to a specific age group (18-25 years), and none restricted their analyses to a specific educational level. No separate estimates for young/old or low/high educational level were available. The majority of studies did not report the labour market conditions and, among those that did, there was hardly any variation in this covariate. All studies reported the maximum entitlement, and almost all studies reported the *individual* entitlement as well (see Table 4.3). As all but one study used *individual* variation in entitlement as part of the identification strategy, and effect estimates were not provided separated by individual entitlement, it was unfortunately not possible to analyse the impact of entitlement on the between-study variation in exhaustion effects. Even a comparison of effect sizes by countries with low/high maximum entitlement does not provide the ‘right’ guidance as to whether a larger effect size was found for lower maximum entitlements. For example, a comparison of the effect size for

the Czech Republic, with a maximum entitlement of 6 months, and the effect size for Spain, with a maximum entitlement of 24 months, would be misleading. The *individual* entitlement in Spain in the period 1987-1992 was 3 months if tenure in the last 48 months was 6-12 months, it then increased in 3-month intervals for each incremental 6 months of tenure in the last 48 months up to a maximum of 24 months with tenure more than 48 months. The effect size for Spain is thus an average of unemployment benefit exhaustion effects at various *individual* entitlements. Thus, a country comparison of the reported effect sizes is not a comparison of high (in Spain) versus low (in Czech Republic) maximum entitlement as one might think.

Concerning the three covariates: type of unemployment benefit, availability of alternative benefits, and compulsory activation, they were either not reported or there was no variation in the covariate.

Five studies provided separate effect estimates by gender. Although these five studies comprise a subset of the included studies, we chose to investigate the impact of gender using effect estimates separated by gender within studies. In general, the strength of inference regarding differences in treatment effects using subsets of studies is controversial. However, making inferences about different effect sizes among subgroups on the basis of between-study differences entails a higher risk compared to inferences made on the basis of within study differences (Oxman & Guyatt, 1992).

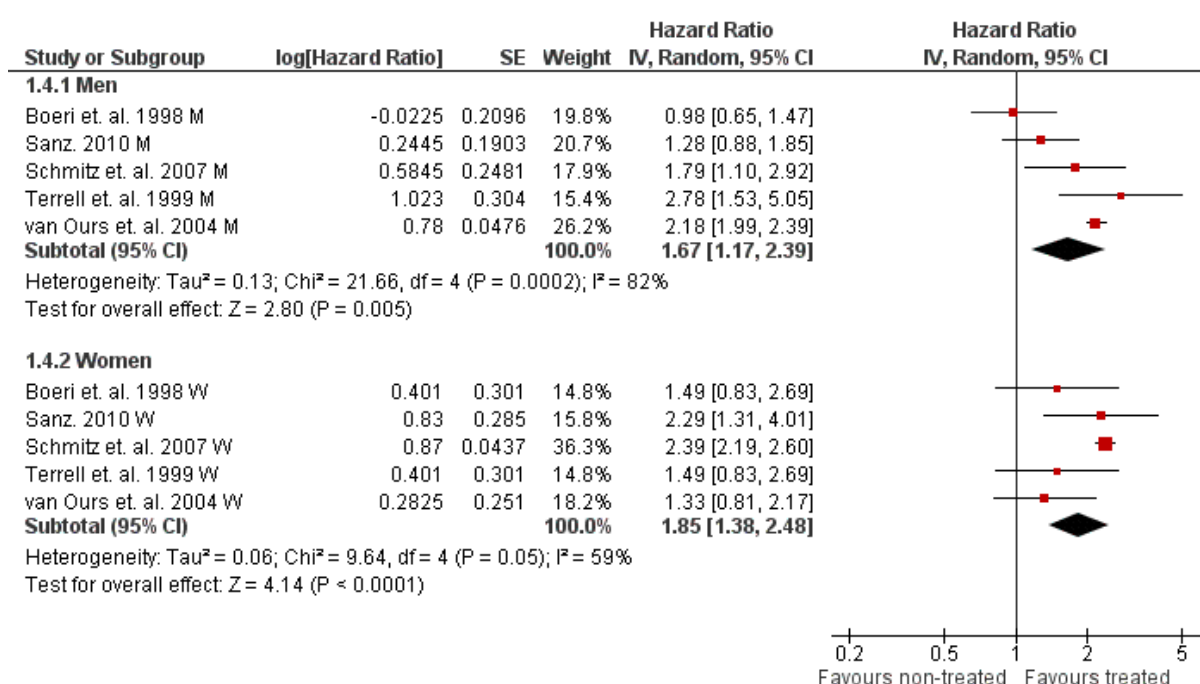
Subgroup analysis was therefore performed using effect estimates separated by gender from the five studies where separate estimates were available. We have drawn no overall conclusion because the analysis is based on a subset of the studies used in the data synthesis. The assessment of any difference between the subgroups is based on 95% confidence intervals and interpretation of relationships is cautious.

Of the five studies that reported separate effect measures for men and women, two studies further reported separate effect measures for two different regions, and one study reported separate effect measures for recall and exit to new job. For these two studies, a synthetic (the average) effect size was calculated and used to avoid dependence problems.

### **The Month or Week of Exhaustion**

The forest plot for the five studies reporting gender breakdowns is displayed in Figure 4.8. Pooled results for both subgroups showed significantly positive exhaustion effects; HR=1.67 (95% CI 1.17 to 2.39) for men and HR=1.85 (95% CI 1.38 to 2.48) for women. There was significant heterogeneity of effects among studies in both subgroups ( $\tau^2=0.13$ ,  $Q=21.66$ ,  $df=4$ ,  $p=.0002$  for men and  $\tau^2=0.06$ ,  $Q=9.64$ ,  $df=4$ ,  $p=.05$  for women). The confidence intervals for the subgroups differed only marginally. There was no evidence to support the hypothesis that the exhaustion effect in the week or month of exhaustion differs by gender.

**FIGURE 4.8 FOREST PLOT, SUBGROUP – THE WEEK/MONTH OF EXHAUSTION**

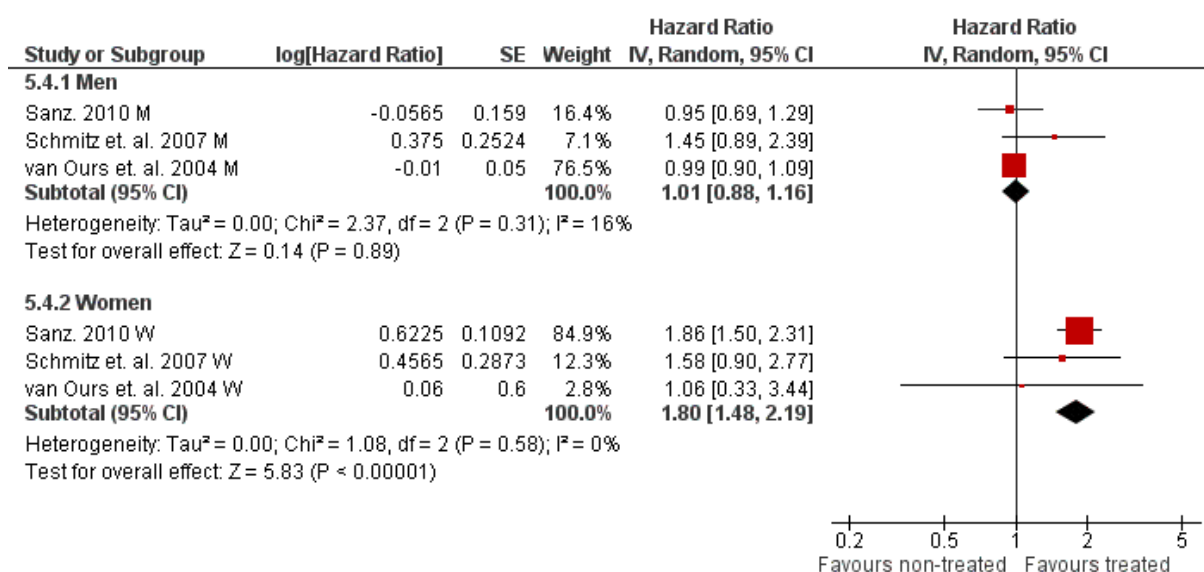


### One Month Before Exhaustion

Three studies reported estimates separated by gender for the one month time period. Pooled results for men showed a nonsignificant exhaustion effect, whereas pooled results for women showed a significant positive exhaustion effect. The forest plot is displayed in Figure 4.9.

There was no significant heterogeneity of effects among studies in either of the subgroups ( $\tau^2=0.00$ ,  $Q= 2.37$ ,  $\text{df}=2$ ,  $p=.31$  for men and  $\tau^2=0.00$ ,  $Q= 1.08$ ,  $\text{df}=2$ ,  $p=.58$  for women). The confidence intervals of the subgroups did not overlap (95% CI 0.88 to 1.16 for men and 95% CI 1.48 to 2.19 for women). However, making inferences about different effect sizes among subgroups entails a higher risk when the difference is not consistent within the studies (Oxman & Guyatt, 1992). Only in the Sanz (2010) study was there a clear difference between men and women. The effect estimates in both the Schmitz and Steiner (2007) and the van Ours and Vodopivec (2004) studies were nonsignificant for both men and women and the 95% confidence interval for women included the 95% confidence interval for men in the van Ours and Vodopivec (2004) study, whereas the 95% confidence interval of men included the 95% confidence interval for women in the Schmitz and Steiner (2007) study. There is no evidence to support the hypothesis that the exhaustion effect one month before exhaustion differs by gender.

**FIGURE 4.9 FOREST PLOT, SUBGROUP – 1 MONTH BEFORE EXHAUSTION**



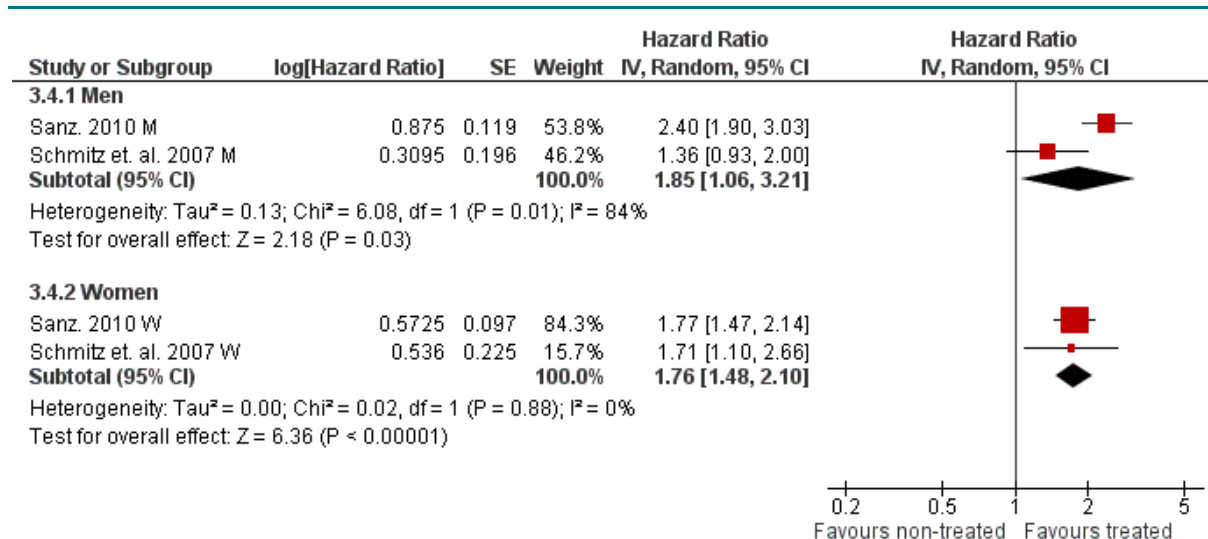
### **Two Months Before Exhaustion**

No effect estimates separated by gender were available.

### **Between Two and Four Months Before Exhaustion**

Two studies reported estimates separated by gender for the 2-4 month time period. Pooled results for both subgroups showed a positive exhaustion effect;  $HR=1.85$  for men and  $HR=1.76$  for women. There was significant heterogeneity of effects among studies in the subgroup of men but no significant heterogeneity of effects among studies in the subgroup of women ( $\tau^2=0.13$ ,  $Q= 6.08$ ,  $df=1$ ,  $p=.01$  for men and  $\tau^2=0.00$ ,  $Q= 0.02$ ,  $df=1$ ,  $p=.88$  for women). The confidence interval of the subgroup of men was wide and inclusive of the confidence interval of the subgroup of women (95% CI 1.06 to 3.21 for men and 95% CI 1.48 to 2.10 for women). There is no evidence to support the hypothesis that the exhaustion effect between two and four months before exhaustion differs by gender. The forest plot is displayed in figure 4.10.

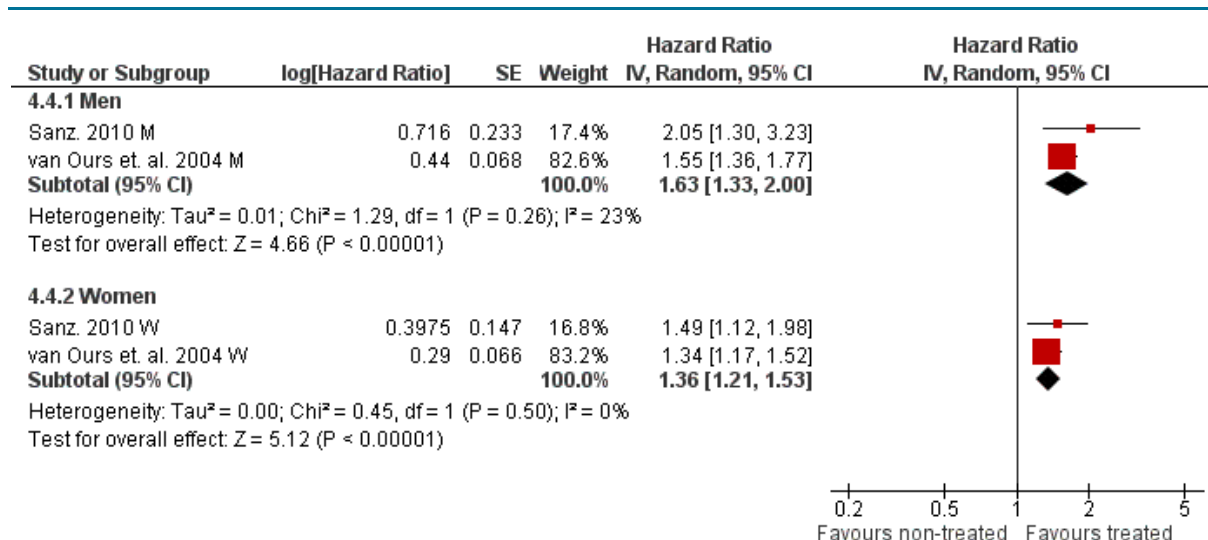
**FIGURE 4.10 FOREST PLOT, SUBGROUP – BETWEEN 2 AND 4 MONTHS BEFORE EXHAUSTION**



### One Month After Exhaustion

Two studies reported estimates separated by gender for the one-month period after benefit exhaustion. Pooled results for both subgroups showed a positive exhaustion effect;  $\text{HR}=1.63$  for men and  $\text{HR}=1.36$  for women. There was no significant heterogeneity of effects among either of the subgroups ( $\tau^2=0.01$ ,  $Q=1.29$ ,  $\text{df}=1$ ,  $p=.26$  for men and  $\tau^2=0.00$ ,  $Q=0.45$ ,  $\text{df}=1$ ,  $p=.50$  for women). The confidence intervals of the subgroups overlapped (95% CI 1.33 to 2.00 for men and 95% CI 1.21 to 1.53 for women). There is no evidence to support the hypothesis that the exhaustion effect one month after exhaustion differs by gender. The forest plot is displayed in Figure 4.11.

**FIGURE 4.101 FOREST PLOT, SUBGROUP – 1 MONTH AFTER EXHAUSTION**



#### 4.4.4 Sensitivity Analysis

Sensitivity analyses were planned to evaluate whether the pooled effect sizes were robust across study design and components of methodological quality. Due to the fact that we found no randomised controlled trials, we could not evaluate the impact of study design. For methodological quality, we carried out sensitivity analyses for the confounding, incomplete data, and selective reporting components of the risk of bias checklists, respectively. Two sets of sensitivity analyses were performed. First, we examined the robustness of conclusions when we excluded studies with risk of bias scores of 4 on confounding, incomplete data, or selective reporting. Second, we examined the robustness of our conclusions when we excluded studies with risk of bias scores of 3 or 4 on confounding, incomplete data, or selective reporting. Sensitivity analyses were further used to examine the robustness of conclusions in relation to the quality of data (outcome measures based on weekly or monthly data collection and whether data were derived from questionnaires or administrative registers).

Due to the small number of studies providing effect estimates for the time intervals 2-4 months before exhaustion and 1 month after exhaustion, we only performed sensitivity analyses for the following three time points: (1) the month or week of exhaustion, (2) one month before exhaustion, and (3) two months before exhaustion. The results are provided in Table 4.5 and displayed in forest plots in Section 11.1.

For the exhaustion effect in the month or week of exhaustion, there were no appreciable changes in the results either due to exclusion of studies where the effect estimates were based on weekly data or due to exclusion of studies using questionnaire data. There were no appreciable changes in the results either due to exclusion of studies with a high/unclear censoring level or due to exclusion of studies with scores of 4 on the confounding, incomplete data, or selective reporting components of the risk of bias checklists. Finally, there were no appreciable changes in the results due to exclusion of studies with scores of either 3 or 4 on the confounding, incomplete data, or selective reporting components of the risk of bias checklists. The overall conclusion that the hazard rate significantly increases in the month or week of exhaustion does not change. In fact, when we only included the studies with the highest scores on the confounding component of the risk of bias checklist (1 and 2), the pooled effect was higher and the confidence interval was narrower compared to inclusion of all studies.

**TABLE 4.5 SENSITIVITY ANALYSIS - RESULTS**

	Week/month of exhaustion	One month before	Two months before
	HR [CI 95%] (Number of studies)		
All studies	1.78 [1.33, 2.38] (9)	1.30 [1.12, 1.50] (9)	1.10 [1.00, 1.22] (7)
<i>Characteristics of studies excluded from the analysis:</i>			
	Means and Confidence Intervals with Studies Removed		
Estimates based on weekly data	1.60 [1.09, 2.34] (5)	1.39 [1.03, 1.87] (6)	1.15 [1.12, 1.18] (4)
Studies based on questionnaire data	1.60 [1.16, 2.21] (7)	1.28 [1.10, 1.50] (7)	1.10 [0.99, 1.22] (5)
Studies with confounding score of 4	1.60 [1.16, 2.21] (7)	-	-
Studies with confounding score of 4 or 3	2.18 [1.90, 2.50] (4)	1.13 [1.09, 1.17] (5)	1.06 [0.86, 1.32] (3)
Studies with incomplete data score of 4	1.98 [1.27, 3.08] (5)	1.14 [1.06, 1.24] (7)	1.08 [0.98, 1.19] (6)
Studies with incomplete data score of 4 or 3	1.70 [1.15, 2.50] (4)	1.15 [1.05, 1.25] (6)	1.07 [0.97, 1.19] (5)
Studies with selective reporting score of 4	2.03 [1.44, 2.86] (7)	1.14 [1.06, 1.22] (8)	1.08 [0.98, 1.19] (6)
Studies w/ selective reporting score of 4 or 3	1.84 [1.06, 3.19] (3)	1.10 [1.04, 1.15] (4)	1.05 [0.93, 1.18] (3)
Studies with high/unclear censoring level	1.98 [1.27, 3.08] (5)	1.15 [1.05, 1.25] (6)	1.07 [0.97, 1.19] (4)

*Notes: Some of the confidence intervals narrow considerably. For a technical comment see section 11.1. “-” indicates that no studies providing effect estimates at the time intervals in question scored 4 in the confounding item.*

For the effect estimate one month before exhaustion, there were no appreciable changes in the results either due to exclusion of studies where the effect estimates were based on weekly data or due to exclusion of studies using questionnaire data, or due to exclusion of studies with a high/unclear censoring level. There were no appreciable changes in the results either due to exclusion of studies with scores of 4 on the confounding, incomplete data, or selective reporting components of the risk of bias checklists, or due to exclusion of studies with a scores of either 3 or 4 on the confounding, incomplete data, or selective reporting components of the risk of bias checklists. The overall conclusion is that the hazard rate increases but less than at the week or month of exhaustion. The exhaustion effect estimate one month before exhaustion is only sensitive in the sense that the confidence intervals narrow when studies with high/unclear censoring levels or studies with high risk of bias are excluded.

For the effect estimate two months before exhaustion, there were no appreciable changes in the results either due to exclusion of studies where the effect estimates were based on questionnaire data or due to exclusion of studies with scores of 4 on the confounding, incomplete data, or selective reporting components of the risk of bias checklists. Finally, there were no appreciable changes in the results due to exclusion of studies with scores of

either 3 or 4 on the confounding, incomplete data, or selective reporting components of the risk of bias checklists. The exhaustion effect estimate two months before exhaustion is, however, sensitive to the exclusion of studies where the effect estimates were based on weekly data in the sense that the confidence interval narrows. The point estimate increases only slightly but is now very precisely estimated and significant within a 95% confidence interval.

#### **4.4.5 Publication bias**

We have assessed the possibility of publication bias for the three time intervals: (1) the week or month of exhaustion, (2) one month before exhaustion, and (3) two months before exhaustion. We did not consider the remaining time intervals (between two and four months before exhaustion and one month after exhaustion), as there were too few studies that provide effect estimates (three respectively four). We assessed the possibility of publication bias visually by examining funnel plots. The three funnel plots are displayed in Section 11.2. There are too few studies and not enough variation in the standard errors to assess whether the funnel plots are symmetric. However, there is no striking asymmetry visible in any of the funnel plots.

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## 5 Discussion

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### 5.1 SUMMARY OF THE MAIN RESULTS

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This review focused on the incentive effect of benefit exhaustion, i.e., the increase in the exit rate out of unemployment into employment of an unemployed person approaching benefit exhaustion. The available evidence supports the hypothesis that there is an incentive effect of approaching benefit exhaustion but only shortly prior to exhaustion and at the time of exhaustion. Furthermore, the available evidence suggests that this incentive effect diminishes as time increases both before and after the time of expiration.

We found a statistically significant exhaustion effect in the month/week of benefit exhaustion. The effect estimate translates into an increase of approximately 80% in the exit rate from unemployment into employment. The exit rate, also termed the hazard, is the rate within a short time interval at which the unemployed find a job conditional on staying unemployed. In other words, the probability of finding a job in that short time interval is the hazard rate. The time intervals used in primary studies varied; they were either one week or one month. This means that, for an unemployed person who has stayed unemployed until the month/week of benefit exhaustion, the probability of finding a job increases with 80% in that time interval solely due to the prospect of benefit exhaustion.

The increase in the hazard rate associated with benefit exhaustion starts even earlier. We found a statistically significant exhaustion effect one month before benefit exhaustion, though this effect was smaller than that found for the month/week of exhaustion. The effect estimate one month before benefit exhaustion translates into a 30% increase in the exit rate from unemployment into employment. A significant effect of a 10% increase in the exit rate from unemployment to employment was found two months prior to benefit exhaustion. No significant effects for benefit exhaustion were found more than two months before exhaustion, and no significant effects for benefit exhaustion were found after benefits had expired.

Interpretation of the results would ideally involve a measure of the average hazard rates for the comparison. However, none of the included studies reported such rates. Most of the studies, however, displayed figures of the average hazard rates over the entire unemployment period. Using these figures, we were able to estimate that the relevant hazard rates (depending on the reference used in the estimation which varies between studies) lie in

the interval 0.02-0.08, i.e., the conditional probability of finding a job in a short time interval (a week or a month depending on the unit of analysis in the primary studies) lies between 2% and 8%. Thus two months prior to exhaustion the hazard rates have increased with 10% to the interval 0.022-0.088, i.e., the conditional probability of finding a job in a short time interval has increased to 2.2-8.8% solely due to the prospect of benefit exhaustion. One month prior to exhaustion the hazard rates have increased with 30% so the conditional probability of finding a job in a short time interval has increased to 2.6-10.4%. In the week/month of exhaustion the hazard rates have increased with 80% so the conditional probability of finding a job in a short time interval has increased to 3.6-14.4% solely due to the prospect of benefit exhaustion.

It was possible to assess the impact of gender using a subset of studies. We found no evidence to support the hypothesis that the exhaustion effect differs by gender.

Concerning secondary outcomes, we analysed the effect of benefit exhaustion on the subsequent exit rate from the re-employment job. Only three studies could be used in this analysis. Based on the low number of studies the evidence was inconclusive with respect to support of the hypothesis that the prospect of benefit exhaustion has an impact on the quality of the job measured as the exit rate of re-employment.

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## **5.2 OVERALL COMPLETENESS AND APPLICABILITY OF EVIDENCE**

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In this review we included 12 studies in the data synthesis. In the light of the large number of studies meeting the inclusion criteria for this review ( $n=47$ ), this number is quite low. The reduction was caused by three factors. Unfortunately, 26 of the 47 studies did not report effect estimates or provide data that enabled the calculation of an effect size. Disregarding studies without a usable effect estimate, it was still possible to include 21 studies in the data synthesis. Of these 21 studies, four were judged to have a risk of bias of 5 on the point scale. In accordance with the protocol, we did not use these studies in the data synthesis. The score of 5 corresponds to a risk of bias so high that the findings of those studies are more likely to mislead than inform. Due to overlap of the samples used in several studies, the number of studies in the synthesis was further reduced to 12. If all the 47 studies had provided an effect estimate or provided data that enabled the calculation of an effect size, the final list of useable studies in the data synthesis would have been larger which again would have provided a more robust literature on which to base conclusions.

During the reduction from 47 studies to 12 studies, the list of countries represented became shorter. The 47 included studies originated from 19 different countries, whereas the 12 studies used in the data synthesis originated from only 9 different countries. The coverage became narrower as studies from many Western European countries could not be used in the data synthesis, including Norway, Sweden, Finland, Netherlands, Belgium, the UK, and Switzerland. The 12 studies used in the data synthesis covered the US, Canada, Portugal, Spain, Slovenia, Germany, Czech Republic, Austria, and Poland.

The 12 studies included in the data synthesis were used to consider the effect on the primary outcome, namely individuals' exit rate out of unemployment and into employment.

It was not possible to examine if the exhaustion effect differed for particular age or educational groups, or if factors such as good/bad labour market conditions, high/low maximum entitlement, availability of alternative benefits, and whether compulsory activation was part of the system had an impact on the exhaustion effect. It was possible to study the impact of gender but only using a subgroup of five studies.

To obtain a clearer picture of the effect of the prospect of benefit exhaustion on the quality of the job, the intention was to consider the duration of re-employment and the re-employment wage as secondary outcomes. Unfortunately, no studies provided data for re-employment wages. However, four studies provided data for the effect of benefit exhaustion on the subsequent exit rate from the re-employment job. We included the measure of exit rate from the re-employment job in the analysis of secondary outcomes. A high exit rate from the re-employment job could indicate that the exhaustion of benefits forces unemployed individuals to find jobs that do not match their qualifications, and therefore they return to unemployment quickly. Due to overlap of data samples, only three studies were eligible for further analysis. The small number of studies reporting this outcome makes us reluctant to draw a conclusion.

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### **5.3 QUALITY OF THE EVIDENCE**

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The quality of each study from which it was possible to extract an effect size (21 studies) was examined using a newly developed tool for assessing risk of bias incorporating non-randomised studies. One of the main points in this tool is that a study with a risk of bias equal to 5 on the point scale corresponds to a risk of bias so high that the findings should not be considered in the data synthesis. Therefore, the quality of the evidence in this review is guaranteed to be at an acceptable level, as the studies that are more likely to mislead than inform were not used in the data synthesis.

Furthermore, we have performed a sensitivity analysis to check whether the obtained result is robust across methodological quality and data quality. To check the robustness across methodological quality, the studies with relatively high risk of bias in confounding, incomplete data, and selective reporting, respectively, were excluded from the analysis. To check the robustness across data quality, studies with estimates on weekly data were excluded. In addition, studies based on questionnaire data were excluded from the main analysis. The overall conclusion that the hazard rate significantly increases in the month or week of exhaustion did not change. Neither were there appreciable changes in the results for the effect estimate one month and two months before exhaustion. Due to the low number of studies, it was not possible to perform sensitivity analyses for the remaining time intervals.

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## **5.4 POTENTIAL BIASES IN THE REVIEW PROCESS**

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We believe that all of the publicly available studies of the unemployment benefit exhaustion effect on exits to employment up to the censor date were identified during the review process. However, two references were not obtained in full text due to unknown information and long delivery time.

One review author (ADK) and one member of the review team (SHF) independently coded the included studies. Disagreements were resolved by consulting a third review author (TF). Decisions about inclusion of studies and assessment of study quality were made by two review authors (ADK, TF) independently, and disagreements were resolved by discussion. Data extraction was made by one review author (TF) and was checked by a second review author (ADK).

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## **5.5 AGREEMENTS AND DISAGREEMENTS WITH OTHER STUDIES OR REVIEWS**

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To our best knowledge, there is no other systematic review on this topic.

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## 6 Authors' Conclusion

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### 6.1 IMPLICATIONS FOR PRACTICE

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In order to reduce high unemployment levels, policy-makers may wish to reduce the generosity of the unemployment system either in amount (the replacement rate) or in maximum potential duration. The positive correlation between unemployment insurance benefit generosity in terms of the replacement rate and unemployment duration is well established at the empirical level (Layard et al., 2005). However, it may be politically intractable to lower the replacement rate, and there are indeed strong efficiency and equity arguments for having a reasonable value of unemployment benefits (Acemoglu & Shimer, 1999; Marimon & Zilibotti, 1999).

Search theory suggests that the prospect of exhaustion of benefits may result in a significantly increased incentive for finding work. Hence, shortening the benefit eligibility period may reduce the share of long and unproductive job searches.

In this review, we have found clear evidence that the prospect of exhaustion of benefits results in a significantly increased incentive for finding work but only shortly prior to exhaustion and at the time of exhaustion. Thus, the theoretical suggestion of an exhaustion effect on job finding rates has been confirmed empirically.

Whether the increased job finding rate close to benefit expiration implies a decrease in the overall unemployment level depends on whether it is caused mostly by an increase in search intensity or a decrease in reservation wages. If increases in the job finding rates close to benefit exhaustion are explained by decreases in reservation wages, those who are close to benefit exhaustion might accept jobs that do not match their qualifications from which they are more likely to quit in the future. If the increased job finding rates are explained by increases in the search effort, there is no reason to expect that exhaustion of benefits forces unemployed individuals to find jobs that do not match their qualifications.

We found no studies that provided data for re-employment wages. We found three studies that could be used for analysis of the exit rate from the re-employment job. Based on this low number of studies, we found no evidence to support the hypothesis that the prospect of benefit exhaustion has an impact on the quality of the job in terms of the exit rate from the

re-employment job. Thus, whether the unemployed workers who are affected may actually be worse off than policy-makers intend them to be, in the sense that they accept “bad” jobs, have not yet been fully investigated.

It was not possible to examine a number of factors which we have reasons to expect have an impact on the magnitude of the exhaustion effect. Knowledge of whether the effect depends on labour market conditions and benefit system factors such as the maximum entitlement, availability of alternative benefits, and compulsory activation may be crucial to policy-makers. The factors are all potential moderators of the exhaustion effect that policy-makers need to be able to assess in relation to the context of their country. The results of this review, however, merely suggest that across a number of countries there is an overall incentive effect of benefit exhaustion on job finding rates.

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## **6.2 IMPLICATIONS FOR RESEARCH**

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In this review, we have found clear evidence that the prospect of exhaustion of benefits results in a significantly increased incentive for finding work.

Whether the increased job finding rate close to benefit expiration implies a decrease in the overall unemployment level however, remains an open question. More information is needed about what triggers the transition out of unemployment.

Further research should be directed at the possible side effects of the benefit exhaustion, in particular whether the unemployed leave unemployment due to a higher acceptance of low-paid employment or due to an increased search effort.

The planned examination of potential moderators of the exhaustion effect was not possible, as the covariates often were not reported. Further, many of the available studies did not provide data that permitted the calculation of an effect size. If effect sizes of these studies had been available, valuable information about the heterogeneous effects of the exhaustion of benefits derived from moderator analyses may possibly have been provided.

These considerations point to the need for studies considering heterogeneous effects of the exhaustion of benefits and for reporting detailed results that permit their inclusion in systematic reviews.

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## 7 Acknowledgements

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The review authors are responsible for any remaining errors.

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## 8 References

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### 8.1 INCLUDED STUDIES

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#### 8.1.1 Studies with Effect Estimates

This first part consists of references linked to the 21 studies that provided data permitting the calculation of an effect size. References denoted with \* are the primary references.

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#### **Arranz, 2008**

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Sanz, Y. R. (2010). Unemployment insurance and job turnover in Spain. *Working paper series No. 10.08, Universidad Pablo de Olavide, Department of Economics*. **(Used in the data synthesis of primary outcome)**

### **Schmieder, 2009**

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### **Wolff, 1997**

Wolff, J. (1997). Unemployment benefits and incentives in Hungary: New evidence. *Working Paper Number 111*. The William Davidson Institute, The University of Michigan Business School.

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## **8.3 STUDIES AWAITING CLASSIFICATION**

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Lardaro, L. (1985). Unused benefit weeks as a work disincentive: Does the entitlement effect of UI always offset the work disincentive effect?. *Proceedings of the Industrial Relations Research Association*, 409-417.

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## 9 Characteristics of Studies

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### 9.1 CHARACTERISTICS OF INCLUDED STUDIES

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This section lists important characteristics for the 12 studies included in the data synthesis. For further characteristics of all 47 included studies see the supplementary document.

*Portugal, P., & Addison, J. T. (2008)*

Country	Portugal
Analysis period	1992-1997
Type of data	Questionnaire
Sample size	9,451 individuals
Time interval for outcome measure	Monthly

*Jenkins, S. P., & García-Serrano, C. (2004)*

Country	Spain
Analysis period	1987-1993
Type of data	Administrative registers
Sample size	3229,947 spells
Time interval for outcome measure	Monthly

*Card, D., Chetty, R., & Weber, A. (2007)*

Country	Austria
Analysis period	1981-2001
Type of data	Administrative registers
Sample size	92,969 spells
Time interval for outcome measure	Weekly

*Addison, J. T., & Portugal, P. (2004)*

Country	USA
Analysis period	1996-1998
Type of data	Questionnaire
Sample size	2,762 individuals
Time interval for outcome measure	Weekly

*Belzil, C. (2001)*

Country	Canada
Analysis period	1976-1984
Type of data	Administrative registers
Sample size	909 individuals
Time interval for outcome measure	Weekly

*Terrell, K., & Sorm, V. (1999)*

Country	Czech Republic
Analysis period	1992-1994
Type of data	Administrative registers
Sample size	3,000 individuals
Time interval for outcome measure	Weekly

*Vodopivec, M. (1995)*

Country	Slovenia
Analysis period	1990-1992
Type of data	Administrative registers
Sample size	23,242 spells
Time interval for outcome measure	Monthly

*Boeri, T. & Steiner, V. (1998)*

Country	Poland
Analysis period	1990-1993
Type of data	Administrative registers
Sample size	<i>Not available</i>

Time interval for outcome measure	Monthly
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*Van Ours, J., & Vodopivec, M. (2004)*

Country	Slovenia
Analysis period	1997-1999
Type of data	Administrative registers
Sample size	30,337 individuals
Time interval for outcome measure	Monthly

*Schmitz, H., & Steiner, V. (2007)*

Country	Germany
Analysis period	1995-2003
Type of data	Questionnaire
Sample size	4,612 individuals/7,348 spells
Time interval for outcome measure	Monthly

*Sanz (2010)*

Country	Spain
Analysis period	2000-2007
Type of data	Administrative registers
Sample size	NA
Time interval for outcome measure	Monthly

*Katz, L. F., & Meyer, B. D. (1990)*

Country	USA
Analysis period	1979-1981
Type of data	Combination of questionnaire and administrative registers
Sample size	756 observations
Time interval for outcome measure	Weekly

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## 9.2 CHARACTERISTICS OF EXCLUDED STUDIES

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*Lalive, R., van Ours, J., & Zweimüller, J. (2006)*

Reason for exclusion	The study analyzed the effect on benefit exhaustion on exit from unemployment but 14% of the exits where to other destinations than employment. Hence, it did not qualify for inclusion in the review.
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*Nivorozhkin, A. (2008)*

Reason for exclusion	The study analyzed the effect on benefit exhaustion on duration dependence and not exit to employment. Hence, it did not qualify for inclusion in the review.
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*Kyyrä, T., & Ollikainen, V. (2008)*

Reason for exclusion	The study investigated a mixture of the threat effects of compulsory participation in ALMP and benefit exhaustion. Hence, it did not qualify for inclusion in the review.
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*Røed, K., & Zhang, T. (2005)*

Reason for exclusion	The study investigated a mixture of the threat effects of compulsory participation in ALMP and temporary benefit exhaustion. Hence, it did not qualify for inclusion in the review.
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*Card, D., & Levine, P. B. (2000)*

Reason for exclusion	The study analyzed the effect on benefit exhaustion on exit from unemployment but did not separate exits to employment and exits to other destinations than employment. Hence, it did not qualify for inclusion in the review.
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*Meyer, B. D. (1990)*

Reason for exclusion	The study analyzed the effect on benefit exhaustion on exit from unemployment but did not separate exits to employment and exits to other destinations than employment. Hence, it did not qualify for inclusion in the review.
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*Hung, C. Y., & Lo, T. F. (2008)*

Reason for exclusion	The study is written in Chinese. It was not possible to translate it. Hence, we could not use the study in the review.
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*Bratberg, E., & Vaage, K. (2000)*

Reason for exclusion	The study compares one temporary benefit exhaustion with another temporary benefit exhaustion. Hence, it did not qualify for inclusion in the review.
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*Lalive, R., van Ours, J., & Zweimüller, J. (2000)*

Reason for exclusion	The study analyses the threat effects of compulsory participation in ALMP and not benefit exhaustion as stated by the authors. Hence, it did not qualify for inclusion in the review.
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*Bover, O., Arellano, M., & Bentolila, S. (1997)*

Reason for exclusion	The study analyses the effect of receiving unemployment benefit on the duration of unemployment and not the effect of exhaustion of benefits. Hence, it did not qualify for inclusion in the review.
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*Moffitt, R. (1985)*

Reason for exclusion	The study analyzed the effect on benefit exhaustion on exit from unemployment but did not separate exits to employment and exits to other destinations than employment. Hence, it did not qualify for inclusion in the review.
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### 9.3 CHARACTERISTICS OF STUDIES AWAITING CLASSIFICATION

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Here we list the known characteristics of the references that were not possible to retrieve.

	<i>Lardaro, L., 1985</i>	<i>Jurajda, S., 1997</i>
<b>Title</b>	Unused benefit weeks as a work disincentive: Does the entitlement effect of UI always off set the work disincentive effect?	Empirical evaluation of the effects of the U.S. unemployment insurance system on employment and unemployment
<b>Journal</b>	Proceedings of the Industrial Relations Research Association	University of Pittsburgh – Conference paper
<b>Ordered</b>	July 2011	July 2011
<b>Reason for not obtained</b>	Long delivery-time	Unretrievable

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# 10 Appendices

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## APPENDIX 10.1 SEARCH DOCUMENTATION

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### 10.1.1 Electronic searches

Documentation for the search strategies used for searching the literature

#### Business Source Elite

##### **S1 Intervention**

((DE "Social Security") OR (DE "WELFARE recipients") OR (welfare w1 payment\*) OR (welfare w1 recipient\*) OR (welfare w1 support\*) OR (economic w1 support\*) OR (public w1 assistance\*) OR (welfare w1 payment\*) OR (public w1 support\*) OR (financial w1 support\*) OR (welfare w1 service\*) OR (direct\* w1 payment\*) OR (general w1 assistance) OR (Social w1 Support) OR (cash w1 assistance) OR (income w1 assistance) OR (benefit\*) OR (social w1 assistance\* ) OR (social w1 securit\* ) OR (social w1 welfare ) OR (social w1 allowance\*) OR (insurance w1 benefit\* ) OR (social w1 benefit\* ) OR (welfare w1 benefit\*) OR (TANF)) OR insurance

##### **S2 Job Situation**

((DE "EMPLOYABILITY") OR (Employ\*) OR (Job\*) OR (work\*) OR (un-employ\* or unemploy\*) OR (re-employ\* or reemploy\*))

##### **S3 Motivation/threat**

((effect\*) OR (threat\*) OR (incentive\*) OR (disincentive\*) OR (impact\*) OR (motivat\*))

##### **S4 Termination**

((Expir\*) OR (Lapse) OR (Terminat\*) OR (Duration) OR (Generosity) OR (Change OR changes) OR (Entitlement) OR (Length) OR (Extend\*) OR (Extension) OR (Exhaust\*) OR (exit))

S1 AND S2 AND S3 AND S4

Combination of aspects in Business Source Elite pr. 30/11-2010.

Hits: 3773

## **Cochrane Library**

### **S1 Intervention**

((“Social Security”) OR (Welfare NEXT recipient\*) OR (welfare NEAR/1 payment\*) OR (welfare NEAR/1 recipient\*) OR (welfare NEAR/1 support\*) OR (economic NEAR/1 support\*) OR (public NEAR/1 assistance\*) OR (welfare NEAR/1 payment\*) OR (public NEAR/1 support\*) OR (financial NEAR/1 support\*) OR (welfare NEAR/1 service\*) OR (direct\* NEAR/1 payment\*) OR (general NEAR/1 assistance) OR (Social NEAR/1 Support) OR (cash NEAR/1 assistance) OR (income NEAR/1 assistance) OR (benefit\*) OR (social NEAR/1 assistance\* ) OR (social NEAR/1 securit\* ) OR (social NEAR/1 welfare ) OR (social NEAR/1 allowance\*) OR (insurance NEAR/1 benefit\* ) OR (social NEAR/1 benefit\* ) OR (welfare NEAR/1 benefit\*) OR (TANF) OR insurance)

### **S2 Job Situation**

((Employ\*) OR (Job\*) OR (work\*) OR (un?employ\*) OR (unemploy\*) OR (re?employ\*) OR (reemploy\*))

### **S3 Motivation/threat**

((effect\*) OR (threat\*) OR (incentive\*) OR (disincentive\*) OR (impact\*) OR (motivat\*))

### **S4 Termination**

((Expir\*) OR (Lapse) OR (Terminat\*) OR (Duration) OR (Generosity) OR (Change or changes) OR (Entitlement) OR (Length) OR (Extend\*) OR (Extension) OR (Exhaust\*) OR (exit))

Combination of aspects in Cochrane pr. 30/11-2010.

Hits: 721

## **EconLit**

### **S1 Intervention**

((DE= "Social Security") OR (Social within 1 securit\*) OR (Social within 1 support) OR (Social within 1 welfare) OR (Welfare within 1 recipient\*) OR (Welfare within 1 service\*) OR (Support) OR (Assistance) OR (Aid) OR (Relief) OR (Benefit\*) OR (Allowance\*) OR (Payment\*) OR (Securit\*) OR (TANF) OR insurance)

### **S2 Job Situation**

((Employability OR Employ\* OR Job\* OR Work\* OR (Un-employ\* OR Unemploy\*) OR (Re-employ\* OR Reemploy\*))

### **S3 Motivation**

(Effect\* OR Threat\* OR Incentive\* OR Disincentive\* OR Impact\* OR Motivat\*)

#### **S4 Termination**

((Expiration) or(Lapse) or(Expiry) or(Termination) or(Duration) or(Generosity) or(Change) or(Entitlement) or(Length) or(Extend\*) or(Extension) or(Exhaust\*) OR Exit)

Combination of aspects in EconLit pr. 30/11-2010.

Hits: 5870

#### **PsycInfo**

##### **S1 Intervention**

((DE "Social Security") OR (welfare w1 payment\*) OR (welfare w1 recipient\*) OR (welfare w1 support\*) OR (economic w1 support\*) OR (Social w1 support) OR (public w1 assistance\*) OR (public w1 support\*) OR (financial w1 support\*) OR (welfare w1 service\*) OR (direct\* w1 payment\*) OR (general w1 assistance) OR (cash w1 assistance) OR (income w1 assistance) OR benefit\* OR (social w1 assistance\*) OR (social w1 securit\*) OR (social w1 welfare) OR (social w1 allowance\*) OR (insurance w1 benefit\*) OR (social w1 benefit\*) OR (welfare w1 benefit\*) OR TANF OR insurance)

##### **S2 Job Situation**

((DE "EMPLOYABILITY") OR Employ\* OR Job\* OR work\* OR un-employ\* OR unemploy\* OR re-employ\* OR reemploy\*)

##### **S3 Motivation/threat**

(effect\* OR threat\* OR incentive\* OR disincentive\* OR impact\* OR motivat\*)

##### **S4 Termination**

(Expiration OR Lapse OR Expiry OR Termination OR Duration OR Generosity OR Change OR Entitlement OR Length OR Extend\* OR Extension OR Exhaust\* OR exit)

Combination of aspects in PsycInfo pr. 1/12-2010.

Hits: 4453

#### **SocIndex**

##### **S1 Intervention**

((DE "Social Security") OR (DE "WELFARE recipients") OR (Welfare w1 recipient\*) OR (welfare w1 payment\*) OR (welfare w1 support\*) OR (economic w1 support\*) OR (public w1 assistance\*) OR (public w1 support\*) OR (financial w1 support\*) OR (welfare w1 service\*) OR (direct\* w1 payment\*) OR (general w1 assistance) OR (Social w1 Support) OR (cash w1 assistance) OR (income w1 assistance) OR (benefit\*) OR (social w1 assistance\*) OR (social

w1 securit\*) OR (social w1 welfare) OR (social w1 allowance\*) OR (insurance w1 benefit\*) OR (social w1 benefit\*) OR (welfare w1 benefit\*) OR TANF OR insurance)

## **S2 Job Situation**

((DE "EMPLOYABILITY") OR Employ\* OR Job\* OR work\* OR (un-employ\* or unemploy\*) OR (re-employ\* or reemploy\*))

## **S3 Motivation/threat**

(effect\* OR threat\* OR incentive\* OR disincentive\* OR impact\* OR motivat\*)

## **S4 Termination**

(Expiration OR Lapse OR Expiry OR Termination OR Duration OR Generosity OR Change OR Entitlement OR Length OR Extend\* OR Extension OR Exhaust\* OR exit)

Combination of aspects in SocIndex pr. 1/12-2010.

Hits: 4385

## **Social Science Citation Index**

### **Aspects, Search Terms and Limitations**

The search is limited to topic and title. The database was last updated November 27, 2010. The maximum no. of terms in the search string is 50. Therefore the search of the different aspects is performed individually in topic/title and combined afterwards. The quotation marks can only be copied from notepad – they have to be straight.

### **S1 Intervention**

"Welfare recipient\*" OR "welfare payment\*" OR "welfare support\*" OR "economic support\*" OR "public assistance\*" OR "public support\*" OR "financial support\*" OR "welfare service\*" OR "direct\* payment\*" OR "general assistance" OR "Social Support" OR "cash assistance" OR "income assistance" OR "benefit\*" OR "social assistance\*" OR "social securit\*" OR "social welfare" OR "social allowance\*" OR "insurance benefit\*" OR "social benefit\*" OR "welfare benefit\*" OR TANF OR insurance

### **S2 Job situation**

Employ\* OR Job\* OR work\* OR (un-employ\* or unemploy\*) OR (re-employ\* or reemploy\*)

### **S3 Motivation/threat**

effect\* OR threat\* OR incentive\* OR disincentive\* OR impact\* OR motivat\*

### **S4 Termination**

Expiration OR Lapse OR Expiry OR Termination OR Duration OR Generosity OR Change OR Entitlement OR Length OR Extend\* OR Extension OR Exhaust\* OR exit

Combination of aspects in Social Science Citation Index pr. 06/12-2010.

Hits: 339

## **International Bibliography of the Social Sciences**

### **Aspects, Search Terms and Limitations**

The main part of the search was made in advanced search on keywords restricted to: [mp=abstract, title, book title, original title, heading word, subject heading, geographic heading]. Social security was represented in the thesaurus.

#### **S1 Intervention**

DE "social security" OR (Welfare adj recipient\$1) OR ) OR (welfare adj payment\$1) OR (welfare adj support\$1) OR (economic adj support\$1) OR (public adj assistance\$1) OR (public adj support\$1) OR (financial adj support\$1) OR (welfare adj service\$1) OR (direct\$1 adj payment\$1) OR (general adj assistance) OR (Social adj Support) OR (cash adj assistance) OR (income adj assistance) OR (benefit\$1) OR (social adj assistance\$1 ) OR (social adj securit\$) OR (social adj welfare) OR (social adj allowance\$1) OR (insurance adj benefit\$1) OR (social adj benefit\$1) OR (welfare adj benefit\$1) OR TANF OR insurance

#### **S2 job situation**

Employ\$ OR Job\$ OR work\$ OR un-employ\$ OR unemploy\$ OR re-employ\$ OR reemploy\$

#### **S3 motivation/threat**

effect\$1 OR threat\$1 OR incentive\$1 OR disincentive\$1 OR impact\$1 OR motivat\$1

#### **S4 termination**

Expiration OR Lapse OR Expiry OR Termination OR Duration OR Generosity OR Change\$1 OR Entitlement OR Length OR Extend\$1 OR Extension OR Exhaust\$ OR exit

Combination of aspects in International Bibliography of the Social Sciences 6/12-2010.

Hits: 1172

## **IBZ Online**

Internationale Bibliographie der geistes- und sozialwissenschaftlichen Zeitschriftenliteratur

### **Aspects, Search Terms and Limitations**

The search is restricted to title, abstract and keywords.

#### **S1 Intervention**

securit\* OR social OR welfare Support OR Assistance OR Aid OR Relief OR Benefit\* OR Allowance\* OR Payment\* OR TANF OR insurance

## **S2 Job situation**

Employ\* OR Job\* OR work\* OR (un-employ\* or unemploy\*) OR (re-employ\* or reemploy\*)

## **S3 Motivation**

effect\* OR threat\* OR incentive\* OR disincentive\* OR impact\* OR motivat\*

## **S4 Termination**

Expiration OR Lapse Or Expiry OR Termination OR Duration OR Generosity OR Change OR Entitlement OR Length OR Extend\* OR Extension OR Exhaust\* OR exit

Combination of aspects in IBZ Online 11/1-2011

Hits: 13

## **Repec via Ideas**

### **Aspects, Search Terms and Limitations**

The search is performed as a full text search. The search is performed in the entire bibliographical record. The terms accepted are all forms – e.g. when searching “benefit” beneficial, benefits etc. are also accepted. The match is boolean. The search is narrowed down due exceptionally many hits (388.351).

### **S1 Intervention**

(Support | benefit | welfare | insurance)

### **S2 Job Situation**

(employ | job | work)

### **S3 Motivation/threat**

(effect | incentive | impact | motivation)

### **S4 Termination**

(expiration | termination | duration | change | exhaust | exit)

Combination of aspects in Repec via Ideas 09/03-11.

Hits: 3555

## **Economist Online**

### **Aspects, Search Terms and Limitations**

The search is performed as a full text search. The restricted search terms have been selected based on the representation of search terms in the title or abstract from the below texts. This is due to the fact that the system showed an error when performing the search on all terms.

**S1 Intervention**

Benefit\* OR support OR insurance

**S2 Employment**

Employ\* OR job\* OR work

**S3 Motivation/threat**

Effect\* OR impact OR incentive

**S4 Termination**

Duration OR exit OR exhaust\*

Combination of aspects in Economist Online 19/01-2011.

Hits: 551

**SSRN**

Social Science Research Network

**Aspects, Search Terms and Limitations**

The search is limited to title, abstract and keywords.

On March 11 and 16 2011 it was not possible to make a AND/OR search even though it was suggested in the SSRN help text. Therefore the search has been made as follows:

S1 Intervention AND S2 Job situation AND S3 Motivation AND S4 Termination

Or

Benefit AND unemployment AND effects AND duration

This search resulted in 64 hits.

The terms have been selected based on the probability of achieving hits from authors known to make research within UI and ALMP. That is, Title and abstract for known articles have been checked and the one combination with the most hits has been selected.

**Social Care Online****Aspects, Search Terms and Limitations**

The search is performed as an intermediate search within full text (free text). The search was made as follows:

(freetext="social security" or topic="benefits" or freetext="benefit\*" or freetext="support" or freetext="insurance") and (topic="unemployment" or freetext="unemployment" or

freetext="work" or topic="employment" or freetext="employment" or freetext="job\*") and (freetext="effect" or freetext="impact\*" or freetext="incentive\*" or freetext=motivation) and (freetext="duration" or freetext="exit\*" or freetext="exhaust\*" or freetext=entitl\*)

Combination of aspects in Social Care Online 21/06-2011.

Hits: 68

### **Forskningsdatabasen (The Danish National Research database)**

#### **Aspects, Search Terms and Limitations**

The search is performed both in the new and the old database.

##### **S1 Intervention**

(Support=benefit=welfare=insurance)

##### **S2 Job Situation**

(employ=job=work)

##### **S3 Motivation/threat**

(effect=incentive=impact=motivation)

##### **S4 Termination**

(expiration=termination=duration=change=exhaust=exit)

Combination of aspects in Forskningsdatabasen 11/03-11.

Hits: 56

### **Dissertations & Thesis A&I**

#### **Aspects, Search Terms and Limitations**

The search was restricted to title and abstract.

The search was performed on August 24 2011.

##### **S1 Intervention**

AB,TI(benefit OR insurance)

##### **S2 Job situation**

AB,Ti(employ OR job OR work)

##### **S3 Motivation**

AB,TI(effect OR incentive OR impact)

#### **S4 Termination**

AB, TI (expiration OR termination OR duration OR exhaust OR exit)

Combination of aspects in Dissertation & Thesis A&I.

Hits: 355

#### **Google**

##### **Aspects, Search Terms and Limitations**

The search is performed without limitations. The search was performed on May 31 2011.

With Google it is only possible to combine the terms with AND/OR and to pair them like "social security". Advanced search is used with the following combination: effect employment duration "social security".

The search resulted in 32,400,000 hits. The top 300 were saved.

#### **Google Scholar**

Search May 31 2011

Advanced search is used with the following combination: effect employment duration "social security".

This resulted in 64,300 hits – the top 300 have been downloaded for screening.

Furthermore the same search was made but only in title, this resulted in 5 hits, all of which were downloaded.

#### **OpenSigle/OpenGrey**

Search June 9 2011

Simple search is used with the following string:

((security OR social OR welfare Support OR Assistance OR Aid OR Relief OR Benefit OR Allowance OR Payment OR TANF OR insurance) AND (Employment OR Job OR work) AND (effect OR threat OR incentive OR disincentive OR impact OR motivation) AND (Expiration OR Lapse Or Expiry OR Termination OR Duration OR Generosity OR Change OR Entitlement OR Length OR Extend OR Extension OR Exhaust OR exit))

This resulted in 501 hits.

### 10.1.2 Hand-searching

Hand-searching was performed in *Labour Economics* (issn: 0927-5371) and *Journal of Labour Economics* (issn: 0734-306X) for the years 2010 and 2011. This resulted in respectively 145 and 37 articles screened.

### 10.1.3 Other

Search performed May and August

Source	Link	Retrieved for full text screening	Material
Det økonomiske råd	<a href="http://www.dors.dk/sw3321.asp">http://www.dors.dk/sw3321.asp</a>	1	<a href="http://www.dors.dk/graphics/Synkron-Library/Publikationer/Rapporter/Efter%E5r_2002/kap6.pdf">http://www.dors.dk/graphics/Synkron-Library/Publikationer/Rapporter/Efter%E5r_2002/kap6.pdf</a>
OECD	<a href="http://www.oecd.org/document002F7/0,3746,en_2649_201185_44222919_1_1_1_1,00.html">http://www.oecd.org/document002F7/0,3746,en_2649_201185_44222919_1_1_1_1,00.html</a>	0	
IMF	<a href="http://www.imf.org/external/pubs/cat/wp1_sp.aspx">http://www.imf.org/external/pubs/cat/wp1_sp.aspx</a>	1	<a href="http://www.imf.org/external/pubs/ft/wp/2007/wp0736.pdf">http://www.imf.org/external/pubs/ft/wp/2007/wp0736.pdf</a>
AIECE	<a href="http://sites-test.uclouvain.be/aiece/publications/special.html">http://sites-test.uclouvain.be/aiece/publications/special.html</a>	0	
ESRC	<a href="http://www.esrc.ac.uk/">http://www.esrc.ac.uk/</a>	0	
MDRC	<a href="http://www.mdrc.org/sps/wish.cgi?search_phrase=employment%20AND%20duration&amp;chunk_num=0&amp;search_title=&amp;search_index=publications">http://www.mdrc.org/sps/wish.cgi?search_phrase=employment%20AND%20duration&amp;chunk_num=0&amp;search_title=&amp;search_index=publications</a>	35	
Copenhagen Economics	<a href="http://www.copenhageneconomics.com/Publications.aspx">http://www.copenhageneconomics.com/Publications.aspx</a>	0	
Theses Canada	<a href="http://amicus.collectionscanada.gc.ca/s4-bin/Main/AdvSearch?l=0&amp;id=&amp;v=1&amp;coll=18">http://amicus.collectionscanada.gc.ca/s4-bin/Main/AdvSearch?l=0&amp;id=&amp;v=1&amp;coll=18</a>	14	

## APPENDIX 10.2 ENTITLEMENT

*Table 10.1: Range of Individual Entitlement and Eligibility Rules Applicable in the Countries and Time Periods of the Studies Used in the Data Synthesis*

Country	Individual entitlement <sup>a</sup>	Period	Eligibility rules
Spain	3-24 months	1987-1992	3 months if tenure last 48 months is 6-12 months, it then increases in 3-month intervals for each incremental 6 months of tenure the last 48 months up to a maximum of 24 months with tenure more than 48 months.
Spain	4-24 months	2000-2007	At least 4 months extendable in 2-monthly periods up to a maximum of 2 years, depending on the worker's employment record.
Austria	20-30 weeks	1981-2001	Individuals with fewer than 36 months of employment can receive up to 20 weeks of benefits, while those who have worked for 36 months or more can receive 30 weeks of UI.
Portugal	10-30 months	1992-1997	10 months if <25 years of age, 12 months for those aged between 25 and 29 years, it then increases in 3-month intervals for each incremental 5 years of age, up to a maximum of 30 months at age 55.
Czech Republic	6 months	1992-1994	No further is stated
Canada	1-50 weeks	1976-1984, reform in 1977	The mean initial entitlement is 33 months with a standard deviation of 14. Difference before and after the reform is 3 weeks. In 1977 maximum benefit period reduced to 50 weeks
Slovenia	3-9 months	1997-1999, reform in 1998	Before the reform (in 1998): 3 months with 0-1.5 years of experience, 6 months with 1.5-5 years of experience, 9 months with 5-10 years of experience, 12 months with 10-15 years of experience and 18 months with 15-20 years of work experience. After the reform: 3 months with 0-5 years of experience, 6 months with 5-15 years of experience and 9 months with 15-20 years of experience.
Slovenia	3-24 months	1990-1992, reform in 1991	Depends on the duration of previous employment - 3 months for at least nine months of uninterrupted employment, 6 months for at least 30 months of uninterrupted employment, 9 months for employment above five years, but less than ten years; 12 months for employment above ten years, but less than 15 years; Before reform- 24 months, for employment above 15 years. –After reform 18 months for employment above 15 years, but less than 20 years; and -- 24 months for employment above 20 years.

Poland	12 months	1990-1993, reform in 1992	Before the change (1992): open-ended. After the change: 12 months
Germany	6-32 months	1995-2003, reform in 1997	<p>Depends on the number of months worked in the last seven years and the age of the claimant. Before the reform the maximum UI entitlement for workers who were younger than 42 was 6-12 months dependent on tenure. For workers aged 42 to 43 the maximum was 14-18 months dependent on tenure; for workers aged 44 to 48, the maximum entitlement was 20-22 dependent on tenure; for workers aged 49 to 54 the maximum entitlement was 24-26 months dependent on tenure and for workers aged 54+ the maximum entitlement was 28-32 months dependent on tenure.</p> <p>After the reform the maximum UI entitlement for workers who were younger than 45 was 6-12 months dependent on tenure. For workers aged 45 to 46 the maximum was 20-22 months dependent on tenure; for workers aged 47 to 51, the maximum entitlement was 24-26 dependent on tenure; for workers aged 52 to 56 the maximum entitlement was 24-26 months dependent on tenure and for workers aged 57+ the maximum entitlement was 28-32 months dependent on tenure.</p>
US <sup>b</sup>	1-30 weeks	1996-1998	26 or 30 weeks. Two states had maximum durations greater than 26 weeks over the sample period. These were Massachusetts and Washington, each with a maximum entitlement of 30 weeks
US <sup>b</sup>	1-45 weeks	1979-1981, extension in 1980	Regular UI benefits in Pennsylvania had a uniform duration of 30 weeks, while Missouri had a maximum potential duration of 26 weeks with variation in the potential duration that depended on base period and high quarter earnings. Extended benefits were triggered in February 1980 in Pennsylvania and in May 1980 in Missouri. The extensions raised the potential length of benefits to 39 weeks in Pennsylvania and increased the potential length to 45 weeks in Missouri.

Notes: a: Individual entitlement of the unemployed included in the analysis of the primary study;

b: No lower level of entitlement is stated

---

## **APPENDIX 10. 3      FIRST AND SECOND LEVEL SCREENING**

---

First level screening is on the basis of titles and abstracts. Second level is on the basis of full text

Reference id. No.:

Study id. No.:

Reviewers initials:

Source:

Year of publication:

Duration of study:

Country/countries of origin:

Author:

The study is excluded if one or more of the answers to question 1-3 are No. If the answers to question 1 to 3 are yes or uncertain then the full study is retrieved for second level eligibility. All uncertain questions need to be posed again on the basis of full text. If not enough information is available or if the study is unclear the author of the study will be contacted if possible.

### **First level screening questions are based on titles and abstracts**

1. Are the participants' unemployed individuals receiving some kind of benefit during their unemployment?

Yes - include

No – if no then stop here and exclude

Uncertain - include

Question 1 guidance:

This includes all types of unemployment benefits both unemployment insurance benefits, unemployment assistance benefits and social assistance benefits.

2. Does the study focus on time limits in the unemployment benefit eligibility period or exhaustion of unemployment benefits or entitlement to unemployment benefits or maximum duration of unemployment benefits etc.?

Yes - include

No – if no then stop here and exclude

Uncertain – include

Question 2 guidance:

The intervention is the exhaustion of any kind of unemployment benefit with a known expiration date. This intervention can be referred to in different ways.

3. Is this study a primary quantitative study?

Yes - include

No – if no then stop here and exclude

Uncertain – include

Question 3 guidance:

We are only interested in primary quantitative studies, where the authors have analyzed the data. We are not interested in theoretical papers on the topic or surveys/reviews of studies of the topic. (This question may be difficult to answer on the base of titles and abstracts alone.)

Second level screening questions based on full text

4. Does the study estimate an effect, using a control group or using an estimated counterfactual?

Yes - include

No – if no then stop here and exclude

Uncertain – include

Question 4 guidance

E.g. 1) Randomised controlled trials including cluster randomisation and quasi randomised controlled study designs (i.e. participants are allocated by means such as alternate allocation, person's birth date, the date of the week or month, case number or alphabetical order), 2) non randomised controlled study designs (i.e. quasi-experimental designs) such as controlled two group study designs or 3) study designs based on observational data, where the effect is estimated by statistical methods.

5. Does the study examine exits to employment?

Yes – include

No – if no then stop here and exclude

Uncertain – include

Question 3 guidance:

The primary outcome is exits to employment. Studies only looking at exits to other destinations (such as other kinds of benefits or out of the labor force) or studies who do not distinguish between destinations will not be included.

---

**APPENDIX 10.4      CODING FORM**


---

Language
Journal
Year
Country
Time period covered by data
Type of unemployment scheme (UI, social benefit other (specify))
Target group (age, gender, education, eligibility requirements for benefits )
Benefit level/replacement rate
Labour market conditions (unemployment rate)
Benefit level/replacement rate available after exhaustion if any
Is compulsory activation part of the system? If yes, describe the elements of the programme (education, work, training, self-employment, job search assistance)
Maximum duration of unemployment benefits
Type of data used (register, questionnaire, other (specify))
Sampling frequency
Time interval the outcome measure is based on (if different from sampling frequency)
Is there correction for unobserved heterogeneity? If yes, how?
Sample size (Treatment/control)
Censoring level (percent, separate for intervention/control if possible)
Is there correction for censoring (yes/no)

## Outcome measures

Instructions: Please enter outcome measures in the order in which they are described in the report. Note that a single outcome measure can be completed by multiple sources and at multiple points in time (data from specific sources and time-points will be entered later).

#	Outcome & measure	Reliability & Validity	Format	Direction	Source	Blind (outcome assessors) ?	Pg# & notes
1		Info from: Other samples This sample Unclear  Info provided:	Dichotomy Continuous	High score or event is  Positive Negative Can't tell	Questionnaire Admin data Other (specify) Unclear	Yes No Can't tell	

\* Repeat as needed

## Outcome Data

### Dichotomous Outcome Data

OUTCOME	TIME POINT (s) (record exact time from exhaustion, there may be more than one, record them all)	SOURCE	VALID Ns	CASES	NON-CASES	STATISTICS	Pg. # & NOTES
		Questionnaire Admin data Other (specify) Unclear	Exhaustion	Exhaustion	Exhaustion	RR (risk ratio) OR (odds ratio) SE (standard error) 95% CI DF	
						P- value (enter exact p value if available) Chi2 Other	
			Comparison	Comparison	Comparison	Covariates (control variables, age, gender, education, ethnicity, duration dependence, labor market conditions, censoring, other)	

Repeat as needed

## Outcome Data

### Time-To-Event Outcome Data

OUTCOME	TIME POINT (s) (record exact time from exhaustion, there may be more than one, record them all)	SOURCE	Method of estimation			STATISTICS	Pg. # & NOTES
		Questionnaire Admin data Other (specify) Unclear	Non-parametric Semi-parametric Parametric			HR (hazard ratio) SE (standard error) 95% CI DF  P- value (enter exact p value if available) Chi2 Other Covariates (control variables, age, gender, education, ethnicity, duration dependence, labor market conditions, censoring, other)	

Repeat as needed

## Outcome Data

### Continuous Outcome Data

OUTCOME	TIME POINT (s) (record exact time from exhaustion, there may be more than one, record them all)	SOURCE (specify)	VALID Ns	Means	SDs	STATISTICS	Pg. # & NOTES
		Questionnaire Admin data Other (specify) Unclear	Exhaustion	Exhaustion	Exhaustion	P t F Df ES Covariates Other	
			Comparison	Comparison	Comparison		

\*Repeat as needed

## APPENDIX 10.5 ASSESSMENT OF RISK OF BIAS IN INCLUDED STUDIES

**Risk of bias table**

Item	Judgement <sup>a</sup>	Description (quote from paper, or describe key information)
1. Sequence generation		
2. Allocation concealment		
3. Confounding <sup>b,c</sup>		
4. Blinding? <sup>b</sup>		
5. Incomplete outcome data addressed? <sup>b</sup>		
6. Free of selective reporting? <sup>b</sup>		
7. Free of other bias?		
8. <i>A priori</i> protocol? <sup>d</sup>		
9. <i>A priori</i> analysis plan? <sup>e</sup>		

<sup>a</sup> Some items on low/high risk/unclear scale (double-line border), some on 5 point scale/unclear (single line border), some on yes/no/unclear scale (dashed border). For all items, record “unclear” if inadequate reporting prevents a judgement being made.

<sup>b</sup> For each outcome in the study.

<sup>c</sup> This item is only used for NRCTs and NRSs. It is based on list of confounders considered important at the outset and defined in the protocol for the review (*assessment against worksheet*).

<sup>d</sup> Did the researchers write a protocol defining the study population, intervention and comparator, primary and other outcomes, data collection methods, etc. in advance of starting the study?

<sup>e</sup> Did the researchers have an analysis plan defining the primary and other outcomes, statistical methods, subgroup analyses, etc. in advance of starting the study?

## **Risk of bias tool**

### **Studies for which RoB tool is intended**

The risk of bias model is developed by Prof. Barnaby Reeves in association with the Cochrane Non-Randomised Studies Methods Group.<sup>21</sup> This model, an extension of the Cochrane Collaboration's risk of bias tool, covers both risk of bias in randomised controlled trials (RCTs and QRCTs), but also risk of bias in non-randomised studies (NRCTs and NRSs).

The point of departure for the risk of bias model is the Cochrane Handbook for Systematic Reviews of interventions (Higgins & Green, 2008). The existing Cochrane risk of bias tool needs elaboration when assessing non-randomised studies because, for non-randomised studies, particular attention should be paid to selection bias / risk of confounding. Additional item on confounding is used only for non-randomised studies (NRCTs and NRSs) and is not used for randomised controlled trials (RCTs and QRCTs).

### **Assessment of risk of bias**

Issues when using modified RoB tool to assess included non-randomised studies:

- Use existing principle: score judgment and provide information (preferably direct quote) to support judgment
- Additional item on confounding used only for non-randomised studies (NRCTs and NRSs).
- 5-point scale for some items (distinguish “unclear” from intermediate risk of bias).
- Keep in mind the general philosophy – assessment is not about whether researchers could have done better but about risk of bias; the assessment tool must be used in a standard way whatever the difficulty / circumstances of investigating the research question of interest and whatever the study design used.
- Anchors: “1/No/low risk” of bias should correspond to a high quality RCT. “5/high risk” of bias should correspond to a risk of bias that means the findings should not be considered (too risky, too much bias, more likely to mislead than inform)

#### **1. Sequence generation**

- Low/high/unclear RoB item

---

<sup>21</sup> This risk of bias model was introduced by Prof. Reeves at a workshop on risk of bias in non-randomised studies at SFI Campbell, February 2011. The model is a further development of work carried out in the Cochrane Non-Randomised Studies Method Group (NRSMG).

- Always high RoB (not random) for a non-randomised study
- Might argue that this item redundant for NRS since always high – but important to include in RoB table ('level playing field' argument)

## 2. Allocation concealment

- Low/high/unclear RoB item
- Potentially low RoB for a non-randomised study, e.g., quasi-randomised (so high RoB to sequence generation) but concealed (reviewer judges that the people making decisions about including participants didn't know how allocation was being done, e.g. odd/even date of birth/hospital number)

## 3. RoB from confounding (additional item for NRCT and NRS; assess for each outcome)

- Assumes a pre-specified list of potential confounders defined in the protocol
- Low(1) / 2 / 3 / 4 / high(5) / unclear RoB item
- Judgment needs to factor in:
  - proportion of confounders (from pre-specified list) that were considered
  - whether most important confounders (from pre-specified list) were considered
  - resolution/precision with which confounders were measured
  - extent of imbalance between groups at baseline
  - care with which adjustment was done (typically a judgment about the statistical modeling carried out by authors)
- Low RoB requires that all important confounders are balanced at baseline (not primarily/not only a statistical judgment OR measured 'well' and 'carefully' controlled for in the analysis).
- Assess against pre-specified worksheet. Reviewers will make a RoB judgment about each factor first and then 'eyeball' these for the judgment RoB table.

## 4. RoB from lack of blinding (assess for each outcome, as per existing RoB tool)

- Low(1) / 2 / 3 / 4 / high(5) / unclear RoB item

- Judgment needs to factor in:
  - nature of outcome (subjective / objective; source of information)
  - who was / was not blinded and the risk that those who were not blinded could introduce performance or detection bias
  - see Ch.8

5. RoB from incomplete outcome data (assess for each outcome, as per existing RoB tool)

- Low(1) / 2 / 3 / 4 / high(5) / unclear RoB item
- Judgment needs to factor in:
  - reasons for missing data
  - whether amount of missing data balanced across groups, with similar reasons
  - whether censoring is less than or equal to 25% and taken into account
  - see Ch.8

6. RoB from selective reporting (assess for each outcome, NB different to existing Ch.8 recommendation)

- Low(1) / 2 / 3 / 4 / high(5) / unclear RoB item
- Judgment needs to factor in:
  - existing RoB guidance on selective outcome reporting
  - see Ch.8
  - also, extent to which analyses (and potentially other choices) could have been manipulated to bias the findings reported, e.g. choice of method of model fitting, potential confounders considered / included
  - look for evidence that there was a protocol in advance of doing any analysis / obtaining the data (difficult unless explicitly reported); NRS very different from RCTs. RCTs must have a protocol in advance of starting to recruit (for REC/IRB/other regulatory approval); NRS need not (especially older studies)
  - Hence, separate yes/no items asking reviewers whether they think the researchers had a pre-specified protocol and analysis plan.

## Confounding Worksheet

<b>Assessment of how researchers dealt with confounding</b>		
Method for <i>identifying</i> relevant confounders described by researchers:	yes no	<input type="checkbox"/> <input type="checkbox"/>
If yes, describe the method used:		
Relevant confounders described:	yes no	<input type="checkbox"/> <input type="checkbox"/>
List confounders described on next page		
Method used for controlling for confounding At design stage (e.g. matching, regression discontinuity, instrument variable): ..... ..... .....  At analysis stage (e.g. stratification, multivariate regression, difference-indifference): ..... ..... .....  Describe confounders controlled for below		

## Confounders described by researchers

Tick (yes[0]/no[1] judgment) if confounder considered by the researchers [Cons'd?]

Score (1[good precision] to 5[poor precision]) precision with which confounder measured

Score (1[balanced] to 5[major imbalance]) imbalance between groups

Score (1[very careful] to 5[not at all careful]) care with which adjustment for confounder was carried out

Confounder	Considered	Precision	Imbalance	Adjustment
Gender	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Age	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Ethnicity	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Education	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Labour market condition	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Unemployment duration	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

Unobservables <sup>22</sup>	<input type="checkbox"/>	Irrelevant	<input type="checkbox"/>	<input type="checkbox"/>
Censoring	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Other:	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

#### 10.1.4 User guide for unobservables

Selection bias is understood as systematic baseline differences between groups and can therefore compromise comparability between groups. Baseline differences can be observable (e.g., age and gender) and unobservable (to the researcher; e.g., motivation and ‘ability’). There is no single non-randomised study design that always solves the selection problem. Different designs solve the selection problem under different assumptions and require different types of data. Especially how different designs deal with selection on unobservables varies. The “right” method depends on the model generating participation, i.e., assumptions about the nature of the process by which participants are selected into a programme.

As there is no universal correct way to construct counterfactuals, we will assess the extent to which the identifying assumptions (the assumption that makes it possible to identify the counterfactual) are explained and discussed (preferably the authors should make an effort to justify their choice of method). We will look for evidence that authors using e.g. (this is NOT an exhaustive list):

##### **Natural experiments:**

Discuss whether they face a truly random allocation of participants and that there is no change of behavior in anticipation of e.g. policy rules.

##### **Instrument variable (IV):**

Explain and discuss the assumption that the instrument variable does not affect outcomes other than through their effect on participation.

##### **Matching (including propensity scores):**

Explain and discuss the assumption that there is no selection on unobservables, only selection on observables.

##### **(Multivariate) Regression:**

Explain and discuss the assumption that there is no selection on unobservables, only selection on observables. Further discuss the extent to which they compare comparable people.

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<sup>22</sup> See user guide for unobservables

**Regression Discontinuity (RD):**

Explain and discuss the assumption that there is a (strict!) RD treatment rule. It must not be changeable by the agent in an effort to obtain or avoid treatment. Continuity in the expected impact at the discontinuity is required.

**Difference-in-difference (Treatment-control-before-after):**

Explain and discuss the assumption that outcomes of participants and nonparticipants evolve over time in the same way.

# 11 Data and analysis

## 11.1 SENSITIVITY ANALYSIS

*Figure 11.1 – Sensitivity Analysis: Week/Month of Exhaustion*

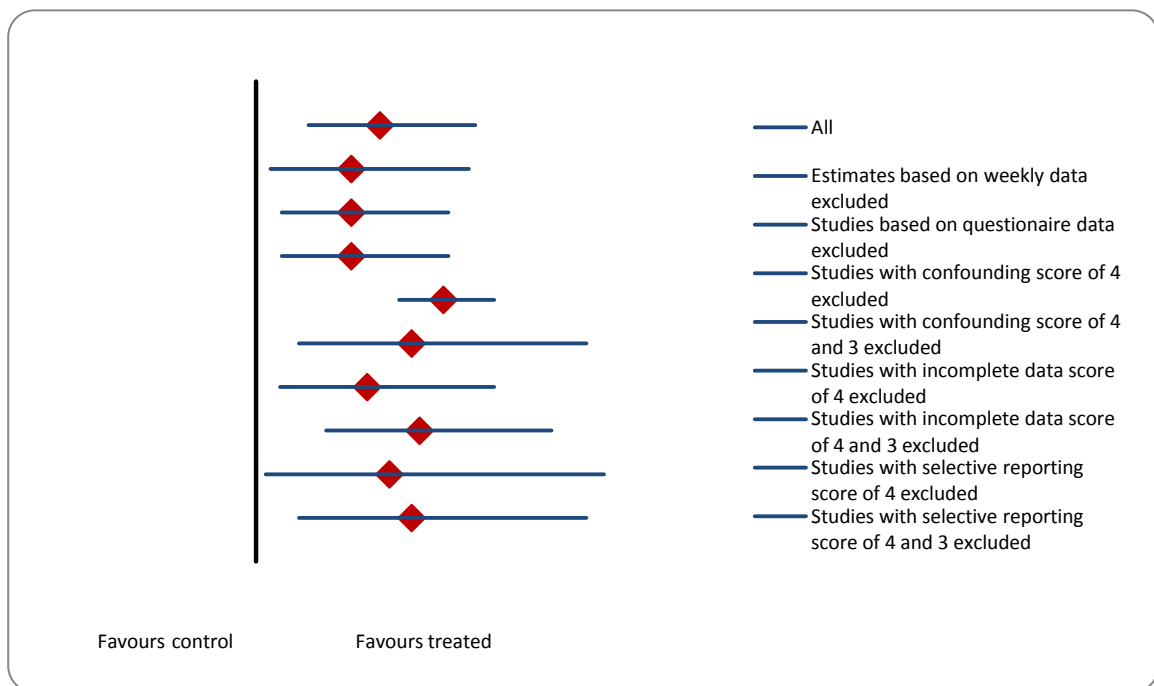
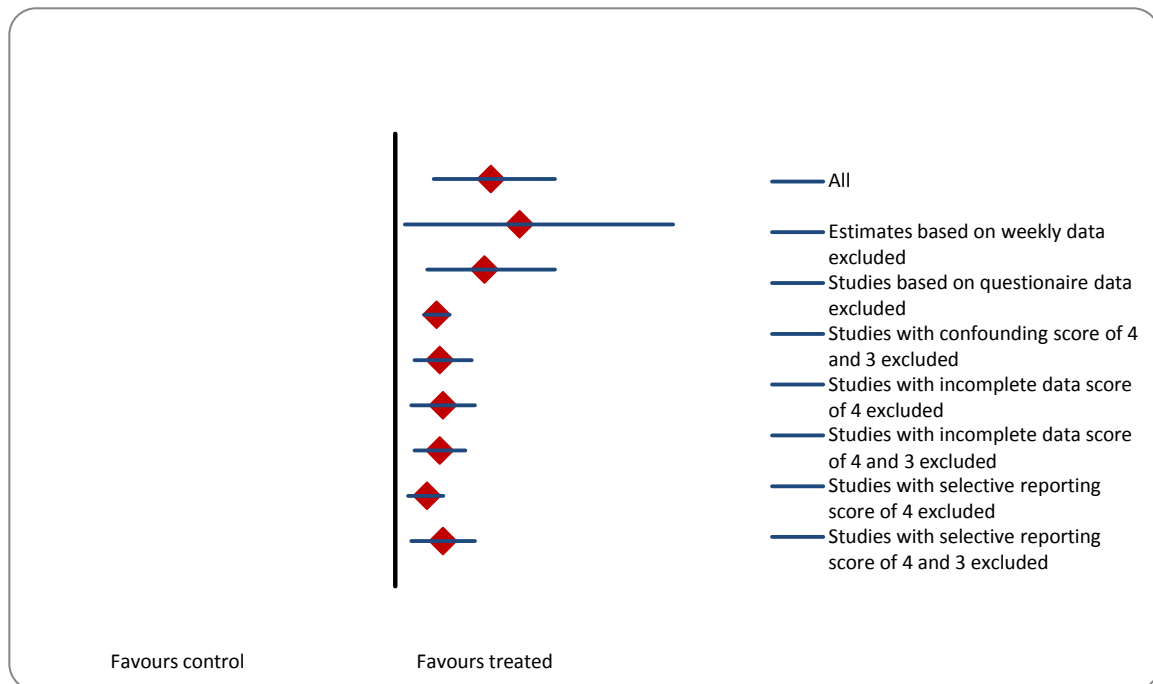


Figure 11.2 – Sensitivity Analysis: 1 Month Before Exhaustion

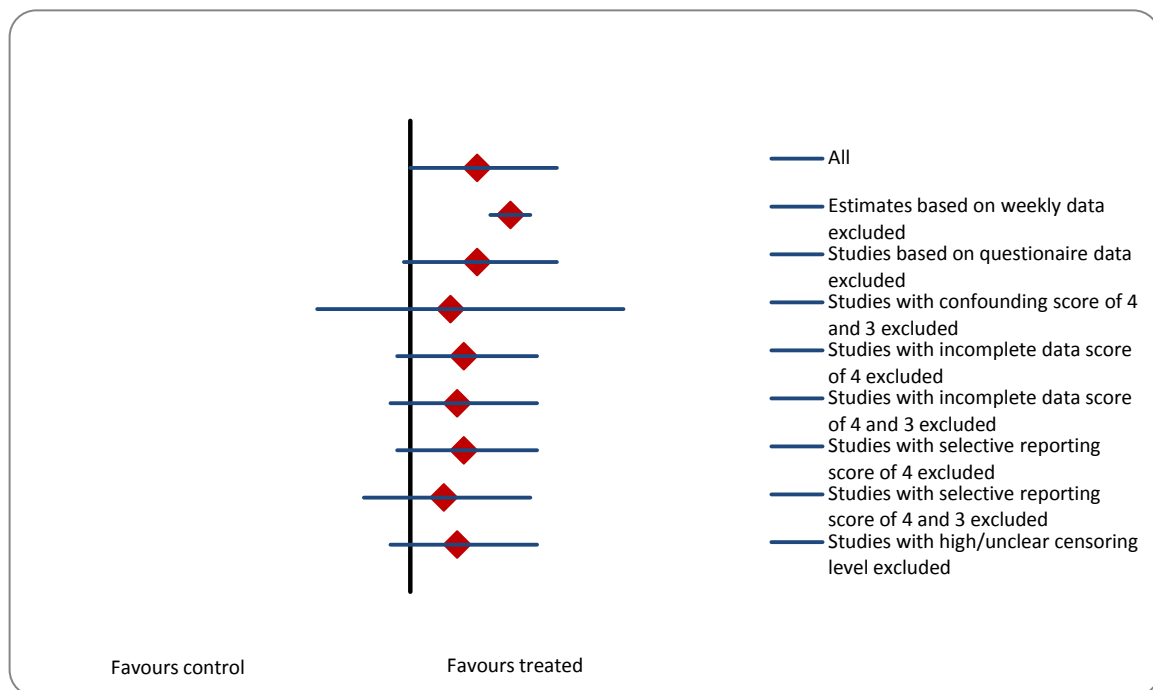


### Technical comment

The exhaustion effect estimate one month before exhaustion is sensitive to the exclusion of studies with a high/unclear censoring level and studies with a score of 3 and 4 in the confounding, incomplete data, and selective reporting components of the risk of bias checklist in the sense that the confidence intervals narrow.

These very narrow confidence intervals are due to the dominance of one (or two) very precisely estimated effect size(s) which has a weight of nearly 100% in the calculation of the pooled effect size. The dominance of this (or these) precise estimated effect size(s) does not appear when all studies are included because among the excluded studies (with a risk of bias score of 3 and 4 or high censoring level) is a relatively high effect size. When this effect size is excluded the estimated  $\tau^2$  falls to zero and the weighting is less balanced.

Figure 11.3 – Sensitivity Analysis: 2 Months Before Exhaustion



### Technical comment

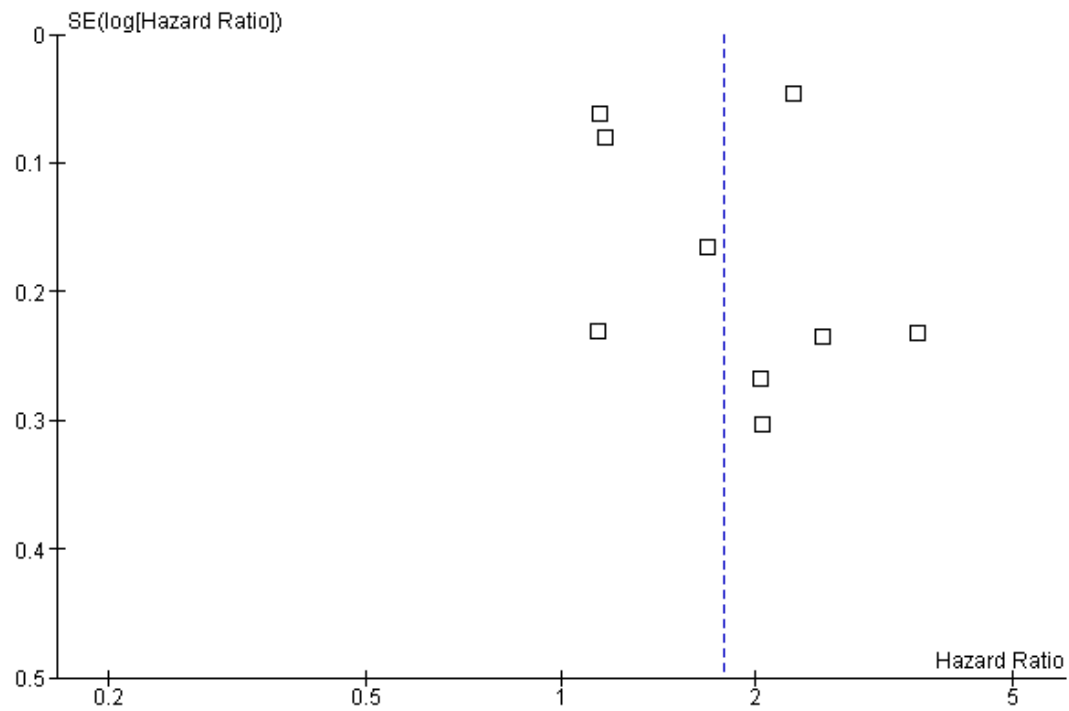
The exhaustion effect estimate two months before exhaustion is sensitive to the exclusion of studies where the effect estimates are based on weekly data in the sense that the confidence interval becomes very narrow and the point estimate, which only increases slightly, is now significant. The very narrow confidence interval is due to the dominance of one very precise estimated effect size, which has a weight of nearly 100% in the calculation of the pooled effect size. The dominance of this precise estimated effect size does not appear when all studies are included because among the excluded studies (based on weekly data) are two relatively small effect sizes. When they are excluded, the estimated  $\tau^2$  falls to zero and the weighting is less balanced.

---

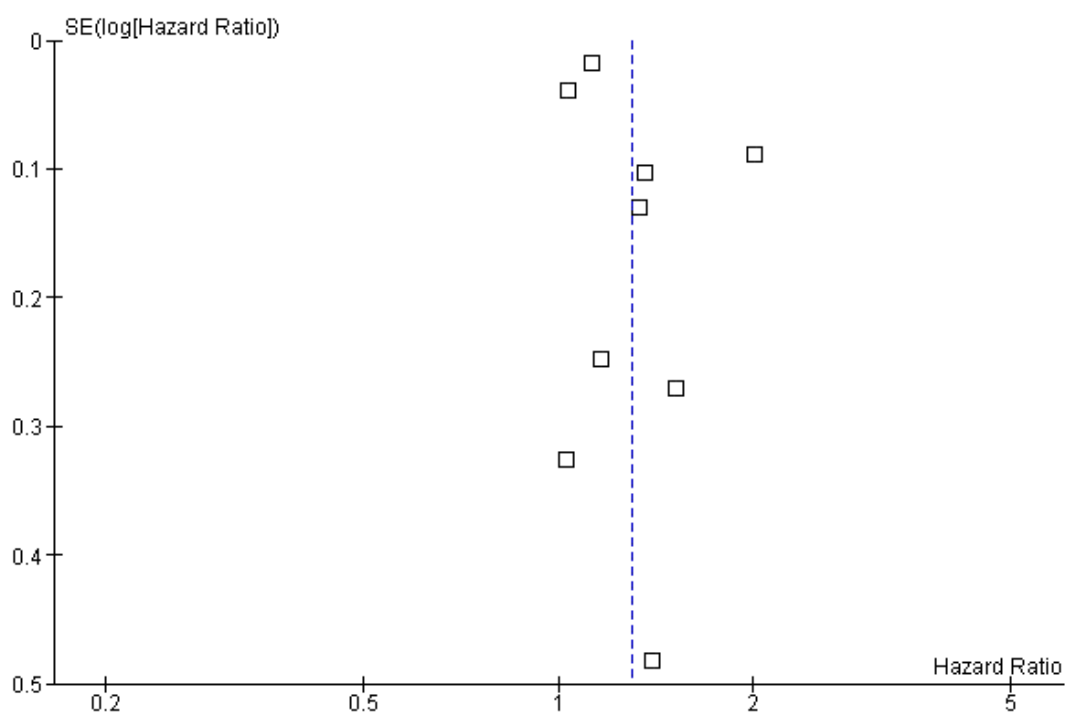
## 11.2 PUBLICATION BIAS

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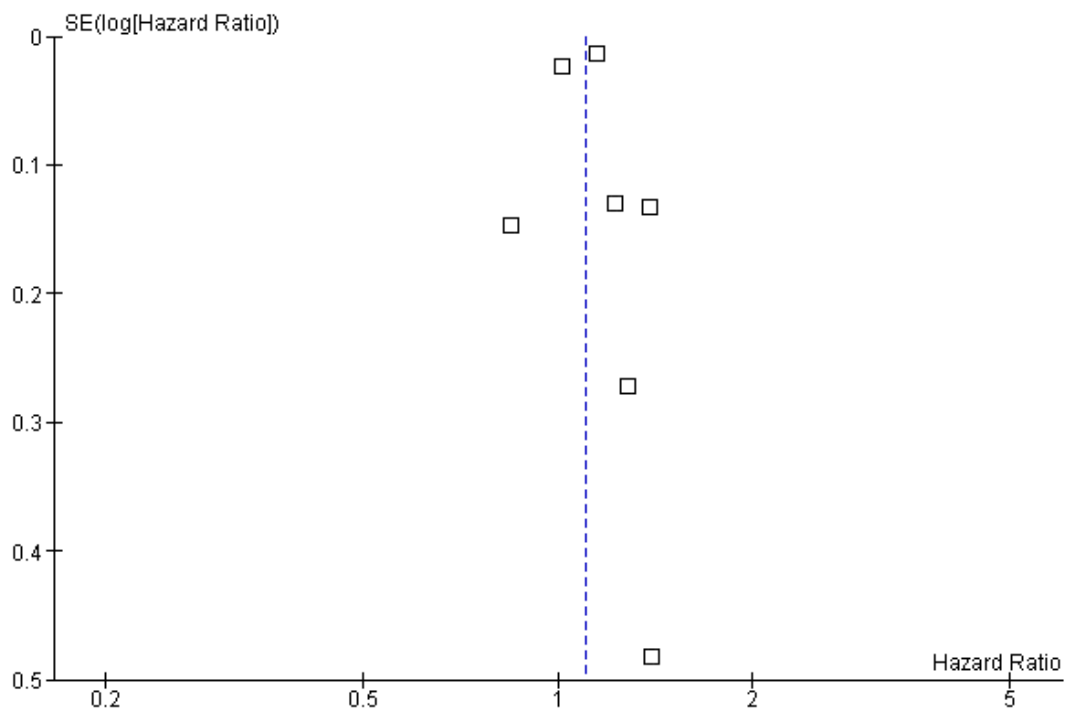
*Figure 11.4 – Funnel Plot: Week/Month of Exhaustion*



*Figure 11.5 – Funnel Plot: 1 Month Before Exhaustion*



*Figure 11.6 – Funnel Plot: 2 Months Before Exhaustion*



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# Supplement

## **Unemployment Benefit Exhaustion: Incentive Effects on Job Finding Rates**

Trine Filges, Lars Pico Geerdsen, Anne-Sofie Due Knudsen,  
Anne-Marie Klint Jørgensen, & Krystyna Kowalski

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## 1. DATA EXTRACTION

### 1.1. Descriptive data for studies with effect estimate

Author	Korpi	van Ours, Vodopivec	Portugal, Addison
Year	1995	2006	2008
Country	Sweden	Slovenia	Portugal
Language	English	English	English
Journal	Economica	Journal of Labor Economics	Scottish Journal of Political Economy
Time period covered by data	1981-1985	August 1, 1997- December 31, 1999	1992(2)-1997(4)
Type of data used	Questionnaire	Administrative registers	Questionnaire
Type of unemployment scheme	UI and UA	UI	UI
Target group	Persons between 16 and 24 who were registered as unemployed at employment agencies in the County of Stockholm in the beginning of 1981. (p. 156)	NA	16-64 years of age, men only (p.398)
Target group - eligibility, requirements for benefits	Persons between 16 and 24 who were registered as unemployed at employment agencies in the County of Stockholm in the beginning of 1981. (p. 156)	Has to register with an unemployment office and depends on work history (p. 355)	Under the Portuguese law, individuals have to have been employed for at least 18 months during the 2 years before the unemployment event to draw UI benefits proper. (p. 396)
Benefit level/replacement rate	NA	Earnings related (p. 354)	65% of the previous wage (p. 397)
Benefit level/replacement rate available after exhaustion	NA	UA - means-tested (p. 355)	Between 70% and 100% of the minimum wage (p. 397)
Is compulsory activation part of the system?	Yes - public labour market training programmes and public relief jobs. (p. 355)	Not compulsory, but unemployed has the opportunity to participate in ALMP activities. (p. 355)	NA

Maximum duration of unemployment benefits	Seven (UA) and 14 (UI) months (p. 355)	Before the reform: 3 months with 0-1.5 years of experience, 6 months with 1.5-5, 9 months with 5-10 years, 12 months with 10-15 years and 18 months with 15-20 years of work experience. After the reform: 3 months with 0-5, 6 months with 5-15 and 9 months with 15-20 (p. 361)	10 months if <25 years, 12 months for those aged between 25 and 29 years, it then increases in 3-month intervals for each incremental 5 years of age, up to a maximum of 30 months at age 55. (p. 397)
Destination	Permanent, temporary jobs, labour market programs	The study distinguishes between job finding and other exit destinations. (p. 358)	Open-ended employment, fixed-term contract, part time, self-employment, public employment, inactivity (table 5 p. 411)
Sampling frequency	Participants were interviewed four times: spring 1981, autumn 1981, spring 1982 and 1985 (p.356) OR 'labour market history' was collected every week between 1981 to 1982 and then monthly until 1985 (p.356)	Daily	Weekly
Time interval the outcome measure is based on	Monthly	Monthly	Weekly
Sample size	830 individuals (p. 356)	9,196 males, 10,853 females (p. 358)	9451 individuals (p. 398)
Are the labor market conditions described?	NA	Since 1995, its unemployment rate has remained remarkably stable, at a level of 6% - 7%. (p. 354)	Yes - over the sample period, Portuguese unemployment rose by almost two-thirds - from 4.1% to 6.7% (p. 403)
Is there correction for unobserved heterogeneity?	NA	Yes (p. 374)	Yes (p. 401)
Censoring level	NA	NA	Approx. 80 %
Is there correction for censoring?	Yes	Yes (p. 371)	Yes (p. 401)

Author	Adamchik	Jenkins, García-Serrano	Card, Chetty, Weber
Year	1999	2004	2007
Country	Poland	Spain	Austria
Language	English	English	English
Journal	Oxford bulletin of economics and statistics	Oxford bulletin of economics and statistics	AEA papers and proceedings
Time period covered by data	February 1994-February 1996	1987-1993	1981-2001
Type of data used	Questionnaire	Administrative registers	Administrative registers
Type of unemployment scheme	UI	UI	UI
Target group	Unemployed, duration less than 12 months	Men aged 20-59 years (p. 244)	Individuals between the ages 20 and 50 and who take up UI benefits within 28 days of job loss and who have worked at their prior employer for at least one year, have worked for between 33 and 38 months in the past 5 years. (p. 114)
Target group - eligibility, requirements for benefits	The individual is required to have worked for at least 180 days during the last year (p.95)	UI was only paid to employees (excluding civil servants and domestic workers) who did not quit their job voluntarily, and who had made contributions for at least 6 months over the previous 48 months. (p. 242)	In Austria, job losers who have worked for 12 months or more in the preceding two years are eligible for benefits... (p. 114)
Benefit level/replacement rate	Benefits were almost equal to the official minimum wage (p. 96). 36 % of the economy-wide average wage in the previous quarter (p.95)	78% in the first 6 months, 67% in months 7-12, 65% thereafter (net replacement rates were quite a lot higher) (p. 247)	55% of the previous (after-tax) wage. (p. 114)
Benefit level/replacement rate available after exhaustion	Social assistance benefits - 28% of the economy-wide average wage in the previous quarter (p. 96)	UA	UA - on average 38% of the UI benefit level (p. 7 in discussion paper)
Is compulsory activation part of the system?	NA	NA	NA
Maximum duration of unemployment benefits	12 months (p. 98)	Table 2 (p. 248)	Individuals with fewer than 36 months of employment can receive up to 20 weeks of benefits, while those who have worked for 36 months or more can receive 30 weeks of UI. (p. 114)

Destination	Employment, not in the labour force, remained unemployed (table 1, p. 97)	Employment	Job or unemployment exit
Sampling frequency	Quarterly (p. 96)	Monthly	Weekly
Time interval the outcome measure is based on	Quarterly	Monthly	Weekly
Sample size	7,339 persons (table 1, p. 97)	329,947 spells (p. 245)	92,969 spells - 47% are eligible for 20 weeks of UI, and 53% are eligible for 30 weeks of UI. (p. 114)
Are the labor market conditions described?	Mid 1990s = 14-16% unemployed (p. 95)	Yes - table 1 (p. 246)	Yes - unemployment rate of an average of 4.1% over the 1993-2004 period (p. 6 in discussion paper)
Is there correction for unobserved heterogeneity?	NA	"We never found the heterogeneity variance to be statistically significant, and all other parameter estimates were very similar to the corresponding ones in models excluding unobserved heterogeneity. (p. 244)	NA
Censoring level	73% (p. 97)	Censoring level is unclear but probably 0 (spells ending in exhaustion is treated as censored (59%), 27 spells ended in employment and 14% ended for other reasons).	0,06
Is there correction for censoring?	Yes	Yes	Yes

Author	Jurajda, Tannery	Addison, Portugal	Belzil
Year	2003	2004	2001
Country	USA	USA	Canada
Language	English	English	English
Journal	Industrial and Labor Relations Review	IZA Discussion Paper	Journal of Applied Econometrics
Time period covered by data	January 1980-December 1985	1996-1998	1976-1984
Type of data used	Administrative registers + Questionnaire	Questionnaire	Administrative registers
Type of unemployment scheme	UI with extension (EB + FSC)	UI	UI
Target group	Table 1 (p. 330)	Included: those who wanted but never found employment after losing their jobs and those who transitioned directly into reemployment. Excluded: individuals who were not economically active at the time of the Questionnaire, part-time workers, employed in agriculture, aged less than 20 years and above 61 years. (p. 9-10)	Young males between 18 and 25 years (p. 625)
Target group - eligibility, requirements for benefits	In order to collect the EB or FSC benefit extensions, the unemployed first have to exhaust their regular UI benefits. (p. 332)	NA	Basically, those individuals who have worked more than a minimum number of weeks (10 to 14 weeks depending on the regional unemployment rate) can qualify for unemployment benefit. (p. 623)
Benefit level/replacement rate	NA	NA	66% of insurable earnings (p. 624)
Benefit level/replacement rate available after exhaustion	NA	NA	NA
Is compulsory activation part of the system?	NA	NA	NA

Maximum duration of unemployment benefits	Regular UI benefits: 30 weeks before 1984 and 26 weeks after 1984 (p. 331)	26 or 30 weeks	Difference between "before September 1977" and "After September 1977" is 3 weeks. (p. 623-624). The mean initial benefit duration in data is 33 weeks but with SD=14 weeks. (p. 625) It is calculated based on the number of weeks worked during the previous year, up to a certain maximum which may depend itself on the local rate of unemployment (p.624)
Destination	Job or recall	Employment	Job (p. 625)
Sampling frequency	Weekly	Weekly	Weekly
Time interval the outcome measure is based on	Weekly	Weekly	Weekly
Sample size	6,658 spells for 5,134 individual workers (p. 329)	2,762 individuals (p. 10)	909 individuals (p. 625)
Are the labor market conditions described?	Yes - unemployment rate reaching 9.9% in Philadelphia and 16.9% in Pittsburgh (p. 325)	NA	NA
Is there correction for unobserved heterogeneity?	Yes (p. 328) - flexible nonparametric approach	Yes (p. 6)	Yes (p. 629)
Censoring level	14.4% (p. 329)	7-13%	0,02
Is there correction for censoring?	Yes	Yes (p. 6)	Yes (p. 629)

Author	Terrell, Sorm	Schmieder, von Wachter, Bender	Vodopivec
Year	1999	2009	1995
Country	Czech Republic	Germany	Slovenia
Language	English	English	English
Journal	Journal of Comparative Economics	Columbia University, Department of Economics	Policy research working paper
Time period covered by data	October 1992-September 1994	1987-1999	1990-1992
Type of data used	Administrative registers	Administrative registers	Administrative registers
Type of unemployment scheme	UI	UI	UI
Target group	Table 3 (p. 44)	Age range 40 to 49	NA
Target group - eligibility, requirements for benefits	Anyone who worked for at least 12 months in the preceding 3 years was immediately eligible for unemployment benefits. (p. 38)	Workers who lose their job without fault (p. 13)	At least nine-months of uninterrupted employment has been required. (p. 5)
Benefit level/replacement rate	60% of the person's previous net wage (p. 38)	63 % of previous net earnings (individual without children)	Before February 1991: 60% (except 80% for those unemployed due to bankruptcy). After February 1991: 70% the first 3 months, 60% thereafter. (p. 25)
Benefit level/replacement rate available after exhaustion	Social assistance (p. 38)	53 %, but UA are reduced substantially by spousal earnings and other sources of income.	Before February 1991: Means-tested and equal to minimum wage. After February 1991: Means-tested and equal to 80% of minimum wage (p. 25)
Is compulsory activation part of the system?	No	NA	NA
Maximum duration of unemployment benefits	6 months (p. 38)	Depends on the age at the beginning of the UI spell.	Depends on the duration of previous employment - from 3 to 24 months. (p. 5) Table 2 (p. 25)
Destination	Employment (p. 39)	Employment	Employment or non-employment (p. 10)
Sampling frequency	Weekly (p.35)	Day-to-day	Daily

Time interval the outcome measure is based on	Weekly	Monthly	Monthly
Sample size	Recipients = 1689 individuals. Nonrecipients = 1218 individuals. (Table 3, p.44)	351731 spells	23,242 spells (table 3, p. 27)
Are the labor market conditions described?	Yes (table 1, p. 34)	NA	Yes (p. 3 and table 1, p. 24)
Is there correction for unobserved heterogeneity?	Yes (p. 40)	No	No
Censoring level	NA	0,23	0,56
Is there correction for censoring?	Yes	Yes (p. 28 + 59)	Yes

Author	Boeri, Steiner	Caliendo, Tatsiramos, Uhlenndorff	Van Ours, Vodopivec
Year	1998	2009	2004
Country	Poland	Germany	Slovenia
Language	English	English	English
Journal	Konjunkturpolitik: Zeitschrift für angewandte Wirtschaftsforschung	Deutsches Institut für Wirtschaftsforschung	IZA Discussion Paper
Time period covered by data	January 1990-December 1993	2001-2003	Unemployment spells that started during August 1, 1997-July 31, 1998 and January 1, 1998-December 31, 1999
Type of data used	Administrative registers	Administrative registers	Administrative registers
Type of unemployment scheme	UI	UI	UI
Target group	NA	Sample A: Restricts the sample to men and women from West Germany who have been employed for at least 36 months in the last seven years when entering unemployment. Restricts the sample to men aged between 44 and 46 and between 43.5 and 46.5 for women. Sample B: individuals who have been working for 12 months in regular employment in the year prior to entering unemployment. (p. 10) Table A.1 and A.2 p. 30-31	21-50 years of age (p.9)
Target group - eligibility, requirements for benefits	Before the change in 1992: almost universal coverage and open-ended. After the change: benefits was confined to those with a least 180 working days in the last year. (p. 290)	To generate a claim for UB workers had to be employed for at least 12 months in the last three years before entering unemployment. (p. 6)	NA
Benefit level/replacement rate	Before the change: benefit levels were earnings-related. After the change: flat-rate system, providing to everybody 36 % of the average wage in the previous quarter. (p. 290)	The replacement rate depends on family status. Unemployed persons with at least one child are entitled to 67 % of previous net remuneration and 60 % otherwise, individual means or needs are not taken into account. (p. 6)	Average UB amounting 37% of the average wage (p. 6)

Benefit level/replacement rate available after exhaustion	Only a relatively small group would seem to qualify for social assistance - if not qualified the individual will experience significant income losses. The shift from unemployment benefit rolls to social assistance involves a reduction in cash transfers of about 20 percent. (p. 292)	UA = 57 %/53 % with/without children. SA = means-tested and flat-rate basis. (p. 6-7)	Unemployment assistance - means-tested (p. 5)
Is compulsory activation part of the system?	NA	NA	No
Maximum duration of unemployment benefits	Before the change: open-ended. After the change: 12 months (p. 290)	The duration depends on age and previous employment duration. At the age of 45 (which is the focus in this study) the maximum benefit duration increases by 6 months - from 12 to 18 months given the workers have been employed for at least 36 months in the last seven years. (p. 6)	Before the reform: 5-10 years of work experience=9 months, 10-15 years=12 months. After the reform: 6 months for both groups (p. 6)
Destination	Employment or non-participation	Employment	Job or other destinations
Sampling frequency	Monthly	Daily basis (p. 10)	Daily
Time interval the outcome measure is based on	Monthly	Monthly (p. 10)	Monthly
Sample size	NA	Sample A: 3,432 males and 3,784 females. Sample B: 2,200 males and 2,700 females. (p. 10)	12,752 males, 17,585 females (p. 21)
Are the labor market conditions described?	The choice of Warsaw and Ciechanov aims at capturing two polar cases of labour market adjustment. (p. 293)	NA	NA
Is there correction for unobserved heterogeneity?	No	Yes (p. 14)	Yes (p. 12)
Censoring level	NA	25-30 % (p. 11)	NA
Is there correction for censoring?	Yes	Yes (p. 8, 12, 16)	Yes

Author	Schmitz, Steiner	Sanz	Katz, Meyer
Year	2007	2010	1990
Country	Germany	Spain	USA
Language	English	English	English
Journal	IZA Discussion Paper	ECON working paper	The Quarterly Journal of Economics,
Time period covered by data	1995-2003	2000-2007	1979-1981
Type of data used	Questionnaire	Administrative registers	Administrative registers + Questionnaire
Type of unemployment scheme	UI	UI	UI
Target group	See table A3 for sample characteristics	Spanish workers aged between 18 and 55. (p. 12) Main sample statistics in table 2 (p. 31)	UI recipients from Missouri (p. 7)
Target group - eligibility, requirements for benefits	Requirements are determined according to age, see table 1 (p.3) One has to be registered as unemployed at the local labour office, be not older than 65 years and available for work on short notice and prepared to accept "suitable" job offers. (p. 3)	All employees who involuntarily become unemployed are entitled to UI benefits, provided that they were employed for at least 12 months over the 72-month period prior to unemployment. (p. 5)	NA
Benefit level/replacement rate	Individuals with children: 67% of their former net income, without children: 60% (p. 4)	70% of average basic pay for the first 4 months - 60% from the fifth month onwards. (p. 5)	Maximum weekly benefit was \$105 in Missouri (p. 13)
Benefit level/replacement rate available after exhaustion	UA - with children: 57% of former net earnings, 53% without children (p. 4)	UA benefits (p. 6)	NA
Is compulsory activation part of the system?	NA	NA	NA
Maximum duration of unemployment benefits	Depends on the number of months worked in the last seven years and the age of the claimant - see table 1 (p. 3)	At least 4 months extendable in 2-monthly periods up to a maximum of 2 years, depending on the worker's employment record. (p. 6)	26 weeks in Missouri. After May 1980: 45 weeks in Missouri. (p. 13)
Destination	Employment or out-of-the-labour-force (p. 8)	Recall or new job entry (p. 22)	Recall or new job

Sampling frequency	Monthly (p. 8)	Monthly	Weekly
Time interval the outcome measure is based on	Monthly (p. 8)	Monthly	Weekly
Sample size	7,348 spells used - 4,612 individuals (p. 8)	NA	756 individuals
Are the labor market conditions described?	NA	Yes (table 2, p. 31)	NA
Is there correction for unobserved heterogeneity?	Yes (p. 5)	Yes (p. 15)	Yes (p. 31)
Censoring level	0,19	10-13%	9.3% (p. 24)
Is there correction for censoring?	Yes (p. 7)	Yes	Yes (p. 26)

Author	Arranz, Bulló, Muro	Jones	Boone, van Ours
Year	2008	1995	2009
Country	Spain	Canada	Slovenia
Language	English	English	English
Journal		Human Resources Development Canada	IZA Discussion Paper
Time period covered by data	1987-1997	January 31-March 7 1992 + April 25-June 5 1992	1997-20011
Type of data used	Administrative registers	Administrative registers + Questionnaire	Administrative registers
Type of unemployment scheme	UI	UI	UI
Target group	18-59 years-old who started receiving UI in 1991 and in 1993 (p. 15) + sample characteristics in table 3 (p. 16)	NA	Table B1 (p. 30)
Target group - eligibility, requirements for benefits	Eligible for UI are workers whose unemployment situation is recognized according to law by the labour authority; i.e., the job was lost involuntarily, including end of a fixed-term contract. Before the reform: Social Security contributions for a minimum of six months during the four years preceding unemployment. After the reform: contributions for a minimum of twelve months during the six years preceding unemployment. (p. 10)	Canadian employers are required to issue a Record of Employment form whenever a job separation occurs. (p. 10)	NA
Benefit level/replacement rate	Before the reform: 80% the first 6 months, 70% from 7. to 12. month, 60% from 13. onwards. After the reform: 70% the first 6 months, 60% the rest of the period (p. 12)	Cohort 1: 60%, cohort 2: 57% (p. 10-11)	70% of the previous wage first 3 months and 60% thereafter (p. 14)
Benefit level/replacement rate available after exhaustion	75% of statutory minimum wage (p.47)	NA	NA
Is compulsory activation part of the system?	NA	NA	NA

Maximum duration of unemployment benefits	Before the reform: 6-12 months contribution=3 months, 13-18 months contribution=6 months -> 48+ contributions=24 months. After the reform: 12-17 months contribution=4 months, 18-23 months contribution=6 months -> 72+ contributions=24 months. (p. 10 + table 1, p. 11)	NA	The analysis focuses on individuals that were entitled to benefits for a maximum duration of 6, 9 or 12 months (p. 14)
Destination	Employment	Employment (p. 14)	Permanent and temporary jobs (p. 16)
Sampling frequency	Monthly	Weekly (p. 10)	Daily
Time interval the outcome measure is based on	Monthly	Weekly	Monthly
Sample size	1991: 42,029 individuals, 1993: 35,845 individuals (p. 18)	Cohort 1: 5,465 individuals, cohort 2: 5,694 individuals (p. 38)	5,583 males, 6,4778 females (p. 30)
Are the labor market conditions described?	Unemployment rate above 15% after the 1992 crisis (p. 7)	NA	NA
Is there correction for unobserved heterogeneity?	Yes (p. 29)	Yes (p. 26)	Yes (p. 21)
Censoring level	77.5% in the 1991-sample, 73% in the 1993-sample (p. 18)	0,61	28-34%
Is there correction for censoring?	Yes (p. 29)	Yes (p. 16 note 8)	Yes (p. 17)

## 1.2. Descriptive data for studies without effect estimate

Author	Gonzalo	Røed, Zhang	Lee, Wilke
Year	2002	2003	2009
Country	Spain	Norway	Germany
Language	English	English	English
Journal	Applied Economics	The Economic Journal	Journal of business & economic statistics
Time period covered by data	First quarter of 1989-last quarter of 1991	1990s	1975-1997
Type of data used	Questionnaire	Administrative registers	Administrative registers
Type of unemployment scheme	UI	UI	UI
Target group	Unemployed men with labour experience in activities of industry and services with age between 25 and 55 years who belong to the group of wage earners in the private sector. (p. 2179) (table B1 p. 2187)	All workers below 60 years of age who became unemployed in Norway during the 1990s, who has a full time job prior to the unemployment spell and who were eligible for unemployment benefits to start with. (Table 1 p. 193)	Individuals aged 44 to 48 years as the reform affects individuals older than 42 and the group 42-43 gets a short extension and therefore is a bad treatment group. (p. 195) The sample is restricted to males (p. 196)
Target group - eligibility, requirements for benefits	Individuals who involuntarily left their job do not have any right to get UI benefits. (p. 2180)	The Norwegian unemployment insurance system is compulsory. The only condition for eligibility is a previous yearly earned income above a fairly low threshold. (p. 192)	An unemployed with sufficient amount of working experience. (p. 194)
Benefit level/replacement rate	NA	The unemployment benefit is calculated as 62.4 % of the labour earnings in the previous calendar year. (p. 192)	NA
Benefit level/replacement rate available after exhaustion	NA	Means-tested social security support. (p. 192)	Unemployment assistance - depends on previous earnings and it is means tested. (p. 195)
Is compulsory activation part of the system?	NA	NA	NA

Maximum duration of unemployment benefits	Depends on tenure, table 2 (p.2180)	Benefits can be maintained for up to 156 weeks. (p. 192)	Maximum length increased from 12 to 22 months for the treatment group - remained constant for the control group (12 months). (p. 195)
Destination	Wage employment (p. 2180)	NA	No specific exit, though recalls are not considered (p.9)
Sampling frequency	Quarterly	Monthly (p. 195)	Daily (p. 195)
Time interval the outcome measure is based on	Quarterly	Monthly	Daily
Sample size	8,873 observations (p. 2187)	58,625 men and 41,874 women (p. 193)	In total: 4,049 spells, of which 2,922 are recorded during the prereform period (p. 197)
Are the labor market conditions described?	Unemployment rate of 18.9 % in the period 1989-1994. (p. 2177)	NA	NA
Is there correction for unobserved heterogeneity?	Yes (p. 2182)	Yes (p. 195)	NA
Censoring level	27.80 % (p. 2180)	NA	Table 2 + 3 (p. 197)
Is there correction for censoring?	Yes (p. 2182)	Yes (p. 197)	Yes (p. 198)

Author	Lindeboom, Theeuwes	Stancanelli	Ham, Rea
Year	1993	1999	1987
Country	Netherlands	Britain	Canada
Language	English	English	English
Journal	Economica	Applied Economics	Journal of Labor Economics
Time period covered by data	October 1982-October 1984	Summer 1983-Autumn 1984	January 1975-December 1980
Type of data used	Administrative registers	Questionnaire	Administrative registers
Type of unemployment scheme	UI	UI	UI
Target group	Unemployed, see table A1 (p. 342)	Either married men or single people of either gender living on their own or with their children; they were aged between 20 and 58; they had been "signing on" continuously for three months following the start of their registered unemployment spell. (p. 1045)	Males aged 18-64 years (p. 338)
Target group - eligibility, requirements for benefits	Involuntarily unemployed private-sector employees who worked at least 130 days in the year preceding unemployment. (p. 331)	NA	NA
Benefit level/replacement rate	Approx. 80% of gross earnings before unemployment, up to a maximum of 210 guilders a day (p. 331)	Flat rate and almost equal to social assistance payments (p. 1044)	Before January 1976: 75% for those with dependents, 67% for all claimants. After January 1976: 67%/60% of insurable earnings. (p. 327)
Benefit level/replacement rate available after exhaustion	Unemployment provision - 75% of previous earnings. (p. 331)	Social assistance payments - very close in amount to UI (p. 1044)	NA
Is compulsory activation part of the system?	NA	NA	NA
Maximum duration of unemployment benefits	130 working days = 26 weeks (p. 331)	52 weeks before 1996 (p. 1043)	52 weeks (p. 339)
Destination	NA	Fulltime job (p. 1048)	Job or recall (p. 339)
Sampling frequency	Weekly	Weekly	Weekly (p. 334)

Time interval the outcome measure is based on	Weekly	Weekly	Weekly
Sample size	114 recipients (table A1, p. 343)	1941 unemployed males (p. 1045)	1058 spells, 282 individuals (p. 339)
Are the labor market conditions described?	NA	NA	NA
Is there correction for unobserved heterogeneity?	Yes	Yes (p. 1047)	Yes (p. 333)
Censoring level	0,32	NA	16% (p. 339)
Is there correction for censoring?	Yes (p. 333)	Yes (p. 1047)	Yes (p. 333)

Author	Katz, Meyer	Lubyova, van Ours	Van Ours, Vodopivec
Year	1990	1997	2006
Country	USA	Slovakia	Slovenia
Language	English	English	English
Journal	Journal of Public Economics	European Economic Review	World Bank Policy Research Working Paper
Time period covered by data	1978-1983	1991/92, 1994/95	Unemployment spells that started during August 1, 1997-July 31, 1998 and January 1, 1998-December 31, 1999
Type of data used	Administrative registers	Administrative registers	Administrative registers
Type of unemployment scheme	UI	UI	UI
Target group	Males from 12 states (p. 57)	NA	For sample statistics see table 2 and 3 (p.27-28)
Target group - eligibility, requirements for benefits	NA	NA	The scheme covers most of the working force. The unemployed has to make themselves available several hours a day and register with an unemployment office. (p.5-6)
Benefit level/replacement rate	NA	Before 1992: 65% of previous wage the first 6 months and 60% the last 6 months. After 1992: 60%/50% in the first/last 3 months. After 1995: Back to pre-1992 level (p. 927)	Earnings related (p. 5)
Benefit level/replacement rate available after exhaustion	NA	NA	Unemployment assistance - means-tested (p. 6)
Is compulsory activation part of the system?	NA	NA	NA
Maximum duration of unemployment benefits	Most states provided 26 weeks of benefits (p. 58)	Before 1992: 12 months. After 1992: 6 months. After 1995: <30 years=no change, 30-45 years=8 months, >45 years=9 months (p. 927)	Before the reform: 5-10 years of work experience=9 months, 10-15 years=12 months. After the reform: 6 months for both groups (p. 6)
Destination	Recall or employment	Regular job or other reasons (e.g. subsidized jobs, retraining, school) (p. 928)	Employment - focus on secondary outcomes as duration, quality and wage

Sampling frequency	Weekly (p. 60)	Monthly	Daily
Time interval the outcome measure is based on	Weekly	Monthly	Monthly
Sample size	3365 males (p. 57)	1991/92: 10,790 observations. 1994/95: 18,603 observations (p. 927)	Males = 6630, females=7245. Wage regression: males=2973, females=3111 (p. 18)
Are the labor market conditions described?	NA	Yes (p. 926)	Yes (p. 5)
Is there correction for unobserved heterogeneity?	NA	Yes (footnote p. 931)	Mentioned in table 3 and 4 concerning duration of the job and wages
Censoring level	0,2	NA	NA
Is there correction for censoring?	Yes	Yes (p. 929)	Yes

Author	Arranz, Muro	Cockx, Ries	Layte, Callan
Year	2004	2004	2001
Country	Spain	Belgium	UK
Language	English	English	English
Journal	Revista de Economica Pública	CESIFO working paper	The Economic and Social Review
Time period covered by data	Individuals who entered UCS during February 1987	January 1997-May 1999	1994-1998
Type of data used	Administrative registers	Questionnaire and administrative registers	Questionnaire
Type of unemployment scheme	UI and UA	UI	UI
Target group	Ages between 18 and 59 years old (p. 141) + table 1 p. 142 for additional characteristics	Less than 50 years, cohabiting with a partner earning some (replacement) labour income. Only women have retained. (p. 4-5) + additional characteristics in table 2 (p. 7)	Table 1 (p. 114)
Target group - eligibility, requirements for benefits	Minimum contribution period of 6 months during the previous 48 months (p. 154)	The unemployed is required to be available for the labour market and comply to certain administrative rules (p.3)	NA
Benefit level/replacement rate	First 6 months=80% of the person's last salary, 7-12 months=70%, 13-=60% (p. 155)	NA	Mean replacement rate mentioned in the appendix table (p. 126)
Benefit level/replacement rate available after exhaustion	UA - p. 155	NA	Mean unemployment assistance rate mentioned in the appendix table (p. 126)
Is compulsory activation part of the system?	NA	NA	NA
Maximum duration of unemployment benefits	Calculated by dividing by 2 the number of months contributed, with the constraints that the result had to be integer multiple of 2. (p. 154-155)	Indefinite period with one exception: benefits may be withdrawn after an unemployment duration, ranging from 2 to 8 years, if one is less than 50 years old and partner of someone with a (replacement) income exceeding a particular level. (p. 3)	15 months (p. 112)
Destination	Job	Employment	Employment or inactivity (p. 119)

Sampling frequency	Daily (p. 141)	Monthly	Monthly
Time interval the outcome measure is based on	Daily	Monthly	Monthly observations (p. 109)
Sample size	11,668 UI claimants, 3,077 UA claimants (p. 141)	Controls=404 individuals, treated=826 individuals (p. 7)	1994: 4,048 households and 9,905 individuals. 1995: 3,584 households and 8,532 individuals. 1998: 2,729 households and 6,324 individuals (p. 108-109)
Are the labor market conditions described?	NA	No	NA
Is there correction for unobserved heterogeneity?	Yes (p. 138)	Not relevant	Yes (p. 118)
Censoring level	15-52% (p. 156)	Left-censored (table 2 p. 7)	NA
Is there correction for censoring?	Yes (p. 139-140)	Not relevant	Yes

Author	Puhani	Saarela	Wolff
Year	2000	2000	1997
Country	Poland	Finland	Hungary
Language	English	Swedish	English
Journal	Journal of Population Economics	Ekonomiska Samfundets Tidskrift	The William Davidson Institute
Time period covered by data	1991-1994	1. January - 18. September 1996	December 1992 - January 1993 (p. 5)
Type of data used	Questionnaire	Administrative registers	Administrative registers
Type of unemployment scheme	Unemployment benefit	Intervention group: UI, control group: social benefits (non-insured -> grunddagpenning and arbetsmarknadsstöd) (p. 200)	UI
Target group	Sample from the population above 15 years of age - table 1 (p. 39)	NA	Table 3 p. 33
Target group - eligibility, requirements for benefits	Before the regime change: qualifying conditions were loose, in that one just had to register with the labour office as unemployed in order to draw benefits. After the regime change: school leavers have a 3-month waiting period (p. 36-37)	Intervention group: has to be registered with an unemployment office and meet the requirements of previous working time (26 or 43 weeks in 2 years). Control group: grunddagpenningen is available if the individual meets the working time requirement but isn't registered with an UI office. Arbetsmarknadsstödet is available if the individual does not meet any requirements. (p. 200)	The individuals are required to have worked for at least one year during the previous 4 years. Additionally the individuals were supposed to search actively after a job, to accept suitable jobs and to co-operate with the labour-centre (p.2)
Benefit level/replacement rate	After the regime change: 36 % of the average wage in the economy during the previous quarter (special rules for school leavers and people in "crisis areas".	Intervention group: 40-65 % of income (p. 200)	Before the change: First phase = 70 %, second phase= 50 %. After the change: First phase=75 %, second phase= 65 % (p. 31)
Benefit level/replacement rate available after exhaustion	NA	Arbetsmarknadsstöd - depends negative on partner's income. 118 FIM per day (p. 200)	Social benefits: 80 % of the minimum pension (p. 3)
Is compulsory activation part of the system?	NA	NA	Not compulsory (p. 4)

Maximum duration of unemployment benefits	Before the regime change: open-ended. After the regime change: 12 months	700 weekdays (=500 working days) (p. 200)	Ten different entitlement periods, depending on the employment record. Before the change: 135-540 days. After the change: reduced by one third. (p. 2)
Destination	Employment	Exit to employment or no-employment (p. 201)	Employment, subsidized employment, training + retraining and (early) retirement. (p. 5)
Sampling frequency	NA (only available for one variable, which is quarterly)	Daily	Duration of spells measured in days (p. 5)
Time interval the outcome measure is based on	Quarterly	Monthly	Monthly
Sample size	4353 men and 4441 women (p. 38)	7309 individuals - treatment=2827 individuals, control=2592 individuals. Excluded=1890 individuals. (p. 200)	54,911 male spells, 25,200 women spells. 37.4 % (=20,519 obs.) of men and 38.1 % (=9,591 obs.) of women are administered by the 1993 benefit provisions. (p. 5)
Are the labor market conditions described?	They mention the rate of unemployment benefit claimants before the regime change (79,0 %) and after the regime change (52,3 %) (p. 37)	NA (only mentioned for 2000 because the study predicts the effect of a new regime change - and use old data to estimate this effect).	NA
Is there correction for unobserved heterogeneity?	Yes, they specify the component to be drawn from a discrete distribution with x mass points and restrictions - and it is assumed that the component is orthogonal to the covariates. (p. 37)	NA	NA
Censoring level	NA	NA	39-49% (p. 32)
Is there correction for censoring?	Yes	NA	Yes (p. 10)

Author	Gritz, MaCurdy	Fallick	Hunter
Year	1992	1991	1990
Country	USA	USA	USA
Language	English	English	English
Journal	Empirical Economics	Review of Economics and Statistics	Ph.D. thesis
Time period covered by data	1978-mid1985	January 1984	January 1979-summer 1980 (p. 62)
Type of data used	Questionnaire	Questionnaire	Questionnaire
Type of unemployment scheme	UI	UI	UI
Target group	Young men, table 3.1 (p. 188) Requirements for participation (p. 187)	Table 1 p. 229	Men aged between 18-65 years (p.65). For further sample statistics see table 3.1 (p.64)
Target group - eligibility, requirements for benefits	NA	NA	Table A-2 p. 198
Benefit level/replacement rate	Summary statistics (p. 188)	NA	Average weekly benefit in US in 1979 was \$90.
Benefit level/replacement rate available after exhaustion	Summary statistics (p. 188)	NA	NA
Is compulsory activation part of the system?	NA	NA	NA
Maximum duration of unemployment benefits	Summary statistics (p. 188)	26 weeks for UI and 39 weeks for extended benefits. (p. 230)	Table A-2 p. 199 - typical maximum period is 26 weeks. (p. 9)
Destination	Employment	Employment (p. 230)	Employment
Sampling frequency	Weekly (p.187)	NA	NA
Time interval the outcome measure is based on	Weekly	Weekly data on unemployment duration (p. 231)	NA
Sample size	1409 individuals. Spells for ineligible individuals=2122, spells for eligible nonrecipients=1190, spells for UI recipients=719 (p. 188)	1290 - 62.3 % UI recipients, 36.7 % nonrecipients, 0.9 % don't know. (p. 229)	31,051 households - duration sample=1,816, completed spells=1,124 (p. 64)

Are the labor market conditions described?	NA	NA	Yes - the unemployment rate in 1979 was 5.8 % and 7.5 % in summer 1980. (p. 62)
Is there correction for unobserved heterogeneity?	NA	NA	NA
Censoring level	NA	NA	NA
Is there correction for censoring?	Yes	Yes (p. 231)	Yes (p. 203)

Author	Solon	Moffitt	Steiger
Year	1978	1985	2007
Country	USA	USA	Switzerland
Language	English	English	English
Journal	Monthly labor review	Unemployment Insurance Occasional Paper	Dissertation
Time period covered by data	September 1972-August 1974	1978-1983	Until December 2003
Type of data used	Administrative registers + Questionnaire	Administrative registers + Questionnaire	Administrative registers
Type of unemployment scheme	UI	UI	UI
Target group	NA	Table III.1 (p. 33)	Table 18 (p. 143)
Target group - eligibility, requirements for benefits	NA	NA	A person is eligible for unemployment benefits if they satisfy the minimum required duration of contribution to the system within the preceding two years from the date of job loss. The minimum duration is set to 12 months (p. 127)
Benefit level/replacement rate	NA	Mean net replacement rate: men=0.50, women=0.79 (p. 33)	70 or 80% of the insured salary of their last employment, conditional on the level of income and dependents (p. 127)
Benefit level/replacement rate available after exhaustion	NA	NA	Additional unemployment aid or social assistance (p. 128)
Is compulsory activation part of the system?	NA	NA	NA
Maximum duration of unemployment benefits	Before February 1974: 26 weeks. After February 1974: 26 weeks + 13 weeks (p. 48)	26 weeks but the program FSB extends benefit duration up to 65 weeks and EB extends up to 39 weeks (p. 2)	Before the change: 2 years. After the change: 18.5 months (p. 128)
Destination	Employment	Employment	Job or left unemployment (p. 143)
Sampling frequency	Questionnaire: 2, 4 and 6 months after the final regular benefit payment (p. 48)	Weekly	Daily

Time interval the outcome measure is based on	Questionnaire: week-by-week information (p. 48)	Weekly	Daily
Sample size	2,213 individuals. Number of cases for no extended benefits=1,041, some extended benefits=1,083, immediate extended benefits=89. (p. 49)	5,167 men, 2,902 women (p. 32)	92,802 persons (p. 140) Different samples (p. 141-143)
Are the labor market conditions described?	NA	NA	NA
Is there correction for unobserved heterogeneity?	NA	NA	NA
Censoring level	NA	NA	NA
Is there correction for censoring?	NA	Yes	No

Author	Micklewright, Nagy	Fujita	Lauringson
Year	1995	2011	2010
Country	Hungary	USA	Estonia
Language	English	English	English
Journal	EUI working papers in economics	Federal Reserve Bank of Philadelphia working paper	University of Tartu
Time period covered by data	Spells starting in December 1992 and January 1993	January 2004-July 2010 without 2008	2007-2008
Type of data used	Administrative registers	Questionnaire	Administrative registers
Type of unemployment scheme	UI	UI	UI
Target group	No spells as the result of quit and receipt of statutory severance pay (p. 7) Sample characteristics table 1 (p. 37)	The individual can only receive UI if the person did not leave his job voluntarily.	Table 1 (p. 10)
Target group - eligibility, requirements for benefits	At least 12 months of work is required in the previous 4 years in order to qualify for any benefits. (p. 4)	If a worker leaves his job voluntarily, he does not qualify for UI benefits (p. 7)	In order to be entitled to receive this benefit, a person has to have made contributions for at least 12 months during the previous 36 months. In addition, only involuntary unemployment is covered. (p. 7)
Benefit level/replacement rate	Before change: Period 1 (two-thirds of the period)=70% of past earnings, period 2=50%. After the change: Period 1 (first quarter)=75%, period 2=60% (p. 5)	NA	50% of the previous average wage during the first 100 days and 40% thereafter. (p. 8)
Benefit level/replacement rate available after exhaustion	Social Benefit= flat-rate equal to cut-off line (two-thirds of minimum wage) (p. 6)	NA	Unemployment allowance - flat-rate. (p. 7)
Is compulsory activation part of the system?	NA	NA	NA
Maximum duration of unemployment benefits	Depends on the working experience. Before the change: Min.= 4½ months, Max.=18 months. After the change: Min.=3 months, Max.=12 months. (p. 4)	26 weeks - but after mid 2008 it could be extended. (p. 6)	Contributions for 12 months=180 days, contributions for 56 months=270 days (p. 7-8)

Destination	Job, a government labour market programme, exhaustion of entitlement, or other reasons (p. 13) See table 3 for potential exits (p.39)	Employment or inactivity (p. 2)	Employment
Sampling frequency	Monthly	Monthly (p. 2)	Monthly
Time interval the outcome measure is based on	Monthly	Monthly	Wage data=monthly (p. 11)
Sample size	92 scheme=50,441 spells, 93 scheme=30,270 spells (p. 8)	114,623 (p. 9)	Number of observations: 180 granted days=2,831, 270 granted days=3,266 (p. 10)
Are the labor market conditions described?	A little (p. 1)	NA	Yes (p. 9)
Is there correction for unobserved heterogeneity?	NA	Yes (p. 13)	Yes (p. 17-18)
Censoring level	Table 3 (p. 39)	NA	NA
Is there correction for censoring?	Yes	Yes	Yes

Author	Gaure, Røed, Westlie	Bennmarker, Carling, Holmlund
Year	2008	2005
Country	Norway	Sweden
Language	English	English
Journal	IZA Discussion Paper	CESIFO working paper
Time period covered by data	1989-2001	2001-2003
Type of data used	Administrative registers	Administrative registers
Type of unemployment scheme	UI	UI
Target group	New entrants into registered unemployment not having had any unemployment experience during the past three years prior to the first spell in our data window. (p. 4) Descriptive statistics in table 1 (p. 5)	Excluded disabled workers and workers over age 54. (p. 19)
Target group - eligibility, requirements for benefits	The individual is required to have earned above a certain level before unemployment (60.000 NOK) in the year prior to unemployment. (note 6, p. 17)	420 hours of work during the 12-month period preceding unemployment. (p. 4)
Benefit level/replacement rate	NA	80% of previous earnings (p. 4) Group A is control group - group B, C and D are treatment groups where the treatment is higher benefits. (p. 6)
Benefit level/replacement rate available after exhaustion	NA	NA
Is compulsory activation part of the system?	NA	NA
Maximum duration of unemployment benefits	Before reform: 80 weeks + 13 weeks (+80 weeks). After the reform: 156 weeks. (p. 6)	60 weeks (p. 3)
Destination	Employment, ordinary education, other benefit, ALMP (p. 8)	Employment
Sampling frequency	NA	Weekly
Time interval the outcome measure is based on	Monthly	Weekly (p.19)
Sample size	373,065 individuals (p. 5)	Table 2 (p. 21)
Are the labor market conditions described?	NA	Yes (p. 11)
Is there correction for unobserved heterogeneity?	Yes (p. 8 + 26)	Yes (p. 27)
Censoring level	19.83% (p. 9)	Table 4 (p. 23)
Is there correction for censoring?	Yes	Yes

### 1.3. Numeric data for studies with effect estimate

Author	Korpi	van Ours, Vodopivec	Portugal, Addison
Year	1995	2006	2008
Country	Sweden	Slovenia	Portugal
Language	English	English	English
Journal	Economica	Journal of Labor Economics	Scottish Journal of Political Economy
Type of outcome data	Time-to-Event	Time-to-Event	Time-to-Event
Outcome	Proportional hazard rate. Table 2 p. 362	Proportional hazard rate. Table 4 p. 372	Proportional hazard rate. Table 5, p. 411 using only exit to open-ended employment (also available: Fixed-term contract, part time, self-employment, public activity and inactivity)
Time Point (s)	UA (reference: no unemployment benefits): 4-7 months before exhaustion and 0-3 months before exhaustion. UB (reference: no unemployment benefits): 7-14 months before exhaustion and 0-6 months before exhaustion	Month of expiration and post expiration (not specified)	1-2 months before expiration, 3-5 months before expiration and 6-11, 12-17 and 18-23 months before expiration (reference: nonrecipients)
Source	Questionnaire	Administrative registers	Questionnaire
Method of estimation	Discrete time hazard (logistic) with piecewise-constant baseline hazard	Proportional hazard rate with piecewise-constant baseline hazard	Proportional hazard rate with piecewise-constant baseline hazard
Statistics	UA: 4-7 months before exhaustion 0.2760 (0.2273) and 0-3 months before exhaustion -0.1403 (0.2558). UB: 7-14 months before exhaustion -0.3291 (0.2420) and 0-6 months before exhaustion -0.6371 (0.4690)	Month of expiration: 0.82 (16.7) males, .91 (18.9) females	1-2 months before expiration: 0.330 (0.481), 3-5 months before expiration: -0.746 (0.444) (reference: nonrecipients)
Notes	Dummy variable coefficients relative to no unemployment benefits (standard errors). Table 2 p. 362. Exit to employment, separated by permanent/temporary also available	Dummy variables (reference no change in PBD) (absolute t-statistic), separated by gender	Dummy variable relative to nonrecipients (standard errors). Only men

Author	Adamchik	Jenkins, García-Serrano	Card, Chetty, Weber
Year	1999	2004	2007
Country	Poland	Spain	Austria
Language	English	English	English
Journal	Oxford bulletin of economics and statistics	Oxford bulletin of economics and statistics	AEA papers and proceedings
Type of outcome data	Time-to-Event	Time-to-Event	Time-to-Event
Outcome	Proportional hazard rate. Table 2, p. 102, estimates for all and separated by gender (also available separated by age and education)	Proportional hazard rate. Table 3, p. 251	Proportional hazard rate. Table 3
Time Point (s)	0-3 months before expiration, 3-6 months before expiration, 6-9 months before expiration and 9-12 months before expiration (reference nonrecipients)	linear spline: 1, 2, 3-4, 5-6, 7-12, 13-18 and 19-24 months away from exhaustion	Exhaustion spline: 9-12 weeks before, 5-8 weeks before, 3-4 weeks before, 1-2 weeks before, 0 weeks before, 1-2 weeks after, 3-4 weeks after, 5-8 weeks after, 9-12 weeks after and more than 12 weeks after
Source	Questionnaire	Administrative registers	Administrative registers
Method of estimation	Cox proportional hazard rate	Discrete time hazard (logistic) with piecewise-constant baseline hazard	Cox proportional hazard rate
Statistics	All: 0-3 months before expiration 0.913 (0.080), 3-6 months before expiration 0.453 (0.095) (reference nonrecipients). Men: 0-3 months before expiration 1.101 (0.101), 3-6 months before expiration 0.659 (0.120) (reference nonrecipients). Women: 0-3 months before expiration 0.594 (0.134), 3-6 months before expiration 0.110 (0.160) (reference nonrecipients)	Linear exhaustion spline, months away from exhaustion: 1: 0.115 (0.018), 2: 0.134 (0.013), 3-4: -0.299 (0.017).	Linear exhaustion spline coefficients (The construction is equivalent to dummies): 9-12 weeks before 1.002 (0.022), 5-8 weeks before 1.014 (0.024), 3-4 weeks before 1.042 (0.036), 1-2 weeks before 1.027 (0.042), 0 weeks before 1.148 (0.062), 1-2 weeks after 1.187 (0.050), 3-4 weeks after 1.062 (0.053), 5-8 weeks after 1.012 (0.046), 9-12 weeks after 0.927 (0.048) and more than 12 weeks after 0.782 (0.047)
Notes	Dummy variable relative to nonrecipients (standard errors)	Table 3, p. 25 Linear exhaustion spline (standard errors)	Table 3, note it is reported as exp(x) (standard errors) (reference more than 12 weeks away)

Author	Jurajda, Tannery	Addison, Portugal	Belzil
Year	2003	2004	2001
Country	USA	USA	Canada
Language	English	English	English
Journal	Industrial and Labor Relations Review	IZA Discussion Paper	Journal of Applied Econometrics
Type of outcome data	Time-to-Event	Time-to-Event	Time-to-Event
Outcome	Proportional hazard rate. Table 2, p. 335	Proportional hazard rate. Table 1, appendix	Proportional hazard rate. Table 2, p. 631
Time Point (s)	0-1 week after exhaustion, 1-3 weeks before exhaustion, 4-18 weeks before exhaustion, 19-27 weeks before exhaustion, 28-36 weeks before exhaustion and 37 or more weeks before exhaustion	Before: 1-25 (29) weeks before exhaustion, At: the week of expiration and After: 1 or more weeks after expiration	exhaustion spline: 1-5 weeks before, 6-9, 10,19, 20-29 and more than 29 weeks before
Source	Administrative registers + Questionnaire	Questionnaire	Administrative registers
Method of estimation	Discrete time hazard (logistic) with piecewise-constant baseline hazard	Proportional hazard rate with piecewise-constant baseline hazard	Proportional hazard rate with Weibull baseline hazard
Statistics	0-1 week after exhaustion: 1.627 (0.097), 1-3 weeks before exhaustion: -0.105 (0.110), 4-18 weeks before exhaustion: -0.812 (0.103)	At: the week of expiration 1.265 (0.231)	exhaustion spline: 1-5 weeks before: -0.3076 (3.01), 6-9: -0.1978 (2.27), 10-19: -0.0567 (2.02), 20-29: 0.0126 (1.18) and more than 29 weeks before: -0.0307 (0.83)
Notes	Dummy variable coefficient (standard errors). Estimates based on the pooled sample allowing for unobserved heterogeneity (column 2, table 2). New job estimates, recall also available. Reference is 2 or more weeks after exhaustion	Dummy relative to nonrecipients (the author states it is asymptotic t-statistic but that must be a mistake. We treat it as a standard error) column 1	Exhaustion spline coefficients (asymptotic t-ratios). Measured as moving one week away from exhaustion (effect of moving one week closer in the interval 1-5 is the sum of all coefficients multiplied by -1)

Author	Terrell, Sorm	Schmieder, von Wachter, Bender	Vodopivec
Year	1999	2009	1995
Country	Czech Republic	Germany	Slovenia
Language	English	English	English
Journal	Journal of Comparative Economics	Columbia University, Department of Economics	Policy research working paper
Type of outcome data	Time-to-Event	Time-to-Event	Time-to-Event
Outcome	Proportional hazard rate. Table 4, p. 47	Proportional hazard rate. Table 11, p. 39	Proportional hazard rate. Table 4, p. 28. Estimate for all and separate divided by entitlement
Time Point (s)	0-1 week before exhaustion and all weeks after exhaustion (not specified)	Last month of UI. Differs by age, separated by age thresholds 42, 44 and 49	Exhaustion spline: monthly (except the last) in the interval 23 months before exhaustion to 2 (or more) months after exhaustion
Source	Administrative registers	Administrative registers	Administrative registers
Method of estimation	Discrete time logistic hazard rate with Weibull baseline hazard	Proportional hazard rate	Cox proportional hazard rate
Statistics	0-1 week before exhaustion: men: 1.023 (0.304), women: 0.401 (0.301)	Age threshold 42 (entitlement of 18 months compared to 12): -0.024 (0.0025), age threshold 44 (entitlement of 22 months compared to 18): -0.013 (0.0022) and age threshold 49 (entitlement of 26 months compared to 22): -0.015 (0.0023)	SPL-2 (2 or more months after) -0.129 (0.097), SPL-I (one month after) -1.121 (0.123), SPLO (month of expiration) 0.155 (0.080), SPLI (1 month before) 0.694 (0.088) and SPL2 (2 months before) 0.324 (0.132)
Notes	Dummy variable separated by gender	RD-design. Local linear regressions (different slopes) on each side of age cutoff (standard errors). Negative coefficients as treated have higher entitlement than control (entitlement of 18 months compared to 12, entitlement of 22 months compared to 18 and entitlement of 26 months compared to 22)	Table 4, p. 28, spline coefficients (standard error). Model 1, total. Also available separated by entitlement.

Author	Boeri, Steiner	Caliendo, Tatsiramos, Uhlenborff	Van Ours, Vodopivec
Year	1998	2009	2004
Country	Poland	Germany	Slovenia
Language	English	English	English
Journal	Konjunkturpolitik: Zeitschrift für angewandte Wirtschaftsforschung	Deutsches Institut für Wirtschaftsforschung	IZA Discussion Paper
Type of outcome data	Time-to-Event	Time-to-Event	Time-to-Event
Outcome	Proportional hazard rate. Table 1-4, p.303-6	Proportional hazard rate. Table 4 p. 22	Proportional hazard rate. Table 3, p. 28
Time Point (s)	month of expiration and after expiration (not specified). 3rd degree polynomial of remaining months also included	9-11 months before, 6-8 months before, 3-5 months before, 0-2 months before, 1-3 months after, 4-6 months after	1 month before exhaustion, month of exhaustion and 1 month after exhaustion
Source	Administrative registers	Administrative registers	Administrative registers
Method of estimation	Discrete time hazard ( multinomial logit hazard) with piecewise-constant baseline hazard	Proportional hazard rate with piecewise-constant baseline hazard	Proportional hazard rate with piecewise-constant baseline hazard
Statistics	Warsaw: men: 0.046 (0.56), women: 0.197 (3.08). Ciechanow, men: -0.091 (-0.27) and women: 0.368 (0.84)	MEN: 3-5 months before: 0.044 (0.196), 0-2 months before: -0.614 (0.209), 1-3 months after: -0.190 (0.223). WOMEN: 3-5 months before: -0.098 (0.165), 0-2 months before: -0.734 (0.170), 1-3 months after: -0.318 (0.186)	1 month before exhaustion: men -0.01 (0.2), women 0.06 (0.1), month of exhaustion: men 0.78 (16.4), women 0.87 (19.9) and 1 month after exhaustion: men 0.44 (6.5), women 0.29 (4.4)
Notes	Table 1-4, p.303-6. Dummy (t-value) separated by region and gender. Into employment (non-participation also available).	Dummy coefficients (T/C) interacted with duration (standard error), sample B (fresh spells), table 4 p. 22. Negative coefficients as treated (entitlement of 18 months) are compared to controls (entitlement of 12 months)	Dummy coefficient (not reported but must be t-value) table 3, p. 28. separated by gender

Author	Scmitz, Steiner	Sanz	Katz, Meyer
Year	2007	2010	1990
Country	Germany	Spain	USA
Language	English	English	English
Journal	IZA Discussion Paper	ECON working paper	The Quarterly Journal of Economics
Type of outcome data	Time-to-Event	Time-to-Event	Time-to-Event
Outcome	Proportional hazard rate. Table 5, p. 16	Proportional hazard rate. Table 3, p.34	Proportional hazard rate. Table 6, p. 995
Time Point (s)	month of expiration, 1 month before, 2 month before, 3-4 months before, 5-6, 7-8, 9-12 and 13-18 months before. Minimum 1 months after exhaustion.	minimum 4 months before exhaustion, 2-3 months before exhaustion, 1 months before, month of exhaustion, 1 month after, 2-3 months after and 4-5 month after exhaustion	week of exhaustion, 1 week before exhaustion, 2-5 weeks before, 6-10 weeks before and minimum 1 week after
Source	Questionnaire	Administrative registers	Administrative registers + Questionnaire
Method of estimation	Discrete time hazard (multinomial logit) with piecewise-constant baseline hazard	Discrete-time competing hazard (complementary log-log) with piecewise-constant baseline hazard	Cox proportional hazard rate
Statistics	month of expiration: West: men 0.47 (1.92), women 0.928 (3.23). East: men 0.699 (2.78), women 0.732 (2.59), 1 month before: West: men 0.189 (0.74), women 0.538 (1.88). East: men 0.561 (2.25), women 0.375 (1.30), 2 month before: West: men 0.005 (0.02), women 0.317 (1.07), East: men 0.169 (0.65), women 0.488 (1.74), 3-4 months before: West: men 0.462 (2.42), women 0.626 (2.93), East: men 0.157 (0.78), women 0.446 (1.88)	2-3 months before exhaustion: Recall: men -0.738 (-5.9), women -0.330 (-3.5), Diff. Firm: men -1.012 (-8.9), women -0.815 (-8.2), 1 months before: Recall: men 0.429 (2.5), women 1.493 (15.8), Diff. firm: men -0.542 (-3.7), women -0.248 (-2.0), month of exhaustion: Recall: men 0.433 (1.8), women 1.409 (8.5), Diff. firm: men 0.056 (0.4), women 0.175 (1.5), 1 month after: Recall: men -0.577 (-1.9), women -0.061 (0.4), Diff. firm: men -0.855 (-5.3), women -0.734 (-5.2), 2-3 months: Recall: men -0.688 (-2.3), women 0.526 (2.4), Diff. firm: men -0.795 (-5.5), women -0.820 (-6.1) after	Week of exhaustion: Total: 0.928 (0.235), recall: 0.835 (0.371), new job: 0.789 (0.329), 1 week before exhaustion: Total: 0.393 (0.300), recall: 0.385 (0.479), new job: 0.410 (0.405), 2-5 weeks before: Total: -0.090 (0.194), recall: -0.045 (0.273), new job: -0.164 (0.291), 6-10 weeks before: Total: -0.167 (0.146), recall: -0.166 (0.208), new job: -0.182 (0.220), 1 week after: Total: -0.636 (0.732), recall: -0.470 (0.416), new job: -1.423 (0.976)
Notes	Dummy coefficient (t-value) table 3, p. 28. separated by gender and region	Dummy coefficient (t-statistic) separated by gender, recall/not recall and permanent contract in previous job/ temporary contract in previous job. Only exhaustion estimates for involuntary unemployed	Dummy coefficients (standard errors) relative to 11 or more weeks before exhaustion. Total and separate for recall and new job

Author	Arranz, Bulló, Muro	Jones	Boone, van Ours
Year	2008	1995	2009
Country	Spain	Canada	Slovenia
Language	English	English	English
Journal		Human Resources Development Canada	IZA Discussion Paper
Type of outcome data	Time-to-Event	Time-to-Event	Time-to-Event
Outcome	Proportional hazard rate. Table 7, p. 33	Proportional hazard rate. Table 10+11, very different results dependent upon which controls are included	Proportional hazard rate. Table 2, p. 37, panel C (with flexible duration dependence)
Time Point (s)	month of exhaustion, 1-3 months before, 4-6 months before, 7-12, 13-18 and 19-24 months before	1-3 weeks before expiration, 4-6 weeks before and 7-12 weeks before	month of expiration
Source	Administrative registers	Administrative registers + Questionnaire	Administrative registers
Method of estimation	Discrete-time hazard (complementary log-log) with piecewise constant baseline hazard.	Cox proportional hazard rate	Proportional hazard rate with piecewise-constant baseline hazard
Statistics	month of exhaustion: is reference, 1-3 months before: -0.134 (0.031), 4-6 months before: -0.243 (0.033), 7-12: -0.189 (0.031), 13-18: -0.147 (0.034) and 19-24: 0.061 (0.038) months before	1-3 weeks before expiration, 4-6 weeks before and 7-12 weeks before	month of expiration: Permanent job: men 1.63 (11.3), women 1.60 (11.6), Temporary job: men 0.59 (6.1), women 0.66 (7.8)
Notes	Dummy coefficients (standard error). Table 7, p. 33, model 2	Table 10 or 11 page 48-50, very different results dependent upon which controls are included	Dummy coefficients (absolute t-statistic), table 2, p. 37, panel C (with flexible duration dependence). Distinguish between permanent/temporary job and separated by gender.

#### 1.4. Numeric data/reason for not in the data synthesis for studies without effect estimate

Author	Gonzalo	Røed, Zhang	Lee, Wilke	Lindeboom, Theeuwes	Stancanelli	Ham, Rea
Year	2002	2003	2009	1993	1999	1987
Country	Spain	Norway	Germany	The Netherlands	Britain	Canada
Language	English	English	English	English	English	English
Journal	Applied Economics	The Economic Journal	Journal of business & economic statistics	Economica	Applied Economics	Journal of Labor Economics
Type of outcome data	Time-to-Event	Time-to-Event	Time-to-Event	Time-to-Event	Time-to-Event	Time-to-Event
Outcome	Proportional hazard rate	Figure of relative hazard rate	Survival probability, bounds analysis, figure only	Proportional hazard rate	The presence of an unemployment benefit exhaustion effect is tested by means of likelihood ratio tests, having allowed the baseline hazard to shift for the unemployed that expect their benefit to exhaust. (p. 1047) Test statistic not reported	Proportional hazard rate
Time Point (s)	Time to exhaustion not specified			Time to exhaustion not specified, second-order polynomial for remaining weeks		Time to exhaustion not specified

<b>Author</b>	Katz, Meyer	Lubyova, van Ours	van Ours, Vodopivec	Arranz, Muro	Cockx, Ries	Layte, Callan
<b>Year</b>	1990	1997	2006	2004	2004	2001
<b>Country</b>	USA	Slovakia	Slovenia	Spain	Belgium	UK
<b>Language</b>	English	English	English	English	English	English
<b>Journal</b>	Journal of Public Economics	European Economic Review	World Bank Policy Research Working Paper	Revista de Economica Pública	CESIFO working paper	The Economic and Social Review
<b>Type of outcome data</b>	Time-to-Event	Time-to-Event	Continuous	Time-to-Event	Dichotomous	Time-to-Event
<b>Outcome</b>	Kaplan-Meyer figure of exit to job without time to exhaustion specified.	Proportional hazard rate	No transition estimates reported, only probability of finding a permanent job to overall probability of finding a job (permanent and temporary). Wage change in post-unemployment job compared to pre-unemployment	Proportional hazard rate	Monthly employment probability difference	Proportional hazard rate
<b>Time Point (s)</b>	Time to exhaustion not specified	Time to exhaustion not specified	Month of expiration and post expiration (not specified).	Time to exhaustion not specified	3 months before, 2 months before, 1 month before, month of exhaustion, 3 months after, 6 months after, 12 months after and 14 months after (table 5, p. 17)	Time to exhaustion not specified

Author	Puhani	Saarela	Wolff	Gritz, MaCurdy	Fallick	Hunter
Year	2000	2000	1997	1992	1991	1990
Country	Poland	Finland	Hungary	USA	USA	USA
Language	English	Swedish	English	English	English	English
Journal	Journal of Population Economics	Ekonomiska Samfundets Tidskrift	The William Davidson Institute	Empirical Economics	Review of Economics and Statistics	Ph.d. thesis
Type of outcome data	Time-to-Event	Time-to-Event	Time-to-Event	Time-to-Event	Time-to-Event	Time-to-Event
Outcome	Proportional hazard rate	Figure of baseline hazard, no confidence interval.	Proportional hazard rate	Survival probability	Proportional hazard rate	Proportional hazard rate for unemployment exit (to employment). Wage following unemployment with duration (continuous variable) and initial entitlement in covariates (no exhaustion effect)
Time Point (s)	Time to exhaustion not specified		Not specified for exits to employment (only specified for exits to subsidised employment and training)	Time to exhaustion not specified	Time to exhaustion not specified	Time to exhaustion not specified

Author	Solon	Moffitt	Steiger	Micklewright, Nagy	Fujita	Lauringson
Year	1978	1985	2007	1995	2011	2010
Country	USA	USA	Switzerland	Hungary	USA	Estonia
Language	English	English	English	English	English	English
Journal	Monthly labor review	Unemployment Insurance Occasional Paper	Dissertation	EUI working papers in economics	Federal Reserve Bank of Philadelphia working paper	University of Tartu
Type of outcome data	Continuous	Time-to-Event	Time-to-Event	Time-to-Event	Time-to-Event	Time-to-Event
Outcome	Average employment weeks in the 26 weeks following exhaustion of regular benefits	Proportional hazard rate	Employment probability, matching and RD	Non-parametric HR for two groups (separate) with different entitlement, figures only	Proportional hazard rate	Proportional HR for two groups (separate) with different entitlement.
Time Point (s)	0-26 weeks after exhaustion of regular benefits	Time to exhaustion not specified	Time to exhaustion not specified		Time to exhaustion not specified	Time to exhaustion not specified

Author	Gaure, Røed, Westlie	Bennmarker, Carling, Holmlund
Year	2008	2005
Country	Norway	Sweden
Language	English	English
Journal	IZA Discussion Paper	CESIFO working paper
Type of outcome data	Time-to-Event	Time-to-Event
Outcome	Proportional hazard rate, exit from unemployment and exit from employment (and monthly earnings). Figures only	Proportional hazard rates
Time Point (s)	Month of expiration and 1,2,3,4,5 and 6 months before exhaustion (for all outcomes)	Time to exhaustion not specified

### 1.5. Numeric data for studies with effect estimate and used for secondary outcome analysis

Author	Belzil	van Ours, Vodopivec	Caliendo, Tatsiramos, Uhlenborff	Sanz	Gaure, Røed, Westlie	Boone, van Ours
Year	2001	2006	2009	2010	2008	2009
Country	Canada	Slovenia	Germany	Spain	Norway	Slovenia
Language	English	English	English	English	English	English
Journal	Journal of Applied Econometrics	World Bank Policy Research	DIW	WP ECON	IZA	IZA
Type of outcome data	Time-to-Event	Time-to-Event	Time-to-Event	Time-to-Event	Time-to-Event	Time-to-Event
Outcome	Proportional hazard rate. Table 2, p. 631 , employment hazard	Proportional hazard rate. Table 3, p. 28.	Proportional hazard rate. Table 5 p. 23	Employment hazard, but no exhaustion effect estimated (only entry requirement effect)	Proportional hazard rate, exit from employment (and monthly earnings). Figures only	Proportional hazard rate. Table 3, p. 38)
Time Point (s)	Exhaustion spline: 1-5 weeks before, 6-9, 10,19, 20-29 and more than 29 weeks before	Month of expiration and post expiration (not specified).	9-11 months before, 6-8 months before, 3-5 months before, 0-2 months before, 1-3 months after, 4-6 months after			Month of expiration and post expiration (not specified).
Source	Administrative registers	Administrative registers	Administrative registers	Administrative registers	Administrative registers	Administrative registers
Method of estimation	Proportional hazard rate with Weibull baseline hazard	Proportional hazard rate with piecewise-constant baseline hazard	Proportional hazard rate with piecewise-constant baseline hazard			Proportional hazard rate with piecewise-constant baseline hazard
Statistics	Exhaustion spline: 1-5 weeks before: -0.0247 (1.88), 6-9: -0.0393 (1.68)	Month of expiration: Permanent job: men -0.13 (0.8), women 0.07 (0.5), Temporary job: men -0.15 (2.2), women -0.10 (1.1)	MEN: 0-2 months before: -0.338 (0.294). WOMEN: 0-2 months before: -0.133 (0.254)			month of expiration: Permanent job: men -0.35 (2.3), women 0.08 (0.3), Temporary job: men -0.26 (2.0), women 0.01 (0.1)

Notes	Exhaustion spline coefficients (asymptotic t-ratios). Measured as moving one week away from exhaustion (effect of moving one week closer in the interval 1-5 is the sum of all coefficients multiplied by -1)	Dummy coefficients (absolute t-statistic), table 3, p. 28. Distinguish between permanent/temporary job and separated by gender.	Dummy coefficients (T/C) interacted with duration (standard error), sample B (fresh spells), table 5 p. 23. Negative coefficients as treated (entitlement of 18 months) are compared to controls (entitlement of 12 months)			Dummy coefficients (absolute t-statistic), table 3, p. 38. Distinguish between permanent/temporary job and separated by gender.
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## 2. RISK OF BIAS

### 2.1. Risk of bias for studies used in data synthesis for primary outcome

Author	Korpi	van Ours, Vodopivec	Portugal, Addison	Adamchik
Year	1995	2006	2008	1999
Sequence generation (judgment)	High	High	High	High
Allocation concealment (judgment)	High	High	High	High
Blinding (judgment)	Not relevant	Not relevant	Not relevant	Not relevant
Incomplete outcome data addressed (judgment)	4	3	3	4
Incomplete outcome data addressed	Censoring level not reported. Missing data not reported.	Level of incomplete data not reported (p.358). Censoring level not reported.	Data discussed at page 395-398 and descriptive statistics given in table A1. Missing data level 2%. Censoring level 78-87%	Data discussed at page 96-98 and descriptive statistics given in table 1. Missing data not reported. Censoring level 73%
Free of selective reporting (judgment)	Not relevant	1	3	Not relevant
Free of selective reporting	Not relevant	1	3	Not relevant
Free of other bias (judgment)	Not relevant	1	1	Not relevant
Free of other bias	Not relevant	1	1	Not relevant
A priori protocol (judgment)	No	No	No	No
A priori analysis plan (judgment)	No	No	No	No
Confounding (judgment)	5	2	3	5
Confounding	No variation in remaining benefit has been modelled (and duration is modelled with dummies), the comparison is simply non-recipients at the same duration level.	-	-	No variation in remaining benefit has been modelled (apparently remaining benefit and duration is identical for all and modelled with dummies); the comparison is simply non-recipients at the same duration level.

<b>Method for identifying relevant confounders described by researchers</b>	No	No	No	No
<b>Relevant confounders described</b>	All, except unobservables (+ more)	All, except ethnicity (+ more)	All, except ethnicity (+ more)	All, except ethnicity, unemployment duration, labor market conditions (+ more)
<b>Method used for controlling for confounding at design stage</b>	None	Legislative changes and individual variation in entitlement due to labor market history	Individual variation in entitlement due to age	Reference group is non-recipients
<b>Method used for controlling for confounding at analysis stage</b>	Effect not identified	Regression	Regression	Effect not identified
<b>Method of estimation</b>	Discrete time hazard (logistic) with piecewise-constant baseline hazard	Proportional hazard rate with piecewise-constant baseline hazard	Proportional hazard rate with piecewise-constant baseline hazard	Cox proportional hazard rate

Author	Jenkins, García-Serrano	Card, Chetty, Weber	Jurajda, Tannery	Addison, Portugal
Year	2004	2007	2003	2004
Sequence generation (judgment)	High	High	High	High
Allocation concealment (judgment)	High	High	High	High
Blinding (judgment)	Not relevant	Not relevant	Not relevant	Not relevant
Incomplete outcome data addressed (judgment)	2	2	2	3
Incomplete outcome data addressed	Data discussed at page 244-247 and descriptive statistics given in table 1. Missing data level 1%. Censoring level is unclear but probably 0 (spells ending in exhaustion is treated as censored (59%), 27 spells ended in employment and 14% ended for other reasons)	Data discussed at page 7-9 and 17-18. Descriptive statistics given in table 2. Missing data level on covariates for the unrestricted sample: 34% of job losses occurring before 1987 and 25% of job losses occurring after 1998. Level in the analysis sample is unclear. Censoring level is 6%	Data discussed at page 328-331 and descriptive statistics given in table 1. Missing data level (including left censored spells) is 14%. Censoring level is 14%. Fresh spells only	Data discussed at page 7-10 and descriptive statistics given in the appendix. Missing data not discussed. Censoring level 7-13%.
Free of selective reporting (judgment)	1	2	3	3
Free of selective reporting	1	2	3	3
Free of other bias (judgment)	1	2	1	1
Free of other bias	1	2	1	1
A priori protocol (judgment)	No	No	No	No
A priori analysis plan (judgment)	No	No	No	No
Confounding (judgment)	2	3	3	4
Confounding	-	-	-	-

Method for identifying relevant confounders described by researchers	No	No	No	No
Relevant confounders described	All (+ more)	All, except unemployment duration and labor market conditions (+ more)	All, except education (+ more)	All (+ more)
Method used for controlling for confounding at design stage	Individual variation in entitlement due to employment history	Individual variation in entitlement due to employment history	Legislative changes and variation in entitlement due to extended benefits programs.	Comparison with non-recipients
Method used for controlling for confounding at analysis stage	Regression	Regression	Regression	Regression
Method of estimation	Discrete time hazard (logistic) with piecewise-constant baseline hazard	Cox proportional hazard rate	Discrete time hazard (logistic) with piecewise-constant baseline hazard	Proportional hazard rate with piecewise-constant baseline hazard

Author	Belzil	Terrell, Sorm	Schmieder, Wachter, Bender	Vodopivec
Year	2001	1999	2009	1995
Sequence generation (judgment)	High	High	High	High
Allocation concealment (judgment)	High	High	High	High
Blinding (judgment)	Not relevant	Not relevant	Not relevant	Not relevant
Incomplete outcome data addressed (judgment)	2	4	4	4
Incomplete outcome data addressed	Data discussed at page 624-626 and descriptive statistics given in table 1. Missing data level is 1%. Censoring level is 2%. Involuntary job loss and no recall only.	Data discussed at page 43-45 and descriptive statistics given in table 3. Missing data level not reported and not discussed. Censoring level not reported. Only recipients.	Data discussed at page 7-8, descriptive statistics in table 2. Missing data not mentioned. Censoring level not mentioned. Only fresh spells with max. entitlement for their age group	Data discussed at page 12, censoring level high (56%)
Free of selective reporting (judgment)	3	3	Not relevant	4
Free of selective reporting	3	3	Not relevant	4
Free of other bias (judgment)	1	1	Not relevant	2
Free of other bias	1	1	Not relevant	2
A priori protocol (judgment)	No	No	No	No
A priori analysis plan (judgment)	No	No	No	No
Confounding (judgment)	3	4	5	3
Confounding	-	-	The authors test the RD assumption by testing whether observable characteristics (Education, gender, foreign citizen, pre wage, tenure in last job, tenure in occupation, tenure in industry and experience) vary continuously at the points of discontinuity. Further they look at the smoothness of the density around the	-

			cutoffs and discuss the results.	
Method for identifying relevant confounders described by researchers	No	No	No	No
Relevant confounders described	All, except education, ethnicity and labor market conditions ( + more)	All (+ more)	Age and unobservables. "It turns out that for most of the outcomes we consider, in particular unemployment and non-employment durations, other variables in our dataset have little explanatory power (partly because we estimate our model on a relatively homogenous sample of workers) The efficiency gain from this is very small, so that we prefer to present the raw estimates without controlling for additional variables." (page 9)	All except unemployment duration, labor market condition (+ more)
Method used for controlling for confounding at design stage	Legislative changes and individual variation in entitlement due to labor market history and local unemployment rate.	Individual variation in entitlement due to late registration and multiple spells	Individual variation in entitlement due to age	Legislative changes and individual variation in entitlement due to labor market history
Method used for controlling for confounding at analysis stage	Regression	Regression	Regression	Regression
Method of estimation	Proportional hazard rate with Weibull baseline hazard	Discrete time logistic hazard rate with Weibull baseline hazard	Proportional hazard rate	Cox proportional hazard rate

Author	Boeri, Steiner	Caliendo, Tatsiramos, Uhlendorff	van Ours, Vodopivec	Schmitz, Steiner
Year	1998	2009	2004	2007
Sequence generation (judgment)	High	High	High	High
Allocation concealment (judgment)	High	High	High	High
Blinding (judgment)	Not relevant	Not relevant	Not relevant	Not relevant
Incomplete outcome data addressed (judgment)	4	3	4	2
Incomplete outcome data addressed	Data discussed at page 292-293, censoring level not mentioned. Missing data not mentioned.	Data discussed at page 9-10. Missing data not discussed. Censoring level 25-30%	Data discussed at page 7-10 and 14-15. Censoring level not mentioned. Missing data not mentioned.	Data discussed at page 8-12. Missing data reported. Censoring level 19%
Free of selective reporting (judgment)	4	1	1	3
Free of selective reporting	4	1	1	3
Free of other bias (judgment)	2	1	1	1
Free of other bias	2	1	1	1
A priori protocol (judgment)	No	No	No	No
A priori analysis plan (judgment)	No	No	No	No
Confounding (judgment)	3	3	2	1
Confounding	-	-	-	-
Method for identifying relevant confounders described by researchers	No	No	No	No
Relevant confounders described	All except ethnicity (+ more)	All (+ more)	All, except ethnicity (+ more)	All (+ more)

<b>Method used for controlling for confounding at design stage</b>	Legislative changes and individual variation in entitlement due to labor market history and reason for unemployment.	Individual variation in entitlement due to age. Regression discontinuity design	Legislative changes and individual variation in entitlement due to labor market history	Legislative changes and individual variation in entitlement due to labor market history and age
<b>Method used for controlling for confounding at analysis stage</b>	Regression	Regression	Regression	Regression
<b>Method of estimation</b>	Discrete time hazard ( multinomial logit hazard) with piecewise-constant baseline hazard	Proportional hazard rate with piecewise-constant baseline hazard	Proportional hazard rate with piecewise-constant baseline hazard	Discrete time hazard (multinomial logit) with piecewise-constant baseline hazard

Author	Sanz	Katz, Meyer	Arranz, Bulló, Muro	Jones	Boone, van Ours
Year	2010	1990	2008	1995	2009
Sequence generation (judgment)	High	High	High	High	High
Allocation concealment (judgment)	High	High	High	High	High
Blinding (judgment)	Not relevant	Not relevant	Not relevant	Not relevant	Not relevant
Incomplete outcome data addressed (judgment)	2	2	3	3	3
Incomplete outcome data addressed	Data discussed at page 10-14. Missing data not discussed. Censoring level 10-13%	Data discussed at page 975-979. Missing data reported but not discussed. Censoring level 9%	Data discussed at page 14-18. Missing data not discussed. Censoring level 73-77.5%	Data discussed at page 9-13. Missing data discussed (attrition rate is 19.9) and sensitivity analysis carried out. "some sensitivity testing for the effects of attrition was conducted, and it was concluded that attrition does not seem to drive the main patterns of the results" (p. 13). Censoring level 61%	Data discussed at page 13-14 and table in appendix B. Missing data not reported. Censoring level 28-34%.
Free of selective reporting (judgment)	3	1	1	Not relevant	1
Free of selective reporting	3	1	1	Not relevant	1
Free of other bias (judgment)	1	1	1	Not relevant	1
Free of other bias	1	1	1	Not relevant	1
A priori protocol (judgment)	No	No	No	No	No
A priori analysis plan (judgment)	No	No	No	No	No
Confounding (judgment)	2	2	2	5	2
Confounding	-	-	-	Uses complete disenittlement for dismissals and voluntary quitters	-

Method for identifying relevant confounders described by researchers	No	No	Discusses the control variables used	No	No
Relevant confounders described	All, except education (+ more)	All except unemployment duration (+ more)	All, except ethnicity (+ more)	All, except unemployment duration (+ more)	All, except ethnicity (+ more)
Method used for controlling for confounding at design stage	Individual variation in entitlement due to labor market history.	Extensions of benefits through federal programs and individual variation in entitlement due to differences in base period and high quarter earnings. (p. 979)	Legislative changes and individual variation due to labor market history	Legislative changes	Legislative changes and individual variation in entitlement due to labor market history
Method used for controlling for confounding at analysis stage	Regression	Regression	Regression	Regression	Regression
Method of estimation	Discrete-time competing hazard (complementary log-log) with piecewise-constant baseline hazard	Cox proportional hazard rate	Discrete-time hazard (complementary log-log) with piecewise constant baseline hazard.	Cox proportional hazard rate	Proportional hazard rate with piecewise-constant baseline hazard

## 2.2.Risk of bias for studies used in data synthesis for secondary outcome

Author	Belzil	van Ours, Vodopivec	Caliendo, Tatsiramos, Uhrendorff	Boone, van Ours
Year	2001	2006	2009	2009
Sequence generation (judgment)	High	High	High	High
Allocation concealment (judgment)	High	High	High	High
Blinding (judgment)	Not relevant	Not relevant	Not relevant	Not relevant
Incomplete outcome data addressed (judgment)	2	3	3	3
Incomplete outcome data addressed	Data discussed at page 624-626 and descriptive statistics given in table 1. Missing data level is 1%. Censoring level is 2%. Involuntary job loss and no recall only.	Data discussed at page 7-8. Missing data not reported. Censoring level 28-34%.	Data discussed at page 9-10. Missing data not discussed. Censoring level 25-30%	Data discussed at page 13-14 and table in appendix B. Missing data not reported. Censoring level 28-34%.
Free of selective reporting (judgment)	3	1	1	1
Free of selective reporting	Unobserved heterogeneity included	Unobserved heterogeneity included. Sensitivity analysis, bivariate duration model (p. 17)	Sensitivity analysis (p. 15). Among other things unobserved heterogeneity. "Modeling unobserved heterogeneity significantly improves the model fit", "The effect of extended benefit duration does not differ qualitatively between the models with and without unobserved heterogeneity" (estimates not reported)	Sensitivity analysis, three duration specifications (p. 18). Unobserved heterogeneity included.
Free of other bias (judgment)	1	1	1	1
Free of other bias	-	-	-	-
A priori protocol (judgment)	No	No	No	No
A priori analysis plan (judgment)	No	No	No	No
Confounding (judgment)	3	2	3	2
Confounding	-	-	-	-

<b>Method for identifying relevant confounders described by researchers</b>	No	No	No	No
<b>Relevant confounders described</b>	All, except education, ethnicity and labor market conditions ( + more)	All, except ethnicity (+ more)	All (+ more)	All, except ethnicity (+ more)
<b>Method used for controlling for confounding at design stage</b>	Legislative changes and individual variation in entitlement due to labor market history and local unemployment rate.	Legislative changes and individual variation in entitlement due to labor market history	Individual variation in entitlement due to age. Regression discontinuity design	Legislative changes and individual variation in entitlement due to labor market history
<b>Method used for controlling for confounding at analysis stage</b>	Regression	Regression	Regression	Regression
<b>Method of estimation</b>	Proportional hazard rate with Weibull baseline hazard	Proportional hazard rate with piecewise-constant baseline hazard	Proportional hazard rate with piecewise-constant baseline hazard	Proportional hazard rate with piecewise-constant baseline hazard