Getting Stuck in Unemployment: Pitfalls and Helping Hands

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Table of Contents

List of Figures

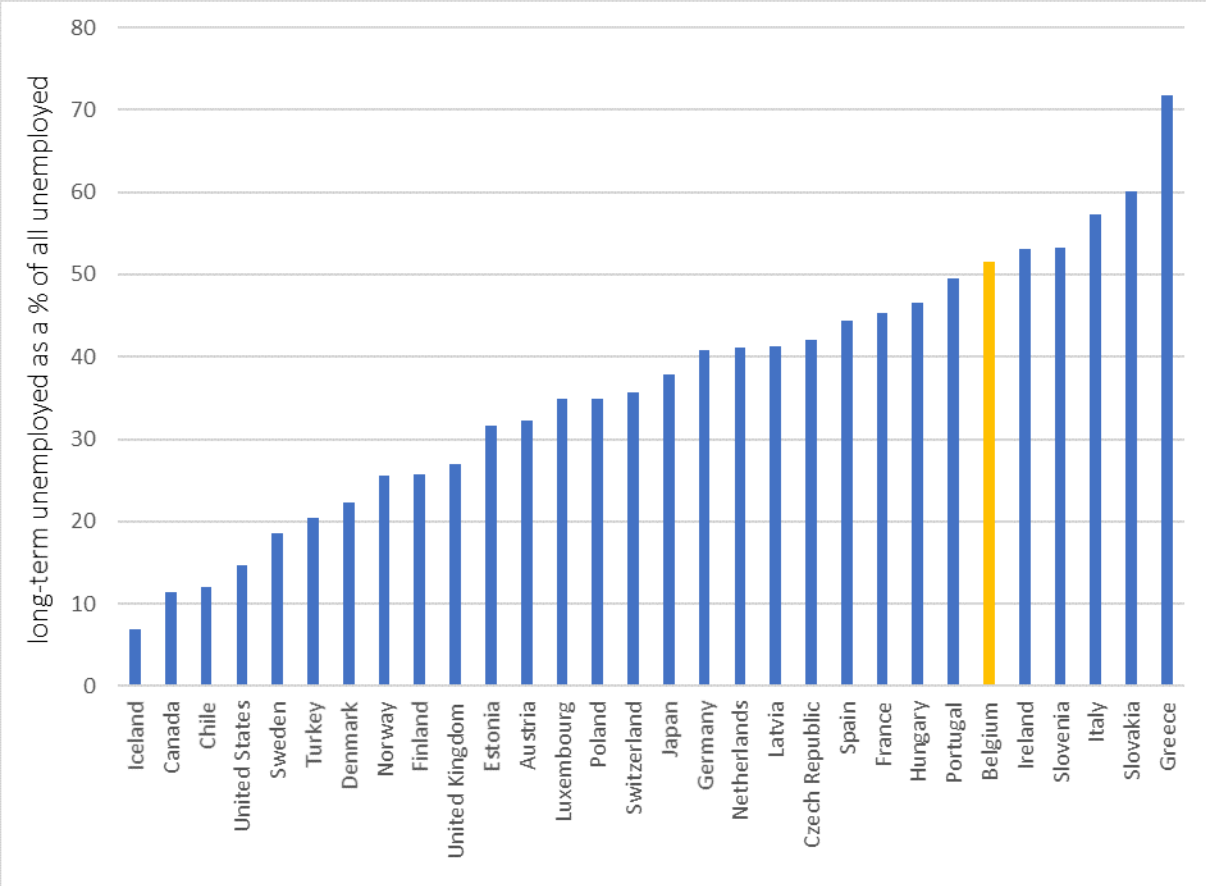
List of Tables

Chapter 1

Introduction

Unemployment is without a doubt a hot topic. The tiniest fluctuation in the unemployment rate is the topic of a newspaper article, while members of our government regularly use catchphrases like "jobs, jobs, jobs". But why are we so concerned with unemployment? There is of course the most straightforward reason that individuals need a job in order to make a livelihood and have the ability to fulfil basic needs like food and housing. Furthermore, unemployment has been related to a myriad of different issues. On a personal level, it has been shown that an unemployment spell is linked to lower physical health (Mathers and Schofield, 1998) and an increased mortality rate (Roelfs et al., 2011). In addition, even stronger negative effects have been reported on mental health and well-being. Dooley et al. (1996), Jefferis et al. (2011) and Mathers and Schofield (1998) show that an unemployment spell increases the probability of depression and suicide rates. These findings are confirmed by a number of meta-analyses (Murphy and Athanasou, 1999; Paul and Moser, 2009). Moreover, being unemployed also negatively influences the extent of one's social network, which is in itself related to a lower wellbeing (Atkinson et al., 1986; Kieselbach, 2003). For a society, a higher unemployment rate also entails large costs. First and foremost, the higher the unemployment rate, the more stress is placed on the welfare state, as there is a smaller share of active contributors financing higher expenditures on unemployment insurance (UI). In addition, a society with a high unemployment rate does not make full use of its human resources and can therefore experience lower economic growth. Moreover, a higher unemployment rate has been related to higher incidence of (property) crime (Aaltonen et al., 2013; Bennett and Ouazad, 2016; Raphael and Winter-Ebmer, 2001) and a higher incidence of domestic violence (Anderberg et al., 2016). Finally, it has been demonstrated that societies with a higher unemployment rate

Figure 1.1: The Percentage of all Unemployed who Have Been so for More than 1 year (OECD-countries)



Note. Data are collected from ILOSTAT. Data is for the last available year.

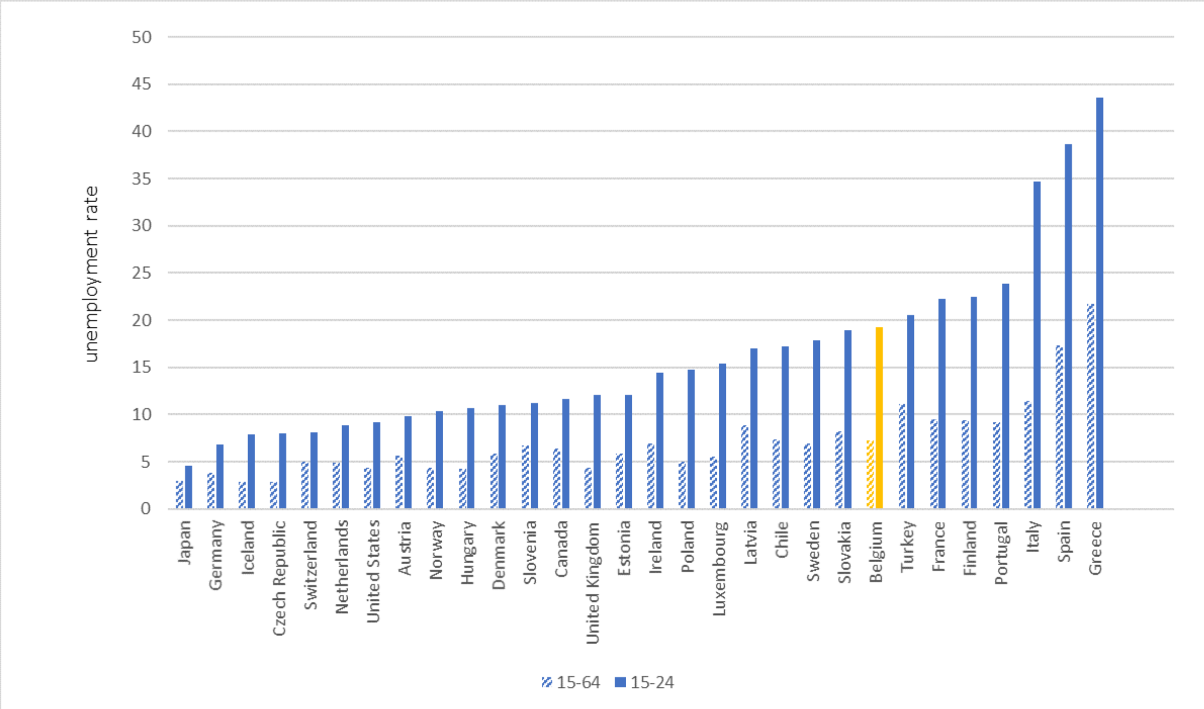
exhibit a higher prevalence of xenophobia (Steininger and Rotte, 2009). Nevertheless, a certain level of unemployment is unavoidable–and not necessarily bad–as people need some time to look for a job that fully matches their interests and capacities. The above mentioned issues prevail when this short-term frictional unemployment becomes long-term or structural. The European Union (EU) defines long-term unemployment as unemployment lasting more than one year. Looking at the data, we have to conclude that long-term unemployment is a reality in many countries. According to data from ILOSTAT, worldwide, an average of 39% of the unemployed have been so for more than one year. Figure 1.1 depicts this indicator for OECD-countries.

Overall, 36 percent of all unemployed have been in this position for more than one year. However, this average value hides large cross-country differences. Iceland, Canada, Chile and the US all have a relative low percentage of long-term unemployed (i.e. around 10% of all unemployed), while at the other side of the spectrum Greece exhibits an extremely high rate of 72%. Belgium–

depicted in yellow–is one of the worse students of the class. In our country, more than half of all unemployed have been so for at least one year. Off course, the simple observation that a large share of all unemployed individuals has been unemployed for at least one year does not tell us anything about the reasons why this might be the case. Nevertheless, we know that the longer an individual has been unemployed, the lower her/his chances are of exiting this state (Biewen and Steffes, 2010; Cockx and Dejemeppe, 2005; Cockx and Picchio, 2013; Imbens and Lynch, 2006; Luijkx and Wolbers, 2009; Mooi-Reci and Ganzeboom, 2015; Plum and Ayllón, 2015; Shimer, 2008). The economic literature has offered two alternative explanations for this observation. On the one hand, it could be the case that some people are simply less employable than others because they are less capable or have lower skills. These individuals will exhibit both a higher chance of being unemployed and a lower chance of finding employment. In other words, the most employable individuals exit unemployment first and what remains is a pool of individuals who will have a lot of trouble finding work. On the other hand, it might also be the case that it is the unemployment spell that causes the lower hiring probabilities. In this case, individuals that experience a short unemployment spell throughout the course of their working life face the threat of ending up in a vicious circle, where an unemployment spell lowers ones chances of finding work, which in turn lengthens the unemployment spell. This is what is referred to as the scarring effect of unemployment. Recently, some large scale field experiments have shown that at least part of the explanation is the scarring effect (Eriksson and Rooth, 2014; Kroft et al., 2013). Two broad theories could explain this effect. Firstly, it could be that the length of the unemployment spell constitutes a negative signal to potential employers (Arrow et al., 1973; Blanchard and Diamond, 1994; Jarosch and Pilossoph, 2018; Spence, 1978; Vishwanath, 1989). A second theory is that of human capital depreciation (Becker, 1962, 1994) which state that the unemployed will see a deterioration of their skills over the course of the unemployment spell (Acemoglu, 1995; Mincer and Ofek, 1982). I will have a closer look at the causes of the observed scarring effect of unemployment in the first study (Chapter 2) of this dissertation.

A second type of unemployment that is of special concern to society–and this dissertation–is youth unemployment. Indeed, literature has shown that an unemployment spell in the beginning of ones career can have long-lasting impacts on the rest of her/his working life (Cockx and Picchio, 2013). Moreover, as Figure 1.2 clearly shows, unemployment rates among youth are systematically higher than overall unemployment in OECD-countries. Belgium (again depicted

Figure 1.2: Unemployment Rates by Age



Note. Data are collected from ILOSTAT. Data is for the last available year.

in yellow) is once more at the left side of the spectrum, indicating that–again–only a couple of countries (mainly in Southern Europe) exhibit higher unemployment rates among youths than here. Chapter 4, will have a closer look at these issues.

Agreeing on the fact that unemployment–and especially long-term unemployment and youth unemployment–is an issue of concern, there is a scope for adequate labour market institutions and policies. Furthermore, given that the first study confirmed that an unemployment spell constitutes a negative signal towards employers, it seems of crucial importance that labour market policies aim to help the unemployed to find a job from the very first moment they become unemployed. Labour market policies are usually classified in two broad categories: Active Labour Market Policies as opposed to the passive income support policies. While income support policies exist to provide a financial buffer to bridge temporary periods of unemployment (think about unemployment benefits or social assistance), active policies aim to enhance the beneficiary's prospect of finding gainful employment. These types of policies usually take on the form of job application counselling and monitoring, training, employment subsidies or direct job creation. While different viewpoints exist on what constitute an 'Active Labour Market Policy', in this dissertation, I will follow the most common definition and will only look at policies of this nature that are temporary and targeted.

While originally labour market policy consisted mainly of passive policies, the rising unemployment rates since the 1970s has led to a marked increase in the use of active labour market policies (ALMPs) in order to enhance the transition from unemployment to employment (ILO, 2014; Martin, 2014). In the early days of active labour market policies, it was believed that in order to activate the unemployed, public spending needed to shift from passive to active policies (Martin, 2014). However, this view ignored some important linkages between the generosity of unemployment insurance, the size and structure of ALMPs and to what extent the first depends on participation in the second. The lack of evidence showing that countries applying this activation strategy exhibited better labour market outcomes has led to a new view in which both active and passive policies are recognized as essential components of a broader labour market policy.

Question remains whether labour market policies are really the best way to tackle unemployment. One could argue that these programs merely increase the employability for those individuals participating in the program but, unless more private-sector jobs are created, this will be at the expense of other individuals. The second study of this dissertation (Chapter 3) investigates this issue. In this study, we look at labour market policies from a macroeconomic perspective, i.e. we answer the question whether countries that invest more funds in labour market policies have better–overall–labour market outcomes. The results of Chapter 3 clearly show that there is indeed scope for labour market policies as spending in Active Labour Market Policies lower unemployment and increase employment and labour force participation. Nevertheless, these effects only materialize given that sufficient amounts are spent in Passive Labour Market Policies. This demonstrated overall effectiveness of labour market policies highlights the importance for a scientifically sound evaluation of which specific policies work best (or which don't) (Card et al., 2010, 2017; Greenberg et al., 2003; Heckman et al., 1999; Kluve, 2010). In the remainder of the dissertation I will analyse the effectiveness of labour market policies from two different perspectives. Chapter 4 evaluates the impact of a specific combination of an active policy (the youth work plan) and a passive policy (the waiting benefit) on the duration in unemployment for Belgium. Finally, in Chapter 5, I venture beyond the question whether or not a policy is effective and assess the mechanisms behind this observation for a specific ALMP, namely the job vacancy referral in Flanders, Belgium.

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Chapter 2

Why Are Employers Put Off by Long Spells of Unemployment?[1]

2.1 Introduction

In social stratification research, the experience of unemployment has been described as a *trigger event* (DiPrete, 2002; Gangl, 2004, 2006), that is, a critical, stressful and potentially disruptive life course event often taking a severe economic and psychological toll on those affected (for a review: Brand, 2015). With the economic downturn of recent years, the number of people going through a spell of unemployment as well as the average length of unemployment spells have been on the rise (OECD, 2013), drawing renewed attention to the potential scarring effect of unemployment on future re-employment chances. As employers are particularly wary of lengthy gaps in the résumé that are unaccounted for (Bills, 1990), unemployment tends to be self-reinforcing, possibly stigmatising the long-term unemployed (hereafter: LTU) in employers' perceptions. Indeed, a number of studies in both sociology and economics have pointed to the negative duration dependence of unemployment—the observation that an individual's probability of exiting unemployment decreases the longer she/he is unemployed (e.g. Cockx and Picchio, 2013; Luijkx and Wolbers, 2009; Mooi-Reci and Ganzeboom, 2015).

Recently, large-scale field experiments conducted in Sweden and the United States have shown that at least part of the negative duration dependence of unemployment has a demand-side

[1] In collaboration with Stijn Baert, Ralf Caers, Marijke De Couck, and Valentina Di Stasio.

explanation: employers are reluctant to hire LTU (Eriksson and Rooth, 2014; Kroft et al., 2013). In these résumé-based audit studies, fictitious job applicants with a longer unemployment spell received significantly fewer job interview invitations than identical applicants with a shorter spell. However, while field experiments of this kind are convincing for the clean measurement of unemployment scarring, they do not allow disentangling the reasons for this pattern: long-term unemployment is shown to be used as a negative signal by employers, but it remains unclear what exactly is signalled by longer unemployment spells.

In this study, we explore the empirical importance of four perceptions potentially underlying employers' reluctance to hire LTU, namely, the perception that LTU: (i) possess skills or characteristics that are not directly observed but considered less than optimal for the job, (ii) have experienced a deterioration of skills during the unemployment spell, (iii) are less trainable than candidates without long unemployment spells, and (iv) have been negatively evaluated by other employers and therefore deemed undesirable employees. To this end, we propose a state-of-the-art vignette experiment in which Flemish professionals involved in real-life hiring processes reveal their hiring intentions with respect to job candidates with different unemployment durations.[2] In addition, the survey module in which the vignette experiment is embedded provides us with rich information about the reasons underlying employers' preferences. This allows us to examine the empirical power of the four signals by estimating a multiple mediation model. Thereby, our study complements (and is consistent with) the evidence obtained from employer surveys (Atkinson et al., 1996; Bonoli, 2014) which, however, are more likely to be biased by socially desirable response patterns. In comparison, vignettes are a powerful method to analyse socially sensitive questions (Auspurg, Hinz, et al., 2014) and the possibility they afford to present employers with detailed scenarios is an important methodological advantage as employers are more likely to report negative views of specific unemployed applicants than when questioned in very general terms (Bonoli, 2014).

This study contributes to the literature on unemployment scarring by looking more closely at the demand-side mechanisms that can trap unemployed job seekers in long-lasting periods of joblessness. Our findings show that employers' reluctance to hire LTU is to a large extent

[2] Belgium is a federal state with three regions. Flanders is the largest region, situated in the North. While unemployment rates in Belgium are comparable to the average of the Eurozone, the share of long-term unemployment (i.e. one year or more) is more than 50% (ILOSTAT), which is fairly high in international comparison. In particular, in Flanders, the share of long-term unemployment was 50.3% in 2018 (source: Public Employment Agency of Flanders).

mediated by their perception of unemployment as signalling lower motivation. A smaller fraction of the total effect of unemployment duration on hiring intentions is associated with rational herding, that is, the belief that other employers found the candidate's productivity to be low. Understanding why employers refrain from hiring LTU is crucial to design activation policies that are effective in re-inserting them into the labor market. Our study is a contribution in this direction. For example, if the unemployed (and caseworkers) are made aware of the (mis)perceptions standing in the way of their employment opportunities, they may attempt to compensate for these perceptions, for instance, by underlining relevant personal characteristics and attainments in their résumé.

The remainder of this article is structured as follows. Section 2.2 gives a brief overview of the four theoretical explanations for employers' reluctance to hire LTU, and the associated signals, as found in the multidisciplinary literature on this topic. Section 2.3 describes the experiment we conducted. The experimental data is then analysed in Section 2.4. Section 2.5 5 concludes with some take-away messages for scholars as well as for interested policy makers. In addition, in this last section, we discuss the limitations of our experimental design.

2.2 Theoretical Framework

Theories explaining the phenomenon of negative duration dependence of unemployment are abundant in both the fields of sociology of work and occupations and labour economics. The observed reluctance to hire LTU can have many possible sources, both on the demand- and supply-side of the labour market. While the demand-side explanations reviewed in this study influence the unemployment duration through the perceptions of employers, supply-side explanations attribute to the negative duration dependence by actual changes in the behaviour or productivity of workers over the course of the unemployment spell.[3] However, in our vignette experiment,

[3] We note three such supply-side explanations. First, a long unemployment spell might reduce one's search intensity when looking for a job. Clark et al. (2001) showed that the unemployed can become indifferent to the prospect of becoming employed after a lengthy unemployment spell. A second explanation is the lack of a network experienced by LTU (Calvo-Armengol and Jackson, 2004). Finally, human capital theory (Becker, 1962, 1994) predicts that LTU will experience skill loss over the course of the unemployment spell. It is important to note that these supply-side explanations could have a demand-side effect through the associated perceptions of employers. Indeed, the important difference between both groups of explanations is the mechanism behind them. While the demand-side explanations assume that the hiring process is characterised by asymmetric information and that, as a result, employers make assumptions based on group differences, the supply-side explanations on the other hand assume that employers adequately evaluate changes in productivity due to the long unemployment spell.

explanations for the negative duration dependence of unemployment that are situated on the supply-side are ruled out by design.

Under the umbrella of signalling theory, we can bracket various models in the social and behavioural sciences, arguing that when people are confronted with asymmetric information, they use the limited available information as a signal for other, unobserved factors related to one's productivity (Arrow et al., 1973; Eriksson and Rooth, 2014; Kroft et al., 2013; Spence, 1978; Vishwanath, 1989). Accordingly, employers could rely on candidates' employment history as a screening device to filter out job candidates. What remains unclear however, is what exactly is signalled by a long unemployment spell. In this study, we focus on four signals that are related in the literature to long-term unemployment: (i) a signal of (lower) fixed skills and characteristics, (ii) a signal of skill loss, (iii) a signal of (lower) trainability, and (iv) a signal of rejection by other employers.

In the most direct interpretation of signalling theory, employers could see a long unemployment duration as a signal of unobserved skills or characteristics that are innate or fixed over time. In this sense, a long unemployment spell can be a signal of lower motivation (Luijkx and Wolbers, 2009) or lower intellectual and social capabilities (Vishwanath, 1989), both of which are negatively associated with productivity. As these characteristics are unobserved by employers at point of hire, unemployment spells may be used as proxies instead.

On the other hand, employers could also believe that a worker's productivity is dynamic and deteriorates over the course of an unemployment spell. Put differently, employers could believe in skill loss or skill depreciation. This mechanism is related to human capital theory, as first described by Becker (1962, 1994). Crucial is that it is costly for the unemployed to maintain their skill level during the stretch of unemployment (Acemoglu, 1995; Mincer and Ofek, 1982). Moreover, employers cannot detect the genuine level of skill depreciation of a (long-term) unemployed applicant. As shown by Acemoglu (1995), these two observations may result in an inefficient equilibrium in which employers discriminate against LTU due to the perceived skill loss (and, as a result, the unemployed do not invest to maintain their skill level).

Two more specific applications of signalling theory are also widely cited in this context. A first particular application relates long-term unemployment to (a signal of) lower trainability. Following queuing theory (Thurow, 1975), employers may rank all job candidates by their (perceived) trainability, with the person they believe will be easiest to train holding the first position in the queue and the person they perceive as the least trainable holding the last. Subsequently, these employers decide on a cut-off and only the individuals above the cut-off are invited for a job interview. Because employers, again, do not possess full information, they have to use the limited information available to assess a job applicant's trainability (Di Stasio, 2014). If employers believe unemployment has a negative effect on trainability, people with a longer unemployment spell will be ranked lower in the labour queue and, as a consequence, have a lower chance of getting invited for a job interview.

The final application of signalling theory we consider stipulates that, when making the decision to invite someone for a job interview, employers follow the behaviour of other employers —a behaviour also known as rational herding (Banerjee, 1992; Bonoli and Hinrichs, 2012; Oberholzer-Gee, 2008). One such factor from which employers might infer the screening behaviour of their colleagues is job candidates' unemployment durations. Qualitative studies have indicated that employers assume the time out of work is spent looking for a job, but, since the candidate is still unemployed, this search must have been unsuccessful (Bonoli, 2014). If the unemployment spell is relatively long, employers might conclude that other employers have repeatedly found the candidate's productivity to be low and decide that it is unprofitable to hire the candidate.

In what follows, we will explore how these key perceptions mediate the effect of unemployment duration on hiring intentions. Note that the we do not intend to demonstrate that, for example, LTU actually lose specific skills or become less motivated while out of work (i.e. to test supply-side explanations), but only that employers believe they do. In other words, when looking at unemployment scarring from a demand-side perspective, employers' perceptions are both crucial and sufficient for scarring effects to materialise.

Correspondence tests have provided evidence for negative signalling effects related to long-term unemployment. In this kind of experiment, sets of fictitious résumés, differing only in the characteristic of interest that is randomly assigned, are sent to real job openings. By measuring the subsequent invitations received from employers (i.e. callbacks) unequal treatment can be identified in a causal manner (Baert, 2018). Using this methodology, it has been shown that a wide range of factors constitute a signal in the hiring process, including ethnicity (Baert, Cockx, et al., 2015; Kaas and Manger, 2012; Oreopoulos, 2011), gender (Baert, De Pauw, and Deschacht, 2016; Petit, 2007; Riach and Rich, 2006), and age (Ahmed et al., 2012; Baert, Norga, et al., 2016; Lahey, 2008). Studies using this methodology have also looked at the signal of long unemployment durations. While Farber et al. (2016) found no significant scarring effect of long unemployment spells on callbacks, the majority of studies reported, indeed, lower callback probabilities for LTU (Eriksson and Rooth, 2014; Kroft et al., 2013; Oberholzer-Gee, 2008).

Having established that a long unemployment spell is a negative signal towards employers, the question remains what is signalled by this long unemployment spell. This has been the topic of a number of qualitative studies. Atkinson et al. (1996) administered a telephone survey with 800 representative employers in the United Kingdom. They concluded that employers believe LTU do possess the necessary skills but they are nevertheless less attractive due to a recent deterioration in these skills—pointing towards a negative signal of skill loss—and, most importantly, a lower motivation. A perceived lower motivation was also the main reason why 722 Swiss employers surveyed by Bonoli (2014) were reluctant to hire LTU. Bonoli and Hinrichs (2012) reached similar conclusions based on 41 semi-structured interviews with employers in six European countries. In addition, they found evidence for rational herding, i.e. the employers stated that LTU must have been deemed unproductive by previous employers. Lastly, Oberholzer-Gee (2008) carried out 766 telephone surveys with Swiss employers and found evidence for a signal of skill loss and a signal of negative evaluation by other employers. To the best of our knowledge, we are the first to approach this question using experimental methods (and to tease out the signals' relative importance).

2.3 Experiment

In order to not only determine whether job candidates' unemployment duration affects their hiring chances, but also gain an insight into the thought process leading to this pattern, we conducted a vignette study. Vignette studies are based on the factorial survey method (Auspurg and Hinz, 2014; Rossi and Nock, 1982) and are commonly used to study human judgements (Jasso, 2006; Wallander, 2009). In recent years, this method has been increasingly used to study dynamics in hiring decisions (e.g. Di Stasio, 2014; Liechti et al., 2017).

Each participant in a vignette experiment is asked to judge several short hypothetical descriptions of situations or individuals described on vignettes, whose characteristics (*factors*) vary randomly or systematically over a defined number of categories (*levels*). As a consequence, correlations between the vignette factors are minimised to a value close to 0. This orthogonal design allows a causal interpretation of the effects of the vignette factors on participants' judgements. When employed to study hiring intentions, vignettes typically list various characteristics of fictitious job applicants who are evaluated by the participants of the experiment. The simultaneous manipulation of different applicant characteristics closely resembles the multidimensional nature of selection decisions in the field, as in practice employers also compare candidates who vary on a number of characteristics, such as gender, level of education, and employment history.

2.3.1 Vignette Design

We asked a sample of professionals familiar with real-life hiring processes (referred to as employers from here on) to evaluate a set of five vignettes describing each a fictitious job applicant. The job applicants varied in five factors, presented in Table 2.1.[4] The vignette factor of main interest for our study is the unemployment duration, operationalised as the number of months a candidate reported to have been unemployed prior to the job application. In line with Kroft et al. (2013), this number could take on any integer from 1 to 36 (resulting in 36 vignette levels for this factor). By means of this flexible approach, we did not have to make any prior judgement on the time-pattern of unemployment scarring. As can be seen from Table 2.1, the fictitious candidates also differed in gender (male or female), highest degree obtained (secondary education or bachelor's degree), work experience (two or five years), and participation in social activities (none or volunteering activities). These factors were chosen on the basis of our literature review and tested over the course of explorative interviews with three HR professionals. We also ran a pilot study with 30 master's students in economics to assess whether our vignettes were perceived as credible, which reassured us that no crucial information was omitted. We should make two important notes. Firstly, our choice to include a continuous unemployment duration, resulting in one vignette factor with 36-levels (as opposed to two levels for the other factors), can cause a 'number of levels' effect (Auspurg and Hinz, 2014). However, as the aim of our study is not to compare the relative importance of different vignette factors, we do not think this is a major issue. Moreover, including these 36 levels in our models allows us to exploit a larger variance

[4] In the methodological literature on vignette experiments (Auspurg and Hinz, 2014), five is the lower bound suggested for the number of vignette factors. We decided to stick to this minimum to limit respondents' fatigue, taking into account the relatively large number of judgements we asked them to make for each vignette (see Section 2.3.2).

in this variable and avoids a choice for arbitrary vignette levels capturing short- and long-term unemployment. Secondly, it could be the case that some combinations of vignette factors are implausible. Indeed, even though long-term unemployment is high in Belgium (see endnote i) one could imagine that employers are unlikely to have been confronted with, for instance, candidates with a bachelor degree and/or five years of experience who have been unemployed for the full 36 months. Therefore, we will report on a robustness check in which implausible vignettes were excluded.

Table 2.1: Vignette Factors and Levels

Vignette factors	**Vignette levels**
Gender	{Male, Female}
Highest degree obtained	{Secondary education degree, Bachelor's degree}
Previous work experience	{Two years of experience, Five years of experience}
Mentioned social activities	{None, Volunteering}
Unemployment duration	{1 month, 2 months, . . . , 36 months}

Note. The factorial product of the vignette levels (2 x 2 x 2 x 2 x 36) resulted in 576 possible combinations. Three hundred vignettes were sampled from this universe using a D-efficient design (D-efficiency: 99.820; Auspurg and Hinz, 2014). These vignettes were blocked into 60 decks containing five vignettes each. These decks were distributed at random to the participants. This guaranteed that the vignette factors were nearly orthogonal, as shown in Table 2.6.

After fully crossing all the vignette levels for the five factors, we obtained a vignette universe of 576 (i.e. 36 x 2 x 2 x 2 x 2) vignettes. We sampled 300 vignettes out of this universe using a D-efficient randomisation following the Kuhfeld (2010) algorithm as explained in Auspurg and Hinz (2014). This resulted in a very high D-efficiency of 99.820. In a second step, we grouped these vignettes (again following Kuhfeld (2010)) to create 60 decks with five vignettes each. These decks were distributed at random to the participants. It is important to note that one of these decks was not effectively evaluated, while the other (59) decks were evaluated at least once. This could result in a low efficiency of the post-survey sample. The ensuing post-survey correlations among the vignette factors are shown in Table 2.6 (in Section 2.6). While this is no test of post-survey efficiency, it is nevertheless comforting that all of these correlations are sufficiently small and not statistically different from 0.

2.3.2 Data Collection

Our vignette experiment was integrated into a large-scale web-based survey sent to individuals living in Flanders, in January 2017. More concretely, the survey was sent to 89,847 individuals who selected themselves into a database of people interested in participating in research on human resource management (in response to calls via e-mail and social media). In the first question, each individual was asked whether she/he had been involved in evaluating job candidates for a minimum of five vacancies over the last year. In order to closely mimic real-life hiring decisions, we wanted to conduct our experiment exclusively with professionals familiar with the hiring process. Therefore, the answer to this first question determined whether a person was eligible to take part in our experiment. If this first question was answered positively, she/he was assigned with a chance of 0.50 to our experiment (and with a similar chance to another one). Otherwise, she/he was referred to a regular, policy-oriented survey on burnout. A total of 10,488 individuals answered this first question, giving us an overall response rate of about 12%. Out of these respondents, 475 indicated being actively involved in the hiring process a minimum of five times over the last year, of which 242 were assigned to our experiment. Twenty-three among them left one or more questions unanswered, leaving us with a final sample of 219 participants with complete responses. These 219 participants were comparable to the initial 242 participants in terms of the participant characteristics that are discussed below and reported in Table 2.7 in Section 2.6.[5] As each participant rated five vignettes, the number of (participant x vignette) observations is 1,095.

At the beginning of the web-based survey, participants were introduced to their role as employer at a fictitious company selling building materials. This company was in search of a counter assistant, which corresponds to ISCO-08 category 4200 (customer services clerks). We selected this occupation because it is transversal to a number of industries, thus increasing the chance that respondents would be familiar with it (we discuss the research limitations related to this choice in Section 2.5). Participants were explicitly informed that this counter assistant should be (i) customer-oriented, (ii) service-minded, and (iii) commercially oriented. The assistant was also expected to be efficient and reliable in managing administrative tasks. These instructions were presented to all participants in the same way at the beginning of the survey. Subsequently,

[5] We assessed the difference in means between the initial 242 participants and the 219 participants with complete responses using t-tests. The results of these tests are available upon request.

participants were shown the vignettes describing five fictitious candidates. It was stressed that these candidates were formally qualified for the job. Information about the candidates was presented in a tabulated way. We chose this format because 'tabular vignettes might be better suited to decision tasks (i.e. resumes or many consumer product descriptions), which frequently involve lists of decision criteria[, compared to text vignettes]' (Auspurg and Hinz, 2014: p. 70). Participants were not informed about the goal of the experiment.

After this, participants were asked to indicate, for each vignette, their intention to hire the candidate by rating the statements 'The probability that I will invite this candidate for a job interview is high' and 'The probability that I will hire this candidate for the position is high' on a 7-point Likert scale (with 1 'completely disagree' and 7 'completely agree'). We will refer to these items as the 'interview scale' and the 'hiring scale', respectively, and consider both outcomes separately.

In view of investigating the signals associated with the unemployment duration, participants were additionally prompted to rate eight statements for each candidate, linked to the four signals described in Section 2.2, on a 7-point Likert scale. These statements are reported, signal by signal, in Table 2.2.[6]

To make sure that our selection of signals was exhaustive, we complemented our literature review with three exploratory interviews with HR professionals (as described in Section 2.3.1). Here we asked whether they would hire a person with a long unemployment spell and, if not, which reasons they voiced for this decision.[7] Independently, all HR professionals linked long-term unemployment to lower motivation and/or fewer hard or soft skills. Related to skill loss, the fact that the workplace goes through quick technological changes over the course of an unemployment spell was also cited multiple times. Next, we discussed the four signals we selected and whether any of these perceptions had ever driven their hiring decisions in practice. The HR professionals evaluated all four signals as relevant.

Firstly, we included three statements to test for the possibility that long-term unemployment may signal (a lower level of) fixed skills and characteristics. Participants were asked whether

[6] One should note that the order of these statements did not vary between vignettes, therefore we cannot exclude an order effect (McFarland, 1981).

[7] The HR professionals were first shown a résumé of a candidate with an unemployment spell of four years and were asked whether they would consider hiring this candidate, and why (not). In the second part of the interview, we talked about 'long unemployment spells' in more general terms, allowing it up to the discretion of the HR professional to determine how she/he interpreted this.

Table 2.2: Signals and Accompanying Statement(s)

Signal (and related scale)	**Statement: content (and label)**
Fixed skills (fixed skills scale)	1. I think this person will be sufficiently motivated to perform properly in this job (fixed skills: motivation). 2. I think this person possesses sufficient intellectual abilities to perform properly in this job (fixed skills: intellectual capacities). 3. I think this person possesses sufficient social abilities to perform properly in this job (fixed skills: social capacities).
Skill loss (skill loss scale)	4. I think this person is sufficiently aware of the evolutions in the work field to perform properly in this job (skill loss: not up to date with technologies). 5. I think this person has lately had a deterioration in her/his general skills (skill loss: general skill loss). 6. I think this person has lately had a deterioration in her/his social skills (skill loss: social skill loss).
Trainability (trainability scale)	7. I think this person will be easy to train (trainability).
Negative evaluation by other employers (rational herding scale)	8. I think this person has often been rejected by other employers (rational herding).

Note. The potential signals are discussed in Section 2.2. The accompanying statements are transformed into the four mediation scales as described in Section 2.3.2. The scores of statement 4 were reverse scored so that a higher score became consistent with higher perceived skill loss also for this statement.

they thought the candidate was sufficiently motivated (statement 1) and had a high enough level of intellectual ability (statement 2) and social ability (statement 3) for the job. Secondly, three statements tested for perceived skill loss of the candidate. Inspired by the interviews with HR professionals, the candidate was scored with respect to being up to date with technologies (statement 4). In addition, perceived deterioration in general skills (statement 5) and social skills (statement 6) were scored. Thirdly, closely linked to queuing theory, participants were asked to rate the candidate's trainability (statement 7). Fourthly, participants judged whether the candidate had been rejected often by other employers (statement 8), which is the explanation for the negative duration dependence of unemployment put forward by rational herding.[8]

A definition of all variables collected by means of this vignette experiment and used in our analyses is given in Table 2.7 in Section 2.6. An English translation of the experimental instructions and an example of a vignette (and the related items) can be found in Section 2.7.

In the mediation model presented in Section 2.4, we include four mediators, one for each signal, based on the eight statements reported in Table 2.2. The first mediator, the fixed skills scale, groups statements 1 to 3 (Cronbach's alpha for internal consistency: $\alpha = 0.763$). Its value is, for each observation, computed as the average over these three statements. The second mediator, the skill loss scale, is based on the scores of statements 4 to 6 ($\alpha = 0.716$). The scores of statement 4 were reverse-coded (so that a higher score became consistent with higher perceived skill loss). The third mediator, the trainability scale, reflects the score of statement 7. The fourth and final mediator, the rational herding scale, corresponds to the score of statement 8.

Our choice to group statements together as we did is, to some extent, arbitrary. Therefore, we tested the sensibility of our results with respect to other strategies. For instance, an approach in which the scores of the statements were first standardised (by subtracting their sample mean and dividing the result by these scores' sample standard deviation) before grouping them did not substantially affect the results presented in Section 2.4. In addition, factor analysis yielded the same number (i.e. four) of scales, with a comparable composition. Note that we also present the mediating role of the eight separate statements (i.e. without grouping them) in an alternative

[8] Oberholzer-Gee (2008) also prompts participants to rate statements to test for different signals. The statement related to skill loss ('I prefer the candidate with a job because the unemployed applicant has lost some skills and she is not familiar with recent developments in the profession') is very close to our three statements capturing this signal. Additionally, he also includes a statement for rational herding: 'I prefer the candidate with a job because the unemployed applicant is probably not very productive. If she were productive, she would have been hired by another firm.'

mediation model.

After judging the five job candidates, participants were asked to provide some personal information, including their gender, level of education, frequency of taking hiring decisions, and experience with the hiring process (Table 2.7, Section 2.6). Overall, about 57% of our participants were female. They were mainly highly educated (almost 90% had completed some form of tertiary education), with an average age of about 42, and an average of around 10 years of experience as an HR professional. Table 2.8 (in Section 2.6) reports the distribution of our participants according to the unemployment duration of the candidates they judged to check whether our randomisation was successful. For instance, as shown in Panel A, the subsample of vignettes disclosing 3 months of unemployment or fewer and the subsample of vignettes disclosing more than 3 months of unemployment were scored by participants with comparable characteristics.

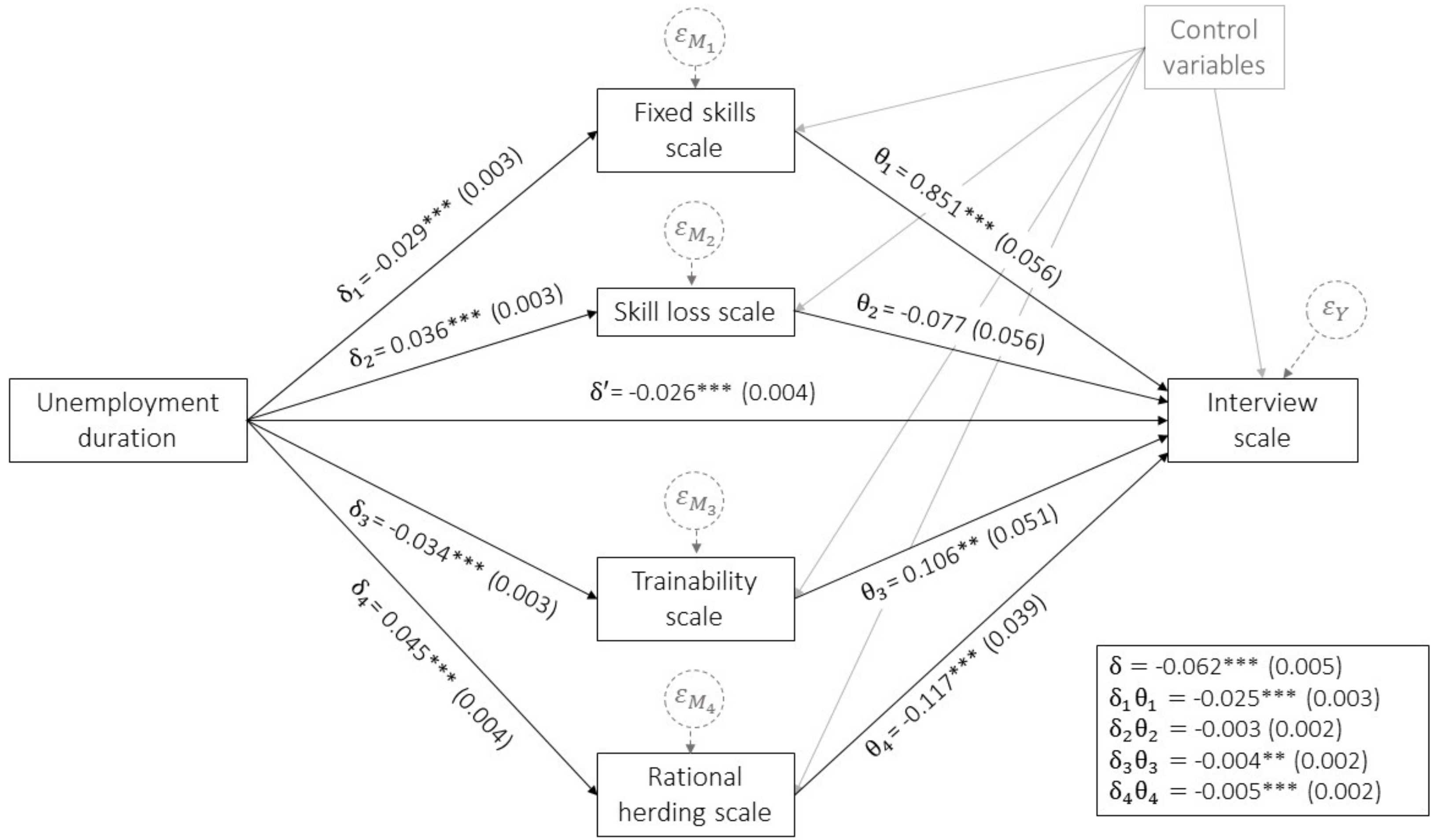
It should be noted that our sample is not representative of the population of Belgian employers, for which a sampling frame is unfortunately not readily available. We do not consider this a substantial shortcoming. Samples gathered by field experiments are similarly non-representative (they only target employers who post their job ads online in specific job banks) but still widely employed to causally test the scarring effects of unemployment. Moreover, our sample is very comparable in age and gender distribution with Belgian HR professionals in the European Social Survey, even though our sample seems slightly higher educated—the formal comparison is included as Table 2.9 in Section 2.6. We come back to this and other issues related to our experimental design in the conclusion.

2.4 Results

We estimate a multiple mediation model (Hayes, 2013) to analyse the total effect of unemployment duration on hiring intentions as well as the part of this effect passing through the four mediators. A simplified version of the estimated model is depicted in Figure 2.1.

In a first step (Section 2.4.1), we estimate the total effect of the unemployment duration of our fictitious job candidates on the employers' hiring intentions. Subsequently, we explore the mediation effects related to the fixed skills, skill loss, trainability, and rational herding scales. Each mediation effect is calculated as the product of the effect of unemployment duration on the

Figure 2.1: Mediation Model with Interview Scale as Outcome



Note. The presented statistics are coefficient estimates and standard errors in parentheses for the mediation model outlined in Section 2.4. δ stands for the total effect, δ' for the direct effect, and δ_i θ_i for the indirect effects of unemployment duration on the interview scale, passing through mediator M_i. Standard errors are corrected for clustering of the observations at the participant level. The confidence intervals for the mediation effects are based on 10,000 bootstrap samples. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level.

respective mediation scale and the association of this scale on the outcome scale (i.e. $\delta_i\theta_i$, with i ranging from 1 to 4, in Figure 2.1). In Section 2.4.2 we explore the mediation effects separately and in Section 2.4.3 we estimate the complete mediation model, in which the mediation scales are included jointly. The latter model allows us to decompose the total effect of unemployment duration into four 'indirect' effects via the mediators and a remaining 'direct' effect δ' (so that the total effect δ equates $\delta' + \sum_{i=1}^{4} \delta_i\theta_i$).

We stress that we follow the literature when labelling $\delta_i\theta_i$ as mediation *effects* but refrain from giving them a causal interpretation. The unemployment duration of our fictitious job candidates is experimentally manipulated and, as a consequence, δ and δ_i are causal effects. However, our mediators are not exogenous. Although we attempt to capture, based on our literature review, the most relevant signals potentially explaining the lower hiring chances of LTU, it is still possible that our mediators correlate with other, unobserved, employer perceptions related to candidates' unemployment. For this reason, θ_i should be seen as associations rather than as causal effects. We return to this point in Section 2.5.

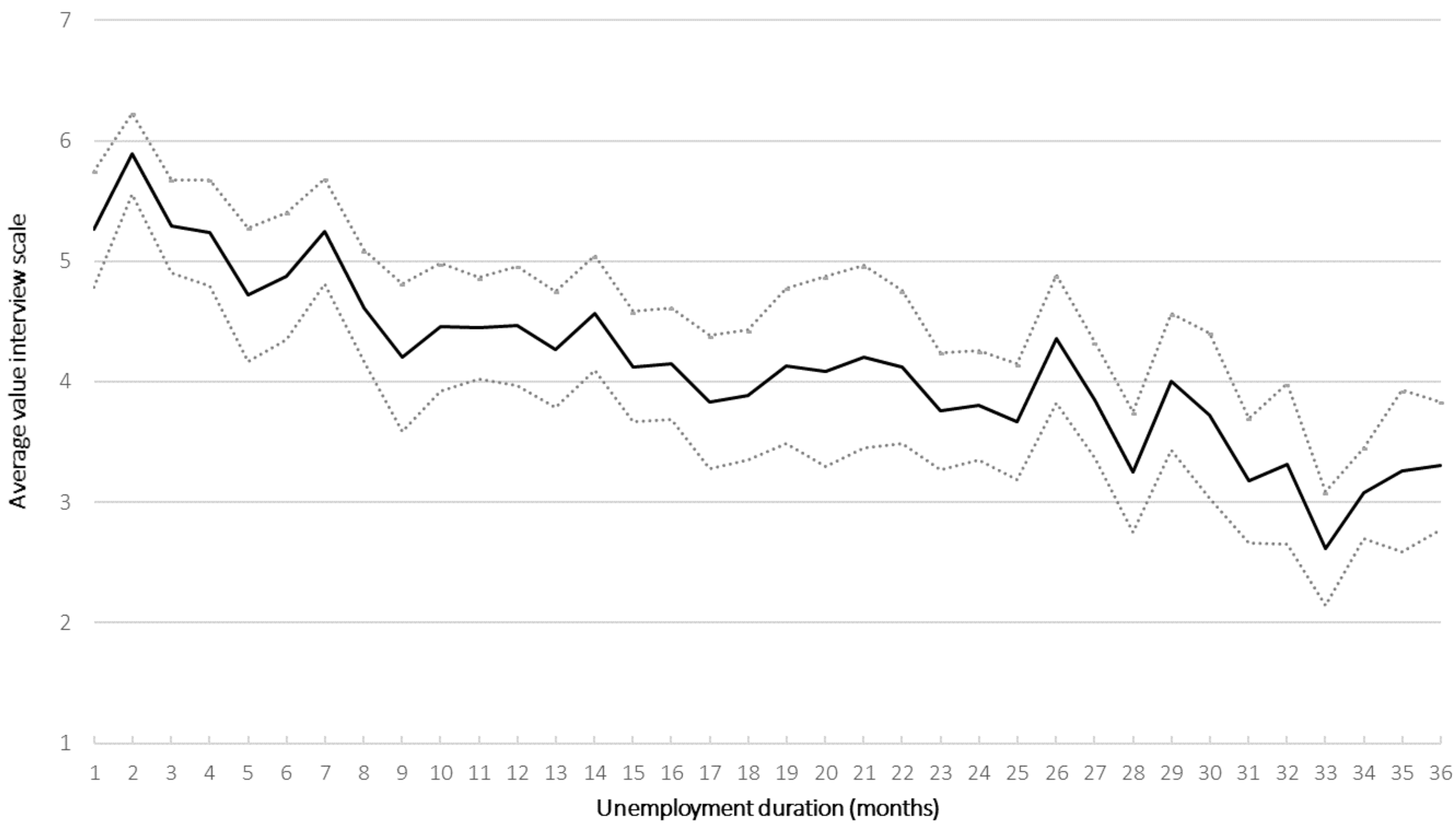
2.4.1 Unemployment Duration and Hiring Intentions

To get a first impression of the (total) effect of the candidates' unemployment duration on their hiring intentions, we plot the average scores on the interview scale of the 1,095 evaluated vignettes, by unemployment duration. As is clear from Figure 2.2, the likelihood of getting invited for an interview exhibits a clear downward trend as the unemployment duration increases. A similar pattern emerges for the hiring scale.

However, due to the relatively low number of observations for each potential unemployment duration (between 23 and 40 observations), Figure 2.2 captures some noise. A clearer picture of the total effect is presented in Table 2.3, where we compare the outcome scales for candidates with an unemployment spell of 3 months or fewer to the outcome scales for candidates with an unemployment spell of more than 3 months (Panel A), and repeat this with 12 months (Panel B) and 24 months (Panel C) as cut-off points. A t-test is used to determine whether the difference in invitation and hiring probability between these subsamples are significantly different from zero.[9]

[9] With respect to the calculation of these t-statistics, it is important to account for the nested structure of data collected through a vignette experiment, with multiple vignettes judged by the same participant (Jasso, 2006). To this end, we take into account the dependence of the error term within participants by clustering all estimated t-values at the participant level.

Figure 2.2: Average Value on Interview Scale by Unemployment Duration



Note. The thick line shows the average value on the interview scale for each unemployment duration. The dotted lines show the upper and lower bounds of the 95%-level confidence interval around these average values. The confidence bounds are corrected for clustering of the observations at the participant level.

Table 2.3: Effect of Unemployment Duration on the Score of the Outcome Scales

	A. Threshold of candidate's UD: 3 months			B. Threshold of candidate's UD: 12 months			C. Threshold of candidate's UD: 24 months		
	Mean		Difference: (A.2) – (A.1)	Mean		Difference: (B.2) – (B.1)	Mean		Difference: (C.2) – (C.1)
	UD ≤ 3 months N = 99	UD > 3 months N = 996		UD ≤ 12 months N = 395	UD > 12 months N = 700		UD ≤ 24 months N = 739	UD > 24 months N = 356	
	(A.1)	(A.2)	(A.3)	(B.1)	(B.2)	(B.3)	(C.1)	(C.2)	(C.3)
Interview scale	5.515	4.050	-1.465*** [10.660]	4.911	3.771	-1.140*** [11.047]	4.518	3.486	-1.032*** [9.997]
Hiring scale	4.859	3.583	-1.275*** [10.200]	4.339	3.337	-1.002*** [11.752]	3.988	3.098	-0.890*** [11.069]

Note. UD stands for unemployment duration. T-tests are performed to test whether the differences presented are significantly different from 0. Standard errors are corrected for clustering of the observations at the participant level. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level. T-statistics are in brackets.

As shown in Table 2.3, the probability of getting invited for a job interview is always significantly higher for candidates belonging to a subsample with a shorter unemployment spell compared to candidates belonging to a subsample with a longer unemployment spell, regardless of the chosen cut-off. For instance, the average score on the interview scale for those with an unemployment duration of 3 months or fewer is 5.515 (i.e. just between an evaluation of 'somewhat agree' and 'agree' with respect to the statement 'The probability that I will invite this candidate for a job interview is high') while it is 4.050 (i.e. close to 'neither agree or disagree') for those with an unemployment duration of more than 3 months. A similar pattern is found for the probability that a candidate is hired for the position.

Due to the orthogonal design, candidates with a longer unemployment spell are (on average) equal to candidates with a shorter unemployment spell on all vignette factors, other than their unemployment duration. In other words, the measured differences in interview invitations presented in Table 2.3 can only be driven by differences in unemployment duration. A regression-based approach yields exactly the same conclusion: a clear scarring effect of long-term unemployment.

2.4.2 Exploration of the Mediation Effects

A significant role for the mediation scales in explaining the negative relationship between unemployment duration and hiring intentions is conditional on two things. Firstly, candidates' unemployment duration should affect the mediation scales (left part of Figure 2.1). Secondly, these mediation scales should affect participants' hiring intentions (right part of Figure 2.1). In this subsection, we explore both conditions separately.

To get a first idea of the effect of unemployment duration on the four mediation scales, we examine the candidates' scores for these scales by their unemployment duration. In addition to the scores at the aggregate level, we present the scores on the level of the individual statements. As Table 2.4 shows, the unemployment duration has a significant effect on all four mediators. Candidates with a longer unemployment spell score significantly lower on the 'positive' mediators (fixed skills and trainability), while they score significantly higher on the 'negative' mediators (skill loss and rational herding). When we look at the individual statements, it is apparent that the subsample means differ highly significantly for all statements and in the expected direction. We have also checked that a positive evaluation with respect to the mediation scales is correlated

Table 2.4: Effect of Unemployment Duration on the Score of the Mediation scales and Statements

	A. Threshold of candidate's UD: 3 months			B. Threshold of candidate's UD: 12 months			C. Threshold of candidate's UD: 24 months		
	Mean		Difference: (A.2) – (A.1)	Mean		Difference: (B.2) – (B.1)	Mean		Difference: (C.2) – (C.1)
	UD ≤ 3 months N = 99	UD > 3 months N = 996		UD ≤ 12 months N = 395	UD > 12 months N = 700		UD ≤ 24 months N = 739	UD > 24 months N = 356	
	(A.1)	(A.2)	(A.3)	(B.1)	(B.2)	(B.3)	(C.1)	(C.2)	(C.3)
Fixed skills scale	4.949	4.288	-0.661***	4.660	4.172	-0.488***	4.510	4.012	-0.498***
			[6.897]			[7.540]			[6.748]
Fixed skills: motivation	4.919	4.020	-0.899***	4.516	3.867	-0.649***	4.322	3.643	-0.679***
			[7.766]			[7.933]			[7.452]
Fixed skills: intellectual capacities	5.172	4.616	-0.555***	4.914	4.527	-0.387***	4.812	4.365	-0.447***
			[5.021]			[4.897]			[5.222]
Fixed skills: social capacities	4.758	4.229	-0.529***	4.549	4.123	-0.427***	4.396	4.028	-0.368***
			[4.097]			[5.844]			[4.374]
Skill loss scale	3.182	4.054	0.872***	3.534	4.224	0.690***	3.794	4.352	0.558***
			[7.875]			[9.623]			[8.322]
Skill loss: not up to date with technologies	3.465	4.418	0.953***	3.835	4.611	0.776***	4.095	4.823	0.728***
			[7.199]			[9.149]			[8.892]
Skill loss: general skill loss	3.071	4.030	0.959***	3.430	4.233	0.802***	3.752	4.340	0.588***
			[6.772]			[8.448]			[6.278]
Skill loss: social skill loss	3.010	3.715	0.705***	3.337	3.829	0.492***	3.535	3.893	0.359***
			[4.911]			[5.930]			[4.283]
Trainability scale	4.859	4.208	-0.651***	4.653	4.049	-0.605***	4.451	3.885	-0.566***
			[5.748]			[7.996]			[8.079]
Rational herding scale	3.364	4.629	1.265***	3.922	4.849	0.927***	4.319	4.919	0.599***
			[9.028]			[10.841]			[6.546]

Note. UD stands for unemployment duration. T-tests are performed to test whether the differences presented are significantly different from 0. Standard errors are corrected for clustering of the observations at the participant level. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level. T-statistics are in brackets.

with higher hiring intentions. To this end, we calculated correlations between the mediation scales (and their underlying statements) and the interview and hiring scales. A correlation matrix is presented in Table 2.10 (in Section 2.6): all correlations are significantly different from 0 and have the expected sign.

2.4.3 Multiple Mediation Regression Model

In the multiple mediation regression model all four mediators are included jointly, following a system of linear regression equations (by analogy with Hayes, 2013):

$$M_1 = \alpha_{M_1} + \beta_{M_1} CC + \gamma_{M_1} PC + \delta_1 UD + \varepsilon_{M_1}; \tag{2.1}$$

$$M_2 = \alpha_{M_2} + \beta_{M_2} CC + \gamma_{M_2} PC + \delta_2 UD + \varepsilon_{M_2}; \tag{2.2}$$

$$M_3 = \alpha_{M_3} + \beta_{M_3} CC + \gamma_{M_3} PC + \delta_3 UD + \varepsilon_{M_3}; \tag{2.3}$$

$$M_4 = \alpha_{M_4} + \beta_{M_4} CC + \gamma_{M_4} PC + \delta_4 UD + \varepsilon_{M_4}; \tag{2.4}$$

$$Y = \alpha_Y + \beta_Y CC + \gamma_Y PC + \delta' UD + \theta_1 M_1 + \theta_2 M_2 + \theta_3 M_3 + \theta_4 M_4 + \varepsilon_Y. \tag{2.5}$$

M_1, M_2, M_3, and M_4 are fixed skills, skill loss, trainability, and rational herding mediation scales, respectively; UD is the candidate's unemployment duration; CC is a vector of other vignette factors; PC is a vector of participant characteristics; and Y is the interview or hiring scale. β_{M_i}, γ_{M_i}, and δ_i are the (vectors of) parameters associated with CC, PC, and UD in the equations with M_i as dependent variable, with α_{M_i} being the intercept. β_Y, γ_Y, δ', and α_Y are the corresponding parameters in the equation with Y as dependent variable. Finally, θ_1, θ_2, θ_3, and θ_4 are the parameters associated with the mediator scales in the latter equation. As a consequence, δ' is the remaining direct effect of the unemployment duration after controlling for the mediators. Our main interest lies in the products $\delta_i\theta_i$, namely the indirect effects of the unemployment duration on Y through each mediator M_i. In line with Hayes (2013), we estimate equations 2.1 to 2.5

simultaneously and correct the standard errors ε_{M_1}, ε_{M_2}, ε_{M_3}, ε_{M_4}, and ε_Y for clustering of the observations at the participant level.

In order to capture hiring intentions, we look at two outcomes: the interview and the hiring scale. The main results of our mediation analysis with the interview scale (hiring scale) as the Y -variable are depicted in Figure 2.1 (Figure 2.3 in Section 2.6). The corresponding full estimation results are reported in Table 2.5 and Table 2.11.

The total effect of unemployment duration on the interview scale (δ = -0.062; $p \leq 0.000$) is in line with what was reported in Section 2.4.1 One additional month of unemployment decreases the interview scale by 0.062 (i.e. about one sixteenth of a unit decrease on this scale ranging from 1 to 7). This total effect can be broken down into one direct effect and four indirect effects (one for each mediator). The direct effect, which can be interpreted as the part of the total effect that does not pass through any of the four mediators, is substantial (δ' = -0.026; $p \leq 0.000$). It accounts for 41.9% (i.e. 0.026 divided by 0.062) of the total effect, while all mediation effects together account for the remaining 58.1% —we will come back to this in Section 2.5.

Next, we investigate the relative importance of the four mediators. On the one hand, unemployment duration significantly affects all four mediation scales in the expected direction. On the other hand, only three of the mediation scales—the fixed skills scale (θ_1 = 0.851; $p \leq 0.000$), the trainability scale (θ_3 = 0.106; $p \leq 0.039$), and the rational herding scale (θ_4 = -0.117; $p \leq 0.003$)—appear to significantly influence the interview probability. Multiplying the first set of coefficients by the second set yields the mediation effects. In line with Hayes (2013), the confidence intervals for these mediation effects are based on 10,000 bootstrap samples. We find three significant mediation effects. Firstly, the effect of the unemployment duration on the interview outcome is highly significantly mediated by the fixed skills scale ($\delta_1\theta_1$ = -0.025, i.e. the product of -0.029 and 0.851; $p \leq 0.000$). This mediation effect accounts for 38.7% of the total effect. In addition, we find a smaller—but still highly significant—mediation via rational herding ($\delta_4\theta_4$ = -0.005; $p \leq 0.005$) and a small mediation via perceived trainability ($\delta_3\theta_3$ = -0.004; $p \leq 0.049$). No significant mediation via perceived skill loss is found. In other words, employers seem to believe that unemployment duration correlates with fixed (unobservable) employee characteristics rather than that the unemployment spell causes skills to deteriorate.

The total, direct, and indirect effects of unemployment duration on the hiring scale are similar to what is found with respect to the interview scale. Other secondary results, pertaining to the

Table 2.5: Mediation Analysis with Interview Scale as Outcome

Explanatory variables	**Outcome variables**				
	Fixed skills scale	Skill loss scale	Trainability scale	Rational herding scale	Interview scale
A. Candidate characteristics					
Female gender	0.112*** (0.041)	-0.084* (0.050)	0.050 (0.053)	-0.045 (0.062)	0.170*** (0.056)
Bachelor's degree	0.354*** (0.050)	-0.232*** (0.056)	0.743*** (0.066)	-0.196*** (0.066)	-0.213*** (0.071)
Five years of experience	0.146*** (0.045)	-0.179*** (0.052)	0.047 (0.056)	-0.062 (0.067)	0.061 (0.057)
Volunteering	0.475*** (0.054)	-0.361*** (0.059)	0.158*** (0.056)	-0.165*** (0.064)	0.054 (0.059)
Unemployment duration	-0.029*** (0.003)	0.036*** (0.003)	-0.034*** (0.003)	0.045*** (0.004)	-0.026*** (0.004)
B. Participant characteristics					
Female gender	0.083 (0.108)	-0.161* (0.094)	-0.065 (0.108)	-0.372*** (0.120)	-0.007 (0.106)
Age	0.005 (0.005)	-0.009* (0.005)	0.010* (0.006)	-0.019*** (0.007)	0.005 (0.006)
Highest degree obtained					
Secondary education or lower	0.181 (0.143)	-0.262* (0.159)	0.162 (0.143)	-0.211 (0.184)	0.215 (0.199)
Tertiary education: outside university	0.177* (0.105)	-0.293*** (0.091)	0.228** (0.105)	-0.016 (0.122)	0.133 (0.110)
Tertiary education: university (reference)					
Frequency of hiring: weekly	-0.055 (0.108)	0.195* (0.100)	-0.047 (0.109)	0.175 (0.117)	-0.144 (0.118)
Experience as HR professional: ≥ 10 years	-0.055 (0.122)	0.142 (0.101)	-0.085 (0.134)	0.104 (0.146)	-0.402*** (0.134)
C. Mediation scales					
Fixed skills scale					0.851*** (0.056)
Skill loss scale					-0.077 (0.056)
Trainability scale					0.106** (0.051)
Rational herding scale					-0.117*** (0.039)
Observations			1,095		

Note. The presented statistics are coefficient estimates and standard errors in parentheses for the mediation model outlined in Section 2.4. Standard errors are corrected for clustering of the observations at the participant level. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level.

role of employers' characteristics, are reported in Panel B and Panel C of both Table 2.5 and Table 2.11 in Section 2.6. We do not discuss them any further as they fall outside the scope of this paper.

As stated in Section 2.3.1 we perform a robustness analysis where we exclude candidates with a bachelor degree and/or five years of experience in combination with an unemployment duration of two years or more, as these combinations of vignette levels could be perceived as implausible. The results of this analysis (in which 108 of the 300 sampled vignettes are excluded) are reported in Figure 2.4 in Section 2.6. It is clear that our results are robust to the exclusion of these potentially implausible vignettes.

To get a picture of the relative weights of the individual statements, we re-estimate our mediation model using eight separate mediators instead of the four mediation scales. Estimation results are given in Table 2.12 and Table 2.13 (Section 2.6). These results indicate that the dominant mediation through the fixed skills scale is mainly driven by a long unemployment spell being viewed as a signal of lower motivation. Moreover, there is some evidence for an indirect effect through the 'not up to date with technologies' statement. This did not translate into a significant effect of the overall skill loss scale in our benchmark mediation model because of the (insignificant) effect of the statements capturing general skill loss and/or social skill loss.

2.5 Discussion and Conclusion

This study contributed to the multidisciplinary literature on the negative duration dependence of unemployment. It complemented recent large-scale field experiments showing that at least part of this negative duration dependence can be given a demand-side explanation: employers are reluctant to hire long-term unemployed job candidates. Using vignettes, we took the logical next step in this literature and empirically explored four theoretical explanations for unemployment scarring. Our analyses provided evidence that employers' reluctance to hire LTU is to a large extent mediated by their perception of unemployment as a signal of lower motivation. This is very much in line with findings from the qualitative study of Bonoli and Hinrichs (2012) as well as with results obtained by Atkinson et al. (1996) and Bonoli (2014) on the basis of employer surveys. We also found that a smaller fraction of the total effect of unemployment duration on hiring intentions was associated with rational herding, that is, the belief that other employers found the candidate's productivity to be low (in line with Oberholzer-Gee, 2008).

From a policy point of view, our findings show that LTU might benefit from including in their job applications a detailed statement about their motivation to find work as well as a credible justification for their time out of work. We believe that the focus in this respect should be on work motivation and not on general motivation because an additional mediation analysis with interaction variables showed that the effect of unemployment duration on hiring intentions was not moderated by applicants' engagement in volunteer work.[10] Furthermore, labour market policies should also take into account potential asymmetric information between employers and job candidates. Indeed, policies aiming to increase productivity of LTU might be ineffective if this increased productivity is not properly signalled to employers when applying to their vacancies.

We end this article by acknowledging limitations inherent to our experiment and briefly highlighting related directions for further research. Most importantly, while the estimated total effect of unemployment duration on hiring intentions (i.e. the δ of our mediation model) and its effect on the tested candidate perceptions (i.e. our δ_i) can be given a causal interpretation, this is not the case for the estimated association of these perceptions with hiring intentions (i.e. our θ_i). Given that the aim of our study is to explore all potential signals related to a long unemployment duration, we would have to experimentally manipulate these perceptions separately to be able to measure their causal impact. However, we do not see a setting in which jointly manipulating these perceptions would be feasible. Indeed, it would be very difficult to signal, for example, skill loss in a vignette in a realistic way. Nevertheless, it would make an interesting follow-up study to experimentally manipulate some of the different signals. Another interesting avenue for future research into the mechanisms behind signalling would be to experimentally manipulate the timing and continuity of the unemployment spell(s). In this way one could causally test whether these factors serve as independent signals or whether they substitute or reinforce one another.

While we found a number of interesting and significant mediation effects, we nevertheless also reported a large and significant direct effect, indicating that a considerable portion of the scarring effect of unemployment still remained unexplained (Shrout and Bolger, 2002; Zhao et al., 2010). This suggests the need for further theoretical development going beyond the four signals included. Our experiment does not allow us to identify the direction this future theory

[10] The results of this analysis are available on request.

development should take, so we can only speculate. One interesting avenue could be to look into a signal of overqualification. It could indeed be the case that when a person remains unemployed for a longer period, she/he will cast a wider net during the job search and apply for positions for which she/he is overqualified. If employers assume this to be the case, this could be a potential negative signal associated with a long unemployment spell (as overqualified candidates may not fit their low-status vacancy). The negative effect of a bachelor degree on hiring intentions is consistent with this explanation. On the other hand, the significant direct effect can also result from our statements imprecisely measuring the four signals. Indeed, measurement errors in our mediators may have resulted in downward-biased estimates for the mediation effects and an upward-biased estimate for the direct effect (Judd and Kenny, 1981; VanderWeele et al., 2012).

Contrary to field experiments, the data collection within a vignette experiment does not take place under real-life circumstances and participants are aware to take part in an experiment. Although this is an advantage from a research-ethical point of view (Charness et al., 2013; Riach and Rich, 2004) and necessary to get an insight into thought processes (Baert and De Pauw, 2014; Van Hoye and Lievens, 2003), participants may answer in a socially desirable way when not exposed to the urgency of real-life decision-making. While this is considered a serious issue for direct question-based surveys (Auspurg and Hinz, 2014), we believe this to be less of a concern in vignette experiments in general, and in our design in particular, for two main reasons. Firstly, the widespread use of vignette studies in the social and behavioural sciences is related to the fact that self-reported measures of perceptions have been shown to correlate highly with actual behaviour and that changes in intentions clearly result in actual behavioural changes (Hainmueller et al., 2015). Secondly, in a vignette experiment each participant is only shown a small number of vignettes that vary with regard to multiple factors and therefore it is almost impossible for the participant to know what the socially desirable answer is (Auspurg and Hinz, 2014; Liechti et al., 2017; Mutz, 2011). In this respect, the reader should also note that the factor of interest in our study (unemployment duration) is a generally socially acceptable screen (Bills, 1990)—much less sensitive than, for example, race—and, as a consequence, socially desirable answers are expected to be negligible.

With respect to the generalisability of our findings, our approach is subject to the same limitations as those found in the field experiments we mimicked. We only measured unequal treatment based on a single recent unemployment spell towards individuals with a specific profile (i.e. two or five years of experience, with a secondary education degree or a bachelor's degree) applying for a specific position in a specific context (i.e. Flanders). As a consequence, our findings cannot be easily generalised to settings with jobs and candidate profiles different from those used in this study, or to other geographical regions. Indeed, it is possible that the stigma of unemployment is more or less present in other settings. In particular, there may be systematic variation across countries, as unemployment is differently regulated across institutional contexts (Gangl, 2004). Similarly, the relative value of some signals related to unemployment may differ across occupations. For instance, the value of social capabilities could be lower in occupations without (much) contact with customers or co-workers. Alternatively, the reported lack of significance for the skill loss scale may be due to the fact that the occupation of counter assistant requires mainly general skills that are less subject to depreciation. More generally, Mosthaf (2014) argues that as the incidence of unemployment is more typical for low-skilled workers, the negative signals related to long-term unemployment may be weaker for them (compared with high-skilled workers).

This being said, the consistency of our results with findings from earlier studies conducted in very different contexts, namely Switzerland (Bonoli, 2014) and the United Kingdom (Atkinson et al., 1996), and different populations, including low-educated LTU in six European countries (Bonoli and Hinrichs, 2012), suggests—at the very least—that the belief that LTU are particularly lacking in motivation is widespread across employers. Nevertheless, further research is necessary to ensure the robustness of our results in other settings. With the recent economic downturn, many people have suffered a spell of unemployment: we welcome a program of research that looks more closely at the scars they carry from a demand-side perspective. For instance, semi-structured interviews with employers (e.g. Bonoli and Hinrichs, 2012) and/or employees could deepen the insights from our study. In addition, research that combines testing in the field with psychological tests in the manner of Rooth (2010) or that integrates vignettes in large-scale and possibly representative employer surveys could be very fruitful.

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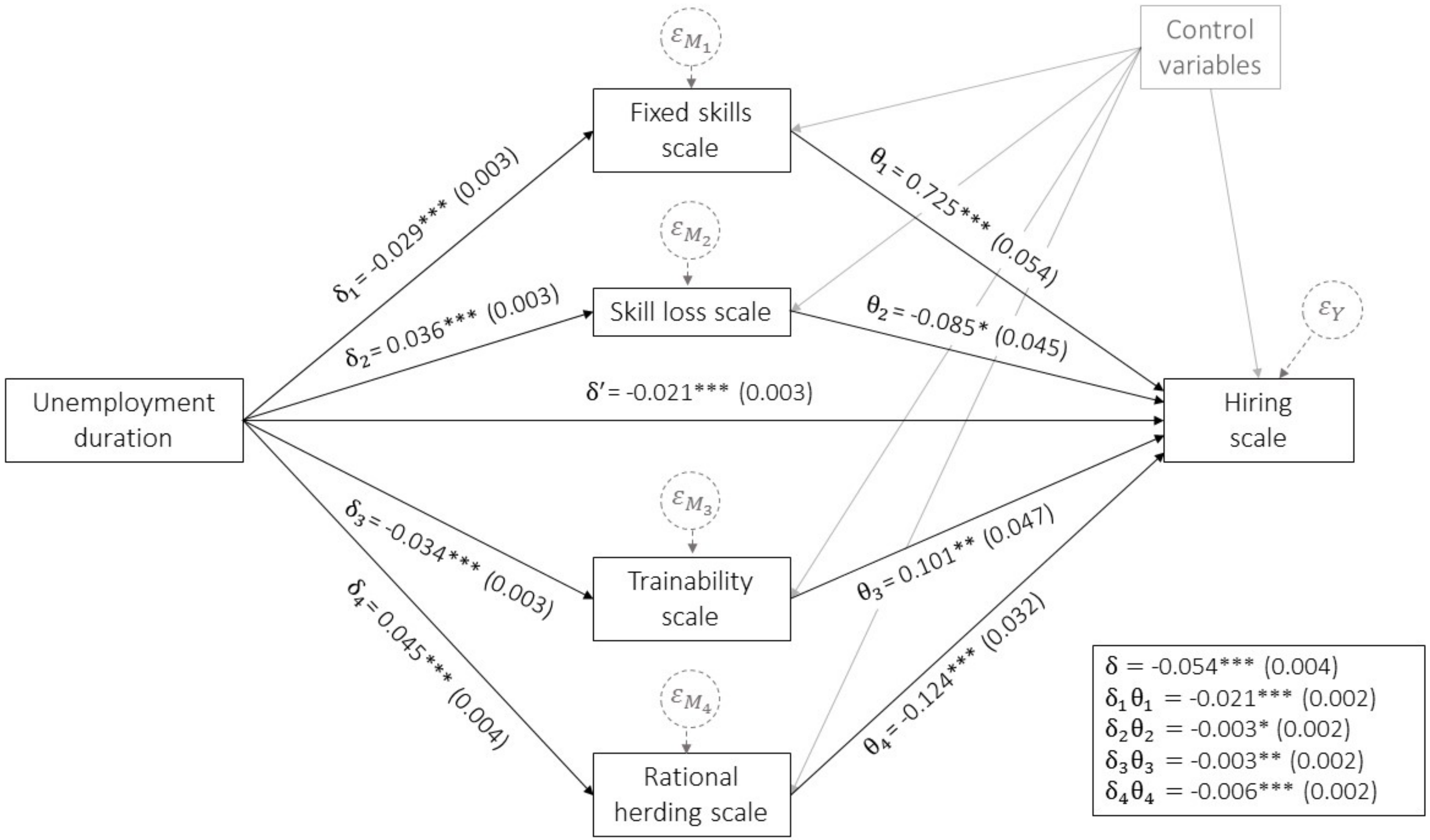
2.6 Appendix A: Additional Figures and Tables

Table 2.6: Correlations Between Vignette Factors

	1	2	3	4	5
1 Gender	1.000				
2 Highest degree obtained	0.036	1.000			
3 Previous work experience	-0.083	0.034	1.000		
4 Mentioned social activities	0.026	-0.009	0.021	1.000	
5 Unemployment duration	-0.003	-0.017	-0.005	0.023	1.000

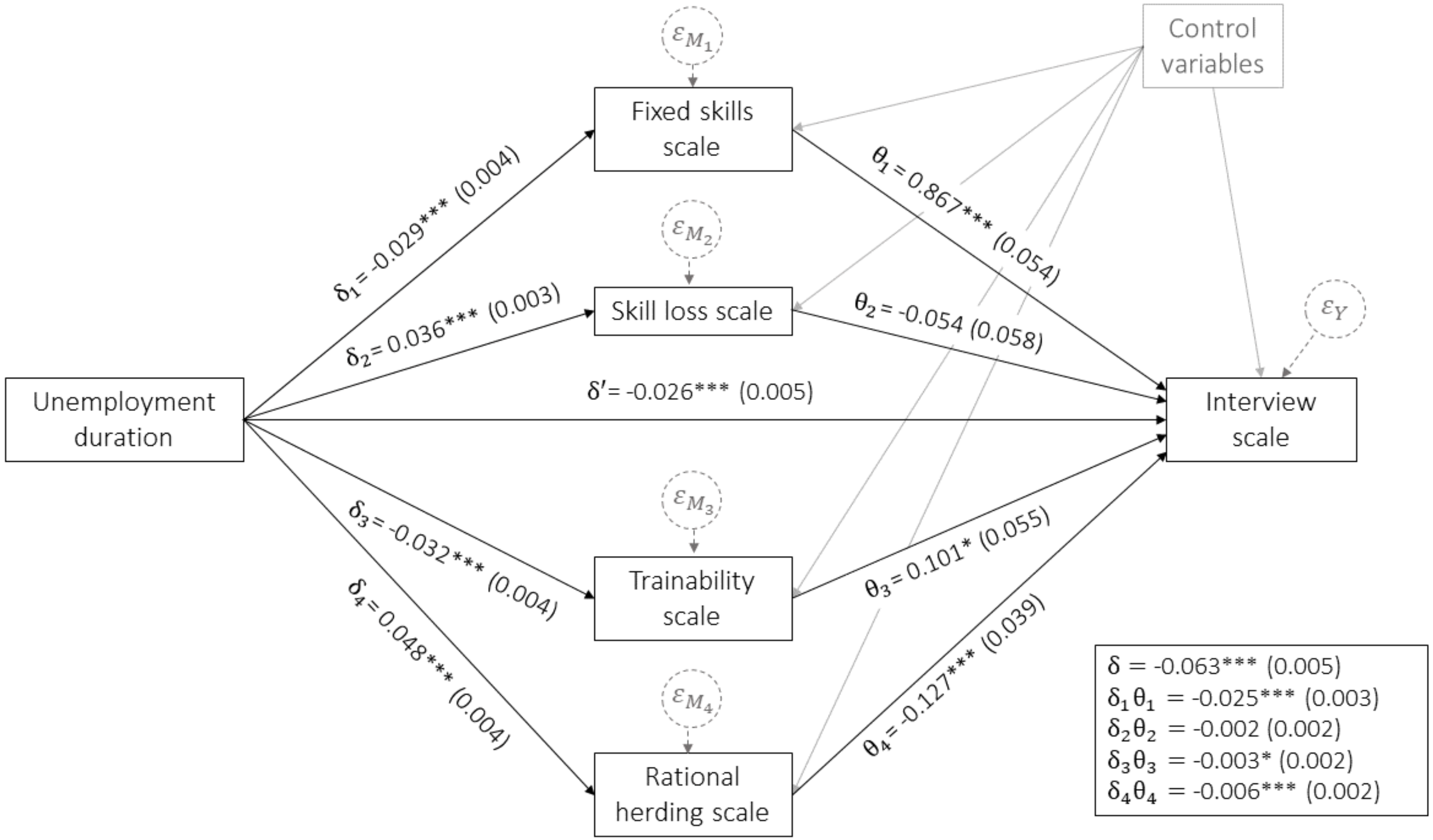
Note. Cramer's V is reported as all values are categorical. These statistics are based on the full sample of 1,095 observations.

Figure 2.3: Mediation Model with Hiring Scale as Outcome



Note. The presented statistics are coefficient estimates and standard errors in parentheses for the mediation model outlined in Section 2.4. δ stands for the total effect, δ' for the direct effect, and δ_i θ_i for the indirect effects of unemployment duration on the hiring scale, passing through mediator M_i. Standard errors are corrected for clustering of the observations at the participant level. The confidence intervals for the mediation effects are based on 10,000 bootstrap samples. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level.

Figure 2.4: Mediation Model with Interview Scale as Outcome after Exclusion of Vignettes with a Bachelor degree and/or Five years of Experience in Combination with an Unemployment Duration of Two Years or More



Note. The presented statistics are coefficient estimates and standard errors in parentheses for the mediation model outlined in Section 2.4. δ stands for the total effect, δ' for the direct effect, and δ_i θ_i for the indirect effects of unemployment duration on the interview scale, passing through mediator M_i. Standard errors are corrected for clustering of the observations at the participant level. The confidence intervals for the mediation effects are based on 10,000 bootstrap samples. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level.

Table 2.7: Summary Statistics: Outcome Variables, Candidate Characteristics and Participant Characteristics

Variable name	Definition	Mean	SD	N
A. Outcome variables				
Interview scale	Rating of the statement 'The probability that I will invite this candidate for a job interview is high' (on a 7-point Likert-scale)	4.183	1.609	1,095
Hiring scale	Rating of the statement 'The probability that I will hire this candidate for the position is high' (on a 7-point Likert-scale)	3.699	1.352	1,095
B. Candidate characteristics				
Female gender	The fictitious candidate is female	0.511	-	1,095
Bachelor degree	The fictitious candidate has a bachelor degree	0.503	-	1,095
Five years of experience	The fictitious candidate has five years of work experience	0.495	-	1,095
Volunteering	The fictitious candidate mentions volunteering activities	0.497	-	1,095
Unemployment duration	The number of months the fictitious candidate has been unemployed immediately prior to the application (from 1 to 36)	18.012	10.489	1,095
C. Participant characteristics				
Female gender	The participant indicates to be female	0.566	-	1,095
Age	The self-reported age of the participant	42.379	11.677	1,095
Highest degree obtained				
Secondary education or lower	The participant indicates that her/his highest obtained degree is secondary education or lower	0.100	-	1,095
Tertiary education: outside university	The participant indicates that her/his highest obtained degree is tertiary education obtained outside of University	0.457	-	1,095
Tertiary education: university	The participant indicates that her/his highest obtained degree is tertiary education obtained within University	0.443	-	1,095
Frequency of hiring: weekly	The participant indicates to be involved in hiring at least on a weekly basis	0.534	-	1,095
Experience as HR professional: $\geq$ 10 years	The participant indicates to have been involved in the hiring process for ten years or more	0.553	-	1,095
D. Mediation scales				
Fixed skills scale	Average rating of statements 1,2 and 3 as depicted in Table 2.2 (on a 7-point Likert-scale)	4.348	1.018	1,095
Skill loss scale	Average rating of statements 4, 5 and 6 as depicted in Table 2.2 (on a 7-point Likert-scale), where statement 4 is reverse scored	3.975	1.046	1,095
Trainability scale	Rating of statement 7 as depicted in Table 2.2 (on a 7-point Likert-scale)	4.227	1.144	1,095
Rational herding scale	Rating of statement 8 as depicted in Table 2.2 (on a 7-point Likert-scale)	4.514	1.313	1,095

Note. No standard deviations are reported for binary variables.

Table 2.8: Summary Statistics: Participant Characteristics by Unemployment Duration of the Fictitious Candidate

	A. Threshold of candidate's UD: 3 months			B. Threshold of candidate's UD: 12 months			C. Threshold of candidate's UD: 24 months		
	Mean		Difference:	Mean		Difference:	Mean		Difference:
	UD ≤ 3 months N = 99	UD > 3 months N = 996	(A.2) – (A.1)	UD ≤ 12 months N = 395	UD > 12 months N = 700	(B.2) – (B.1)	UD ≤ 24 months N = 739	UD > 24 months N = 356	(C.2) – (C.1)
	(A.1)	(A.2)	(A.3)	(B.1)	(B.2)	(B.3)	(C.1)	(C.2)	(C.3)
Female gender	0.616	0.561	-0.055 [1.202]	0.562	0.569	0.007 [0.221]	0.579	0.539	-0.040 [1.198]
Age	42.707	42.346	-0.361 [0.328]	41.903	42.647	0.743 [0.426]	42.042	43.079	1.037 [1.232]
Highest degree obtained									
Secondary education or lower	0.121	0.098	-0.023 [0.889]	0.119	0.090	-0.029* [1.679]	0.104	0.093	-0.011 [0.643]
Tertiary education: outside university	0.485	0.454	-0.031 [0.657]	0.438	0.467	0.029 [0.976]	0.456	0.458	0.002 [0.055]
Tertiary education: university	0.394	0.448	0.054 [1.147]	0.443	0.443	0.000 [0.006]	0.440	0.449	0.010 [0.291]
Frequency of hiring: weekly	0.566	0.531	-0.035 [0.723]	0.559	0.520	-0.039 [1.346]	0.549	0.503	-0.047 [1.401]
Experience as HR professional: ≥ 10 years	0.535	0.554	0.019 [0.399]	0.525	0.569	0.045 [1.499]	0.525	0.610	0.085** [2.576]

Note. UD stands for unemployment duration. T-tests are performed to test whether the differences presented are significantly different from 0. Standard errors are corrected for clustering of the observations at the participant level. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level. T-statistics are in brackets.

Table 2.9: Comparison Between Participant Characteristics and Characteristics of HR Professionals in ESS

Participant characteristics	Mean in experiment	Mean among HR professionals in ESS
Female gender	0.566	0.582
Age	42.379	40.900
Highest degree obtained		
Secondary education or lower	0.100	0.216
Tertiary education: outside university	0.457	0.252
Tertiary education: university	0.443	0.532

Note. We combined waves 1 to 8 of the European Social Survey, conducted between 2002 (wave 1) and 2016 (wave 8) and selected all respondents with ISCO-88 occupation codes 1232 (Personnel and industrial relations department managers), 2412 (Personnel and careers professionals) and 3423 (Employment agents and labour contractors) for waves 1 to 6 and ISCO-08 codes 1212 (Human resource managers), 2423 (Personnel and careers professionals), 3333 (Employment agents and contractors) and 4416 (Personnel clerks) for waves 7 and 8.

Table 2.10: Correlation Matrix Between Mediation Scales and Outcome Scales

	Interview scale	Hiring scale
Fixed skills scale	0.695***	0.710***
Fixed skills: motivation	0.688***	0.705***
Fixed skills: intellectual capacities	0.490***	0.489***
Fixed skills: social capacities	0.542***	0.564***
Skill loss scale	-0.515***	-0.536***
Skill loss: not up to date with technology	-0.577***	-0.610***
Skill loss: general skills	-0.358***	-0.358***
Skill loss: social skills	-0.327***	-0.344***
Trainability scale	0.530***	0.544***
Rational herding scale	-0.333***	-0.355***

Note. Cramer's V is reported as all values are categorical. These statistics are based on the full sample of 1,095 observations. Standard errors are corrected for clustering of the observations at the participant level. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level.

Table 2.11: Mediation Analysis with Hiring Scale as Outcome

Explanatory variables	**Outcome variables**				
	Fixed skills scale	Skill loss scale	Trainability scale	Rational herding scale	Interview scale
A. Candidate characteristics					
Female gender	0.112*** (0.041)	-0.084* (0.050)	0.050 (0.053)	-0.045 (0.062)	0.073 (0.046)
Bachelor's degree	0.354*** (0.050)	-0.232*** (0.056)	0.743*** (0.066)	-0.196*** (0.066)	-0.231*** (0.058)
Five years of experience	0.146*** (0.045)	-0.179*** (0.052)	0.047 (0.056)	-0.062 (0.067)	0.048 (0.047)
Volunteering	0.475*** (0.054)	-0.361*** (0.059)	0.158*** (0.056)	-0.165*** (0.064)	0.024 (0.051)
Unemployment duration	-0.029*** (0.003)	0.036*** (0.003)	-0.034*** (0.003)	0.045*** (0.004)	-0.021*** (0.003)
B. Participant characteristics					
Female gender	0.083 (0.108)	-0.161* (0.094)	-0.065 (0.108)	-0.372*** (0.120)	-0.076 (0.089)
Age	0.005 (0.005)	-0.009* (0.005)	0.010* (0.006)	-0.019*** (0.007)	-0.004 (0.005)
Highest degree obtained					
Secondary education or lower	0.181 (0.143)	-0.262* (0.159)	0.162 (0.143)	-0.211 (0.184)	0.117 (0.148)
Tertiary education: outside university	0.177* (0.105)	-0.293*** (0.091)	0.228** (0.105)	-0.016 (0.122)	0.096 (0.088)
Tertiary education: university (reference)					
Frequency of hiring: weekly	-0.055 (0.108)	0.195* (0.100)	-0.047 (0.109)	0.175 (0.117)	-0.080 (0.095)
Experience as HR professional: ≥ 10 years	-0.055 (0.122)	0.142 (0.101)	-0.085 (0.134)	0.104 (0.146)	-0.110 (0.105)
C. Mediation scales					
Fixed skills scale					0.725*** (0.054)
Skill loss scale					-0.085* (0.045)
Trainability scale					0.101** (0.047)
Rational herding scale					-0.124*** (0.032)
Observations			1,095		

Note. The presented statistics are coefficient estimates and standard errors in parentheses for the mediation model outlined in Section 2.4. Standard errors are corrected for clustering of the observations at the participant level. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level.

Table 2.12: Mediation Analysis with Interview Scale as Outcome and Eight Statements as Mediators

	Outcome variables								
Explanatory variables	Fixed skills: motivation	Fixed skills: intellectual capacities	Fixed skills: social capacities	Skill loss: not up to date with technologies	Skill loss: general skills	Skill loss: social skills	Trainability	Rational herding	Interview scale
A. CANDIDATE CHARACTERISTICS									
Female gender	0.170*** (0.059)	0.046 (0.050)	0.119** (0.053)	-0.048 (0.056)	-0.050 (0.062)	-0.154** (0.065)	0.050 (0.053)	-0.045 (0.062)	0.164*** (0.055)
Bachelor's degree	-0.078 (0.071)	0.917*** (0.067)	0.221*** (0.058)	-0.312*** (0.065)	-0.217*** (0.073)	-0.166** (0.070)	0.743*** (0.066)	-0.196*** (0.066)	-0.065 (0.068)
Five years of experience	0.097 (0.062)	0.183*** (0.053)	0.158*** (0.055)	-0.239*** (0.057)	-0.170** (0.074)	-0.128** (0.066)	0.047 (0.056)	-0.062 (0.067)	0.065 (0.055)
Volunteering	0.349*** (0.064)	0.150*** (0.057)	0.926*** (0.080)	-0.233*** (0.062)	-0.240*** (0.072)	-0.611*** (0.080)	0.158*** (0.056)	-0.165*** (0.064)	0.160** (0.063)
Unemployment duration	-0.038*** (0.004)	-0.024*** (0.004)	-0.024*** (0.003)	0.043*** (0.004)	0.039*** (0.004)	0.024*** (0.004)	-0.034*** (0.003)	0.045*** (0.004)	-0.022*** (0.004)
textscB. Participant characteristics									
Female gender	-0.050 (0.131)	0.152 (0.137)	0.147 (0.105)	-0.218** (0.110)	-0.042 (0.130)	-0.222* (0.122)	-0.065 (0.108)	-0.372*** (0.120)	0.038 (0.103)
Age	0.013** (0.006)	0.004 (0.007)	-0.003 (0.005)	-0.010* (0.005)	-0.009 (0.007)	-0.008 (0.006)	0.010* (0.006)	-0.019*** (0.007)	0.002 (0.006)
Highest degree obtained									
Secondary education or lower	0.105 (0.176)	0.218 (0.174)	0.221 (0.151)	-0.085 (0.173)	-0.271 (0.232)	-0.430* (0.220)	0.162 (0.143)	-0.211 (0.184)	0.281 (0.190)
Tertiary education: outside university	0.234* (0.126)	0.111 (0.128)	0.186* (0.101)	-0.208* (0.113)	-0.319** (0.128)	-0.351*** (0.116)	0.228** (0.105)	-0.016 (0.122)	0.125 (0.106)
Tertiary education: university (reference)									
Frequency of hiring: weekly	0.010 (0.131)	-0.093 (0.132)	-0.083 (0.105)	0.197* (0.118)	0.289** (0.139)	0.100 (0.131)	-0.047 (0.109)	0.175 (0.117)	-0.155 (0.117)
Experience as HR professional: ≥ 10 years	-0.269* (0.156)	0.056 (0.162)	0.050 (0.110)	0.094 (0.118)	0.220 (0.149)	0.112 (0.134)	-0.085 (0.134)	0.104 (0.146)	-0.328*** (0.125)
C. MEDIATION SCALES									
Fixed skills: motivation									0.498*** (0.048)
Fixed skills: intellectual capacities									0.156*** (0.046)
Fixed skills: social capacities									0.126*** (0.047)
Skill loss: not up to date with technologies									-0.171*** (0.051)
Skill loss: general skills									-0.020 (0.038)
Skill loss: social skills									0.044 (0.040)
Trainability scale									0.095* (0.050)
Rational herding scale									-0.094** (0.039)
Observations					1,095				

Note. The presented statistics are coefficient estimates and standard errors in parentheses for the mediation model outlined in Section 2.4. Standard errors are corrected for clustering of the observations at the participant level. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level.

Table 2.13: Mediation Analysis with Hiring Scale as Outcome and Eight Statements as Mediators

	Outcome variables								
Explanatory variables	Fixed skills: motivation	Fixed skills: intellectual capacities	Fixed skills: social capacities	Skill loss: not up to date with technologies	Skill loss: general skills	Skill loss: social skills	Trainability	Rational herding	Hiring scale
A. CANDIDATE CHARACTERISTICS									
Female gender	0.170*** (0.059)	0.046 (0.050)	0.119** (0.053)	-0.048 (0.056)	-0.050 (0.062)	-0.154** (0.065)	0.050 (0.053)	-0.045 (0.062)	0.064 (0.044)
Bachelor's degree	-0.078 (0.071)	0.917*** (0.067)	0.221*** (0.058)	-0.312*** (0.065)	-0.217*** (0.073)	-0.166** (0.070)	0.743*** (0.066)	-0.196*** (0.066)	-0.087 (0.054)
Five years of experience	0.097 (0.062)	0.183*** (0.053)	0.158*** (0.055)	-0.239*** (0.057)	-0.170** (0.074)	-0.128** (0.066)	0.047 (0.056)	-0.062 (0.067)	0.050 (0.045)
Volunteering	0.349*** (0.064)	0.150*** (0.057)	0.926*** (0.080)	-0.233*** (0.062)	-0.240*** (0.072)	-0.611*** (0.080)	0.158*** (0.056)	-0.165*** (0.064)	0.090* (0.052)
Unemployment duration	-0.038*** (0.004)	-0.024*** (0.004)	-0.024*** (0.003)	0.043*** (0.004)	0.039*** (0.004)	0.024*** (0.004)	-0.034*** (0.003)	0.045*** (0.004)	-0.017*** (0.003)
B. PARTICIPANT CHARACTERISTICS									
Female gender	-0.050 (0.131)	0.152 (0.137)	0.147 (0.105)	-0.218** (0.110)	-0.042 (0.130)	-0.222* (0.122)	-0.065 (0.108)	-0.372*** (0.120)	-0.049 (0.085)
Age	0.013** (0.006)	0.004 (0.007)	-0.003 (0.005)	-0.010* (0.005)	-0.009 (0.007)	-0.008 (0.006)	0.010* (0.006)	-0.019*** (0.007)	-0.006 (0.005)
Highest degree obtained									
Secondary education or lower	0.105 (0.176)	0.218 (0.174)	0.221 (0.151)	-0.085 (0.173)	-0.271 (0.232)	-0.430* (0.220)	0.162 (0.143)	-0.211 (0.184)	0.173 (0.144)
Tertiary education: outside university	0.234* (0.126)	0.111 (0.128)	0.186* (0.101)	-0.208* (0.113)	-0.319** (0.128)	-0.351*** (0.116)	0.228** (0.105)	-0.016 (0.122)	0.088 (0.083)
Tertiary education: university (reference)									
Frequency of hiring: weekly	0.010 (0.131)	-0.093 (0.132)	-0.083 (0.105)	0.197* (0.118)	0.289** (0.139)	0.100 (0.131)	-0.047 (0.109)	0.175 (0.117)	-0.093 (0.094)
Experience as HR professional: ≥ 10 years	-0.269* (0.156)	0.056 (0.162)	0.050 (0.110)	0.094 (0.118)	0.220 (0.149)	0.112 (0.134)	-0.085 (0.134)	0.104 (0.146)	-0.050 (0.093)
C. MEDIATION SCALES									
Fixed skills: motivation									0.420*** (0.043)
Fixed skills: intellectual capacities									0.1104** (0.043)
Fixed skills: social capacities									0.125*** (0.036)
Skill loss: not up to date with technologies									-0.186*** (0.043)
Skill loss: general skills									0.017 (0.031)
Skill loss: social skills									0.006 (0.032)
Trainability scale									0.089* (0.047)
Rational herding scale									-0.107*** (0.032)
Observations					1,095				

Note. The presented statistics are coefficient estimates and standard errors in parentheses for the mediation model outlined in Section 2.4. Standard errors are corrected for clustering of the observations at the participant level. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level.

2.7 Appendix B: Survey Experiment

2.7.1 Introduction to the Experiment (translated from Dutch)

Imagine, you have been working for the firm ‘Building & Co’, a firm that sells building materials to both individuals and professionals in the building industry, for several years.

You are the head of the human resources department at ‘Building & Co’ and currently looking for a new counter assistant.

This employee will be responsible for a friendly first reception and will give advice to customers in the store. In addition, the employee will answer questions and provide helpful information concerning orders over telephone.

You are consequently looking for someone who is customer-oriented, service-minded and a commercial talent. The employee has to be efficient and trustworthy concerning administrative work and has to have a good PC knowledge. There is no specific education nor experience required for this position.

The candidates have been pre-screened by an administrative assistant. Five candidates are formally considered. Some of their characteristics have been summarised by the administrative assistant and can be found on the next pages.

Read, candidate by candidate, the profile **thoroughly**. You have to make a decision concerning whether or not to invite these candidates for a job interview based on this information. You can judge the candidates in a random order and you can return to previous candidates. There is no restriction in the number of candidates you invite for a job interview. **Judge all candidates before continuing to the additional questionnaire. It is important that you fill in all the questions and do not leave anything open.**

2.7.2 Example of a Vignette and Subsequent Questions (translated from Dutch)

Profile candidate 1

Gender	Woman
Highest degree obtained	Bachelor
Previous work experience	2 years
Mentioned social activities	None
Unemployment duration	9 months

As head of the human resources department you now have to decide whether to invite the candidate for a job interview based on the above given information.

Please, take your decision now

I will invite the candidate for a job interview at ‘Building & Co’ for the function of counter assistant:

Completely disagree						Completely agree
1	2	3	4	5	6	7

There is a large probability that I will hire this candidate for the function of counter assistant at ‘Building & Co’:

Completely disagree						Completely agree
1	2	3	4	5	6	7

Please answer the following statements concerning the candidate.

	Completely disagree						**Completely agree**
1. I think this person will be sufficiently motivated to perform properly in this job (fixed skills: motivation).	1	2	3	4	5	6	7
2. I think this person possesses sufficient intellectual abilities to perform properly in this job (fixed skills: intellectual capacities).	1	2	3	4	5	6	7
3. I think this person possesses sufficient social abilities to perform properly in this job (fixed skills: social capacities).	1	2	3	4	5	6	7
4. I think this person is sufficiently aware of the evolutions in the work field to perform properly in this job (skill loss: not up to date with technologies).	1	2	3	4	5	6	7
5. I think this person has lately had a deterioration in her/his general skills (skill loss: general skill loss).	1	2	3	4	5	6	7
6. I think this person has lately had a deterioration in her/his social skills (skill loss: social skill loss).	1	2	3	4	5	6	7
7. I think this person will be easy to train (trainability).	1	2	3	4	5	6	7
8. I think this person has often been rejected by other employers (rational herding).	1	2	3	4	5	6	7

Chapter 3

Better together: Active and Passive Labour Market Policies in Developed and Emerging Economies[1]

3.1 Introduction

The rise in unemployment in developed economies during the 1980s led governments to increasingly use the coordination of passive and active labour market policies to offer social protection, while at the same time enhancing the transition from unemployment to employment (Estevão, 2003; ILO, 2014). This policy trend regained a central stage since the eruption of the global financial crisis, which reinforced the need for governments to channel spending towards interventions that could at the same time protect workers' income and raise their employability (Martin, 2014). While active labour market policies (ALMPs) have a long history in OECD countries, at the beginning the important potential linkages between the generosity of the unemployment benefits, the size and composition of ALMPs and the degree to which unemployment benefits' eligibility was determined by participation in ALMPs were largely ignored (Martin, 2014). Indeed, it was believed that in order to activate the unemployed, public spending needed to shift from passive labour market policies (PLMPs) to active policies. However, evidence showed that countries implementing this activation strategy did not automatically increase their labour

[1] In collaboration with Clemente Pignatti – Preliminary, please do not cite.

market performance, suggesting that active and passive policies should be seen as two essential components of a broader social protection system (ILO, 2012).

In the developing world, social protection systems were originally implemented as short-term interventions in response to crises and structural adjustments (Barrientos, 2010; McCord, 2012; Sabates-Wheeler and Devereux, 2011). However, rising poverty and stagnating productivity following the 1980s 'lost decade' in Latin America, the financial crises in Asia in the 1990s and rapid economic transformation in transition economies demonstrated the need for strong and stable labour market institutions concerned with poverty reduction and employment promotion (Barrientos, 2010; McCord, 2012). This led to two parallel developments. On the one hand, there has been a marked increase in conditional cash transfers and public works programs aiming to tackle basic income security (Barrientos and Hulme, 2009).[2] On the other hand, social protection is increasingly linked with other complementary measures (i.e. skills programs). These social protection systems serve not only the present basic income security role, but also aim to increase the opportunities to improve capabilities and break the poverty cycle (DFID, 2011). At the same time, ALMPs in emerging economies are rarely promoted as independent interventions (i.e. without a connection with income support programs) (Escudero et al., 2016).

The increased importance of active labour market and income support policies in both developed and emerging economies has generated an interest by researchers in evaluating their effectiveness (Greenberg et al., 2003; Heckman et al., 1999; Kluve, 2010; Liechti et al., 2017; Van Belle et al., 2018). While most evaluations look at the effects of one specific intervention, few studies have looked at the combination of active and income support policies. For developed economies, the microeconomic evidence is rather mixed. Overall, results suggest that – after an initial lock-in period – both the job finding rate and the quality of the employment increase due to the policy mix (Bolhaar et al., 2016; Crépon et al., 2012; Graversen and Van Ours, 2008; Markussen and Røed, 2016). Nevertheless, some studies find no or adverse effects on labour market outcomes (Cockx and Van Belle, 2016; McGuinness et al., 2013). For emerging economies, consensus seems even less likely. While positive results on employment and income were found for programs combining cash transfers and training in Nicaragua and Chile (Macours et al., 2012; Martínez et al., 2015) and a program combining public works and training in Bangladesh (Hashemi and

[2] In line with the horizontal dimension of the ILO recommendation on social protection (ILO, 2012), i.e. the implementation of a social protection floor.

Rosenberg, 2006) similar programs appeared to produce adverse effects in Argentina (Almeida and Galasso, 2010; Galasso et al., 2004). Moreover, a program combining an employment subsidy with training in Colombia led to a decrease in employment 18 months after participation (Medina et al., 2013).

While this evidence is compelling, very few studies have taken a macroeconomic approach to the assessment of active and passive interventions. This relates to general problems of econometric identification in cross-country analyses as well as the lack of adequate information on spending in passive and active policies beyond OECD economies. Filling this void is particularly important, as macroeconomic studies can address critical questions such as the presence of general equilibrium effects (e.g. disincentives or displacement effects) that are generally not taken into account in single impact evaluations. Similarly, cross-country analyses can generate conclusions whose validity goes beyond the single intervention at the centre of the impact evaluation. A number of studies have looked at the effectiveness of spending in ALMPs in developed economies, sometimes controlling for the level of unemployment benefits. Escudero (2018) examines the effect of spending in ALMPs in OECD countries and finds that they can improve employment outcomes (especially for low-skilled individual), provided that they are correctly implemented. Gal and Theising (2015) look both at unemployment insurance (UI) benefit replacement rates and spending in ALMPs and find that both lower UI replacement rates and a higher spending in ALMPs increase employment. A similar conclusion is reached by Estevão (2003); while Hujer et al. (2009) find no effect of ALMPs on the matching process in West-Germany. A second strand of literature looks at the macroeconomic effects of labour market institutions and reforms, of which both ALMPs and UI are important components. The studies by Blanchard and Wolfers (2000), Murtin and Serres (2014) and Murtin and Robin (2016) confirm that additional spending in ALMPs increases employment, while a higher UI replacement rate has the opposite effect. Some of these studies have also explored the possible interaction between active and passive interventions. For instance, Bassanini and Duval (2006, 2009) find that the adverse impact of the generosity of UI is lower in countries that spend more in ALMPs. Boone and Van Ours (2004) estimate the same interaction but with specific types of ALMPs and find that spending in training is more effective for countries with a more generous UI. Elmeskov et al. (1998) find an inverted U-shape relationship between the detrimental effects of UI and spending in ALMPs (i.e. with the negative effects of UI being the lowest in countries with an average amount of spending in ALMPs).

The current study complements the existing macroeconomic literature in two important ways. Firstly, we expand the analysis – compared to the OECD focus of previous contributions – by also including data from a number of emerging economies. In particular, we look at data from 58 countries – of which about one third is outside the OECD. Given differences in the functioning of labour markets as well as differences in the way in which labour market policies are implemented between developed and emerging countries, results from previous studies cannot be easily generalised to emerging countries. Secondly, while some of the previous studies looked at the effects of both spending in active and passive labour market policies, only a handful of studies have explicitly taken the interaction between both into account – as reviewed above. In this paper we explicitly take this interaction effect into account and test for its presence, which can be extremely important given the set of countries at the centre of the analysis. Indeed and as mentioned above, active and passive labour market policies in emerging economies do not often follow a clear-cut distinction and are often provided in combination.

The results reveal that ALMPs alone have only a marginal effect on labour market outcomes (i.e. they increase labour force participation and unemployment as expected, but the coefficients are small in magnitude and only marginally significant). At the same time, spending in PLMPs alone has the anticipated adverse effect on labour market performances as suggested by previous studies. In particular, it increases the unemployment rate and decreases the employment rate – while no significant result is reported on labour force participation. However, when taking into consideration both policies at the same time as well as their interaction a different picture emerges. In particular, each type of (active or passive) intervention is more effective if spending in the other type increases. As a result, even the negative effect of PLMPs disappears (and eventually becomes positive) for a given level of spending in ALMPs. When looking in more detail at the type of active or passive intervention it appears that additional spending in Public Employment Services (PES) and administration, direct job creation and employment incentives positively impact labour market outcomes, while spending in supported employment and rehabilitation has the opposite effect. Spending in training programs has the desired effect on unemployment rates, but at the cost of important discouragement effects. Turning to passive policies, both unemployment insurance and unemployment assistance increase unemployment and lower employment. Nevertheless, increased spending in unemployment insurance appears to incentivize individuals to remain in the labour force, while spending in unemployment assistance has the opposite effect. With regards to to the globally positive interaction effects between ALMPs and PLMPs, these seem to be driven by a positive interaction between spending in the different types of active interventions and spending in unemployment insurance.

The rest of the paper is structured as follows. Section 3.2 gives an overview of the data sources and some insight into the data gathering process; section 3.3 describes the empirical strategy adopted; section 3.4 reports the main results and the robustness tests for the overall analysis (i.e. on total spending in active and passive policies); section 3.5 looks at the detailed results by type of active and passive intervention; section 3.6 concludes with some take-away messages and policy recommendations.

3.2 Data

The aim of the paper is to capture the effects of labour market policies on main employment dynamics. Following previous studies, we look at three main indicators: the unemployment rate, the employment-to-population ratio and the labour force participation rate. All this information is gathered from the ILO World Economic and Social Outlook (WESO) database, which produces harmonized series for 189 countries from 1991.

The main regressors of interest in our model are the variables capturing the intensity of active and passive labour market policies. While different options are available, we look at spending in active and passive labour market policies as a percentage of GDP. As Estevão (2003) argues, using this measure may downward bias the results as aggregate output shocks change unemployment in the same direction as spending in labour market policies as a share of GDP. Therefore, the final estimates should be interpreted as a lower bound for the true effect of labour market policies on employment outcomes. Other studies alternatively use the spending per unemployed individual (Escudero, 2018; Gal and Theising, 2015; Murtin and Robin, 2016; Murtin and Serres, 2014), as this is more representative of the true policy stance (Escudero, 2018). Nevertheless, the level of spending per unemployed individual is also an imperfect measure of policy intensity; especially when the policy is targeted towards individuals who already have a job or those outside of the labour market (e.g. labour market services, unemployment assistance). The OECD defines expenditure in ALMPs as all expenditure aimed at improving the beneficiaries' prospect of finding gainful employment. This includes spending in (i) public employment services and administration; (ii) training; (iii) employment incentives; (iv) sheltered and supported employment; (v) direct job creation; and (vi) start-up incentives. Of course, the structure and content of ALMPs differs in emerging economies; where such a clear categorization does not often apply since interventions tend to combine different components (Escudero et al., 2016). Spending in PLMPs on the other hand consist of spending in (i) unemployment insurance; (ii) unemployment assistance; and (iii) programs for early retirement (OECD, 2007). Even in this case, such a clear-cut differentiation does not often apply in emerging economies; where income support programs tend to target only the most vulnerable groups in the population without a strict labour market conditionality.

Given the broad geographical coverage of the present study, data on public expenditure in active and passive labour market policies is necessarily collected from different sources. Firstly, data for OECD countries comes from the OECD Labour Market Programs database. This database contains information on spending in ALMPs and PLMPs for 34 countries from 1985 to 2015 with the exception of some (mainly Eastern-European) countries – for which the information is available for a more limited time period. Secondly, data for EU member states who are not part of the OECD was collected from the Eurostat Labour Market Policy database. This gives us information for an additional five countries from 1985 to 2015, with some exceptions. Data from Eurostat and the OECD are fully comparable (i.e. we can compare the information for EU countries in the OECD from the two databases) and therefore the use of these different data sources does not generate any inconsistency. Thirdly, we obtained access to detailed data on spending in active and passive labour market programs in 18 countries in Latin America and the Caribbean from roughly 2000 to 2015 from the World Bank ASPIRE Database. This data source overlaps with data from the OECD for both Chile and Mexico. Unfortunately, the information is not fully comparable and for both countries we use OECD data since it reports a longer time series. Fourthly and finally, data from fourteen Asian countries was collected from the Social Protection Index (SPI) database from the Asian Development Bank (ADB). Data is mainly available for countries in Central, East and South-East Asia, from 2008 till 2013. In contrast to the other data sources, the data on PLMPs is limited to spending in unemployment insurance. Even in this case, data from ADB overlaps for two countries (Japan and Korea) with the OECD database and we opt for the latter source for the longer and more complete information. The lack of comparability between the data collected from different sources could be due to a number of factors. First, the data reported in the World Bank ASPIRE Database only takes central government expenditures into account. Secondly, the data collected from different sources is based on different definitions of active and passive policies. For example, while the OECD data on PLMPs includes spending in early retirement benefits, this is not the case for the other two sources. Table 3.1 gives an overview of the available data and their respective sources.

For the purpose of the estimation strategy, we also extract information on GDP growth rates (from the ILO WESO database) and on the governments' primary balance (from the World Economic Outlook database of the IMF). Table 3.2 gives the descriptive statistics for the outcome variables, the main regressors and the control variables in Panel A, Panel B and Panel C respectively. The descriptive statistics are provided both for the entire sample and by development status. A first interesting observation is that the classic labour market outcomes appear to be consistently better in emerging economies than in developed economies. This can be explained by the fact that the lack of social protection in emerging economies makes open unemployment unaffordable (Madrid, 2006). Rather than becoming unemployed, these individuals take up various forms of, often vulnerable, employment (Schmitt, 2011). Moreover, from the observations in Panel B of Table 3.2 it is clear that emerging economies spend far lesser shares of their GDP in both active and passive labour market programs. However, we have to bear in mind that the expenditure data sourced from different databases is not fully comparable – and the empirical analysis will take care of this inconsistency. Lastly, Panel C gives the descriptive statistics for the control variables. In particular, the output gap seems to be more favourable for the emerging countries who have experienced higher growth rates in the recent decades as part of the process of economic convergence. The primary balance seems fairly comparable between country groups, with both running on average a primary deficit.

Figure 3.1 depicts a more detailed image of spending in ALMPs and PLMPs in the different countries considered in this study. The bottom of the distribution for both spending in ALMPs and PLMPs is made up by Asian countries and countries in Latin America and the Caribbean, while at the other side of the spectrum we find Western European countries. The majority of Eastern-European countries can be found somewhere halfway along the distribution. While most countries spend larger parts of their GDP on passive policies than they spend on active policies (the average value is almost twice as large), this is not always the case. Especially

Table 3.1: Data Availability on Spending in Active and Passive Labour Market Policies

Country	Time period covered	Source	Country	Time period covered	Source
Argentina	2000 – 2015	ASPIRE	**Kyrgyz Republic** [i]	2008 – 2013	ADB
Armenia	2008 – 2013	ADB	Latvia	2003 – 2015	OECD
Australia	1985 – 2015	OECD	Lithuania	2003 – 2015	OECD
Austria	1985 – 2015	OECD	Luxemburg [i]	1985 – 2015	OECD
Azerbaijan	2008 – 2013	ADB	**Malaysia** [i]	2008 – 2013	ADB
Belgium	1985 – 2015	OECD	Malta	2006 – 2014	Eurostat
Brazil	2005 – 2015	ASPIRE	**Mexico**	1998 – 2015	OECD
Bulgaria	2004 – 2015	Eurostat	**Mongolia**	2008 – 2013	ADB
Canada	1985 – 2015	OECD	Netherlands	1985 – 2015	OECD
Chile [i]	2004 – 2015	OECD	New Zealand	1985 – 2014	OECD
China	2008 – 2013	ADB	Norway	1985 – 2015	OECD
Colombia [ii]	2000 – 2015	ASPIRE	**Papua New Guinea** [i]	2008 – 2013	ADB
Croatia	2012 – 2015	Eurostat	**Peru** [ii]	2000 – 2015	ASPIRE
Cyprus	2006 – 2016	Eurostat	**Philippines** [i]	2008 – 2013	ADB
Czech Republic	1991 – 2015	OECD	Poland	1991 – 2015	OECD
Denmark	1986 – 2015	OECD	Portugal	1985 – 2015	OECD
Ecuador [i]	2000 – 2015	ASPIRE	**Romania**	2003 – 2015	Eurostat
Estonia	2003 – 2014	OECD	Slovak Republic	1991 – 2015	OECD
Finland	1985 – 2015	OECD	Slovenia [i]	2003 – 2015	OECD
France	1985 – 2015	OECD	Spain	1985 – 2015	OECD
Germany	1985 – 2015	OECD	Sweden	1985 – 2015	OECD
Greece [i]	1985 – 1997	OECD	Switzerland	1985 – 2015	OECD
Honduras [ii]	2003 – 2015	ASPIRE	**Tajikistan**	2008 – 2013	ADB
Hungary	1992 – 2015	OECD	**Thailand** [i]	2008 – 2013	ADB
Ireland	1985 – 2015	OECD	United Kingdom [i]	1985 – 2011	OECD
Israel	2004 – 2015	OECD	United States	1985 – 2015	OECD
Italy [i]	1990 – 2015	OECD	Uruguay [i]	2000 – 2015	ASPIRE
Japan [i]	1985 – 2015	OECD	**Uzbekistan**	2009 – 2013	ADB
Korea	2000 – 2015	OECD	**Viet Nam**	2008 – 2013	ADB

Note. Data availability based on the data collection as discussed in Section 3.2. Where different sources overlap preference was given to OECD data. Countries in bold are emerging economies according to the worldbank classification. [i] Data is not equally available for active and passive policies. [ii] No data is available for spending in passive labour market policies.

Table 3.2: Descriptive Statistics

	Entire sample		Emerging economies		Advanced economies	
	N	Mean	N	Mean	N	Mean
A. OUTCOME VARIABLES						
Unemployment rate	1,782	7.757	810	7.315	972	8.125
Employment to population ratio	1,782	56.914	810	59.415	972	54.830
Labour force participation rate	1,782	61.594	810	63.972	972	59.612
B. MAIN REGRESSORS						
Spending in ALMPs (% of GDP)	1,047	0.507	214	0.130	833	0.604
Spending in PLMPs (% of GDP)	1,049	0.930	155	0.142	894	1.067
C. CONTROL VARIABLES						
GDP growth gap	1,518	3.295	690	4.121	828	2.606
Primary balance	1,483	-0.132	514	-0.279	969	-0.054

Note. The variables are defined as described in Section 3.2. The number of observations and means are calculated for the entire period of 1985 - 2015, where the data is available.

Asian countries (i.e. Papua New Guinea, the Philippines, Mongolia, China, Viet Nam, Kyrgyz Republic, Korea, Malaysia and Tajikistan), Scandinavian countries (i.e. Sweden and Norway), Argentina, Colombia, Mexico and Lithuania all spend a larger share of their GDP in ALMPs than they spend in PLMPs.

3.3 Empirical Strategy

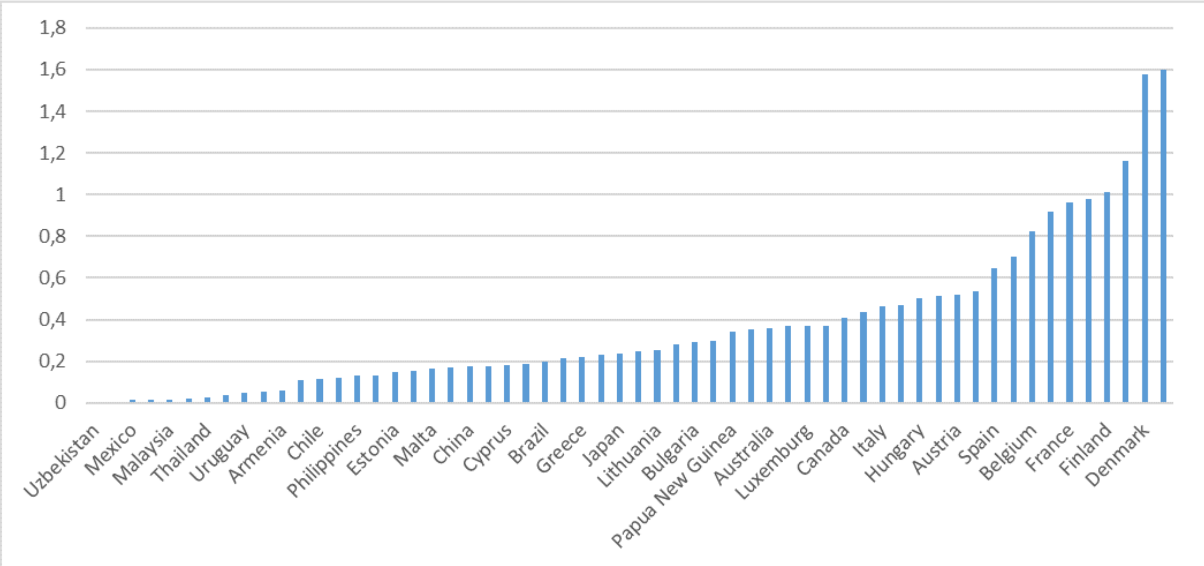
The purpose of the analysis is to investigate the causal effect of spending in active and passive labour market policies (and their interactions) on aggregate employment performances in a panel analysis. Following previous contributions (Escudero, 2018; Estevão, 2003), we estimate the following model:

$$Y_{i,t} = c + \beta_1 LMP_{i,t} + \beta_2 X_{i,t} + \beta_3 T_t + \beta_4 C_t + \varepsilon_{i,t} \tag{3.1}$$

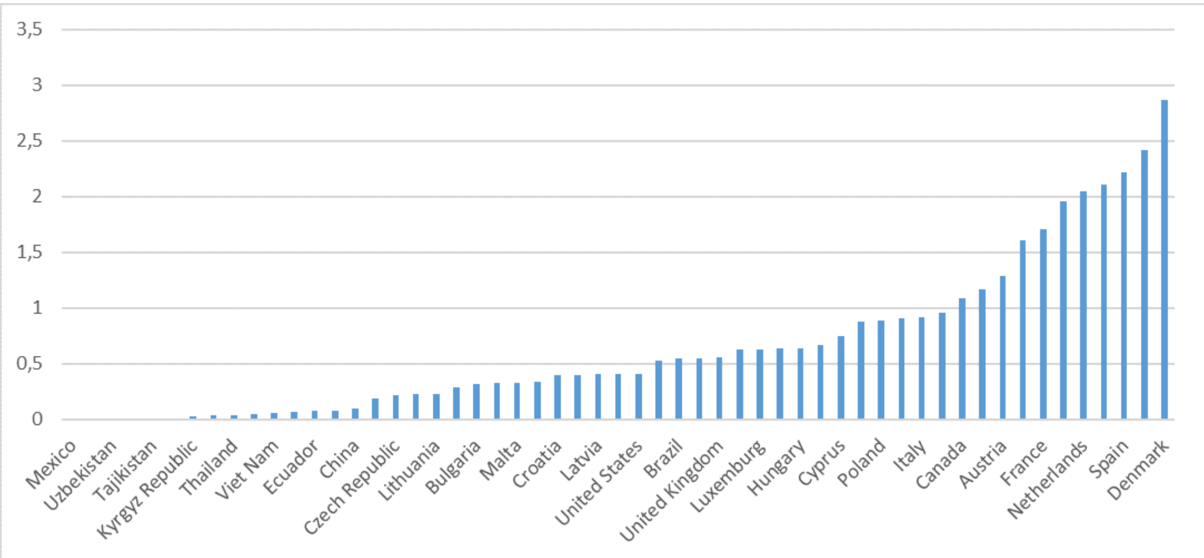
Where $Y_{i,t}$ represents the outcome of interest (unemployment, employment and labour force participation rates) in country i and year t; c is a constant; $LMP_{i,t}$ is the (vector of) spending

Figure 3.1: Average Spending in Active and Passive Policies by Country

(a) Average spending in ALMPs (% of GDP)



(b) Average spending in PLMPs (%of GDP)



Note. Data collected from different sources as discussed in Section 3.2. Where different sources overlap preference was given to OECD data.

in labour market policies; $X_{i,t}$ is the vector of control variables; T_t are year fixed effects; C_t are country fixed effects; and $\varepsilon_{i,t}$ is the error term.

Apart from the intensity of active and passive policies, labour market outcomes are likely determined by a number of factors. While we control for some aspects, we are fairly limited by data availability – especially for the emerging economies in our sample. In general, the literature has defined four groups of factors possibly influencing labour market outcomes. A first set of factors are demand conditions. In this sense, we include the difference between the GDP growth rate and its five-year average to capture cyclical fluctuations.[3] While Escudero (2018) controls for the terms of trade, this data is not available for our entire sample. In order to partially accommodate for this, we include year fixed effects to control for time variant shocks that affect all countries in the same way. A second set of factors deals with the structure of the labour market. For instance, Escudero (2018) controls for this by including the share of the population on a certain skill level, information which is again not available for our sample of countries. Thirdly, labour market outcomes are likely determined by institutional factors. Previous studies have controlled for the union density and the employment protection legislation (EPL) (Bassanini and Duval, 2006, 2009; Boone and Van Ours, 2004; Elmeskov et al., 1998; Escudero, 2018; Estevão, 2003; Gal and Theising, 2015), the prevalence of a minimum wage (Bassanini and Duval, 2006, 2009; Elmeskov et al., 1998; Gal and Theising, 2015), the tax wedge (Boone and Van Ours, 2004; Elmeskov et al., 1998; Estevão, 2003; Gal and Theising, 2015) and whether or not a country was part of the European Union (Escudero, 2018). Most of these variables are however less informative in our setting, as labour market institutions are often less binding in emerging economies due to lower compliance with labour law. In any case, data on institutional factors are often very scant and including these variables would require to substantially restrict the sample.[4] In order to partially account for these (generally constant) institutional characteristics, we follow previous contributions and include country fixed effects that capture any time-invariant difference at the country level. A fourth and final set of determinants are fiscal measures. In line

[3] Ideally, we would include the output gap as is done by Gal and Theising (2015) and Elmeskov et al. (1998) but this data is not publicly available for all countries in our sample.

[4] Data on the EPL is available for a large subset of our sample, therefore we control for this institution in a robustness check. Additionally, we also test whether our results are robust to the inclusion of a dummy variable for EU membership, as this is closely related to the degree of labour mobility. For both types of tests, the results of the regressors of interest do not significantly change in either magnitude or significance. For ease of exposition, these tetss are not reported in the paper but are available upon request.

with Gal and Theising (2015), we include the governments' primary balance to make sure that the measured effects of active and passive policies do not result from an overall fiscal stimulus. Estevão (2003) and Gal and Theising (2015) control in addition for the level of government employment, data which is nevertheless not available for our sample.

After having discussed the inclusion of covariates, the other main step concerns the choice of the empirical model. In particular, different econometric problems could affect simple OLS estimates in the present context. First, panel data are likely to be plagued by serial correlation in the idiosyncratic error term (Escudero, 2018; Lusinyan and Bonato, 2007). Although this does not necessarily affect identification, it would definitely influence inference (i.e. coefficients estimated would be consistent but not efficient). In order to test for this autocorrelation, we use the Arellano-Bond post-estimation technique (Roodman et al., 2009). Given that this test confirms the presence of first order autocorrelation (AR(1)), we estimate the model using feasible generalized least squares (FGLS) as proposed in Escudero (2018). FGLS is a viable alternative to OLS as it allows for the presence of AR(1) autocorrelation within panels and for cross-sectional correlation and heteroskedasticity across panels (Escudero, 2018). The second main econometric problem relates to endogeneity due to omitted variable bias or reversed causality, which would directly affect consistency of the estimated parameters (i.e. OLS results might be biased). While the choice of the covariates (as presented above) has aimed at alleviating the risk of omitted variable bias, it is impossible to rule out the risk that we are omitting variables that at the same time influence the outcome and the regressors of interest. This is particularly the case given the relatively large sample of countries included, for many of which we lack detailed information on labour market and institutional characteristics. With respect to the possible risk of reversed causality, it can be expected that when unemployment is high governments decide to increase spending in active labour market programs in order to increase enrolment.[5] This reverse causality might be even more important for passive labour market policies, as in this case the level of spending (almost) mechanically increases with the unemployment rate (at least in developed economies where this policy adjustment is in place). In order to control for these sources of bias, we follow previous contributions and estimate the panel models described above by instrumenting the expenditure in active and passive labour market policies (or their interactions) with their

[5] For instance, the opportunity cost of enrolling in a training program is lower during times of crises due to the reduced job opportunities.

one-year lagged values (Escudero, 2018; Estevão, 2003; Hujer et al., 2009).

The rationale behind this instrumental variable strategy is that spending in active and passive labour market policies might vary over time in ways that do not necessarily reflect labour market dynamics. In this way, previous spending is correlated with current spending (first stage relationship) without being otherwise correlated with the outcome of interest (exclusion restriction). In particular, we might think about two possible sources of exogenous variation in spending levels that could be exploited in the present context. First, there could be policy changes concerning the eligibility, duration or coverage of labour market policies from one period to the other. These policy changes would generate variations in spending levels across years that are not (necessarily) entirely associated with changes in the state of the labour market. For instance, during the years of the economic crisis between 2008 and 2015 there have been 801 legislative changes in the area of ALMPs reported in European countries (LABREF Database). Similarly, spending in active and passive policies might vary over years in ways that do not necessarily reflect the state of the labour market due to hysteresis effects. In particular, spending in PLMPs might decrease after the peak of the recession (despite the unemployment rate remaining high) as the bulk of the unemployment becomes long-term and the replacement rates gradually decrease. Of course, a main threat to the identification assumption is represented by concurrent time persistence in both labour market dynamics (e.g. current unemployment levels being determined by the previous unemployment levels) and spending in labour market policies. In that case, the lagged value of spending is a strong function of both current spending and current labour market outcomes. Although there is no conclusive solution to this problem, concerns could be alleviated by taking longer values of the lags as instruments (i.e. instrumenting current expenditure with their values x years before). We will check how the results vary by changing the length of the lag used as a robustness test, the results of which are presented in Section 3.4.2.

3.4 Aggregate Impact of Active and Passive Labour Market Policies

This Section will present the main empirical results of the macroeconomic analysis on the impact of overall spending in active and passive labour market policies (i.e. without differentiating by type of intervention) on labour market indicators. In particular, Section 3.4.1 will present the results of our preferred specification (both for the overall sample and splitting the countries according to their development status); while Section 3.4.2 reports a large set of robustness tests aimed at exploring the extent to which our results are sensitive to slight changes in the empirical analysis.

3.4.1 Main Results

As mentioned above, following previous contributions our preferred specification estimates equation 3.1 above with a 2SLS model using the previous level of spending (in active and passive policies) as an instrument for the current spending values. The results of this estimation are reported in Table 3.3 below for the unemployment rate, the employment rate and the labour force participation rate. For each outcome of interest, we present four different specifications; adding spending in active and passive policies first in isolation, then together and finally taking also into account their interaction.

At first glance, both types of spending seem to increase the unemployment rate – with the effect of spending in passive policies being particularly strong (Columns 1 and 2). However, once we introduce the two spending measures simultaneously (Column 3) the findings become more informative: additional spending in ALMPs has a negative (albeit small and non-significant) effect on the unemployment rate; while additional spending in PLMPs still increases unemployment. This result is expected from a theoretical point of view, as ALMPs are intended to activate the unemployed and help them attain gainful employment (OECD, 2007). PLMPs on the other hand make the option of being unemployed more attractive and increase the reservation wage, potentially increasing unemployment (Estevão, 2003; Gal and Theising, 2015). In Column 4, we also add the interaction between spending in active and passive labour market policies. The results reveal that in the complete specification spending in ALMPs and PLMPs both increase the unemployment rate, while the interaction between the policies significantly decreases it. In terms of magnitude, for any given value of spending in PLMPs (ALMPs) x, the effect of an additional unit (here, one percent of GDP) spent in ALMPs (PLMPs) is equal to 3.867-3.554x (8.321-3.554x). In other words, while both spending in ALMPs and PLMPs increase the unemployment rate if the spending in the other policy is equal to zero, the point estimate decreases gradually when the spending in the other type of intervention increases – and it turns negative at a certain point. Figure 3.2 shows this more clearly for each of the three labour market outcomes. Panel A plots the equations quantified here above. The figure confirms that spending in both ALMPs and PLMPs increases the unemployment rate when spending on the other policy package is zero. For the effect of spending in ALMPs (the dashed line) the effect turns quickly negative when spending in PLMPs increases (it reaches zero for a level of spending in PLMPs just above 1 per cent of GDP). A similar pattern is visible for the effect of spending in PLMPs (the full line) albeit that the initial positive effect is larger and more persistent (it reaches zero for a level of spending in ALMPs around 2.4 per cent of GDP). As a matter of comparison, the median value of spending in ALMPs (PLMPs) in 2014 was equal to 0.3 per cent (0.5 per cent) of GDP. This means that spending in ALMPs is beneficial for all those countries above the 75 percentile of the distribution of spending in PLMPs (e.g. France, Portugal, Spain and the Netherlands); while there is no country that currently reaches the threshold level of spending in ALMPs for making spending in PLMPs beneficial to unemployment reduction (Denmark had in 2014 a value of spending in ALMPs just above 2 per cent of GDP).

The results for the employment-to-population ratio and the labour force participation rate are in line with the findings for the unemployment rate. In particular, additional spending in ALMPs increases labour force participation rates (while it does not have a statistically significant effect on the employment rate), while additional spending in PLMPs has a negative effect on the employment rate (but no effect on the labour force participation rate). These results are in line with the economic theory, as ALMPs aim to activate individuals that would otherwise remain outside of the labour market (i.e. these individuals will enter both employment and unemployment) while PLMPs might generate disincentive effects for those that are already in the labour market (i.e. these forms of supports do not generally cover inactive individuals). As above, the inclusion of the interaction term reveals how ALMPs and PLMPs might have detrimental effect if implemented in isolation. In particular, both ALMPs and PLMPs have a negative effect on the employment rate; while PLMPs have also a negative (although smaller) effect on the labour force participation rates. However, the interaction between active and passive interventions is positive and statistically significant – meaning that both types of interventions can have a positive labour market effect provided that enough is spent in the other type of policy. For the labour force participation rate, the critical threshold of spending is lower than for the case of the unemployment rate. In particular, spending in ALMPs (PLMPs) increases labour force participation provided that around 0.6 per cent (1.3 per cent) of GDP is spent in PLMPs (ALMPs).

The results discussed above are generally in line with previous studies as reviewed in the introduction. The only notable difference is that we do not find evidence of the fact that ALMPs alone can improve labour market performances. Rather, this positive mechanism works only when ALMPs and PLMPs are jointly implemented (and an adequate level of spending is devoted to both types of interventions). The main motivation behind this difference in the results could lie in the differences in the countries covered in the present analysis (i.e. for the first time we include also emerging economies). In order to better investigate this hypothesis, we conduct the analysis by adding an interaction term between the main regressors of interest (i.e. spending in ALMPs, PLMPs and their interactions) and a dummy variable for development status (i.e. taking the value of one for emerging countries). The equation presented above takes therefore the following form:

$$Y_{i,t} = c + \gamma_1 LMP_{i,t} + \gamma_2 DEV_{i,t} + \gamma_3 DEV_{i,t} * LMP_{i,t} + \gamma_4 X_{i,t} + \gamma_5 T_t + \gamma_6 C_t + \varepsilon_{i,t} \tag{3.2}$$

Where $DEV_{i,t}$ takes the value of one for emerging economies and all the other covariates have the same interpretation as before. Note that in this case we have two endogenous regressors ($LMP_{i,t}$ and $DEV_{i,t} * LMP_{i,t}$) that are both instrumented with their lagged values. The results of this exercise is presented in Table 3.4 below, where the parameters of interest are in this case represented by the interaction terms between the development status and the variables for the labour market policies. The main finding from Table 3.4 is that PLMPs seem to have a stronger disincentive effect on labour market performances (i.e. increasing unemployment and decreasing employment) in emerging compared to advanced economies. However, the coefficient is not very stable across specifications and it also changes sign–suggesting that the parameters might be imprecisely estimated, potentially due to the limited sample size. At the same time, we also find some evidence of the fact that ALMPs might be more effective in increasing labour force participation rates in emerging economies (i.e. the coefficients of the interaction term between emerging economies and spending in ALMPs is positive and significant in columns 9 and 12). For both ALMPs and PLMPS, these results might be driven by the fact that emerging economies

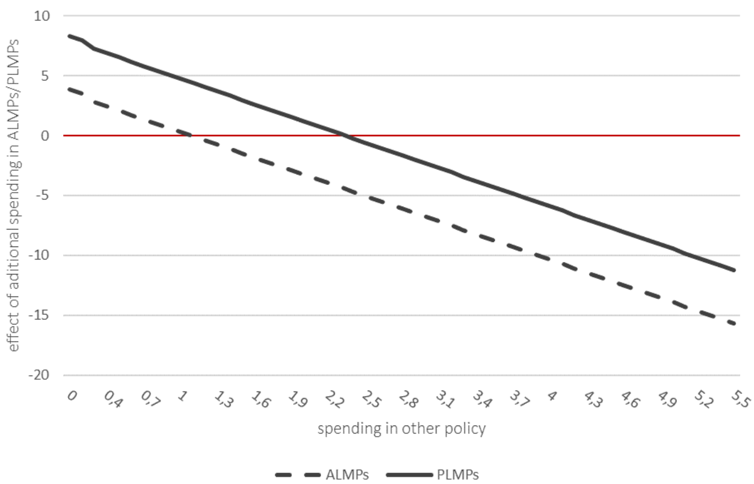
Table 3.3: Results for the Unemployment Rate, Employment-to-population Ratio and Labour Force Participation Rate Estimated with 2SLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Unemp. rate	Unemp. rate	Unemp. rate	Unemp. rate	Empl. rate	Empl. rate	Empl. rate	Empl. rate	Labour force part. rate	Labour force part. rate	Labour force part. rate	Labour force part. rate
	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
	N = 806	N = 805	N = 749	N = 749	N = 806	N = 805	N = 749	N = 749	N = 806	N = 805	N = 749	N = 749
Spending in ALMPs	1.430**		-0.848	3.867***	-0.339		0.822	-2.843***	0.744*		0.543	-0.473
(% of GDP)	(0.613)		(0.750)	(0.848)	(0.575)		(0.746)	(0.857)	(0.435)		(0.487)	(0.615)
Spending in PLMPs		4.066***	4.213***	8.321***		-2.500***	-2.616***	-5.809***		-0.0705	-0.117	-1.002**
(% of GDP)		(0.392)	(0.462)	(0.624)		(0.355)	(0.426)	(0.548)		(0.218)	(0.252)	(0.416)
Interaction				-3.554***				2.762***				0.766***
				(0.453)				(0.391)				(0.268)
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Country fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

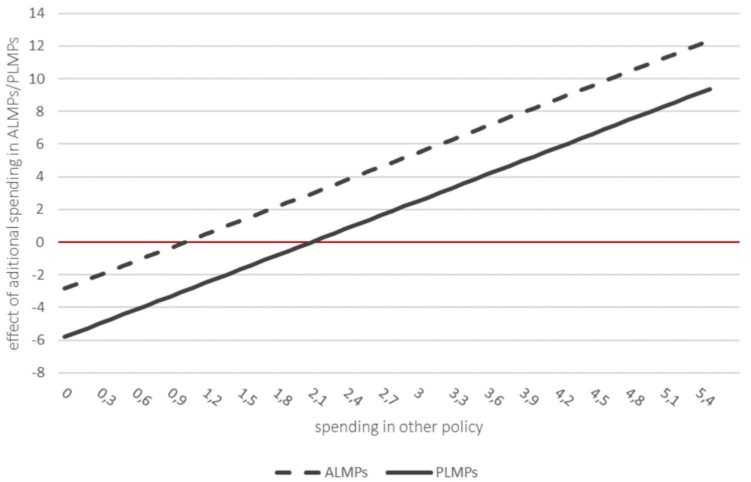
Note. The presented statistics are coefficient estimates and robust standard errors in parentheses for the panel model outlined in Section 3.3. The spending in ALMPs and spending in PLMPs variables are instrumented by its lagged values. ***(**)((*)) indicates significance at the 1%(5%)((10%)) significance level.

Figure 3.2: The Effect of one Additional Unit Spending in ALMPs (PLMPs) Given Spending in PLMPs (ALMPs)

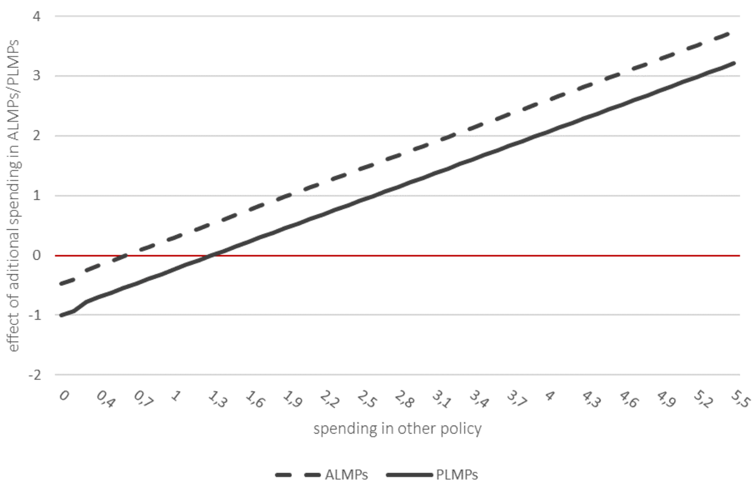
(a) Unemployment rate



(b) Employment rate



(c) Labour force participation rate



Note. Interaction effects as described in Section 3.4. The presented statistics are coefficient estimates for the panel mode outlined in Section 3.3.

spend a relatively low amount on these types of interventions–an additional percentage of GDP spent can therefore quickly generate large (positive or negative) labour market effects. Finally, the results obtained for the differential effect of the interaction between active and passive policies across development groups are implausibly large. In passing by, it is worth noting how the results of the overall effects of active and passive policies and their interactions (i.e. the variables for the labour market policies not interacted with the dummy of development status) have remained mostly unchanged (i.e. in terms of both magnitude and significance) compared to those reported in Table 3.3 above.

3.4.2 Robustness Tests

This section presents the different tests conducted in order to verify the soundness of the methodological approach adopted in the paper. First, we have run the regressions departing from the 2SLS model introduced before. In particular, we re-estimated equation 3.1 above by means of OLS, Arellano-Bond and FGLS. The results are available in the Appendix (Table 3.11 in Appendix 3.7) and–given that they are mostly in line with those discussed above–we proceed in the rest of the analysis with the 2SLS model.

The main issue with the instrumental variable approach as presented above concerns the plausibility of the exclusion restriction (i.e. lagged values of spending do not directly affect current labour market performances). Given possible time persistence in both the regressors of interest and the dependent variable, this condition could be violated. Although there is no conclusive solution to this problem, we might get a measure of the extent to which this constitutes a threat to the current estimation strategy by using previous lags of spending levels (i.e. going back in time). Although this will reduce the available sample size for the estimation (and the strength of the instrument), it should probably weaken concerns over the plausibility of the exclusion restriction.[6] The results of the exercise are available in Table 3.5, where for ease of exposition we presented only the full specification for the unemployment rate (i.e. corresponding to column 4 in Table 3.3 above). Moving from the left to the right of the Table, each column uses a different lagged value of the instrument (i.e. from the first to the tenth lag). Of course, the sample size also varies and therefore the results are not directly comparable. However, the regressors of

[6] Additionally, the sample size becomes increasingly biased towards advanced economies for which we have longer time series.

Table 3.4: Results for the Heterogeneity Analysis by Development Status Estimated Using 2SLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Unemp. rate	Unemp. rate	Unemp. rate	Unemp. rate	Empl. rate	Empl. rate	Empl. rate	Empl. rate	Labour force part. rate	Labour force part. rate	Labour force part. rate	Labour force part. rate
	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
	N = 806	N = 805	N = 749	N = 749	N = 806	N = 805	N = 749	N = 749	N = 806	N = 805	N = 749	N = 749
Spending in ALMPs	1.372**		-1.060	4.013***	-0.600		0.904	-3.151***	0.443		0.501	-0.709
(%of GDP)	(0.621)		(0.724)	(0.804)	(0.625)		(0.765)	(0.902)	(0.466)		(0.504)	(0.647)
Spending in PLMPs		4.058***	4.206***	8.349***		-2.494***	-2.608***	-5.903***		-0.0684	-0.112	-1.081**
(% of GDP)		(0.392)	(0.464)	(0.624)		(0.355)	(0.427)	(0.563)		(0.218)	(0.252)	(0.425)
Interaction				-3.577***				2.856***				0.850***
				(0.447)				(0.402)				(0.276)
Dev*ALMP	0.693		4.425	2.238	3.137		-2.006	4.354	3.613**		0.510	6.219***
	(3.233)		(4.190)	(5.289)	(2.318)		(2.424)	(3.176)	(1.717)		(1.856)	(1.769)
Dev*PLMP		6.232*	5.515	13.04**		-4.901*	-4.772	0.147		-1.793	-2.068	8.433**
		(3.428)	(3.562)	(6.650)		(2.631)	(2.988)	(4.582)		(1.884)	(1.977)	(4.111)
Dev*Interaction				-43.74*				-16.75				-47.73***
				(23.47)				(14.17)				(14.33)
Dev	2.483	1.247	3.683*	4.911**	-6.797***	-3.835***	-6.717***	-8.846***	-5.632***	-3.346***	-4.757***	-6.341***
	(1.760)	(1.209)	(2.035)	(2.417)	(1.060)	(0.846)	(1.162)	(1.346)	(0.700)	(0.439)	(0.751)	(0.731)
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Country fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Note. The presented statistics are coefficient estimates and robust standard errors in parentheses for the panel model outlined in Section 3.4.1. The spending in ALMPs and spending in PLMPs variables are instrumented by its lagged values. ***(**)((*)) indicates significance at the 1%(5%)((10%)) significance level.

interest remain remarkably constant across the different specifications both in terms of magnitude and statistical significance (with the possible exceptions of models from 5 to 7, which are less precisely estimated). This suggests that using longer lags–for which the exclusion restriction is more likely to hold–should not necessarily change the validity of the results discussed above. Since previous lags are also available for identification purposes, the model presented above can be over-identified (i.e. using more instruments than endogenous regressors). In that case, a GMM specification could be preferred to 2SLS. In order to check the robustness to changes in the model, we therefore run equation 3.1 above with a GMM model that uses as instruments all the lags up to four years.[7] The results are available in Table 3.6 below and they largely confirm those obtained before in the just-identified case.

After having discussed the validity of the instrument, another set of robustness tests concerns introducing slight changes to the preferred specification introduced above. The first point of concern is the unbalanced nature of our data, especially the fact that we have longer time series for developed than emerging economies. In order to make sure that what we are estimating is not an effect that is only present for OECD countries in the 1990s we restrict our sample to the years after 1999, for which we have a more comparable number of observation. These results (estimated with 2SLS, using one-year lag as an instrument) are reported in the first three Columns of Table 3.7 below. They confirm the findings obtained for the overall sample with the exception of regressions using labour force participation as the outcome of interest, when the coefficients of the regressors of interest become smaller in magnitude and statistically non-significant. As an additional test, we re-estimate the baseline equation using the same methodology as before (2SLS with lagged values as instruments) but without additional covariates (i.e. apart from the year and the country fixed effects). Indeed, it could be that the inclusion of those covariates (i.e. difference between the GDP growth rate and its five-year average to capture cyclical fluctuations and government primary balance) is spuriously driving the results for active and passive labour market policies. The issue is particularly important for the primary balance, since controlling for that means that we are considering variations in spending in labour market policies that are somehow compensated by reductions in spending in other items of the public budget. In that case, one may wonder whether the effect that we identify is truly associated with the change in spending in ALMPs or PLMPs–rather than with the reduction in some other type of spending.

[7] A different choice of the length of the lag would not substantially change the results.

Table 3.5: Results for the Robustness Test Using Different Lags of Spending as Instruments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Unemp. rate	Unemp. rate	Unemp. rate	Unemp. rate	Unemp. rate	Unemp. rate	Unemp. rate	Unemp. rate	Unemp. rate	Unemp. rate
	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
	N = 749	N = 719	N = 689	N = 663	N = 634	N = 607	N = 584	N = 561	N = 523	N = 488
Spending in ALMPs	3.867***	4.181***	4.857***	4.500**	9.540	-1.175	5.252**	7.412***	10.74***	12.00**
(% of GDP)	(0.674)	(0.943)	(1.282)	(1.785)	(8.837)	(6.329)	(2.634)	(2.745)	(3.730)	(5.374)
Spending in PLMPs	8.321***	10.02***	11.67***	12.78***	22.48	-5.889	2.026	4.843**	8.260***	11.79***
(% of GDP)	(0.469)	(0.667)	(1.062)	(2.000)	(16.61)	(13.31)	(2.614)	(2.047)	(2.889)	(4.442)
Interaction	-3.554***	-4.479***	-5.831***	-7.052***	-16.74	10.00	1.769	-0.596	-2.661	-4.462*
	(0.330)	(0.486)	(0.836)	(1.680)	(15.31)	(13.31)	(2.429)	(1.567)	(1.790)	(2.609)
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Country fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Note. The presented statistics are coefficient estimates and robust standard errors in parentheses for the robustness analysis outlined in Section 3.4.2. The spending on ALMPs and spending on PLMPs variables are instrumented by its lagged values. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level.

Table 3.6: Results for the Over-identified Model Estimated Using GMM

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Unemp. rate	Unemp. rate	Unemp. rate	Unemp. rate	Empl. rate	Empl. rate	Empl. rate	Empl. rate	Labour force part. rate	Labour force part. rate	Labour force part. rate	Labour force part. rate
	GMM	GMM	GMM	GMM	GMM	GMM	GMM	GMM	GMM	GMM	GMM	GMM
	N = 696	N = 717	N = 653	N = 653	N = 696	N = 717	N = 653	N = 653	N = 696	N = 717	N = 653	N = 653
Spending in ALMPs	-0.0804		-1.416	3.874***	-0.0931		0.643	-2.955***	0.114		0.487	0.130
(% of GDP)	(1.093)		(0.991)	(1.154)	(0.898)		(0.878)	(1.013)	(0.668)		(0.615)	(0.801)
Spending in PLMPs		3.855***	4.124***	8.632***		-2.326***	-2.367***	-6.062***		-0.00958	-0.0106	-1.089***
(% of GDP)		(0.393)	(0.448)	(0.682)		(0.354)	(0.377)	(0.561)		(0.220)	(0.255)	(0.419)
Interaction				-3.828***				2.991***				0.742**
				(0.525)				(0.411)				(0.293)
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Country fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Note. The presented statistics are coefficient estimates and robust standard errors in parentheses for the robustness analysis outlined in Section 3.4.2. The spending on ALMPs and spending on PLMPs variables are instrumented by its lagged values. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level.

The last three Columns of Table 3.7 below presents the result of this new exercise, which once again provides reassuring evidence that the results discussed above are not sensitive to slight modifications in the baseline specification.

3.5 Effects by Type of Intervention

As stated previously, we have detailed data on the type of active and passive interventions for two regions (OECD countries and Latin America and the Caribbean), which we will exploit in this section. From a policy perspective, this is particularly important as it helps us to understand the relative effectiveness of the different (active and passive) interventions. Moreover, from a research perspective, the exercise is also interesting because few studies have explore the macroeconomic effectiveness of these policies by types of interventions (and their eventual interaction type by type). In order to do so, we re-estimate our preferred specification (as described above) and replace the aggregate spending variables by spending (as a % of GDP) in the specific labor market policy components. We follow the OECD classification and split spending in ALMPs into the following six components: (i) PES and administration; (ii) training; (iii) employment incentives; (iv) sheltered and supported employment and rehabilitation; (v) direct job creation; and (vi) start-up incentives.[8] For the passive policies, we make the distinction between unemployment insurance and unemployment assistance (i.e. contributory and non-contributory unemployment benefits).[9] Table 3.8 gives an overview of the spending in these labour market programs for the overall sample and for both regions.

A few things stand out from Table 3.8. First, it is clear that the vast majority of our data points again come from OECD countries. Nevertheless, we can see some clear distinctions in the types of labour market programs prevalent in both regions. Looking at active policies, we can observe that in the OECD, the biggest chunk of spending goes to training programs and PES.

[8] PES and administration refers to programs including (i) counselling and case management of jobseekers; (ii) open information services; and (iii) referral to work, training or other assistance, as well as the budget of the institutions that manage unemployment benefits. Training includes both institutional and workplace training, as well as support for apprenticeships. Employment incentives refers both to recruitment- and employment maintenance incentives, usually in the form of subsidies or reduced social security contributions. Sheltered and supported employment and rehabilitation are programs targeted at people with a permanently reduced capacity to work. Direct job creation creates additional jobs, usually for community benefits. The final category, start-up incentives, are programs promoting entrepreneurship. For the complete definitions of these active interventions, see OECD.org.

[9] We do not include early retirement benefits here, as this information is only available for OECD countries.

Table 3.7: Results for the Robustness Test Using Only Observations After 2000 and Without Covariates

	(a) Only since 2000			(b) No covariates		
	(1)	(2)	(3)	(4)	(5)	(6)
	Unempl. rate	Empl. rate	Labour force part. rate	Unempl. rate	Empl. rate	Labour force part. rate
	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
	N = 602	N = 602	N = 602	N = 820	N = 820	N = 820
Spending in ALMPs (% of GDP)	4.657***	-3.574***	-0.741	3.244***	-2.211**	-0.237
	(1.547)	(1.264)	(0.939)	(0.782)	(0.862)	(0.640)
Spending in PLMPs (% of GDP)	12.58***	-7.517***	-0.0558	7.423***	-5.054***	-0.748*
	(0.980)	(0.675)	(0.446)	(0.531)	(0.529)	(0.397)
Interaction	-7.663***	4.730***	0.168	-3.038***	2.079***	0.357
	(0.983)	(0.722)	(0.469)	(0.407)	(0.377)	(0.269)
Constant	8.611***	56.50***	61.82***	13.78***	52.40***	60.87***
	(1.431)	(0.863)	(0.454)	(1.452)	(0.905)	(0.344)
Controls	YES	YES	YES	NO	NO	NO
Country fixed effects	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES

Note. The presented statistics are coefficient estimates and standard errors in parentheses for the robustness analysis outlined in Section 3.5. The spending on ALMPs and spending on PLMPs variables are instrumented by its lagged values. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level.

Table 3.8: Descriptive Statistics by Program Component

	Entire sample		**OECD**		**LAC**	
	N	Mean	N	Mean	N	Mean
A. SPENDING IN LABOR MARKET PROGRAMS						
PES and administration	765	0.122	712	0.131	53	0.004
Training	815	0.153	737	0.162	78	0.068
Employment incentives	768	0.095	739	0.095	29	0.100
Supported employment and rehabilitation	751	0.076	711	0.079	40	0.020
Direct job creation	774	0.079	736	0.076	38	0.137
Start-up incentives	783	0.015	747	0.015	36	0.011
Unemployment insurance	573	0.506	514	0.553	59	0.268
Unemployment assistance	507	0.203	496	0.208	11	0.001

Note. The variables are defined as described in Section 3.5. OECD stands for Organisation of Economic Co-operation and Development and LAC for Latin America and the Caribbean. The number of observations and means are calculated for the entire period of 1985 – 2015, where the data is available.

Indeed, the spending in training programs is about ten times what is spent in start-up incentives. In Latin America and the Caribbean (LAC) this picture is quite different. Here, the biggest programs (in terms of spending) are direct job creation programs and employment incentives. Moreover, and in striking contrast with the OECD countries, almost nothing is spent in PES and administration. Concerning passive programs, both regions spend the largest amounts in unemployment insurance. For LAC countries, the percentage of GDP spent in unemployment assistance is even virtually zero.

Table 3.9 gives the results of the 2SLS regression with the individual components rather than the aggregate labour market spending variables. Spending in PES and administration appears to have an overall positive impact on labour market outcomes (i.e. additional spending lowers the unemployment rate, and increases the employment and labour force participation rates). This is in line with economic theory, stating that employment services increase the quality of labour market matching, reducing frictional unemployment and the duration of the unemployment spell. Training programs on the other hand lower the unemployment rate but also the labour force participation rate, pointing towards an important discouragement effect making people shift from unemployment to inactivity. Spending in employment incentives lowers the unemployment rate and increases the employment rate, while no effect is found on labour force participation. These findings are in accordance to previous evidence from micro-econometric evaluations (Card et al., 2010, 2017; Kluve, 2010) and macroeconomic evidence for OECD countries (Escudero, 2018), who also find large positive effects for employment services and employment incentives, and smaller positive effects for training programs. Spending in supported employment and rehabilitation has an overall adverse effect on labour market outcomes, as we see no effect on unemployment and significant negative effects on employment and labour force participation. This is rather suprising, as previous evaluations have found mainly positive employment effects (Burns et al., 2007; Crowther et al., 2001; Hoffmann et al., 2012). To the extent that participation in these programs is mandatory, our findings could be due to important discouragement effects. Spending in direct job creation has an overall favourable labour market impact. This is expected, as direct job creation increases the total number of jobs, and is in line with findings for OECD countries (Escudero, 2018). Nevertheless, previous micro-econometric studies have indicated that this type of interventions have low or adverse effects on the employability of beneficiaries (Card et al., 2010, 2017; Kluve, 2010). Lastly, spending in start-up incentives does not appear to have any labour market consequences for our sample of countries. This is in contrast to the evidence for OECD countries presented by Escudero (2018).

For the passive measures, the results are again in line with general economic theory. Spending in unemployment insurance increases unemployment and lowers employment, while at the same time labour force participation increases, indicating that higher contributory unemployment benefits incentivize individuals to remain in the labour force. For unemployment assistance, we find that additional spending increases unemployment and lowers employment and labour force participation significantly.

As one of the most important results of our main analysis was that the interaction between active and passive policies appears to be of vital importance, we repeat the exercise including all possible interactions between active and passive policies here (resulting in twelve interactions). The results of this exercise are reported in Table 3.10. In general, the interaction between active policies and unemployment insurance appears to have favourable labour market outcomes, while the interaction with unemployment assistance has the opposite effect. This is the case for spending in PES and administration, employment incentives, direct job creation and start-up

Table 3.9: Results for the Analysis by Component Estimated Using 2SLS

	(1)	(2)	(3)
	Unempl. rate	Empl. rate	Labour force part. rate
	2SLS	2SLS	2SLS
	N = 436	N = 436	N = 436
PES and administration	-5.066	8.953**	6.762**
	(4.526)	(3.839)	(2.662)
Training	-7.644***	1.535	-3.403***
	(1.680)	(1.329)	(1.057)
Employment incentives	-7.199**	5.739**	1.667
	(3.513)	(2.647)	(2.088)
Supported empl. and rehabilitation	3.799	-13.064***	-11.647***
	(3.975)	(3.657)	(2.346)
Direct job creation	-1.495	4.579***	4.095***
	(1.587)	(1.750)	(1.443)
Start-up incentives	-5.492	9.783	7.379
	(10.992)	(10.579)	(6.798)
Unemployment insurance	7.941***	-3.098***	1.804***
	(1.172)	(0.983)	(0.600)
Unemployment assistance	7.757***	-5.649***	-1.059**
	(0.546)	(0.526)	(0.426)
Controls	YES	YES	YES
Country fixed effects	YES	YES	YES
Year fired effects	YES	YES	YES

Note. The presented statistics are coefficient estimates and standard errors in parentheses for the panel model outlined in Section 3.3. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level.

incentives. For supported employment and rehabilitation on the other hand we find the exact opposite results. Indeed, it appears that the interaction between this group of active policies and unemployment assistance counters the overall negative effect of these policies on labour market outcomes.

3.6 Conclusion

In this paper, we investigate the causal effect of spending in active and passive labour market policies on key labour market outcomes in both developed and emerging economies. We do this by means of a panel model using a rich database containing expenditure information on 58 countries, of which about one third is outside of the OECD. We follow the existing literature (Escudero, 2018; Estevão, 2003; Gal and Theising, 2015; Murtin and Robin, 2016; Murtin and Serres, 2014) and control for the likely presence of reverse causality between the labour market outcomes and the spending measures by estimating an instrumental variable model where the spending variables are instrumented by their lagged values.

We extend the existing literature in two important ways. Firstly, to the best of our knowledge, this is the first contributions that includes observations from non-OECD countries. Indeed, very little evidence exists to show that policies which work well in one labour market context can be easily translated to another labour market context. Moreover, micro-econometric evidence indicated that, overall, policies combining an active and a passive component exhibit more mixed success rates in emerging economies than in developed economies (e.g. Almeida and Galasso, 2010; Cockx and Van Belle, 2016; Crépon et al., 2012; Graversen and Van Ours, 2008; Martínez et al., 2015; Medina et al., 2013). Secondly, we explicitly take into account the possible presence of complementarities between active and passive labour market policies. This is particularly important given the wider country coverage of the present contribution, since active and passive policies in emerging economies do not generally have an independent status but they are rather provided in combination.

If labour market policies are considered in isolation, we find that spending in ALMPs does not have a significant effect on labour market performances; while spending in PLMPs might generate disincentive effects (i.e. increase in unemployment or decrease in employment levels). However, when we consider the two policies in combination we find that the interaction between

Table 3.10: Results for the Analysis by Component Including Interactions Estimated Using 2SLS

	(1) Unempl. rate	(2) Empl. rate	(3) Labour force part. rate
	2SLS N = 436	2SLS N = 436	2SLS N = 436
PES and administration	11.586 (7.567)	4.988 (6.615)	13.037*** (4.375)
Training	4.028 (3.952)	-4.744 (4.078)	-2.673 (2.633)
Employment incentives	1.852 (3.482)	1.690 (3.593)	3.024 (2.620)
Supported empl. and rehabilitation	13.091 (8.216)	-20.360** (8.938)	-13.637** (5.908)
Direct job creation	3.558 (2.530)	1.475 (2.581)	4.005** (1.900)
Start-up incentives	-33.010 (20.716)	-24.686 (16.751)	-47.234*** (12.007)
Unemployment insurance	14.977*** (2.314)	-10.744*** (1.981)	-1.935* (1.079)
Unemployment assistance	8.832** (3.593)	0.120 (3.111)	5.791*** (2.058)
PES*UI	-40.695*** (14.521)	43.709*** (16.498)	20.692* (11.616)
Training*UI	-2.915 (4.928)	-0.288 (5.626)	-2.244 (3.743)
Emp. inc.*UI	-10.119 (6.443)	21.662*** (7.196)	16.545*** (5.155)
Sup. empl.*UI	2.526 (12.826)	-20.476 (14.840)	-19.758* (10.966)
Direct job creation*UI	-10.092** (4.968)	5.822 (4.879)	0.153 (3.269)
Start-up inc.*UI	-22.865 (21.865)	49.624** (21.778)	39.609** (16.372)
PES*UA	11.762 (18.054)	-46.760*** (18.109)	-41.744*** (12.127)
Training*UA	-4.188 (5.848)	7.188 (5.428)	4.964 (3.975)
Emp. inc.*UA	-8.597 (15.704)	-42.719*** (15.682)	-50.356*** (11.802)
Sup. empl.*UA	-9.784 (15.115)	25.847* (14.905)	20.773** (9.599)
Direct job creation*UA	-10.257 (7.927)	9.089 (6.753)	2.630 (5.575)
Start-up inc.*UA	93.323*** (35.152)	-31.928 (38.026)	23.389 (28.797)
Controls	YES	YES	YES
Country fixed effects	YES	YES	YES
Year fired effects	YES	YES	YES

Note. The presented statistics are coefficient estimates and standard errors in parentheses for the panel model outlined in Section 3.3. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level.

active and passive labour market policies generate substantial beneficial effects in terms of both employment, unemployment and labour force participation. This means that the more is spent on one type of policy, the more the other policy becomes effective. As a result, even the disincentive effects of PLMPs disappear (and eventually become positive) provided that enough is spent in ALMPs.

When looking in detail at the type of active and passive interventions, we find that spending in PES and administration, direct job creation and employment incentives have overall positive labour market effects, while no effects are found for additional spending in start-up incentives. Additional spending in supported employment and rehabilitation on the other hand has an overall negative effect on labour market outcomes. Finally, spending in training programs has the desired effects on the unemployment rate but at the cost of a lower labour force participation rate. For the passive policies, our findings are in line with economic theory. Both spending in unemployment insurance and unemployment assistance increases unemployment and lowers employment, but while unemployment assistance decreases labour force participation, unemployment insurance incentivizes individuals to remain in the labour force. In line with this, we also find that the positive interaction effects between active and passive interventions are completely driven by a positive interaction between active policies and unemployment insurance.

This leads us to the main take-away message from this paper. Ignoring the important interaction effects between active and passive labour market policies is detrimental, both form an academic point of view as for policy makers. Indeed, if we would not take the interaction into account we might conclude that spending in passive labour market policies leads to negative labour market outcomes, and that it therefore should be kept limited – and potentially considered as a sunk cost meant to provide support to the unemployed at the expense of labour market efficiency. However, as we learn from our interaction analysis, spending in ALMPs can only effectively ameliorate labour market outcomes if spending in PLMPs is sufficiently high. This can be explained by the fact that participation in active interventions is not attractive (or not effective) if individuals are not provided with adequate income support while being in the active program. Indeed and especially in emerging economies, individuals cannot afford to spend long periods without a job and participating in ALMPs (without a source of income) often represents an unaffordable investment whose returns will eventually materialise only in the medium (or even long) run. In this context, any investment in ALMPs alone becomes largely ineffective. A similar reasoning can be made for the spending in PLMPs. Indeed and while it is true that this spending deteriorates labour market outcomes when no money is spent in ALMPs, additional spending in PLMPs becomes beneficial for the labour market once a certain amount is spent in ALMPs. This can be explained by the fact that the provision of income support does not generate disincentive effects when adequate measures are implemented in parallel to activate the unemployed. Rather, guaranteeing income security can increase the efficiency of labour market matching (i.e. higher wages, longer job tenure) if individuals are not forced to accept the first available job in the presence of an adequate income support. Of course, the design and implementation of this support is critical to avoid any disincentive effects (e.g. duration, rate, conditionality).

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3.7 Appendix A: Additional Tables

Table 3.11: Results for the Robustness Test using Different Specifications

	(1) Unemp. rate	(2) Empl. rate	(3) Labour force part. rate	(4) Unemp. rate	(5) Empl. rate	(6) Labour force part. rate	(7) Unemp. rate	(8) Empl. rate	(9) Labour force part. rate
	OLS	OLS	OLS	Arellano-Bond	Arellano-Bond	Arellano-Bond	FGLS	FGLS	FGLS
	N = 788	N = 788	N = 788	N = 723	N = 723	N = 723	N = 788	N = 788	N = 788
Spending in ALMPs (% of GDP)	3.594***	-2.608***	-0.431	0.951	-0.837	-0.156	2.366***	-1.942***	-0.630*
	(0.685)	(0.710)	(0.551)	(0.889)	(0.663)	(0.418)	(0.558)	(0.486)	(0.364)
Spending in PLMPs (% of GDP)	6.396***	-4.194***	-0.473	3.757***	-2.805***	-0.488**	5.098***	-3.414***	-0.489**
	(0.488)	(0.438)	(0.373)	(0.768)	(0.525)	(0.190)	(0.358)	(0.311)	(0.231)
Interaction	-2.659***	2.010***	0.529**	-1.563***	1.152***	0.198	-1.760***	1.373***	0.431**
	(0.322)	(0.297)	(0.248)	(0.519)	(0.386)	(0.196)	(0.271)	(0.238)	(0.179)
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Country fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES

Note. The presented statistics are coefficient estimates and robust standard errors in parentheses for the robustness analysis outlined in Section 3.4.2. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level.

Chapter 4

Waiting Longer Before Claiming, and Activating Youth. No Point?[1]

4.1 Introduction

The Great Depression of 2008 has had a devastating impact on youth unemployment in Europe. By 2012 the youth unemployment rate in the European Union (EU27) had attained an unprecedented height of 22.8 percent, causing multiple countries to raise the alarm. Belgium was no exception. In 2009 the Flemish[2] government introduced a Youth Work Plan (YWP) in which job seekers below the age of 25 were followed-up more intensively when entering unemployment. In 2012 the Belgian government extended, for school-leavers aged less than 26, the waiting period before entitlement to unemployment insurance (UI) by three months, from nine to twelve months. This study aims at evaluating the effectiveness of these two policies.

Belgium is one of the few countries in the world where school-leavers need not have worked to be eligible to (flat rate) *non*-means-tested UI benefits. To the best of our knowledge, Australia and New-Zealand are the only other OECD countries that share these features of UI, even for non-school-leavers. While a waiting period in UI is usually justified as a means to discourage voluntary job quits (Fredriksson and Holmlund, 2006), this argument cannot apply for school-leavers who enter the labour market for the first time. Rather, the extension mainly aimed at reinforcing job search incentives. Indeed, standard job search theory predicts an unambiguous

[1] In collaboration with Bart Cockx.

[2] Belgium is a federal state. Flanders is the Dutch speaking region in the North.

increase in the job finding rate throughout the unemployment spell as opposed to job seekers facing a shorter waiting period. This job finding rate will gradually decrease, until the point of benefit entitlement (Mortensen, 1977). Moreover, empirical predictions from this job search theory have been largely confirmed in the literature (Tatsiramos and Ours, 2014).[3]

One may question that these predictions realise for at least two reasons. First, they are based on the assumption that other income sources are exogenously fixed. This may be unrealistic for school-leavers who may be financially supported by their parents. Whether this is possible could depend on the financial situation of parents, which we will test for in the analysis. Second, these predictions are based on the assumption that job seekers form rational and unbiased expectations about the likelihood of finding jobs. Since the seminal work of Tversky and Kahneman (1974) there is, however, growing evidence that expectations can be severely biased. For example, it is shown that individuals are overly optimistic regarding positive events and pessimistic with respect to negative events (Moore and Healy, 2008). Job seekers in particular strongly underestimate how long they will remain unemployed (Spinnewijn, 2015), and behave according to time-inconsistent (hyperbolic) preferences (DellaVigna and Paserman, 2005). Both tend to make job seekers less responsive to future incentives (Paserman, 2008; Spinnewijn, 2015). Hence, an extension of the waiting period may have much weaker behavioural impact than the one predicted by the standard job search model.

The empirical evidence on the effects of an intensification of counselling and training for youth such as in the aforementioned YWP in Flanders is mixed. In their most recent meta-analysis of active labour market programme (ALMP) evaluations Card, Kluve, et al. (2017) find that “job search assistance and sanction programs emphasizing "work first" have relatively large short term impacts on average. Training and private sector employment programs have smaller short term impacts but larger effects in the medium and longer runs.” Since the YWP comprises both components, it might be expected to increase the job finding rate of those who are assisted in searching for jobs, while locking-in participants in training programmes. However, ALMPs are also found to generally work less well for youth than for prime aged populations. Experimental evidence in Denmark has shown that a combination of meetings, job search courses and early activation could significantly enhance transitions from unemployment to employment. In this case the treatment seemed even particularly effective for youth (Graversen and Van Ours, 2008).

[3] We refer the reader to Section 4.7 for a graph based discussion of the predictions of standard job search theory

However, these large treatment effects appear to be largely due to the *threat* of participation requirements rather than the program itself (Pedersen et al., 2012; Rosholm, 2008; Vikström et al., 2013). In Denmark the intensity of meetings is much higher than in the Flemish YWP where youth are invited to a first meeting only from the third month. Moreover, even if participation is in principle mandatory this is, in contrast to Denmark, not very strictly enforced. Therefore, we cannot expect as strong effects of the YWP as the intensified early meetings and activation in Denmark.

Our research strategy consists in exploiting two distinct age discontinuities. First, a discontinuity in the duration of the waiting period at age 26 that was present prior to the reform in 2012: school-leavers younger than 26 were eligible to UI after 9 months, while those older had to wait one year. We investigate whether this discontinuity translates in a discontinuity in a number of labour market outcomes and, hence, provides causal evidence on the effectiveness of the 2012 reform (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). Secondly, participation in the YWP is also conditioned on an age threshold. We will therefore evaluate these two policies simultaneously. We consider the effect on unemployment duration and transitions from unemployment to employment. Search theory predicts that the longer waiting period may not only induce youth to search harder for jobs, but also to be less selective in accepting job offers (Mortensen, 1977). We therefore also consider the effect on a number of indicators of job quality, such as the daily wage, the time spent in employment, the incidence of part-time work and annual earnings from salaried employment.

Our analysis is based on a follow-up of *all* first registrations in Flanders, from July to October between 2008 and 2010. In order to obtain information on job quality, these registers are matched to those of diverse social security institutions. The population of interest is restricted to individuals with at least a bachelor's degree to avoid confounding the analysis by a hiring subsidy targeted to youth with a lower level of education, which applied also below age 26. One might argue that this limits the policy implications of our study, as most policy makers are primarily interested in the effects on the most vulnerable population i.e. low-skilled youth. Nevertheless, we believe that highly educated youth are an interesting and important target group for labour market policy. Indeed, as depicted in Figure 4.1, in many OECD countries youth unemployment is high, even for youth with advanced education. In some countries - i.e. Denmark, Portugal, Slovakia and others - the unemployment rate of youth with advanced education even outnumbers the unemployment rate of youth with basic or intermediate education.

We are not aware of any other study that investigates the impact (of an extension) of the waiting period. This is probably because the waiting period, if it exists, mostly lasts only a couple of days (Tatsiramos and Ours, 2014) and job search incentives are rather induced through much stricter job search requirements and follow-up by counsellors, especially in Australia (Langenbucher, 2015). As youths below the age of 25 are also counselled more intensively early on in the unemployment spell, our study design allows us to disentangle the impact of both policies.

The remainder of this paper is structured as follows. In Section 4.2 we describe in more detail the institutional setting and, in particular, the features that may influence the causal regression discontinuity design (RDD). Section 4.3 describes the data and Section 4.4 the empirical approach. Section 4.5 reports the results of our analysis, including some sensitivity analysis. Section 4.6 concludes with a summary of the empirical findings and a brief discussion of policy implications, the limitations of this study, and suggestions for further research.

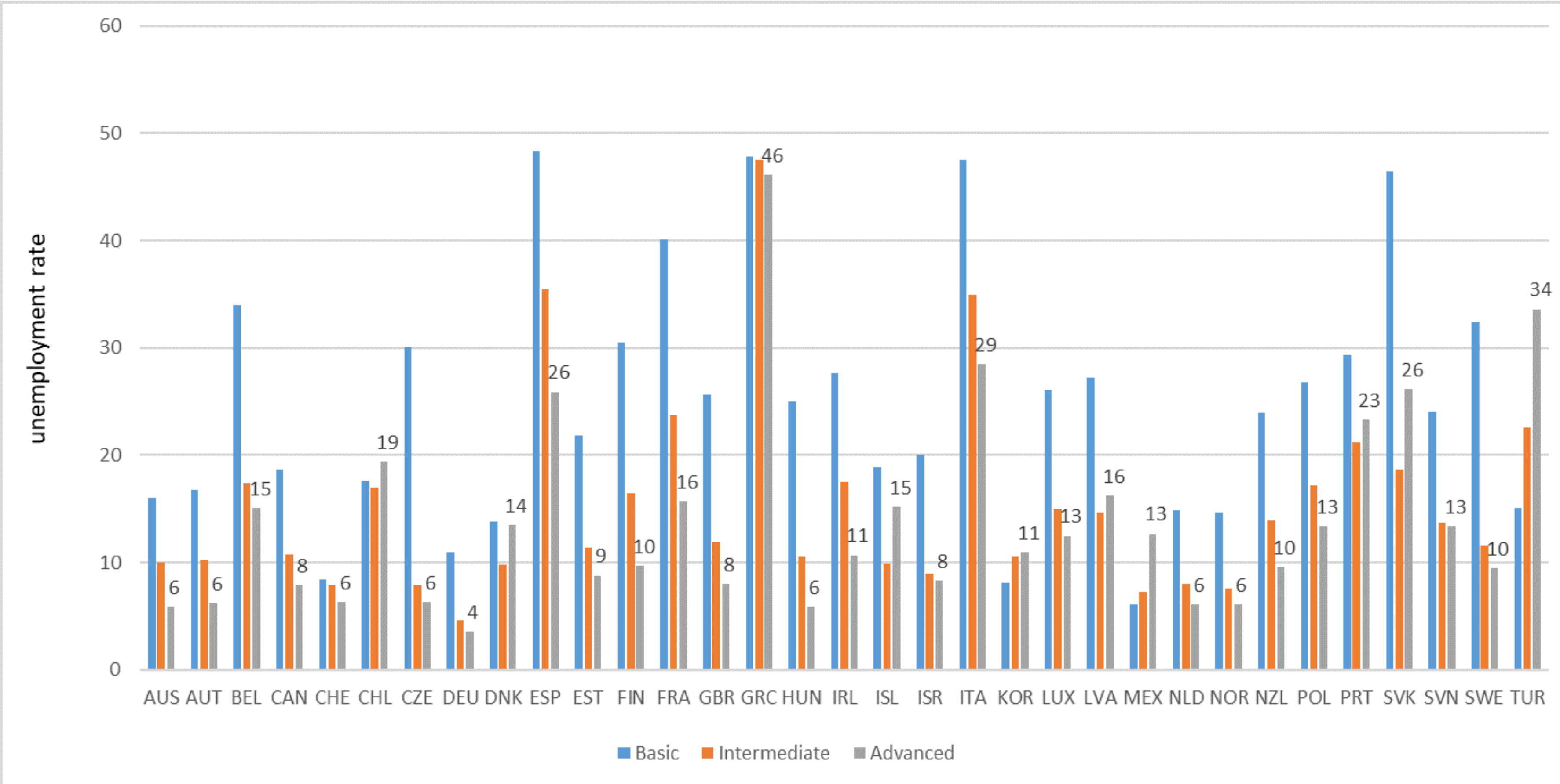
4.2 Institutional Framework

4.2.1 UI, the Waiting Period and Recent Reforms Regarding Youth

In Belgium a worker is eligible to UI in two instances: (i) after graduation from school conditional on a waiting period; (ii) after involuntary dismissal from a sufficiently long-lasting job. School-leavers are entitled to flat rate benefits, which depend on the family status and are non-means tested. In contrast to many other countries there is no time limit to the payment of UI.

Before January 2012 the required waiting period for eligibility to UI lasted 9 months if the applicant was younger than 26 at the *end* of this period, while it lasted 12 months for those older. The period starts after school completion from the first registration as job seeker at the regional PES. Since the secondary school year usually runs from 1 September to 30 June, first registrations occur usually in July. However, regulations state that the waiting period cannot start before August 1, unless registration starts after drop-out in the middle of the school year. In order to discourage drop-out, eligibility is conditional on a minimum acquired level of education.

Figure 4.1: Unemployment Rates by Educational Attainment in OECD Countries



Note. Data are collected from ILOstat and refer to the latest available year.

During the waiting period one is supposed to be actively seeking jobs. However, before 2012 this was not explicitly monitored and from an international perspective the imposed requirements on job seekers are relatively lenient. Any intervening employment spell or participation in short- to medium-run part-time vocational training counts for the waiting period. By contrast, participation in long-term or full-time training programmes, or resumption of full-time education, resets the waiting time to zero. The waiting period is interrupted (without reset) for any other intervening period of inactivity, such as sickness or incarceration.

Since January 1, 2012 the waiting period for those younger than 26 was extended by three months, so that it became as long as that for the older school-leavers. Furthermore, two additional restrictions were imposed. First, a time limit of three years was imposed on the entitlement to UI. However, this time limit applies only before the age of 30 for individuals living with other household members with income above some threshold. Second, job search effort is evaluated every 6 months since 2012 and school-leavers are only eligible to UI if they satisfy the job search requirements. Before 2012 these evaluations were only implemented after 15 or 21 months, respectively for those younger or older than 25. In 2015, the UI scheme for school-leavers was further reformed. UI can no longer be claimed if older than 25 and school-leavers younger than 21 must at least have successfully completed six years of secondary education.

Apart from UI, school-leavers are entitled to means-tested social assistance. While this may lower the financial incentives provided by the extension of the waiting period, the number of youths claiming social assistance in our sample is nevertheless very small (i.e. less than 30 individuals in the entire sample or less than 0.4%). This is probably because most of these young people still live with their parents and are thus not entitled.

4.2.2 The Youth Work Plan

In 2008 the Flemish PES introduced the Youth Work Plan as a pilot project targeted at low-educated youth in the largest Flemish cities. From 2009 onwards the YWP was extended to everyone younger than 25 one month after registration. The YWP consists of a set of specific actions targeted at those still unemployed three months after registration. At that moment a PES counsellor contacts the job seeker by telephone. If impediments to work are detected, the job seeker is invited for a meeting and counselling or training actions are proposed. If no action has been undertaken after this first contact and the job seeker is still unemployed three months later, she is invited directly to a meeting with a PES counsellor who may then propose particular actions. To the extent that the PES strictly denies these services for those older than 25 one month after registration, this could generate an age discontinuity close to the one that determines the length of the waiting period. For the latter the discontinuity occurs at 26,9 months after registration as job seeker. If the age is measured 9 months after registration, the potential age discontinuity of the YWP would occur at the age of 25 and 8 months.[4] In the analysis below we therefore explicitly allow for this second discontinuity.

4.2.3 Policies Potentially Threatening the RDD

In the period of analysis (2008-2012) several hiring subsidies were targeted to youth below the age of 26. These could potentially threaten our RDD. Two policies, a flat rate reduction in employer's social security contribution and the so-called "Activa" advantages, do not pose a threat because they do not constitute a discontinuity or the discontinuity is at a different point. Between 2010 and 2011 the Activa advantages were temporarily replaced by the so-called "Win-Win" to fight the persisting crisis since 2008. The Win-Win was targeted at youth with at most a secondary school degree who were less than 26 years old at hiring. By targeting youth below the same age threshold as the one at which the waiting period is extended, this policy in principle threatens our design. However, since the subsidy is targeted at youths with at most secondary education, the RDD remains valid if the analysis is restricted to youths with a higher educational degree: bachelors or masters. Not many observations are lost by imposing this, since the analysis focuses on youths entering unemployment directly after their studies. Within this target group only a small minority does not have a higher educational degree around the age discontinuity of 26 years.

[4] For the YWP the age (25 years) is measured one month after registration. Consequently, if age is measured at the end of the waiting period, i.e. 9 months after registration, participants in the YWP should be younger than 25 years and 8 months at that point.

4.3 Data

4.3.1 Data Sources and Sample Selection Criteria

The empirical analysis is based on Flemish PES register data of the full population registering for the first time as job seeker between July 1 and October 31 for the years 2008 through 2013. The PES data only informs about the potential type of UI entitlement – based on sufficient work experience or educational attainment – not about the effective benefit entitlement, neither about the activity state (education, employment or inactivity) prior to the first registration. They cannot, hence, distinguish between youths who just left school and those who had some intervening spell of employment or inactivity. Since employment spells count for the waiting period and we do not have reliable information on the exact starting date of employment we restrict the population in the following ways. First, since the Belgian school year ends on June 30 and the academic year starts end of September, restricting first registrations to the July-October period targets the group registering immediately after graduation.[5] Second, we requested the Cross Roads Bank of Social Security (CBSS) to match the register data of the PES to those of the different federal institutions of social security in Belgium. These data contain quarterly information on salaried and self- employment (since 2007) and monthly information on receipt of UI. Based on this information we dropped all individuals who (i) were observed in employment prior to the first registration; (ii) were reported to have left unemployment for a job according to the PES, but were not found to be unemployed in the CBSS dataset; and (iii) who were observed to be entitled to UI earlier than they could have been based on their first registration date and their age. The latter inconsistency is likely due to measurement error, since there are only few.

We mentioned in Section 4.2.3 that we could only include school-leavers with a bachelors or master degree, because otherwise the RDD would be confounded by a wage subsidy targeted at low educated youth. Furthermore, since the focus of the analysis is on the impact of the extension of the waiting period and the identification strategy is based on the discontinuity in the duration of the waiting period at the age of 26, we restrict the sample of analysis to an age window of 1.5 years to the left and to the right of this age. We do not consider a wider window

[5] For a few observations the unemployment spell was recorded to start at a different date than the first registration. These observations were dropped from the analysis.

because there are only very few individuals (178) who are older than 27.5 years. Finally, we restrict the analysis to youths entering the labour market between 2008 and 2010. The inflow in 2011 is not considered, because the waiting period of those younger than 26 was extended in the middle of their waiting period, on January 1, 2012. The entrants in 2012 are retained for a placebo analysis. The 2013 inflow is ignored, because the available PES registration data are right censored at the end of November 2013. In conclusion, while the initial population consists of 151,744 individuals, the final sample size retained for the analysis reduces to 5,495 individuals of whom 4,495 are younger than 26 and 1,000 older. Section 4.8 indicates how the sample size diminishes as particular selection criteria are imposed.

4.3.2 Descriptive Statistics

Table 4.1 reports the descriptive statistics of the explanatory variables retained in the empirical analysis. All variables except the household type originate from the PES registers and are measured at registration. The household type originates from the CBSS and is measured on December 31 of the year preceding the first registration at the PES. Descriptive statistics are reported for the complete sample, the group aged between 24.5 and 26 and the group aged between 26 and 27.5. The age is calculated (with daily precision) at the (counterfactual) end of the waiting period.

There is an asymmetry in the sample size around the age discontinuity. The majority in the retained sample is younger than 26 as most individuals complete education before this age. Youths ending higher education so late typically have repeated a couple of grades, since 22 or 23 is the age at which a master degree without any schooling delay would be attained. It also explains why about three quarters of the sample have a master degree: bachelors must have even more schooling delay to be observed in this age range. Another interesting observation is that more than 80% of the sample were officially residing at their parent's at the end of the year preceding the first registration. This is an indication that the sampled individuals are still financially dependent on their parents and that, hence, the extension of the waiting period might not have any important financial impact.

Table 4.2 displays the descriptive statistics of the outcomes of interest. We report the number of

Table 4.1: Descriptive Statistics of Explanatory Variables

	Total	**<26**	**≥26**
Number of Individuals	5,495	4,495	1,000
Mean age at the end of the waiting period registration	25.36	25.10	26.52
Variable	%	%	%
Gender: Female	47.68	48.59	43.60
Driver license	99.49	99.51	99.40
Education: master (other = bachelor)	75.18	74.82	76.80
Good knowledge of Dutch	94.54	94.82	92.80
Nationality: Belgian	98.23	98.40	97.50
Household type			
Single or couple with children	0.42	0.42	0.40
Single	3.73	3.38	5.30
Other (couple w/o child., institution,...)	3.55	3.43	4.10
Child living at parent's house	83.99	84.92	79.80
Year of first registration at PES			
2008	32.67	33.24	34.60
2009	34.43	34.73	33.10
2010	32.90	33.04	32.30
Month of first registration at PES			
July	47.90	48.92	43.30
August	18.89	19.18	17.60
September	27.01	26.14	30.90
October	6.21	5.76	8.20
Equivalent household income*	23'978	24'226	22'845

Note. Descriptive statistics of sample of analysis for the RDD. First registration at Flemish PES in July-October 2008-2010 for those aged between 24.5 and 27.5 years 9 months after registration. All variables except the household type originate from the PES registers and are measured at the first registration. The household type comes from the CBSS and is measured on December 31 of the year preceding the first registration. * Measured in the calendar year prior to first registration as job seeker. This includes labour market earnings and social security allowances of all household members excluding the school-leaver. The income has been scaled by the "OECD-modified scale" assigning a value of 1 to the household head, of 0.5 to each adult household member older than 18 (including the school-leaver) and 0.3 to each child. Reported statistics are calculated after dropping 74 missing observations. In the benchmark analysis these 74 observations are retained, because this analysis does not condition on this variable.

observations for which we have non-missing values, and the mean and percentiles (5, 25, 50, 75 and 95) of its distribution. The first variable of interest is the unemployment duration, which comes from the PES registers and is measured at the end of each calendar month. Temporary exits within the month are not recorded. This may lead to a slight measurement error, an issue to which we will return. Moreover, our data are left truncated: the PES did not select individuals at the actual first registration date, but at the end of the calendar month of this registration. This means that individuals who have left unemployment between registration and the end of the month are not retained. Note that we do have exact information at which date the registration of these individuals occurred, so that we can exactly determine the potential end date of the waiting period for each individual.

Unemployment duration is right censored at the end of the observation period in November 2013. However, only 12 observations are right censored, which is negligible and a feature that will be exploited in the analysis. The PES registers inform whether an exit was to employment or another destination, labelled “inactivity” . In Table 4.2 the third variable reports the descriptive statistics for the unemployment duration in case of an exit to employment, while the fifth one considers exits to inactivity only. Relatively few individuals (396) leave to inactivity, so the general distribution hardly differs. Median duration is 4 months, implying that only a minority is unemployed throughout the complete waiting period. 95% has left unemployment within one year. Figure 4.2 shows the profile of the hazard rates of exiting unemployment for the three distinct groups: (i) those falling under the YWP, (ii) those not benefiting from the YWP under the age of 26 and, (iii) those older than 26.

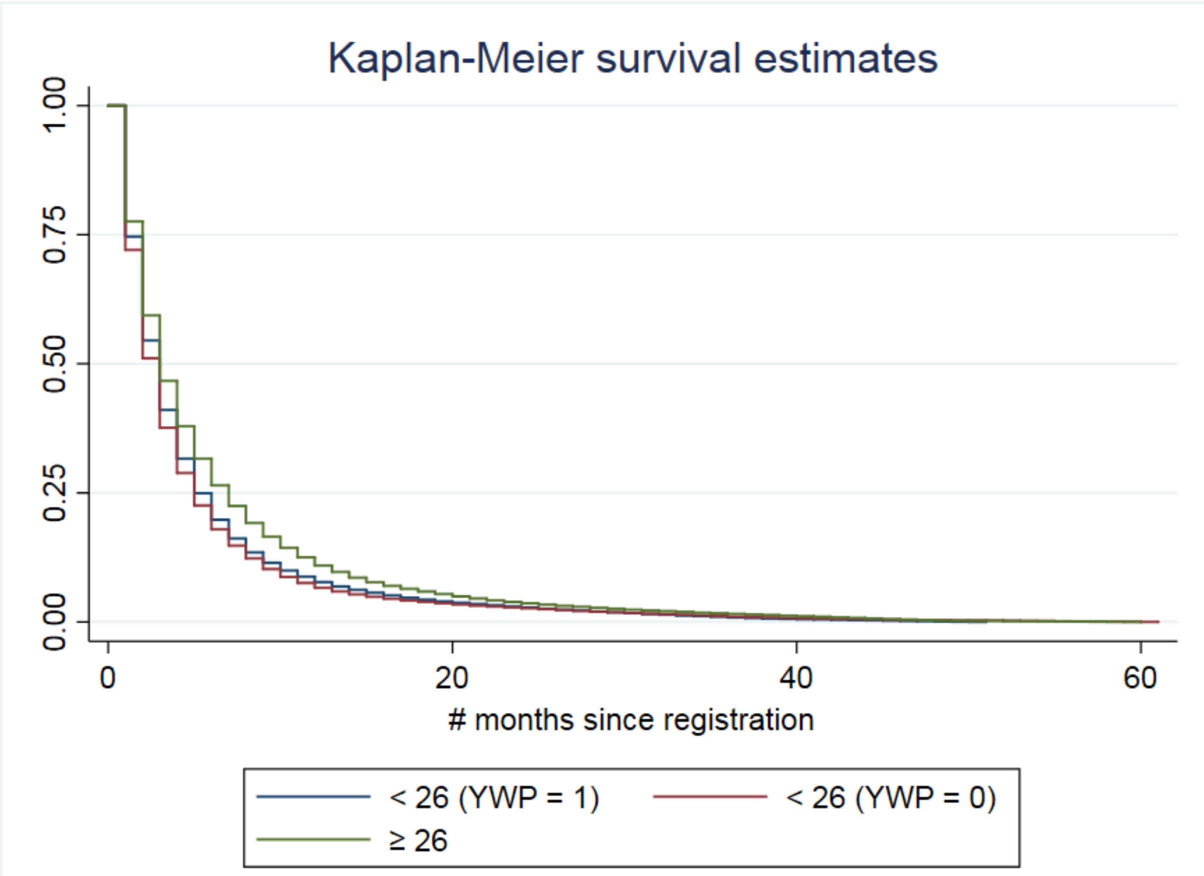
Based on the information of the BCSS, we constructed a number of additional outcomes measuring the quality of employment. We consider the number of working days in salaried employment in the quarter of exit from unemployment and the 4 subsequent quarters, the daily wage in the quarter of exit, the daily wage multiplied by the number of working days in the quarter of exit and the 4 subsequent quarters, and an indicator equal to one if a salaried worker worked part-time in the quarter of exit and zero otherwise. We only observe these variables for individuals who transited to salaried employment. For the daily wage in the quarter of exit, for instance, there is a relatively large number of missing values. This may be a consequence of individuals leaving the unemployment registers near the end of the quarter while not entering employment immediately

Table 4.2: Descriptive Statistics of Outcome Variables

Variabele	# observations Full sample	# observations *< 26*	# observations *≥ 26*	average Full sample	average *< 26*	average *≥ 26*	5% Full sample	25% Full sample	50% Full sample	75% Full sample	95% Full sample
Unemployment duration	5,495	*4,497*	*998*	4.98	*4.86*	*5.53*	2	3	4	6	12
Excluding right censored obs.	5,483	*4,487*	*996*	4.89	*4.76*	*5.84*	2	3	4	6	12
Ending in employment	5,086	*4,158*	*928*	4.81	*4.70*	*5.34*	2	3	4	6	11
Ending in salaried employment	4,785	*3,93*	*855*	4.79	*4.68*	*5.31*	2	3	4	6	11
Ending in inactivity	396	*327*	*69*	5.73	*5.42*	*7.23*	2	3	4	7	14
Working days [1]	4,785	*3,93*	*855*	243.3	*245.2*	*234.5*	72	211	270	295	324
Daily wage (€) [2]	4,259	*3,527*	*732*	106.7	*106.7*	*106.7*	74.7	92.6	104.5	119.2	144.2
Daily wage (€) (corrected) [3]	4,54	*3,733*	*807*	109.5	*109.5*	*109.6*	77.9	95.8	106.7	123.1	147.1
Earnings [4]	4,689	*3,852*	*837*	26,039	*26,341*	*24.651*	3,867	19,28	27,732	33,466	42,552
Part time work [5]	4,785	*3,93*	*855*	0.098	*0.100*	*0.088*	-	-	-	-	-

Note. All monetary values are expressed are expressed in constant 2013 Euros. [1] Number of working days in salaried employment in quarter of exit and 4 following quarters. Excludes workers transiting first to self-employment. [2] Daily wage is the average gross wage (before taxes and personal social security contributions) in the most important salaried job in the quarter of exit to employment. Excludes 526 missing observations. [3] Daily wage in quarter of exit replaced by daily wage in the following quarter if the latter deviates more than 5% from the former. This correction is applied because the daily wage is more sensitive to measurement error (possibly missing) if exit to employment occurs near the end of a quarter and, hence only observed over a short period. Excludes 245 missing observations. [4] In the quarter of exit and each 4 following quarters the product of the average gross daily wage in the most important job and the number of working days in salaried employment is calculated. The reported figure is the sum of these products over all quarters. Excludes 96 observations for which wage data are missing in all quarters. If the wage is missing in any of the other quarters, earnings was set to zero in the corresponding quarter. [5] Indicator variable taking on the value one if a person works part time in the quarter of exit and zero otherwise.

Figure 4.2: Profile of the Hazard Rates by Age



Note. Survival refers to survival in unemployment. Groups are defined as stated in Section 4.3.2

afterwards. Therefore, we also considered a second (corrected) daily wage in which we replace the first wage by the wage measured in the subsequent quarter if this wage deviates more than 5% from the first one and is not zero or missing.

Job seekers who find a salaried job are not all the time employed in the quarter of hiring and 4 subsequent quarters: 50% works less than 270 days. If we consider that in most sectors the workweek lasts 5 days and that an individual enters on average in the middle of a quarter, then someone who would have worked full-time during these 4.5 quarters would have worked 292.5 days. This corresponds roughly to the number of working days of the individual at the 75th percentile, who worked 295 days. Since only about 10% worked part-time, a substantial share of individuals have lost their job within 5 quarters. The median daily gross wage is about €105 which means if, as is common for a full-time occupation in Belgium, 7.2 hours per day is worked, the gross wage per hour would be about 14.6€/hour. Considering that some individuals work part-time, this is a lower bound. Finally, we measure the earnings as the sum over the aforementioned 5 quarters of the product of the average gross daily wage and the number of working days in each quarter. The median individual earned €27,732 in this period, or about €2,054/month.

4.4 The Empirical Approach

The empirical analysis aims at identifying the effect of (i) an extension of the waiting period from 9 to 12 months and (ii) the YWP on the various outcome variables described above. Identification is based on the discontinuity of the length of the waiting period at the age of 26 prior to 2012 and the age discontinuity at 25 years and 8 months for the YWP. The forcing variable A_i is the age of individual ($i = 1, 2, \ldots, N$) 9 months after the first registration as job seeker in the PES[6] measured in days and in deviation from the age discontinuity at 26 years. Let $D_{Wi} \equiv 1\left[A_i \geq 0\right]$ denote the treatment status (extension of the waiting period) of individual i where $1\left[.\right]$ is the indicator function, D_{YWP} is an indicator if the individual is younger than 25 years and 8 months (and hence eligible to the YWP) and zero otherwise,[7] X_i the vector of explanatory variables

[6] Nine months after August 1 if registration is in July (see Section 4.2.1).

[7] Since the YWP was not yet implemented for the high-educated in 2008, this indicator also zero for individuals of any age in 2008.

listed in Table 4.1,[8] and $F(A_i)$ and $F_1(A_i)$ two polynomial functions, assumed to be linear in the benchmark models. The following log-linear regression equation then identifies the proportional treatment effect β_W of the extension of the waiting period and β_{YWP} for the YWP:

$$log(Y_i) = \alpha + \beta_W D_{Wi} + \beta_{YWP} D_{YWPi} + F(A_i) + D_i F_1(A_i) + X_i\gamma + U_i \tag{4.1}$$

where in the benchmark models Y_i is one of the outcome variables listed in Table 4.2[9] and U_i the error term. We implicitly impose the same (linear) polynomial to the left and to the right of the age cut-off for the YWP. This assumption is made, because this cut-off at 25 and 8 months is very close to 26, so that a different polynomial between these two cut-offs would be identified on few data points only. Moreover, based on the graphical analysis below, this assumption does not seem to be violated. We will test this assumption in a sensitivity analysis for those outcomes for which we find a significant effect. We will also check for these outcomes whether the results are sensitive to the age window and the choice of the polynomial function (Section 4.5.4).

In case the outcome variable is a duration, for some individuals this duration is bound to terminate after the end of the observation period, or exits to a particular destination (e.g. employment) are not observed, because an exit to another destination (e.g. to inactivity) precedes exit to the destination of interest. These are instances of right censoring. Because the number of right censoring is very limited (see Table 4.2), we first ignore this and run regression equation 4.1. Subsequently, we treat right censored observations correctly by estimating the discrete duration model as a sequence of monthly binary choices (Jenkins, 1995; Kiefer, 1988).[10]

Let $t \in \{2,3,\ldots\}$ and ε_i denote the elapsed unemployment duration and the unobserved determinants of the exit rate from unemployment for individual i, assumed to be independently distributed from the observed covariates, respectively. This allows to take the dynamic selective sorting of the pool of unemployed over the unemployment spell into account (Salant, 1977). The conditional discrete-time hazards $h(t; A_i, X_i, \varepsilon_i) \equiv P(T_i = t | T_i \geq t; A_i, X_i, \varepsilon_i)$ associated to these binary choices take on the complementary log-log specification if they are derived from a continuous time hazard model:

[8] In the benchmark analyses the equivalent household income is not included as explanatory variable.
[9] Since the data are left truncated at the end of the first month, we normalize the duration by subtracting one.
[10] See Lammers et al. (2013) for a similar treatment of RDD within a hazard modeling framework.

$$h(t;A_i,X_i,\ \varepsilon_i) = 1 - exp\left[-exp\left(\alpha_t + \beta_{Wt}D_{Wi} + \beta_{YWP}D_{YWPi} + F\left(A_i\right) + D_iF_1\left(A_i\right) + X_i\gamma + \varepsilon_i\right)\right] \quad (4.2)$$

where $exp(\alpha_t)$ is the baseline hazard, $exp(\beta_{Wt})$ and $exp(\beta_{YWP})$ the proportional treatment effects on the hazard of, respectively, the extension of the waiting period and the YWP. We consider only linear polynomials. The discrete baseline is assumed to be constant within the following sets of discrete duration months: $\{3,4\}$, $\{5,6\}$, $\{7,8,9\}$, $\{10,11,12\}$, $\{13,14,\ldots\}$. As this is an arbitrary choice, we estimate the same model with a different set of discrete duration months as a robustness test, the results of which are available in Section 4.10. The treatment effects are assumed to be either fixed over the complete unemployment spell ($\beta_{Wt} = \beta_W$ and $\beta_{YWPt} = \beta_{YWP}$) or piecewise-constant. In order to determine the pattern of this piecewise-constant function, we use the predictions of standard non-stationary job search theory. Additionally, we estimate equation 4.1, where, for each month (counted from the first month after registration) the outcome variable is a dummy variables taking on the value 1 if a person is not registered as unemployed in that respective month. The results of this exercise are reported in Section 4.9. This leads us to assume a piecewise-constant over the following sets of months: $\{2,3,4,5,6,7,8,9\}$,$\{10,11,12\}$ and $\{$ 13,14, $\ldots\}$ for the extension of the waiting period and: $\{2,3,4,5,6,7\}$,$\{8,9,10\}$ and $\{11,12,\ldots\}$ for the YWP.

This model is estimated by maximum likelihood. To form the likelihood function, note that the discrete survival rate after an elapsed duration of t months is simply the product of one minus the discrete-time hazards in all preceding periods: $\prod_{s=2}^{t}(1 - h(s;A_i,X_i,\ \varepsilon_i))$. Consequently, if c_i denotes an indicator that is equal to zero in case of right censoring and one otherwise, then the log-likelihood function, from which the unobserved determinants are integrated out, can be written as follows:

$$logL = \sum_{i=1}^{N} log\left[\int_{-\infty}^{+\infty} \left[h\left(t_i;A_i,X_i,\ \varepsilon_i\right)\right]^{c_i} \prod_{s=2}^{t_i-1}\left(1 - h\left(s;A_i,X_i,\ \varepsilon_i\right)\right) dG\left(\varepsilon_i\right)\right] \quad (4.3)$$

where $G(\varepsilon_i)$ is the distribution of unobserved heterogeneity. We perform estimations in which we either assume that there is no unobserved heterogeneity or that it is Normally distributed with mean zero and variance σ^2 .

4.5 The Empirical Findings

4.5.1 Discontinuities in the Timing of Benefit Receipt and in the Participation in the YWP

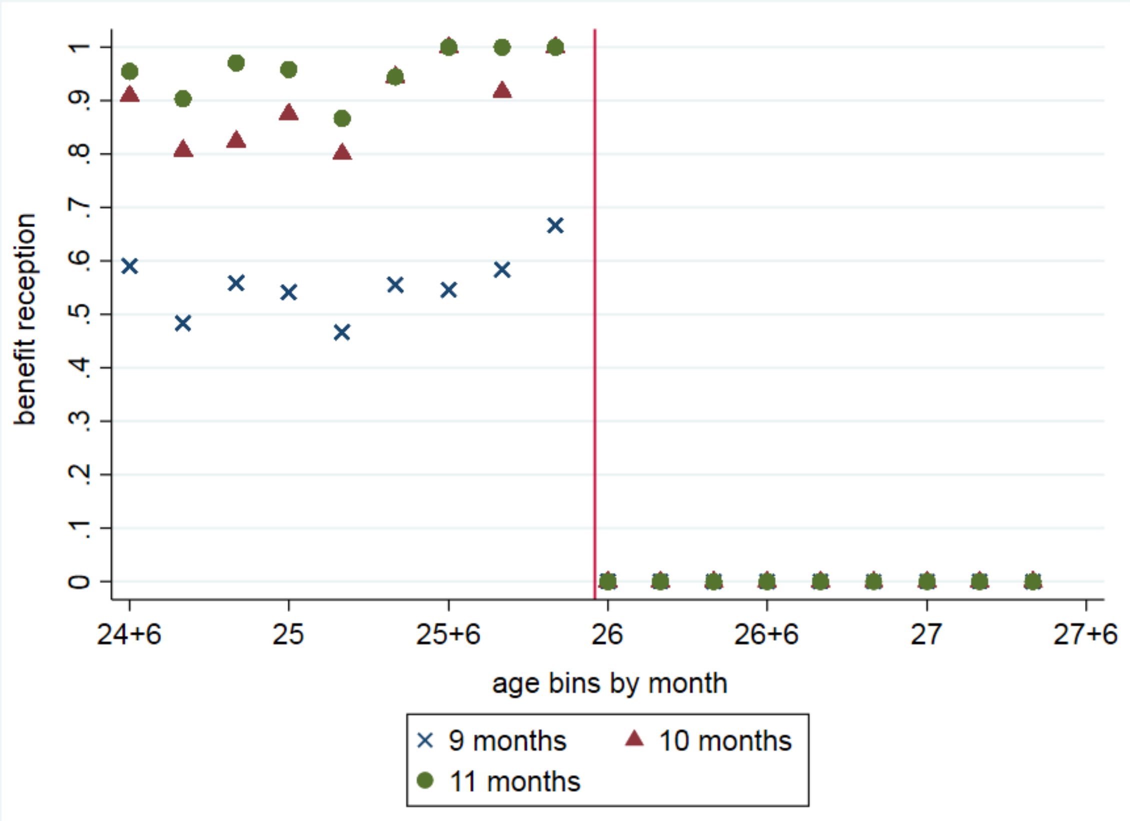
Figure 4.3 displays how UI receipt varies over age at various unemployment durations. By construction nobody is entitled to UI before 9 months. Let us first consider panel A. From 9 months the benefit receipt rate jumps up for those younger than 26 to 40-60%, depending on the specific age. Not everyone is entitled, because the waiting period might be extended due to brief interruptions of inactivity (cf. supra). Indeed, the receipt rate increases further to more than 80% after 10 months and to 90-100% in month 11. For those older than 26 a similar pattern is observed after 12 months (Panel B). The benefit receipt rates are more unstable for this group as a consequence of the small numbers involved: only 5% on average are unemployed for 12 months or more (Table 4.2) and the sample size of the older group is much smaller. We can conclude that there is a clear discontinuity in the waiting period before entitlement to UI at the age of 26. We therefore expect that, if the differential waiting period has an impact on job search, this should show up in a discontinuity in the unemployment duration and, possibly, in the indicators of employment quality. Even if, as a consequence of measurement error, the RDD is not completely sharp and the treatment effects must be, hence, interpreted as "intention-to-treat" effects.

Figure 4.4 displays the evolution of the fraction of individuals that are labelled in the YWP by age, as measured one month after registration as job seeker.[11] This fraction drops sharply at age 25. This justifies the inclusion of a second discontinuity point in the analysis at 25 years and 8 months, if age is measured at the potential start of benefit receipt, i.e. 9 months after registration

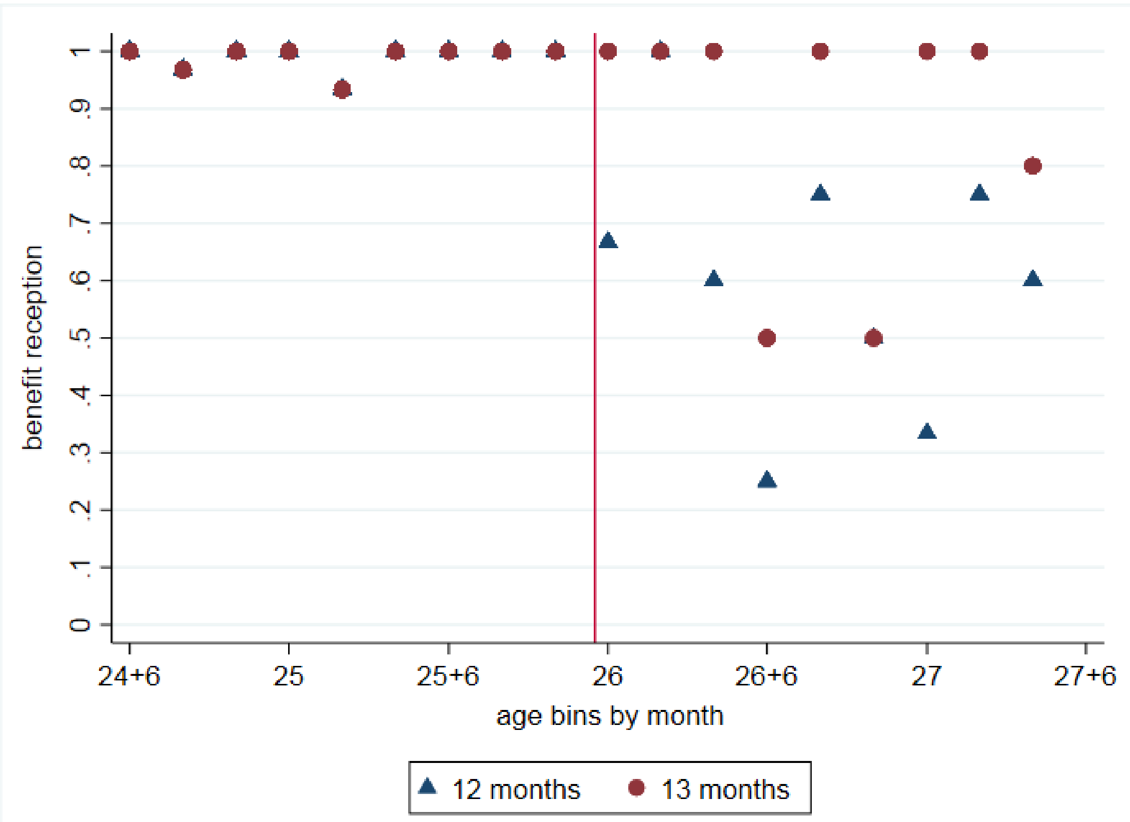
[11] Note that this analysis is based on a larger dataset, since the participation indicator to the YWP is not available in the dataset that was matched to the BCSS. Hence, we could not exclude individuals who experienced employment prior to registration as job seeker, neither could we exclude individuals who were entitled to UI prior to the end of the waiting period and, hence, could not be school-leavers. Individuals who registered in 2008 are excluded from this analysis, because the YWP was then not yet implemented for the high-educated.

Figure 4.3: UI Benefit Receipt at Various Unemployment Durations by Age

(a) From 9 to 11 Months



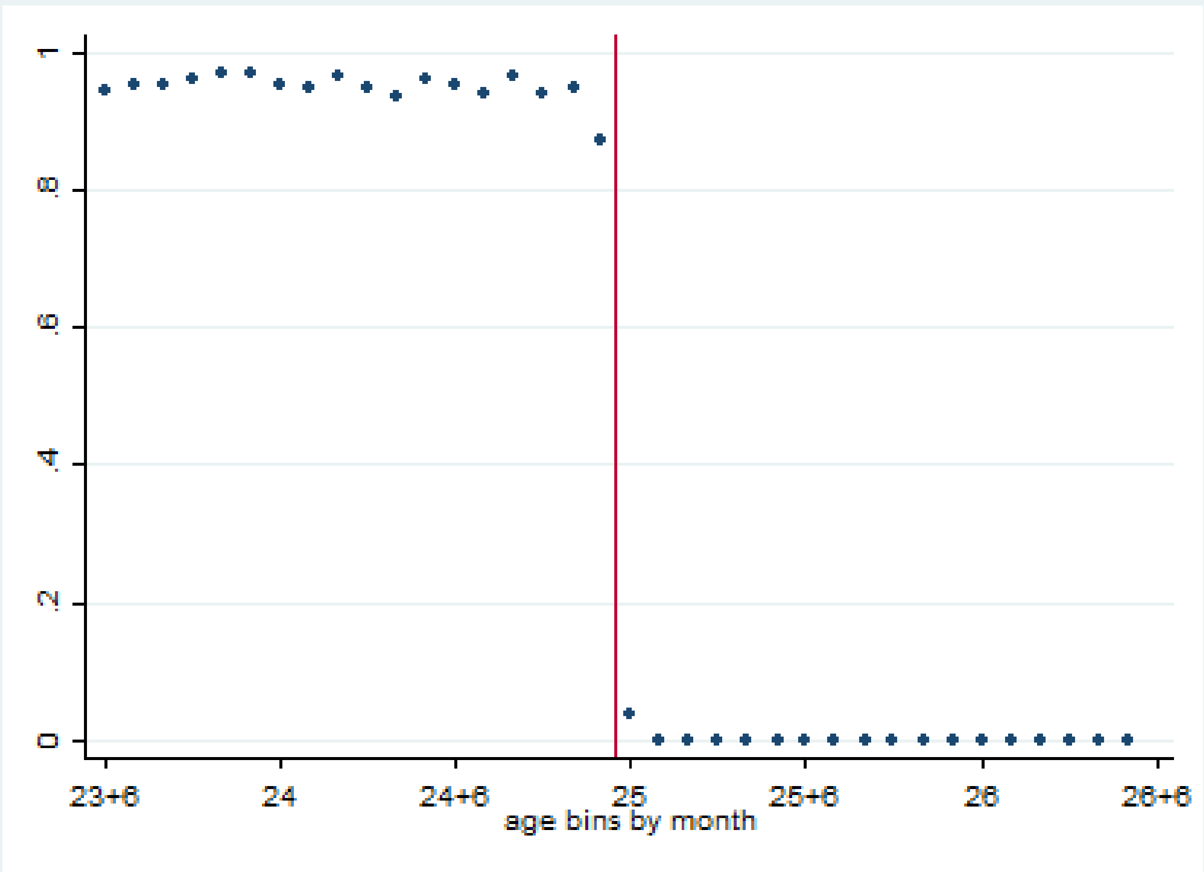
(b) After 12 or 13 Months



Note. Bins are defined as outlined in Section 4.5.1.

as job seeker.

Figure 4.4: Evolution of the Fraction Labelled in the YWP by Age

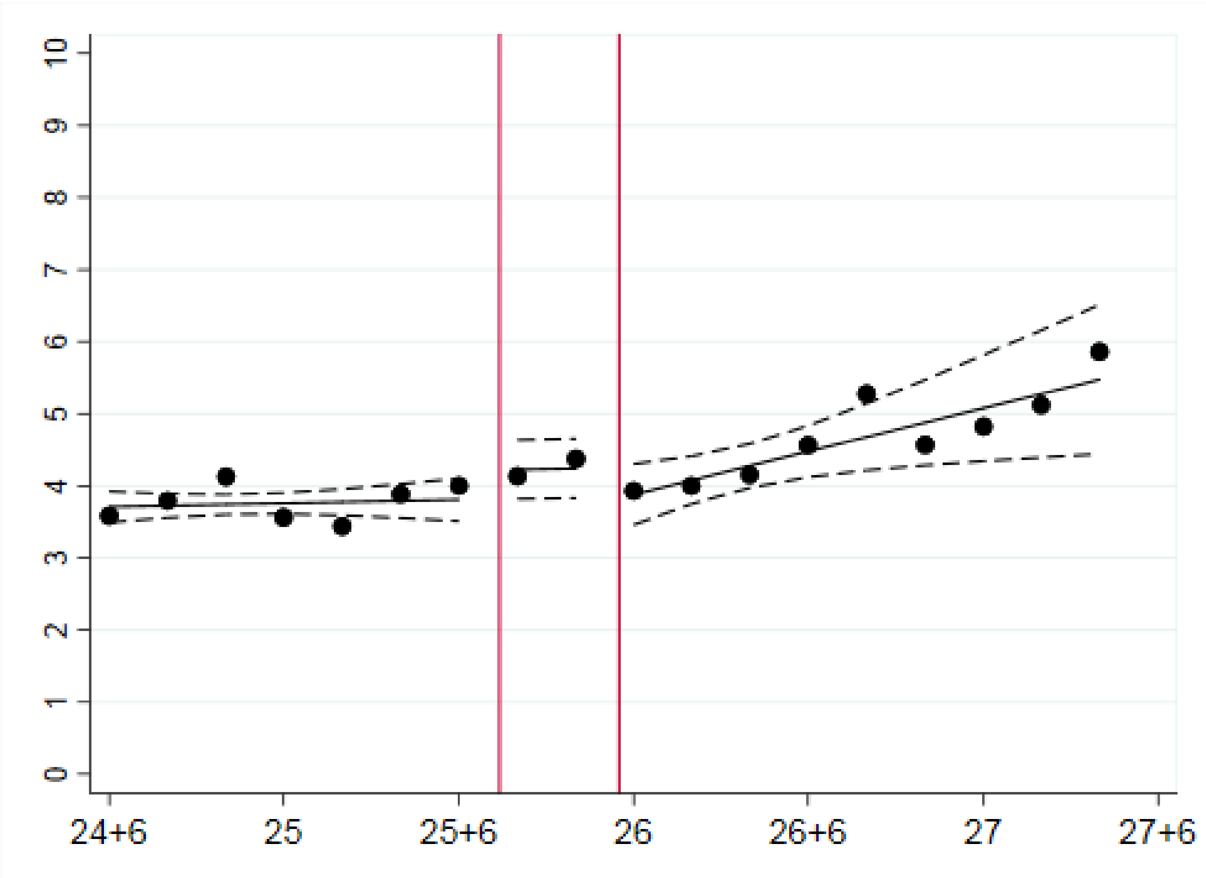


Note. Bins are defined as outlined in Section 4.5.1.

4.5.2 The Effects on Unemployment Duration

Figure 4.5 displays the unemployment duration as a function of age where dots represent the averages by age bins of 2 months. As mentioned, older school-leavers are more likely to have repeated grades and, hence, less attractive for potential employers. This is reflected in an increasing relationship of unemployment duration with age. However, despite the clear discontinuity in benefit receipt at 26 and of participation in the YWP at 25 and 8 months, the unemployment duration only drops slightly at 26 and evolves very smoothly at 25 and 8 months. This means that benefit extension seems to have only a slight impact on job search behaviour, if any, while the YWP not at all.

Figure 4.5: Unemployment Duration by Age



Note. Age bins are grouped by 2 months. 0.24% of the full sample are right censored observations and, hence, dropped.

The graphical evidence is quite salient. The formal econometric analysis just confirms this evidence.

Table 4.3 summarize the findings of the estimations of the linear regression and the discrete hazard model. In all of them the polynomial in age is specified as a linear spline. The first four columns report the estimated bench mark treatment effects (and associated standard errors) of the extension of the waiting period (β_W) and of the YWP (β_{YWP}) obtained by estimating the linear regression. The first two columns consider the (log) unemployment duration to any destination, while in the two subsequent columns the analysis is restricted to (log) durations ending in employment. The last two columns present the results of interest for the discrete hazard model. The coefficients are reported in exponential form, so they can be interpreted as multipliers of the hazard. In Column (5) we report the findings of the model that assumes a constant proportional treatment effect throughout the unemployment spell. Column (6) shows the results for the model that allows a time-varying treatment effect of the extension of the waiting period, while Column (7) shows the results allowing a time-varying treatment effect of the YWP.

In line with theoretical expectations, the extension of the waiting period is found to decrease unemployment duration. However, the extension by 3 months reduces the unemployment duration of a 26 year old job seeker by 0.13 months (Column (1)) or 1.1% (Column (2)) only, and is not statistically different from zero. The effect of the YWP is positive for both the linear and the log-linear model and even closer to zero. If we only look at spells ending in employment (Columns 3 & 4) the findings hardly alter, as only very few job seekers leave unemployment to inactivity. The discrete hazard model in Column (5) displays a slightly larger proportional effect of the extension on the hazard (+4.4%) and a small negative effect of the YWP (-4.7% = (1-0.953)$*$100), but these effects are again not statistically significant. The findings reported in Column (6) show some non-monotonic variation of the extension of the waiting period in the considered sub-periods, but the treatment effects are never significantly different from one. Finally, in Column (7) it is shown that the YWP increases the hazard of exiting unemployment significantly for months 8, 9 and 10. In Section 4.10 we present the complete estimation results of these benchmark models. As an additional robustness test, we have estimated equation 4.1 where the outcome of interest is the chance to be registered as unemployed in month x, where x takes on the value 1, one month after registration. The results of this exercise are reported

Table 4.3: Estimation Results for Unemployment Duration and Transitions to Employment as Outcomes of Interest

Coef.	Any exit		Exit to employment		Exp. (Coef.)	Exit to employment		
	(1)	(2)	(3)	(4)		(5)	(6)	(7)
	Linear	Log-linear	Linear	Log-linear		Hazard	Hazard	Hazard
$\hat{\beta}_w$	-0.126	-0.011	-0.156	-0.012	$exp(\beta_w)$	1.044	-	1.044
	(0.283)	(0.049)	(0.293)	(0.051)		(0.088)		(0.083)
					$exp(\beta_{w2-9})$	-	1.047	-
							(0.088)	
					$exp(\beta_{w10-12})$	-	0.921	-
							(0.183)	
					$exp(\beta_{w13-\infty})$	-	1.180	-
							(0.232)	
$\hat{\beta}_{ywp}$	0.122	0.003	0.049	-0.001	$exp(\beta_{ywp})$	0.953	0.953	-
	(0.243)	(0.041)	(0.246)	(0.042)		(0.067)	(0.066)	
					$exp(\beta_{ywp2-7})$	-	-	0.928
								(0.062)
					$exp(\beta_{ywp8-10})$	-	-	1.385***
								(0.174)
					$exp(\beta_{ywp11-\infty})$	-	-	0.891
								(0.130)
					Variance	0.635***	0.631***	0.542***
					heterogeneity	(0.082)	(0.082)	(0.073)
N	5,483	5,483	5,087	5,087	***N***	5,495	5,495	4,495
R^2	0.043	0.047	0.041	0.047	**Log-likelihood**	-11338.2	-11337.6	-11334.6

Note. Heteroskedastic robust standard errors between parentheses. All models include the control variables mentioned in Table 4.1 (except for the equivalent household income) and a linear spline in age. In the (log-) linear models right-censored observations are dropped: 12 observations in case of exits to any destination reported in the first two columns; an additional 396 individuals who leave from unemployment to inactivity in case duration until exit to employment is considered. In the hazard models the aforementioned dropped observations are right censored. "Variance heterogeneity" is the variance of the Normal mixing distribution of the unobserved heterogeneity. * p-value less than 10%, ** p-value less than 5%, *** p-value less than 1%.

in Section 4.9 and largely confirm our findings. Before providing any interpretation of these potential treatment effects we consider their effects on some indicators of quality of employment.

4.5.3 The Effects on the Quality of Employment

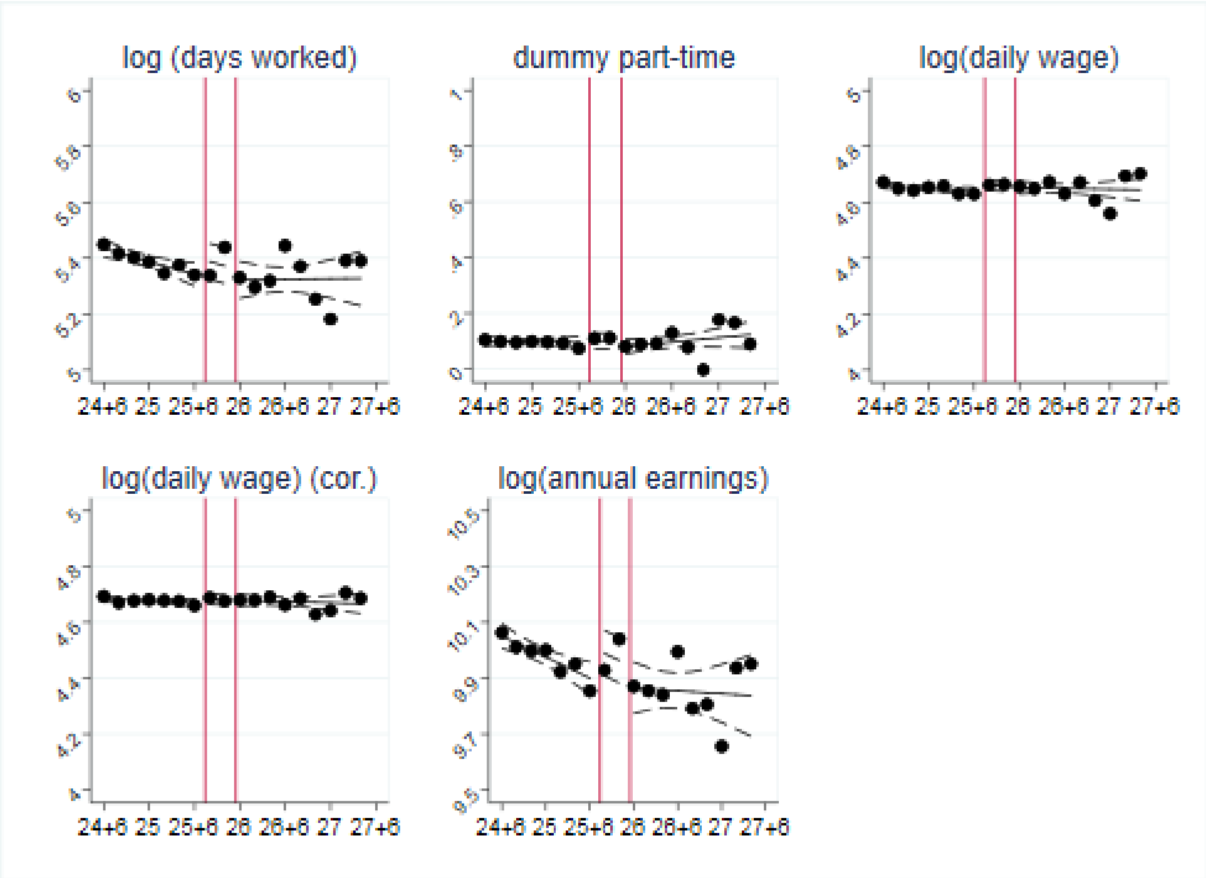
Figure 4.6 displays the evolution over age of various indicators of employment quality: the log number of working days in the quarter of exit and 4 subsequent quarters, the fraction of individuals working part-time, the (possibly corrected) log daily wage at the end of the quarter of exit, the log of annual earnings in the quarter of exit and 4 subsequent quarters. The daily wage and the indicator of part-time work evolve very smoothly at the two discontinuities, providing evidence that these outcomes are not influenced by the policies. For the number of working days and the earnings the figures are less clear. We see a spike at 25 years and 10/11 months. This spike occurs at an instant that neither the extension of the waiting period nor the YWP is in operation. This suggests that *both* policies affect the aforementioned outcomes negatively.[12] This is also the robust conclusion of the formal econometric analysis below.

Table 4.4 presents the formal econometric estimates of the parameters of interest for the aforementioned outcomes. Since these outcomes are only measured for individuals in *salaried* employment for whom there is no missing value, we first check whether we should not be concerned by sample selectivity (Heckman, 1974). We therefore ran a linear probability model specified as in equation 4.1, where the dependent variable is equal to one if the considered outcome has a non-missing value and to zero otherwise. We conclude that there is only a major concern for the non-corrected daily wage, but not for the corrected daily wage. While this is comforting, it is no formal proof of the absence of sample selectivity at the age discontinuities.

In line with the graphical evidence, both the YWP and the extension of the waiting period have a small and statistically insignificant effect on the gross wage and on part-time employment. Both policies are found to have a negative impact on hours worked and earnings, although not statistically significant at the 5% level: the former reduces working time by 6.8% (p-value of 8.3%) and earnings by 5.4% , while for the latter these effects are -3.5% and -5.5% , respectively.

[12] Note that it is unlikely that this spike just reflects lack of statistical precision due to too small bin size. This becomes an issue only from age 26.5 onwards, because very few individuals graduate and register as unemployed at those ages. Before age 26.5 the average outcome is fairly stable by bin, except for the aforementioned spike.

Figure 4.6: Indicators of Quality of Employment by Age



Note. Age bins are grouped by two months. The indicators are defined as explained in Section 4.3.2.

Table 4.4: Estimation Results on Employment Quality and Associated Selection Indicators

	Days worked		**Dummy part time**	**Daily wage**		**Daily wage (corrected)**		**Earnings**	
Coef.	Select D	Log-lin.	Lin.	Select D	Log-lin.	Select D	Log-lin.	Select D	Log-lin.
$\hat{\beta}_w$	-0.027	-0.035	-0.023	-0.071**	0.003	-0.037	0.004	-0.032	-0.055
	(0.018)	(0.047)	(0.019)	(0.027)	(0.015)	(0.024)	(0.014)	(0.020)	(0.063)
$\hat{\beta}_{ywp}$	-0.006	-0.068*	-0.008	-0.044*	-0.010	-0.026	-0.005	-0.015	-0.054
	(0.014)	(0.039)	(0.016)	(0.022)	(0.012)	(0.019)	(0.011)	(0.016)	(0.051)
N	5,495	4,785	4,785	5,495	4,259	5,495	4,54	5,495	4,689
R^2	0.017	0.024	0.053	0.018	0.075	0.020	0.072	0.022	0.036

Note. Heteroskedastic robust standard errors between parentheses. All models include control variables mentioned in Table 4.1 (apart from the equivalent household income) and a linear spline in age. Select D refers to an indicator that is equal to one if the associated outcome is observed and zero if it is missing. It therefore refers to a linear probability regression based on Equation 4.1 that aims at identifying whether there is any sample selectivity at the age discontinuity of 26. Log-lin. Refers to a log-linear regression of the associated indicator of employment quality based on equation 4.1. * p-value less than 10%, ** p-value less than 5%, *** p-value less than 1%.

Because the effects on working days and earnings were barely statistically significant, we performed a couple of sensitivity analyses on these variables. We considered (1) a narrower window around the age discontinuity at 26, (2) a spline at the age discontinuity of 25 and 8 months rather than at the one at 26, (3) a linear and (4) quadratic function of age without spline. These findings are reported in Section 4.11. Even if the effects remain statistically insignificant, the findings are remarkably robust, especially for the YWP. For the extension of the waiting period the sign of the effect is in line with standard job search theory in that job seekers respond by lowering the job acceptance requirements, i.e. jobs with shorter expected duration are accepted. The fact that the reservation wage is not affected is probably related to the binding minimum wage for youth. Lowering job acceptance requirements should generally also lead to more job acceptance and, hence, increase the job finding rate, which we did not find (Table 4.3). This suggests that the acceptance of lower quality jobs does not accelerate job finding very much.

The significant negative impact of the YWP on days worked suggests that PES caseworkers induced unemployed school-leavers to accept more temporary jobs and/or fixed-term contracts then they would have done in absence of the YWP. Caseworkers could have justified this strategy based on the argument that short-term jobs could be stepping stones to a more permanent job. However, the empirical evidence on the effectiveness of this strategy is mixed. For instance, stepping-stone effects have been found by Booth et al. (2002) in the UK, Ichino et al. (2008) and Graaf-Zijl et al. (2011) in the Netherlands and Cockx and Picchio (2012) in Belgium, while other researchers have found adverse effects, such as Güell and Petrongolo (2007) in Spain, and Autor and Houseman (2010) in the US. Givord and Wilner (2015) in a recent paper argue that these mixed findings may be a consequence of lumping together temporary jobs and fixed term contracts. These authors find on French data that "although fixed-term contracts may provide a 'stepping-stone' to permanent positions, temporary agency work is hardly better than unemployment in this regard." The PES explicitly announces on its website its partnership with the sector of temporary work agencies and that it regards temporary jobs as 'stepping stones' to regular work.[13] Even if we do not have hard evidence that caseworkers indeed advice youths to accept temporary jobs, our findings and the aforementioned empirical evidence are consistent with this interpretation. In order to explore this further, two additional analysis on the number

[13] See vdab.be/uitzendsector/samenwerking.shtml [accessed on 13/09/2016].

of days worked are presented in Section 4.12. These confirm our hypothesis that both policies incite youth to take up lower quality temporary jobs but that this does not result in higher job finding probabilities, i.e. the 'stepping-stone' is not realised.

4.5.4 Validity Tests

We have checked for the validity of the RDD based on two standard tests. In Section 4.13 we present the graphical tests proposed by McCrary (2008) as to demonstrate that "manipulation" of the sorting variable does not threaten the validity of the RDD. In Section 4.14 we further justify our approach by reporting for all outcomes the placebo tests on the 2012 data. In 2012 the waiting period has become equally long for all age groups, so that no impact at the age threshold of 26 should be found. This is confirmed.

4.5.5 Treatment Heterogeneity

In Section 4.3 we argued that the extension of the waiting period might not induce that important effects on job search behaviour, because parents might financially compensate for the income loss. The fact that more than 80% of the school-leavers lived at their parent's at the end of the calendar year preceding their first registration as job seeker (Table 4.1) suggests this might be relevant. To investigate this further we split the sample in two groups depending on whether the equivalent income from labour income and social security allowances of other household members was either below or above the median. We only report the results from the baseline model and for the days worked and earnings, for which we found significant effects.

In the first two columns of Table 4.5, we report these findings for the benchmark outcome, i.e. log unemployment duration with exits to all destinations. For this outcome variable the effect of an extension of the waiting period is qualitatively the reverse of what we would have expected i.e. the effect is more negative for youths living in households with high equivalent income. Standard errors are, however, again very large, so that no firm conclusion can be drawn. The next two columns report the results for the hazard model where we allow the treatment effect of the YWP to differ in time. Here, the effect goes in the expected direction. The significant

Table 4.5: Heterogeneous Effects by Equivalent Household Income

	Log duration in unemployment		Hazard out of unemployment		Days worked		Earnings	
Coef.	Income ≤ median	Income > median	Income ≤ median	Income > median	Income ≤ median	Income > median	Income ≤ median	Income > median
$\hat{\beta}_w$	0.018	-0.074			-0.065	-0.032	0.023	-0.015
	(0.071)	(0.069)			(0.069)	(0.064)	(0.021)	(0.022)
$\hat{\beta}_{ywp}$	0.003	0.002			-0.123**	-0.032	-0.003	-0.016
	(0.061)	(0.057)			(0.055)	(0.057)	(0.017)	(0.017)
$exp(\beta_w)$			1.004	1.120				
			(0.125)	(0.120)				
$exp(\beta_{ywp2-7})$			0.923	0.904				
			(0.097)	(0.080)				
$exp(\beta_{ywp8-10})$			1.480**	1.216				
			(0.271)	(0.215)				
$exp(\beta_{ywp11-\infty})$			1.029	0.656**				
			(0.215)	(0.140)				
N	2,705	2,704	2,712	2,709	2,361	2,358	2,104	2,099
R^2	0.059	0.045			0.036	0.023	0.074	0.081
Log-likelihood			-5715.3	-5437.0				

Note. Heteroskedastic robust standard errors between parentheses. All models include control variables mentioned in Table 4.1 (apart from the equivalent household income) and a linear spline in age. * p-value less than 10%, ** p-value less than 5%, *** p-value less than 1%.

positive effect found after 8 months seems to be only present for the youths living in low-income households. Similar effects are found when looking at the number of working days (Columns 5 & 6). A longer waiting period induces low-income-youths to accept jobs that reduce working time by 6.5%, while this reduction is only 3.2% for high-income-youths. While these effects are not significantly different from zero, the impact of the YWP for the low-income group is -12.3% and significant at the 5% level, in contrast to the -3.2% statistically insignificant effect for the high-income group. Unfortunately, this ordering of the effects according to income does not remain as clear if the effect on earnings (the last two columns of Table 4.5) is considered.

4.6 Conclusion

In this study we exploited two age cut-offs to evaluate, by an RDD, the effects of two ALMPs targeted to youth on the transition rate from unemployment to employment and on the quality of this employment. The first policy consisted in an extension of the waiting period from 9 to 12 months that was imposed on Belgian school-leavers before they were entitled to UI. The second was the YWP providing more intensive counselling and training to young job seekers earlier on in their unemployment spell. In order to avoid that the estimated treatment effect would be confounded by a programme targeted to low educated youth, the analysis was restricted to youths who recently graduated from a bachelor's or master's degree.

The study finds that such an extension of the waiting period slightly, but statistically insignificantly, increased the transition rate to employment for these highly educated youth. We argued that a potential explanation of this small impact could be that these youths were not much financially constrained by this extension, as most of them would still be financially dependent on their parents' income. However, we did not find supporting evidence for this hypothesis. Another potential explanation could be that these youths form biased or non-rational expectations that could make them less responsive to future incentives (Paserman, 2008; Spinnewijn, 2015). Even if these elements could play a role, the analysis finds that future incentives do affect job *acceptance* behaviour. While the extension of the waiting period did not affect the level of the accepted wage, we did find some suggestive, but robust, evidence that it did reduce the number of working days and, hence, earnings in the five quarters following exit from unemployment.

This means that the extension of the waiting period induces job seekers to accept short-term job offers more easily. These effects were found to be larger for youths living in poorer households where job acceptance is more guided by liquidity constraints.

The YWP on the other hand did only have a significant positive impact on the exit rate from unemployment to employment after eight months. Furthermore, as for the extension of the waiting period, it did robustly reduce the number of working days by about 6-7%, while leaving the wage unaffected. For youths living in households with below median equivalent income this working time fell even by about 12%. The effect on earnings was also negative, although slightly smaller and never statistically significant. An explanation for these findings is that PES caseworkers advised young unemployed to accept more temporary jobs and fixed-term contracts potentially arguing that these could be stepping stones to permanent jobs. However, our findings point that the stepping stone hypothesis should be refuted as the reduction in working time did not result in a significantly higher probability of exiting unemployment.

Even if the RDD approach is generally a very convincing and powerful method to identify causal effects, we faced an important limitation in the implementation of this method. We were confronted with two policies the participation in which was delineated by two sharp age cut-offs which were only 4 months apart. This sizeably reduced the width of the age window to detect a corresponding discontinuity in behaviour and hence the statistical power.

We nevertheless can formulate some policy conclusions. It is important to note that these apply to the population of this study: highly educated school-leavers and that they cannot easily be generalized beyond this group. First, our analysis revealed that an extension of the waiting period either did not enhance much the transition rate to employment or, if it did, it did so at the cost of reduced working time and, hence, earnings. This suggests that threatening with a sanction is not the right method to activate youth and supportive measures might work better. However, the YWP is precisely offering this kind of support and our analysis revealed that this approach produced very similar, if any, effects as the one that involves a financial sanction. Part of the explanation is that caseworkers might have given misleading advice that temporary jobs are stepping stones to long-term employment. Another reason is that the YWP was not sufficiently intensive. As mentioned, experimental evidence suggests that very intensive (fortnightly) meetings with caseworkers can generate significantly positive effects on the job-finding rate.

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4.7 Appendix A: Job Search Theory

A waiting period is the mirror image of a time limit on UI eligibility. With a time limit the job seeker is informed at entry that the UI benefit will expire after a predetermined period. In the case of a waiting period, UI benefits are zero at the onset of unemployment and will become strictly positive after a predetermined period. We expect therefore that the predicted behaviour should be the mirror image of the one predicted for the case of time limit.

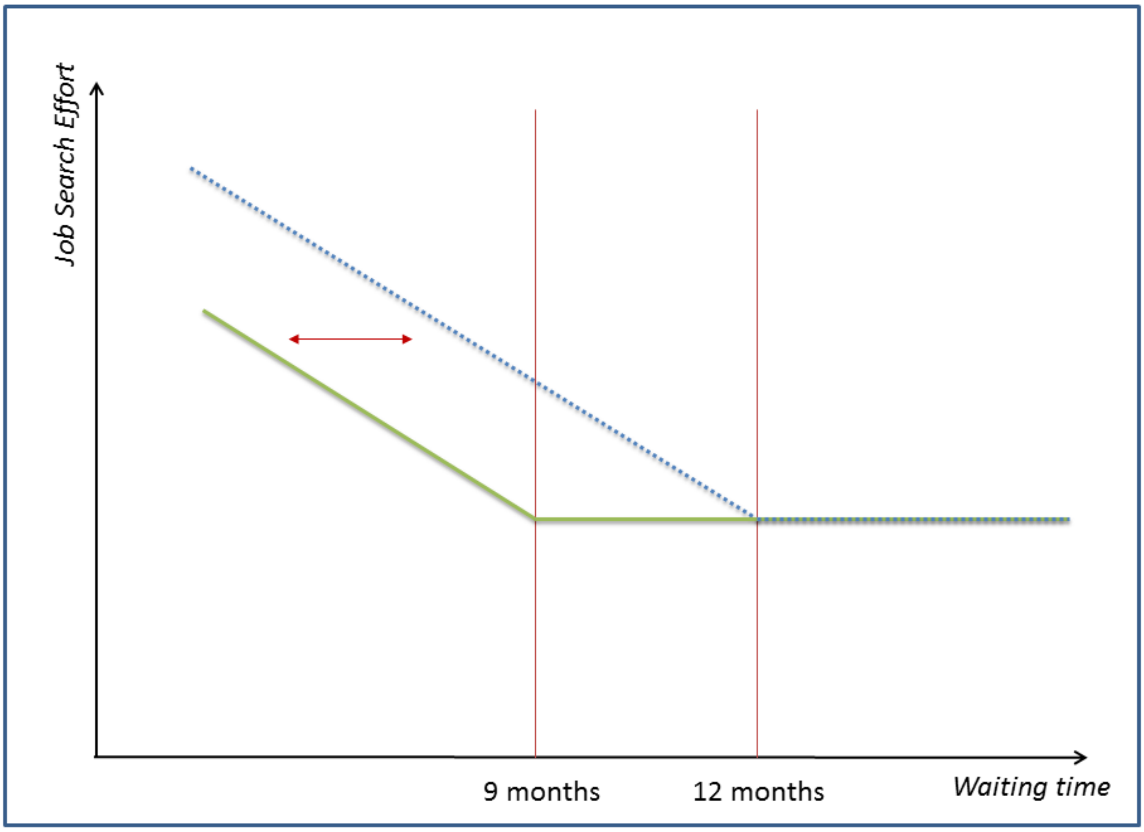
Mortensen (1977) is the first to use non-stationary job search theory to describe the predictions of a time limit in UI on the job search behaviour of rational forward looking individuals. He proves that a job seeker gradually increases job search effort and reduces the reservation wage (or, equivalently, increases the job acceptance probability) as she approaches the moment of benefit exhaustion. This empirical prediction has been largely confirmed in the literature (Tatsiramos and Ours, 2014), although that empirical studies generally find a more abrupt than predicted increase in the job finding rate just before benefit exhaustion, followed by drop immediately afterwards.[14]

In Figure 4.7 we depict the predictions of standard job search theory of an extension of the waiting period from 9 to 12 months. Before the extension the unemployed are not yet claiming UI benefits. Hence, they will then search harder for jobs than when they are entitled to UI. Nevertheless, in anticipation of the entitlement, they will gradually decrease job search effort until the flat rate benefit is paid out. Since this benefit never expires, job search effort remains constant after that point. An extension of the waiting period from 9 to 12 months should induce a parallel shift of the search effort to the right, because rational forward looking individuals make identical decisions in case the future profile of UI benefits is the same. Job search theory therefore predicts that an extension of the waiting period should unambiguously increase the job finding rate throughout the unemployment spell. Depending on the form of the utility function, the time profile may be concave or convex (linear, for simplicity, in Figure 4.7) close to the start of benefit entitlement. Hence, we cannot make general predictions regarding the unemployment

[14] Mortensen (1977) show that such spikes in the job finding rate could theoretically emerge if income and leisure are strict substitutes. Boone and Ours (2012), however, explain that such an assumption is not realistic. Card, Chetty, et al. (2007) attribute part of the spike to measurement error, but according to Boone and Ours (2012, p. 415) this cannot be the full story. Last mentioned authors argue that the spike may be generated by the fact that UI recipients may prefer delaying the beginning of a job until unemployment benefits have expired.

duration at which the maximal impact should be attained. However, because job seekers discount the future, the difference in job search behaviour is expected to diminish closer to the onset of the unemployment spell.

Figure 4.7: Stylized Impact on the Job Finding Rate of an Extension of the Waiting Period



4.8 Appendix B: Reduction of Sample Size after Imposition of Selection Criteria

The initial population consists of 151,744 individuals. We make consecutively the following selections:

- Delete individuals with the onset of unemployment at a different date than the first registration: 197 individuals (0.13%); observations left: 151,547;
- Delete individuals entering unemployment in 2011, 2012 and 2013: 76,716 individuals (50.62%); observations left: 74,831;
- Delete individuals with at most a secondary education degree: 43,150 individuals (57.66%); observations left: 31,681;
- Delete individuals who have worked prior to the onset of the waiting period: 9,003 individuals (28.42%); observations left: 22,678;
- Delete individuals finding a job according to the PES, but not found to be employed in the corresponding quarter in the social security files: 432 individuals (1.90%); observations left: 22,246;
- Delete individuals who receive UI benefit before the end of the waiting period: 80 individuals (0.36%); observations left: 22,166;
- Retain individuals within an age window of 3 years around the age discontinuity at 26 years (1.5 years to the left and to the right): 16,479 individuals deleted to the left of the discontinuity (74.34%) and 192 individuals to the right of the discontinuity (3.38%); observations left: 5,495. The final sample for analysis consists of 5,495 individuals.

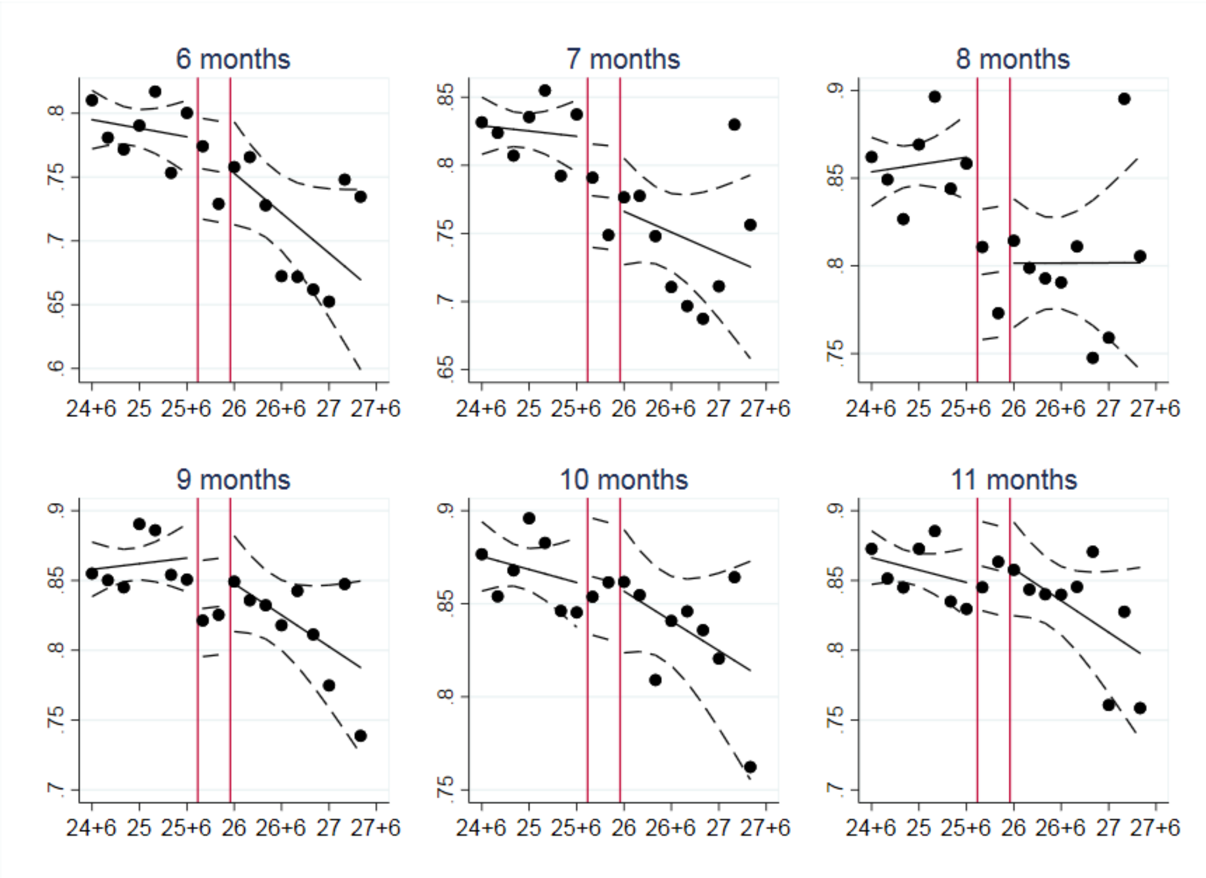
4.9 Appendix C: Linear Probability Model for the Probability of Not Being Unemployed x Months After Registration

Table 4.6: Estimation Results by Month for the Probability of Not Being Unemployed

	Not unemployed x months after registration												
	1 month	2 months	3 months	4 months	5 months	6 months	7 months	8 months	9 months	10 months	11 months	12 months	13 months
$\hat{\beta}_w$	-0.006	-0.019	0.014	0.004	-0.013	-0.016	-0.024	-0.027	0.003	-0.007	0.004	-0.025	-0.033
	(0.023)	(0.031)	(0.031)	(0.030)	(0.029)	(0.028)	(0.027)	(0.025)	(0.024)	(0.023)	(0.023)	(0.024)	(0.023)
$\hat{\beta}_{ywp}$	0.003	0.004	0.014	0.022	-0.000	0.032	0.036	0.046**	0.033*	0.022	0.006	-0.010	-0.003
	(0.019)	(0.026)	(0.026)	(0.025)	(0.024)	(0.023)	(0.022)	(0.021)	(0.020)	(0.019)	(0.019)	(0.020)	(0.019)
N	5,507	5,507	5,507	5,507	5,507	5,507	5,507	5,507	5,507	5,507	5,507	5,507	5,507
R^2	0.046	0.037	0.050	0.050	0.038	0.042	0.041	0.035	0.037	0.038	0.033	0.019	0.019

Note: Heteroskedastic robust standard errors between parentheses. All models include control variables mentioned in Table 4.1 (apart from the equivalent household income) and a linear spline in age. * p-value less than 10%, ** p-value less than 5%, *** p-value less than 1%.

Figure 4.8: Estimation Results by Month for the Probability of Not Being Unemployed



Note. Age bins are grouped by two months. The indicators are defined as explained in Section 4.5.

4.10 Appendix D: Complete Estimation Results for the Benchmark Outcome

Unemployment duration is the benchmark outcome for our analysis. In this appendix we report the full estimation results (except for the region dummies and the year and monthly entry dummies) for both the linear regression model defined by Equation 4.1 and the hazard model defined by Equation 4.2. The full estimation results for the other outcomes can be obtained from the authors upon request.

Table 4.7: Estimation Results for the Benchmark Outcomes

Coef.	**(1)**	**(2)**	**Exp. Coef.**	**(3)**	**(4)**	**(5)**	**(6)**
	Linear	**Log-linear**		**Hazard**	**Hazard**	**Hazard**	**Hazard**
$\hat{\beta}_w$	-0.126	-0.108	$exp(\beta_w)$	1.040	-	1.046	1.046
	(0.283)	(0.049)		(0.083)		(0.084)	(0.084)
	-	-	$exp(\beta_{w2-9})$	-	1.044	-	-
					(0.084)		
	-	-	$exp(\beta_{w10-12})$	-	0.829	-	-
					(0.154)		
	-	-	$exp(\beta_{w13-\infty})$	-	1.267	-	-
					(0.234)		
$\hat{\beta}_{ywp}$	0.122	0.003	$exp(\beta_{ywp})$	0.960	0.959	0.952	-
	(0.243)	(0.041)		(0.064)	(0.063)	(0.063)	
	-	-	$exp(\beta_{ywp2-7})$	-	-	-	0.928
							(0.062)
	-	-	$exp(\beta_{ywp8-10})$	-	-	-	1.385***
							(0.174)
	-	-	$exp(\beta_{ywp11-\infty})$	-	-	-	0.891
							(0.130)
Age	0.001**	0.001		1.000**	1.000**	1.000**	1.000**
	(0.001)	(0.001)		(0.001)	(0.001)	(0.001)	(0.001)
Age*$\hat{\beta}_w$	0.002	0.001		1.000	1.000	1.000	1.000
	(0.001)	(0.001)		(0.001)	(0.001)	(0.001)	(0.001)
Female	-0.737***	-0.152***		1.323***	1.322***	1.321***	1.321***
	(0.106)	(0.020)		(0.047)	(0.047)	(0.047)	(0.047)
Dutch	-0.789**	-0.184***		1.365***	1.366***	1.520***	1.520***
	(0.289)	(0.052)		(0.116)	(0.116)	(0.135)	(0.135)
Bel	-1.126*	-0.176*		1.344**	1.343**	1.346**	1.346**
	(0.580)	(0.091)		(0.181)	(0.180)	(0.185)	(0.185)
Driver's license	-1.751	-0.035		1.163	1.166	1.216	1.216
	(1.708)	(0.190)		(0.286)	(0.286)	(0.305)	(0.305)
Master	-0.097	0.029		0.977	0.977	0.950	0.950
	(0.132)	(0.024)		(0.039)	(0.039)	(0.038)	(0.038)
Family status							
- family	1.966*	0.291		0.509**	0.510**	0.423***	0.423***
	(1.066)	(0.198)		(0.140)	(0.140)	(0.124)	(0.124)
- single	0.406	0.066		0.878	0.879	0.816*	0.816*
	(0.285)	(0.057)		(0.093)	(0.093)	(0.087)	(0.087)
- children	1.041***	0.237***		0.634**	0.635***	0.620***	0.620***
	(0.128)	(0.032)		(0.040)	(0.040)	(0.039)	(0.039)
- other	0.437	0.072		0.820*	0.822*	0.800**	0.800**
	(0.324)	(0.061)		(0.090)	(0.090)	(0.088)	(0.088)
Region dummies	YES	YES		YES	YES	YES	YES
Entry dummies	YES	YES		YES	YES	YES	YES
			λ_{2-3}	2.133***	2.124***		
				(0.119)	(0.118)		

			λ_{4-5}	2.166***	2.148***		
				(0.214)	(0.212)		
			λ_{6-8}	2.482***	2.454***		
				(0.328)	(0.324)		
			λ_{9-11}	2.327***	2.419***		
				(0.393)	(0.422)		
			λ_{12+}	1.933**	1.791**		
				(0.419)	(0.406)		
			λ_{2-4}			2.077***	2.072***
						(0.109)	(0.109)
			λ_{5-7}			2.117***	2.105***
						(0.197)	(0.196)
			λ_{8-10}			2.051***	1.653***
						(0.273)	(0.241)
			λ_{11+}			1.637***	1.645***
						(0.295)	(0.305)
cst	7.027***	1.287***		0.120***	0.120***	0.103***	0.104***
	(1.847)	(0.219)		(0.036)	(0.036)	(0.031)	(0.031)
			Variance	0.588***	0.581***	0.547***	0.542***
			heterogeneity	(0.076)	(0.076)	(0.073)	(0.073)
N	5,483	5,483	N	5,495	5,495	5,495	5,495
R^2	0.043	0.047	log-likelihood	-11884.9	-11883.2	-11341.1	-11334.6

Note. Heteroskedastic robust standard errors between parentheses. All models include the control variables mentioned in Table 4.1 (except for the equivalent household income) and a linear spline in age. In the (log-) linear models right-censored observations are dropped: 12 observations in case of exits to any destination reported in the first two columns; an additional 396 individuals who leave from unemployment to inactivity in case duration until exit to employment is considered. In the hazard models the aforementioned dropped observations are right censored. “Variance heterogeneity” is the variance of the Normal mixing distribution of the unobserved heterogeneity. * p-value less than 10%, ** p-value less than 5%, *** p-value less than 1%.

4.11 Appendix E: Sensitivity Analysis for Days Worked and Annual Earnings

Table 4.8: Estimation Results for Days Worked and Annual Earnings

	Days worked				**Annual earnings**			
Coef.	(1) Age window	(2) Spline YWP	(3) No spline	(4) Quad. spline	(5) Age window	(6) Spline YWP	(7) No spline	(8) Quad. spline
$\hat{\beta}_w$	-0.054	0.016	-0.015	-0.045	-0.069	-0.050	-0.035	-0.070
	(0.054)	(0.061)	(0.043)	(0.047)	(0.072)	(0.085)	(0.057)	(0.064)
$\hat{\beta}_{ywp}$	-0.069	-0.057	-0.058	-0.063	-0.048	-0.080	-0.044	-0.049
	(0.048)	(0.054)	(0.038)	(0.038)	(0.064)	(0.069)	(0.050)	(0.049)
N	2,832	3,213	4,785	4,785	2,762	3,156	4,689	4,689
R^2	0.023	0.021	0.024	0.024	0.035	0.041	0.036	0.037

Note. Heteroskedastic robust standard errors between parentheses. All models include control variables mentioned in Table 4.1 (apart from the equivalent household income) and a linear spline in age. All reported results are based on log-linear regressions of the associated indicator of employment quality as specified in equation 4.1. In columns (1) and (5) the specification is as in the benchmark, but the age window is narrowed down to 1 year to the left and right of the discontinuity at 26 years; in columns (2) and (6) the spline is set at the 25 years and 8 months (i.e. the discontinuity of the YWP) and the effect of the waiting period is captured by a dummy variable at 26 years; in columns (3) and (7) the specification is linear in age without any spline; in columns (4) and (8) the specification is quadratic in age without any spline. * p-value less than 10%, ** p-value less than 5%, *** p-value less than 1%.

4.12 Appendix F: Additional Analyses for Days Worked

In order to get a fuller picture of the pattern of the number of working days after exit out of unemployment we consider two distinct outcomes: the (i) the days worked in the quarter of exit and the subsequent quarter and (ii) the days worked in the next 3 quarters. These results are portrayed in Column (1) and Column (2) of the table below. Column (3) shows the result of an extra sensitivity test where we include all individuals, regardless of whether or not they have left unemployment. We include the number of days worked in the quarter of entry into unemployment and the four subsequent quarters. If an individual did not work, this value was set to zero. In this way we test whether the effect we found on days worked is not the result of a composition effect in the exit to employment.

Table 4.9: Estimation Results Days Worked (Additional Analyses)

	Days worked		
	(1)	(2)	(3)
	Early interval	Later interval	Sensitivity test
Coef.	Log-lin.	Log-lin.	Lin.
$\hat{\beta}_w$	-0.0879*	0.00582	-10.462*
	(0.0478)	(0.0432)	(6.322)
$\hat{\beta}_{ywp}$	-0.0701*	-0.0392	-11.072**
	(0.0382)	(0.0359)	(5.247)
N	4,783	4,626	5,495
R^2	0.013	0.023	0.023

Note. Heteroskedastic robust standard errors between parentheses. All models include control variables mentioned in Table 4.1 (apart from the equivalent household income) and a linear spline in age. All reported results are based on log-linear regressions of the days worked as specified in equation 4.1. * p-value less than 10%, ** p-value less than 5%, *** p-value less than 1%.

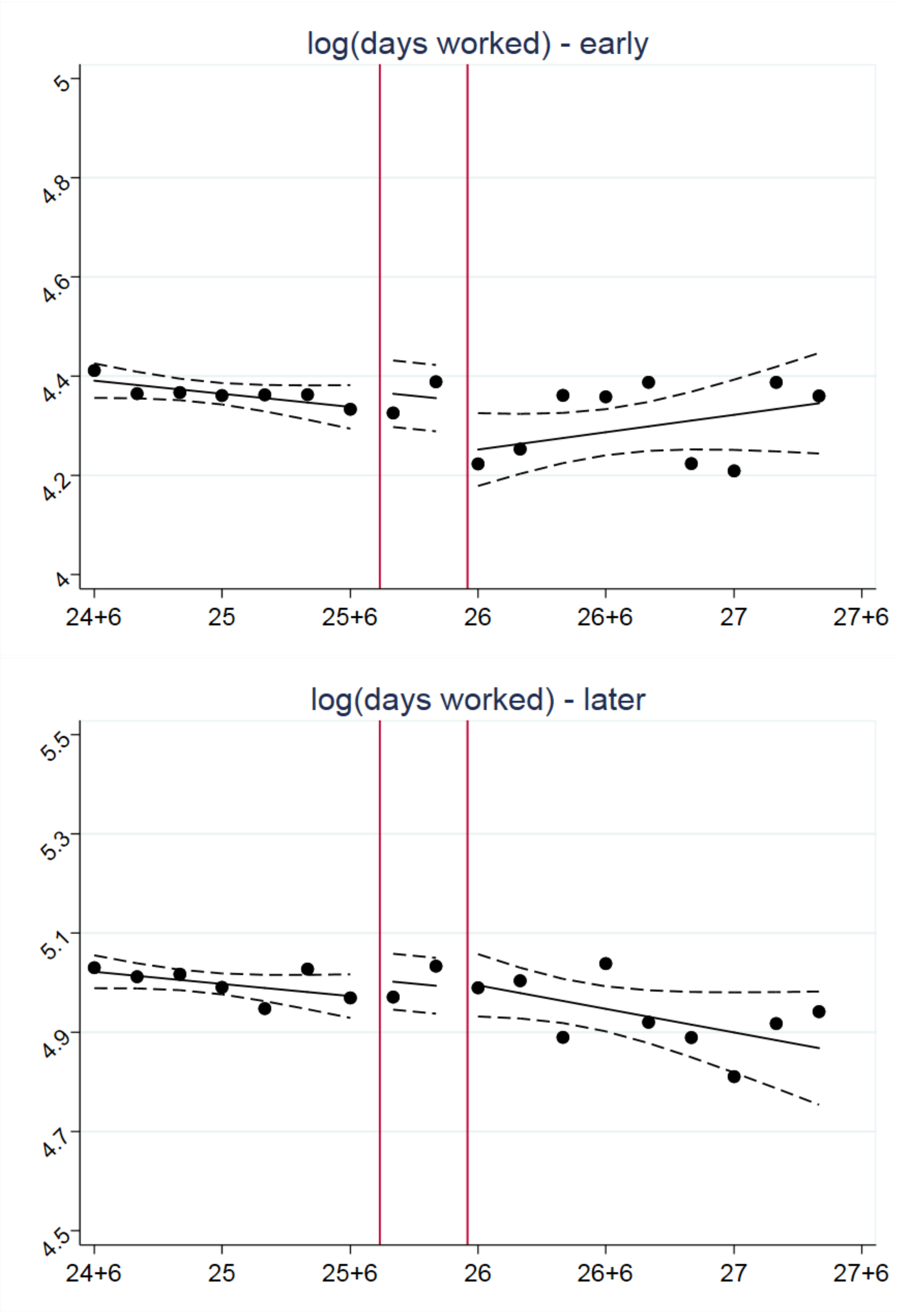
Finally, we re-estimate the model as in Column (1) and (2) of Table 4.9 for the sample divided by the equivalent household income. As is clear from the paper the significant decrease in the number of working days at the beginning of the employment spell is only prevalent for youth coming from low income households.

Table 4.10: Additional Heterogeneity Analyses for Days Worked

	Days worked - early		**Days worked - late**	
Coef.	Income ≤ median	Income > median	Income ≤ median	Income > median
$\hat{\beta}_w$	-0.116*	-0.075	-0.015	0.012
	(0.067)	(0.071)	(0.066)	(0.058)
$\hat{\beta}_{ywp}$	-0.128**	-0.030	-0.022	-0.071
	(0.052)	(0.058)	(0.056)	(0.046)
N	2,36	2,357	2,276	2,29
R^2	0.018	0.012	0.034	0.023

Note. Heteroskedastic robust standard errors between parentheses. All models include control variables mentioned in Table 4.1 (apart from the equivalent household income) and a linear spline in age. All reported results are based on log-linear regressions of the days worked as specified in equation 4.1. * p-value less than 10%, ** p-value less than 5%, *** p-value less than 1%.

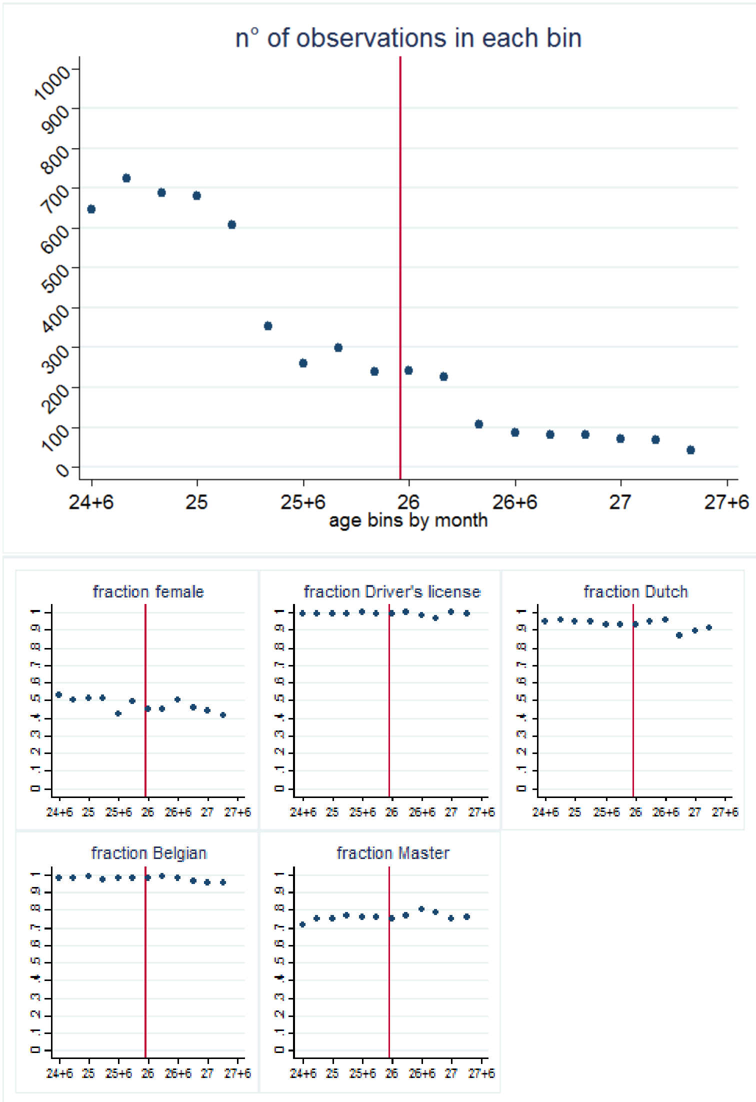
Figure 4.9: Days Worked (Ealry and Later Interval)



Note. Age bins are grouped by two months. The indicators are defined as explained in Section 4.3.2.

4.13 Appendix G: Graphical Tests to Detect Manipulation of the Forcing Variable

Figure 4.10: Manipulation Tests



Note. Age bins are grouped by two months. The manipulation tests are defined in Section 4.5.4.

4.14 Appendix H: Placebo Test on First Registrations at the PES in 2012

Table 4.11: Estimation Results for the Placebo Test

	Duration in unemployment						
	Any exit		Exit to employment				
Coef.	(1) Linear	(2) Log-linear	(3) Linear	(4) Log-linear	Exp. Coef.	(5) Hazard	(6) Hazard
$\hat{\beta}_w$	-0.058 (0.424)	0.013 (0.097)	0.220 (0.426)	0.070 (0.100)	$exp(\beta_w)$	1.060 (0.191)	-
	-	-	-	-	$exp(\beta_{w2-9})$	-	1.040 (0.188)
	-	-	-	-	$exp(\beta_{w10-12})$	-	1.076 (0.324)
	-	-	-	-	$exp(\beta_{w13-\infty})$	-	1.679 (0.569)
$\hat{\beta}_{ywp}$	-0.589 (0.389)	-0.182 (0.091)	-0.366 (0.394)	-0.130 (0.095)	-	-	-
					Variance heterogeneity	0.807*** (0.172)	0.798** (0.174)
N	1,826	1,826	1,676	1,676	N	1,950	1,950
R^2	0.055	0.069	0.052	0.068	Log-likelihood	-4208.10	-4206.90

	(7) Days worked	**(8) Dummy parttime**	**(9) Daily wage**	**(10) Daily wage (cor.)**	**(6) Annual earnings**
Coef.	Exit to employment				
$\hat{\beta}_w$	0.163 (0.106)	0.028 (0.041)	0.018 (0.027)	0.035 (0.027)	0.074 (0.134)
$\hat{\beta}_{ywp}$	0.064 (0.098)	-0.039 (0.038)	0.034 (0.025)	0.031 (0.024)	0.052 (0.126)
N	1,585	1,585	1,451	1,515	1,546
R^2	0.059	0.065	0.091	0.081	0.077

Note. Heteroskedastic robust standard errors between parentheses. All models include the control variables mentioned in Table 4.1 (except for the equivalent household income) and a linear spline in age. In the (log-) linear models right-censored observations are dropped: 12 observations in case of exits to any destination reported in the first two columns; an additional 396 individuals who leave from unemployment to inactivity in case duration until exit to employment is considered. In the hazard models the aforementioned dropped observations are right censored. “Variance heterogeneity” is the variance of the Normal mixing distribution of the unobserved heterogeneity. * p-value less than 10%, ** p-value less than 5%, *** p-value less than 1%.

Chapter 5

The Signal of Applying for a Job Under a Vacancy Referral Scheme[1]

5.1 Introduction

In order to alter the trend of persistent unemployment over recent decades, the majority of OECD countries have invested vast amounts of public funds in active labour market policies (ALMPs) (J. P. Martin, 2014; J. Martin and Grubb, 2001). These investments have logically resulted in a surge in micro-econometric research evaluating the effectiveness of these policy instruments (Card et al., 2010, 2017; Greenberg et al., 2003; Heckman et al., 1999; Kluve, 2010; Liechti et al., 2017). The results of these evaluations are mixed at best. Overall, the effectiveness of ALMPs in terms of exit out of unemployment depends largely on the type of ALMP, its target group and the time horizon of the evaluation (Card et al., 2010, 2017; Kluve, 2010; J. Martin and Grubb, 2001). Moreover, the few studies that have taken the costs of these programmes into account indicate that the benefits of ALMPs do not outweigh their costs (Albanese et al., 2016; Card et al., 2017; Crépon et al., 2013; Jespersen et al., 2008). The ALMP central to this study, a job-vacancy referral scheme, exhibits the same mixed effectiveness. While some studies find positive results of this kind of programme with respect to exit out of unemployment (Bollens and Cockx, 2017; Fougère et al., 2009; Van den Berg and Vikström, 2014), others find no impact at

[1] In collaboration with Stijn Baert, Ralf Caers, Marijke De Couck, and Valentina Di Stasio.

all (Engström et al., 2012; Van den Berg and Van der Klaauw, 2006). Moreover, some of the studies that present positive effects of job-vacancy referrals show that the higher job-finding rate as a result of referral goes hand in hand with a lower job quality (Van den Berg, Hofmann, et al., 2016; Van den Berg and Vikström, 2014).

Measuring the effectiveness of these policies in terms of their desired outcomes is in no doubt important. However, if the aim is to reform ALMPs to increase their effectiveness, we need to go beyond measuring and look at possible explanations for the unsatisfactory effectiveness. One possible explanation documented in the literature is the signal associated with ALMPs. Signalling theory states that when individuals are faced with limited information, they will use particular components of this information to predict unobserved factors (Arrow et al., 1973; Blanchard and Diamond, 1994; Moscarini, 1997; Spence, 1978; Vishwanath, 1989). In this respect, we can expect a positive as well as a negative signal sent to prospective employers by a job-vacancy referral.[2] On the one hand, candidates applying for a position at the request of a public employment service (PES) have gone through an initial screening process and have been deemed suitable for the position by the PES. In other words, the vacancy referral could be seen as a signal of improved suitability (Bellis et al., 2011). On the other hand, employers could see referred applicants as candidates who do not have the ability to succeed in the labour market on their own and/or only apply to comply with benefit rules (Bellis et al., 2011; Bonoli and Hinrichs, 2012; Ingold and Stuart, 2015). As a consequence, being referred to a vacancy has been theoretically related to lower intellectual and/or social abilities (Bellis et al., 2011; Ingold and Stuart, 2015), lower trainability (Thurow, 1975), negative evaluation by previous employers (Banerjee, 1992; Bikhchandani et al., 1992) and, most commonly cited, lower motivation (Bonoli and Hinrichs, 2012; Ingold and Stuart, 2015).

The existing empirical literature on the signal of ALMPs is limited and mainly of a qualitative nature (Bellis et al., 2011; Bonoli and Hinrichs, 2012; Ingold and Stuart, 2015; Liechti et al., 2017). Little or no causal evidence has been presented for signalling as an explanation for the limited effectiveness of these policies. Falk et al. (2005) and Liechti

[2] Throughout the present article, we assume that employers are aware if a candidate has been referred to a vacancy. As explained in Subsection 5.2.1, this is a realistic assumption in the case of the referral scheme central to our study.

et al. (2017) are notable exceptions. By means of a field experiment, Falk et al. (2005) measure how completing a computer training programme impacts job-finding rates. More concretely, they compare the call-back rates for job candidates with and without this kind of training. They find that completing a computer training programme does not yield higher call-back rates. Liechti et al. (2017) quantify the signalling value of different types of ALMPs, including training programmes and subsidised employment, by means of a factorial survey experiment. They find that employers do take ALMP participation into account when making hiring decisions, but the signalling effect of this participation can be both positive as well as negative, depending on the potential candidate's distance from the labour market.

In this study, we investigate the signalling value of a distinct type of ALMP: a job-vacancy referral. To this aim, we conduct a state-of-the-art vignette experiment with human resource (HR) professionals. We ask these participants to make fictitious hiring decisions concerning job candidates described on vignettes. Half of these candidates are indicated as applying under a job-vacancy referral scheme. Besides being rated in terms of hireability, these fictitious candidates are evaluated on statements related to the five potential signals sent by applying in the context of a referral as listed above (motivation, intellectual abilities, social abilities, trainability, and previous unfavourable evaluation by other employers). The data collected by means of this experiment allow us to answer three research questions.

R1: Does applying for a job under a job-vacancy referral scheme yield lower hiring chances?

R2: Is the signalling effect of applying for a job under a vacancy referral scheme heterogeneous by candidate and participant characteristics?

R3: Which particular signals are sent by applying for a job under a vacancy referral scheme?

The present study complements the work by Falk et al. (2005) and Liechti et al. (2017) by quantifying the signalling value of a different type of ALMP. In addition, by means of answering R3, we contribute to this literature by being the first to investigate which signal(s) is (are) particularly sent by participation in ALMPs in general and applying under a vacancy referral scheme in particular. This is of particular policy relevance because it shows which prejudices against the unemployed applying for jobs under a job-vacancy referral scheme should be compensated for.

The remainder of the study is structured as follows. Section 5.2 describes the institutional setting, our experimental design, and the realised data collection. Our research questions are answered in Section 5.3, where results from analysis of the experimental data are presented. Section 5.4 concludes, focussing on both the academic and policy implications of our research.

5.2 Experiment

To answer R1, R2, and R3, we conducted a vignette experiment. In this kind of experiment, participants are asked to judge fictitious descriptions (presented on vignettes) that differ on a pre-defined number of variables (the vignette factors), which are randomly assigned a value (the vignette-levels; Auspurg and Hinz, 2014; Jasso, 2006; Rossi and Nock, 1982; R. M. Sauer, 2015). As a consequence of these design features, correlation between the vignette factors are minimised to a value close to 0 (Rossi and Nock, 1982), a situation which rarely occurs outside an experimental setting. The biggest advantage of vignette experiments is, therefore, that they enable scholars to give a causal interpretation to the measured effects of the included vignette factors on human decisions (Damelang and Abraham, 2016; Wallander, 2009). Moreover, as opposed to field experiments, vignette experiments have the added benefit of creating the opportunity to ask additional questions to unravel the thinking process behind certain decisions and, consequently, to shed light on why we observe certain phenomena.

Vignette experiments have recently been the method of choice for a number of prominent studies in sociology and economics investigating human judgement (Ambuehl and Ockenfels, 2017; Auspurg, Hinz, and C. Sauer, 2017; Eriksson and Rooth, 2014; Mathew, 2017; Rivera and Tilcsik, 2016). In particular, this type of experiment has been increasingly used to study dynamics in hiring decisions (Auer et al., 2018; Damelang and Abraham, 2016; Di Stasio, 2014; Di Stasio and Gërxhani, 2015; Liechti et al., 2017; McDonald, 2017; Van Belle et al., 2017; Van Hoye and Lievens, 2003). In this application of the vignette experimentation framework, participants have to judge fictitious candidates with divergent characteristics as vignette factors. As a consequence, these vignette experiments closely mimic real-life hiring situations, where HR professionals also take a number of characteristics into account when making hiring decisions. In our application, we included as a vignette factor whether job candidates applied to this vacancy under a job-vacancy referral system or on their own initiative. Exogenous variation in this factor would be hard to find in observational data.

The next subsections describe the specific institutional framework under study, the design of our vignettes, and the data gathering process. We return to some potential limitations of our experimental design in Section 5.4.

5.2.1 Institutional Framework

Our study focusses on a job-vacancy referral scheme implemented by the PES of Flanders, the northern part of Belgium. In essence, this scheme matches open vacancies with jobseekers and, subsequently, forces jobseekers to apply for the vacancies matched to them. There are two types of possible referrals. In the case of a classic referral, a case worker, possibly aided by matching software, matches an open vacancy with an unemployed benefit recipient, who is then obliged to apply for it. In the alternative type of referral, the caseworker invites the unemployed person for a meeting, during which they go over a set of potentially relevant vacancies. If the unemployed person and the caseworker agree that a particular vacancy suits the person's profile, (s)he is obliged to apply. In both cases, non-compliance with the referral may result in a reduction or loss of benefits. In the context of monitoring this compliance, the PES informs the employer about the referral. This happens at, or soon after, the time of referral.[3] So, in principle, the employer is aware that a candidate is referred by the PES prior to this candidate's actual job application.[4]

Bollens and Cockx (2017) used a timing-of-events approach to investigate the effectiveness of this job-vacancy referral scheme in terms of entry to employment. They found

[3] More concretely, this happens automatically via a software program for employers with a profile in the PES database and manually—but very soon after the matching is done—for employers without such a profile.

[4] The information on the referral procedure was given to us by caseworkers of the PES in Flanders. A transcript of this information is available upon request.

substantial positive treatment effects both during the month of the referral and over the course of the following months. However, the positive total effect of the referral measured by Bollens and Cockx (2017) combines supply-side and demand-side effects of the programme so that it does not rule out a negative signalling effect of this job-vacancy referral scheme. Moreover, at the time of the evaluation performed by Bollens and Cockx (2017), employers were informed about the referral in approximately only 25% of the cases, so that demand-side dynamics might have been less influential then.

5.2.2 Vignette Design

We asked a sample of HR professionals described in the subsection below to evaluate a set of five vignettes, each describing one potential fictitious candidate for an open vacancy. These job applicants differed in the six vignette factors defined in Table 5.1. The main factor of interest was the one indicating whether or not candidates were referred to the position by the PES.[5] More precisely, and in line with reality, candidate summaries could either mention that the candidate applied for the position directly or was referred by the PES. In the latter case, it was mentioned explicitly that this entailed that the candidate was obliged to apply for this suitable vacancy. Besides this factor, applicants differed in gender (male or female), educational attainment (secondary education certificate or bachelor's degree), previous work experience (two or five years), whether they mentioned social activities (none or volunteering activities), and unemployment duration prior to the application (1 to 36 months). These vignette factors were chosen on the basis of a literature review of drivers of job-application success (Damelang and Abraham, 2016; Di Stasio, 2014; Di Stasio and Gërxhani, 2015; Liechti et al., 2017; Van Belle et al., 2017) and an interview with three HR professionals.[6] In addition, we ran a pilot study with 30 master's students in Economics to test whether our vignettes were perceived as plausible and that no crucial information was omitted.

After fully crossing all vignette levels for the six mentioned factors, we obtained a vignette universe of 1,152 (i.e. $2 \times 2 \times 2 \times 2 \times 2 \times 36$) vignettes. We used a D-efficient randomi-

[5] As argued in Subsection 5.2.1, it is highly likely that a Flemish employer is aware that a candidate is referred to a position by the PES so that this is a realistic vignette factor.

[6] Transcripts of these interviews are available upon request.

Table 5.1: Vignette factors and levels

Vignette factors	Vignette levels
Gender	{Male, Female}
Highest obtained educational certification	{Secondary education, Bachelor's degree}
Previous work experience	{Two years' experience, Five years' experience}
Mentioned social activities	{None, Volunteering}
Unemployment duration	{1 month, 2 months, . . . , 36 months}
Referral	{Referred by the PES, Direct application}

Note. PES stands for the Public Employment Service (of Flanders). The factorial product of the vignette levels (2 × 2 × 2 × 2 × 36 × 2) resulted in 1,152 possible combinations. Sets of five vignettes were drawn from this vignette universe using a D-efficient design (D-efficiency: 99.809; Auspurg & Hinz, 2014) and distributed at random to the participants as described in Subsection 5.2.2. This guaranteed that the vignette factors were nearly orthogonal, as shown in Table 5.5.

sation to minimise correlation between the different vignette factors. More concretely, following the algorithm in Auspurg and Hinz (2014), we selected 60 sets of 5 vignettes, allowing us to achieve a D-efficiency of 99.809.[7] Each participant was randomly assigned one of these 60 sets. The resulting correlations between our vignette factors can be found in Table 5.5 (in Appendix A) and show that our randomisation approach was successful.

5.2.3 Data Collection

We conducted our experiment in January 2017 with Flemish recruiters. The experiment was part of a large-scale survey performed with individuals that selected themselves in a database of volunteers for participation in research on human resource management. At the start of this survey, the participants were asked whether they had been actively involved in the hiring process a minimum of five times during the past year. Participants who answered this question in a positive way had a 50% probability of being assigned to

[7] This design maximises both orthogonality and level balance, thereby enhancing statistical precision (Auspurg and Hinz, 2014). Formally, this approach maximises the D-efficiency, which is given by the following formula:,where is the vector of the vignette variables, indicates the number of vignettes in the sample and presents the number of regression coefficients (including the intercept) in the analysis of the experimental data. For more information, we refer to (Auspurg and Hinz, 2014).

our experiment (and a 50% probability of being assigned to the experiment of (Van Belle et al., 2017)). In total, 234 recruiters took part in our experiment. Of these respondents, 29 left one or more questions unanswered, leaving us with a total sample of 205 participants. Each participant rated five vignettes, yielding a total of 1,025 at the participant-vignette level.[8]

As a first step, each participant received experimental instructions. They were introduced to their role as a recruiter for a fictitious company supplying building materials. In this role, they were responsible for filling an open vacancy for a new counter assistant—we selected this occupation because it is common in a number of industries, thus increasing the probability that participants would be familiar with it. Participants were informed that the successful candidate had to be (i) customer orientated, (ii) service minded, and (iii) commercially orientated. Moreover, the company was looking for someone able to perform administrative tasks in an efficient and reliable way. No specific education or work experience was required. Subsequently, each participant was shown five vignettes (as described in the previous section). It was stressed that all five candidates had passed an initial screening by an administrative staff member. In line with the literature, the applicants' characteristics were presented in a tabulated way. Participants, though aware of participating in an experiment, were not informed of the goal or the topic of the experiment, which was framed in rather general terms.

With this information in hand, participants were asked to reveal, for each candidate, their hiring intentions. They were specifically asked to rate the statements 'The probability that I will invite this candidate for a job interview is high' and 'The probability that I will hire this candidate for the position is high' on a 7-point Likert scale. In the remainder of this article, these scales will be referred to as the 'interview scale' and the 'hiring scale', respectively.

In addition, in view of answering R3, participants were prompted to rate five statements (also on a 7-point Likert scale) linked to the different signals that could be sent by a job-vacancy referral prevalent in the literature and as described in Section 5.1. So, perceptions

[8] (Liechti et al., 2017) argue that the response rate in this type of experiment is of lesser importance as long as each profile is judged by multiple participants. This is the case here, as each profile was judged by an average of 3.417 participants (i.e. 205 [respondents] divided by 60 [sets of profiles]).

concerning the candidates' (i) motivation, (ii) intellectual abilities, (iii) social abilities, (iv) trainability, and (v) previous unfavourable evaluation by other employers were tested. The statements used can be found in Table 5.6. We limited the experimental survey to one statement per signal to keep the overall time taken up by the experiment within a reasonable limit (taking into account that each respondent was asked to review multiple profiles).

Finally, in view of answering R2, some personal characteristics of the respondents were surveyed, including their gender, age, nationality, educational attainment, frequency of hiring, and experience with the hiring process.

Panel A of Table 5.2 presents some summary statistics concerning the participants in our experiment. Participants had an average age of about 43 years, with most being Belgian nationals and having some form of tertiary education. They were slightly more likely to be female than male. A total of 46.8% of the respondents indicated that they recruited someone on at least a weekly basis, while 55.1% indicated having at least ten years' experience as a recruiter.

In columns (2) and (3) of Table 5.2 the research sample is split by the referral status of the judged job candidate. Column (4) presents the difference between these two columns as well as the results of a t-test to determine whether these differences are statistically significant. Given that each respondent judged five candidate profiles, our data are inherently nested. We control for this by clustering at the participant level. Overall, the information in Panel A of Table 5.2 allows us to conclude that referral status was successfully randomised over the participants.

5.3 Results

5.3.1 Does Applying for a Job Under a Job Referral Scheme Yield Lower Hiring Chances?

A first indication of the signalling effect of applying for a job under a job-vacancy referral scheme is given in Figure 5.1. On the left-hand side, the average rating on the interview

Table 5.2: Summary Statistics by Referral Status of the Candidate

	Mean			
	Full sample	Subsample Referral	Subsample: No referral	Difference: (3) - (2)
	N = 1,025	N = 521	N = 504	
	(1)	(2)	(3)	(4)
A. Participant characteristics				
Female gender	0.566	0.564	0.567	-0.003 [0.228]
Age	43.024	43.177	42.867	0.310 [0.966]
Foreign nationality	0.093	0.094	0.091	0.003 [0.345]
Highest obtained educational certification				
Secondary education or lower	0.093	0.100	0.085	0.014* [1.860]
Tertiary education: outside university	0.429	0.436	0.423	0.013 [0.950]
Tertiary education: university	0.478	0.464	0.492	-0.028** [1.993]
Frequency of hiring: weekly	0.468	0.468	0.468	0.001 [0.005]
Experience as HR professional: ≤ 10 years	0.551	0.559	0.544	0.015 [1.074]
B. Evaluation: interview and hiring decisions				
Interview scale	4.329	4.006	4.663	-0.657*** [6.548]
Hiring scale	3.739	3.422	4.065	-0.643*** [7.324]
C. Evaluation: perceived candidate traits				
Perceived motivation	4.115	3.635	4.611	-0.976*** [11.736]
Perceived intellectual abilities	4.774	4.678	4.873	-0.195*** [3.054]
Perceived social abilities	4.386	4.290	4.861	-0.196*** [3.173]
Perceived trainability	4.433	4.288	4.583	-0.295*** [4.649]
Perceived evaluation by other employers	3.629	3.582	3.679	-0.097 [1.207]

Note. More information concerning the scales mentioned in Panel B and Panel C can be found in Subsection 5.2.3. T-tests are performed to test whether the presented differences are significantly different from 0. Standard errors are corrected for clustering of the observations at the participant level. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level. T-statistics are between brackets.

and hiring scales by referral status is depicted. It is clear that candidates referred to the vacancy by the PES are both less likely to be invited for an interview (p = 0.000) and less likely to be hired (p = 0.000) for the position. The average rating on the interview (hiring) scale is 0.657 (0.643) lower for referred candidates than for direct applicants. In other words, given that the standard deviation of the interview (hiring) scale is 1.608 (1.414), being referred to the vacancy decreases the probability of an interview (getting hired) by 40.9% (45.5%) of a standard deviation. The same information can be inferred from Panel B of Table 5.2.

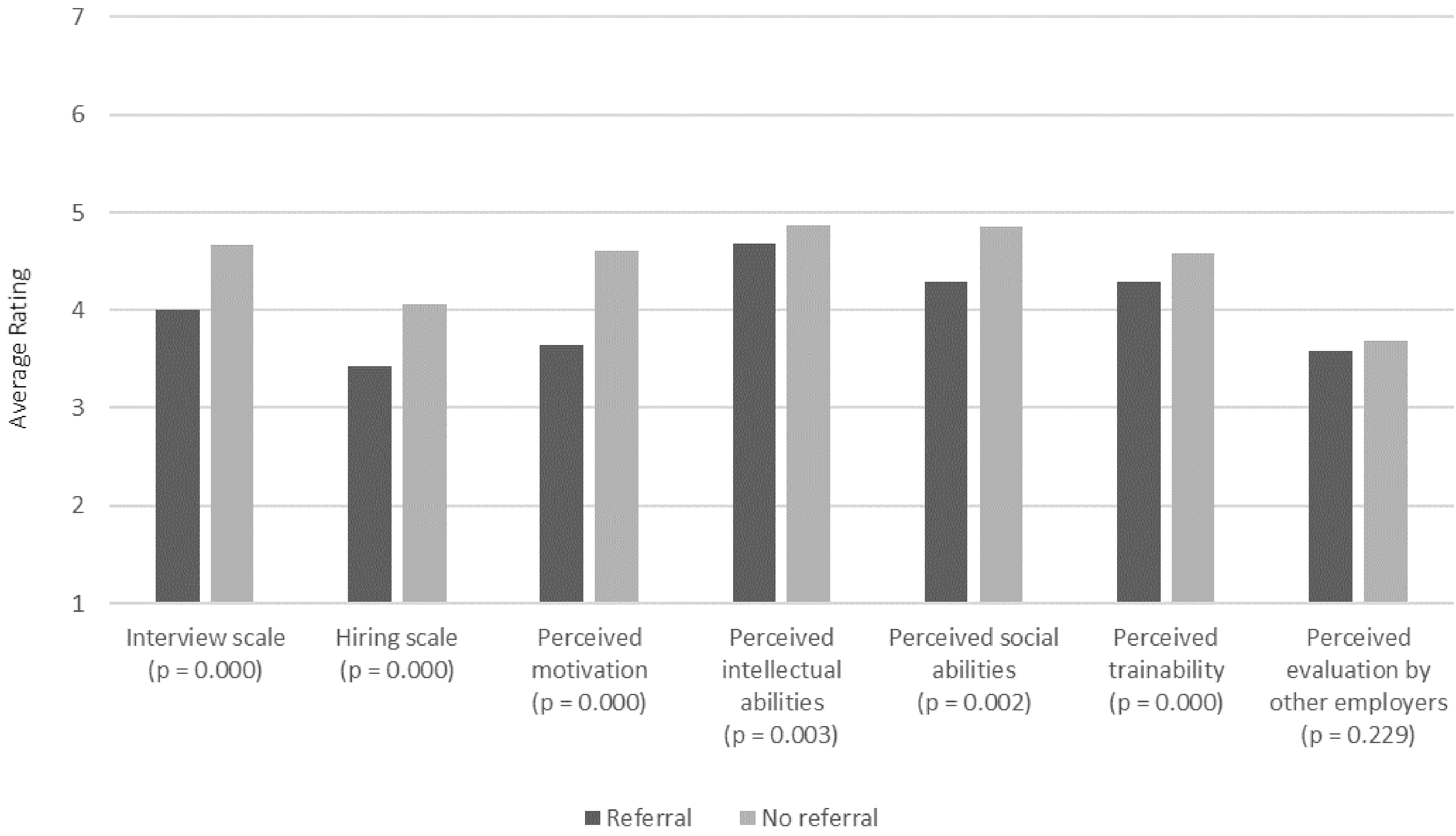
Due to our experimental set-up—the referral dummy is, by design, uncorrelated with any of the other observable candidate characteristics—these effects can be given a causal interpretation. In addition, due to the random allocation of vignette sets to participants, we do not expect any correlation with the participant characteristics. This is also confirmed when we estimate the following econometric model:

$$Y = \alpha + \beta CC + \gamma PC + \delta REF + \varepsilon \tag{5.1}$$

In this model, REF is the candidate's referral status (1 in case of referral, 0 in case of direct application); CC is the vector of the other vignette factors mentioned in Subsection 5.2.2; and PC is the vector of participant characteristics mentioned in the same subsection. The dependent variable of this model, Y, can be either the interview or hiring scale. β, γ, and δ are the (vectors of) parameters associated with CC, PC, and REF and α represents the intercept in this equation. Finally, standard error ε is corrected for clustering of the observations at the participant level.

The estimation results of this model are reported in column (1) of Table 5.3 and Table 5.7, with the interview and hiring scales as respective outcomes. Again, in Panel A of this table, we observe that candidates who are referred to the position by the PES are less likely to be invited for an interview—the related coefficient is substantial in both economic and statistical terms. This clearly indicates that being referred to a position by the PES has a negative signalling effect in respect of employers. This finding corroborates the qualitative evidence on the signalling effect of ALMP participation mentioned in Section 5.1 (Bellis et al., 2011; Bonoli and Hinrichs, 2012; Ingold and Stuart, 2015). Moreover, the negative

Figure 5.1: Differences in Average Ratings by Referral Status of the Candidate



Note. More information concerning the used scales can be found in Subsection 5.2.3. T-tests are performed to test whether the presented differences are significantly different from 0. Standard errors are corrected for clustering of the observations at the participant level.

signal of a job-vacancy referral could be part of the explanation for the unsatisfactory results of these programmes in terms of employment outcomes. Finally, our finding indicates that the positive effect of the Flemish referral system on employment outcomes, as found in (Bollens and Cockx, 2017), could be even larger if this adverse signalling effect could be reduced. We return to this point in Section 5.4.

Concerning the other vignette factors, the estimated coefficients all have the expected signs. While gender has no effect on the likelihood of being invited for an interview, having a bachelor's degree (as opposed to a secondary-level education) and having five (as opposed to two) years' professional experience clearly enhances one's chances of being invited, as does mentioning volunteering activities.[9] On the other hand, in line with what was found by Van Belle et al. (2017), the longer a candidate has been unemployed prior to the position, the lower her/his chances of being invited for an interview. If we turn to the coefficients related to the participant characteristics (in Panel B of Table 5.3), we see that female and older recruiters are more lenient in their judgement, while recruiters who are involved in the hiring process at least once a week tend to be stricter.

The corresponding estimation results when adopting the hiring scale as the outcome variable (Table 5.7, column (1)) are largely in line with the discussed results when using the interview scale as the dependent variable. However, the significant effect of recruiter gender disappears.

5.3.2 Is this Effect Heterogeneous by Candidate and Subject Characteristics?

To determine whether the signalling effect of the job-vacancy referral depends on other candidate characteristics, we re-estimate equation 5.1 including interaction terms between

[9] The fact that mentioning volunteering activities appears to have a larger effect on one's hiring chances than having completed a higher education programme or having three years of additional work experience might seem surprising. However, this result is in line with what is found by Baert and Vujić (in press), who show by means of a field experiment that job candidates mentioning volunteering receive one-third more interview invitations. Moreover, multiple studies have shown that volunteering activities appear to have a positive impact on earnings (Cozzi et al., 2013; Day and Devlin, 1997, 1998; Detollenaere et al., 2017; Hackl et al., 2007; Prouteau and Wolff, 2006; R. M. Sauer, 2015) and that this volunteer work, when done by ethnic minorities, may even cancel out ethnic discrimination in the labour market (Baert et al., 2016).

Table 5.3: Multivariate Analysis: Regression Analysis with Interview Scale as Outcome

Explanatory variables	(1)	(2)	(3)
A. Candidate characteristics			
Female gender	0.075 (0.074)	0.036 (0.117)	0.083 (0.074)
Bachelor's degree	0.344*** (0.083)	0.385*** (0.123)	0.356*** (0.083)
Five years' experience	0.265*** (0.080)	0.360*** (0.111)	0.264*** (0.079)
Unemployment duration	-0.053*** (0.004)	-0.057*** (0.005)	-0.053*** (0.004)
Volunteering	0.356*** (0.067)	0.388*** (0.108)	0.360*** (0.068)
Referral	-0.655*** (0.090)	-0.690*** (0.260)	-0.590*** (0.500)
Referral x Female gender		0.067 (0.179)	
Referral x Bachelor's degree		-0.096 (0.176)	
Referral x Five years' experience		-0.185 (0.187)	
Referral x Unemployment duration		0.009 (0.007)	
Referral x Volunteering		-0.048 (0.169)	
B. Participant characteristics			
Female gender	0.341** (0.161)	0.344** (0.161)	0.363** (0.167)
Age	0.027*** (0.009)	0.027*** (0.009)	0.018* (0.010)
Highets obtained educational certification			
Secondary education and lower	0.005 (0.269)	-0.002 (0.270)	-0.216 (0.319)
Tertiary education: outside university	0.074 (0.147)	0.067 (0.148)	0.119 (0.164)
Tertiary education: university (reference)			
Frequency of hiring: weekly	-0.393** (0.168)	-0.391** (0.168)	-0.460** (0.191)
Experience as HR professional: ≥ 10 years	-0.239 (0.177)	-0.240 (0.178)	-0.153 (0.198)
Referral x Female gender			-0.044 (0.177)
Referral x Age			0.017 (0.011)
Referral x Secondary education and lower			0.405 (0.305)
Referral x Tertiary education			-0.077 (0.187)
Referral x Frequency of hiring: weekly			0.120 (0.183)
Referral x Experience as a HR professional: ≥ 10 years			-0.182 (0.205)
Observations		1,025	

Note. The presented statistics are coefficient estimates and standard errors in parentheses for the regression model outlined in Subsection 5.3.2. Standard errors are corrected for clustering of the observations at the participant level. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level.

the referral dummy and each of the five other vignette factors. After including these interactions, the remaining coefficient of the referral dummy should be interpreted as the effect of a referral for a reference candidate, i.e. a male candidate with high school certification and two years' experience who has been unemployed for zero months and did not mention any social activities on his CV. The results of this exercise are reported in column (2) of Table 5.3 and Table 5.7. We find that the signalling value of the job-vacancy referral scheme is not moderated by any of the other candidate characteristics. In particular, it is interesting to note that the negative effect of a PES referral does not vary with the length of the unemployment spell. If we observed such an interaction effect, this could have been interpreted as suggestive evidence of employers assuming that the long-term unemployed simply apply via the PES to keep receiving benefits (without being intrinsically motivated to fill the vacancy).

Secondly, in column (3) of Table 5.3 and Table 5.7 we present the results of a similar analysis to study whether the effect of the job-vacancy referral is heterogeneous by type of recruiter. More concretely, compared to column (1), interaction terms between referral and the participant characteristics are adopted. In contrast to the candidate characteristics, the observed participant characteristics are not experimentally controlled and can, as a consequence, correlate with unobserved participant traits. Thus, the interaction effects presented in column (3) cannot be given a causal interpretation. However, we find that the effect of being referred to a vacancy is not heterogeneous by any of the participant characteristics.

5.3.3 Which Signals Are Sent by Applying for a Job Under a Job Referral Scheme?

In this subsection, we explore what exactly is signalled by the job-vacancy referral scheme, thereby examining why applying via a referral decreases one's hiring chances. The right-hand side of Figure 5.1 (and Panel C of Table 5.2) gives us a first indication of the empirical importance of the possible signals put forward in the literature and enumerated in Section 5.1. It is revealed that the candidates who apply for the open vacancy directly, without action by the PES, score better, on average, on all five statements related to these signals than those candidates who have been referred to the vacancy by the PES. So, referred candidates are, indeed, perceived as less motivated, less intellectually gifted, less socially gifted and less trainable, as well as having been rejected more often by other employers. These perceptions are statistically significant (at the 1% level), with the exception of the last one. In terms of economic significance, it is striking that the effect of referral on perceived motivation of the candidate is substantially higher than its effect on the other perception scales.

Finally, we estimate a mediation model, where these five potential signals related to referral are included jointly. This has two important advantages. Firstly, we can estimate the prominence of each signal independently. This is important as the signals potentially correlate with each other.[10] Secondly, in addition to looking at the impact of the referral on the signals, the model takes the importance of the signals in terms of invitation (or hiring) probability into account as well. This enables us to look at what part of the total effect of the referral on hiring is explained by each signal. The estimated model consists of the following system of equations (in line with Hayes (2013) and Van Belle et al. (2017)):

$$M_i = \alpha_i + \beta_i CC + \gamma_i PC + \delta_i REF + \varepsilon_i \tag{5.2}$$

$$Y = \alpha' + \beta' CC + \gamma' PC + \delta' REF + \theta M + \varepsilon' \tag{5.3}$$

In this system, CC, PC, REF, and Y are the same variables as those defined in the context of equation 5.1. In addition, M is the vector of mediators capturing perceived motivation, intellectual abilities, social abilities, trainability, and evaluation by other employers, respectively. β_i, γ_i, and δ_i are the (vectors of) parameters associated with CC, PC, and REF in the equations with M_i as a dependent variable, and α_i represents the intercept in these equations. β', γ', δ', and α' are the corresponding parameters in the equation with Y as a dependent variable. Finally, θ is the vector parameters associated with the mediating signals in the latter equation. As a consequence, δ' is the remaining direct effect of the referral after controlling for the five mediators. The products $\delta_i\theta_i$ are

[10] Indeed, these correlations range from r = 0.135 (between perceived social abilities and perceived evaluation by other employers) to r = 0.632 (between perceived social abilities and trainability).

the indirect effects of the referral on Y through each mediator M_i. In line with Hayes (2013), we estimate this system of six equations simultaneously and, as before, correct the standard errors ε_i and ε' for clustering of the observations at the participant level.

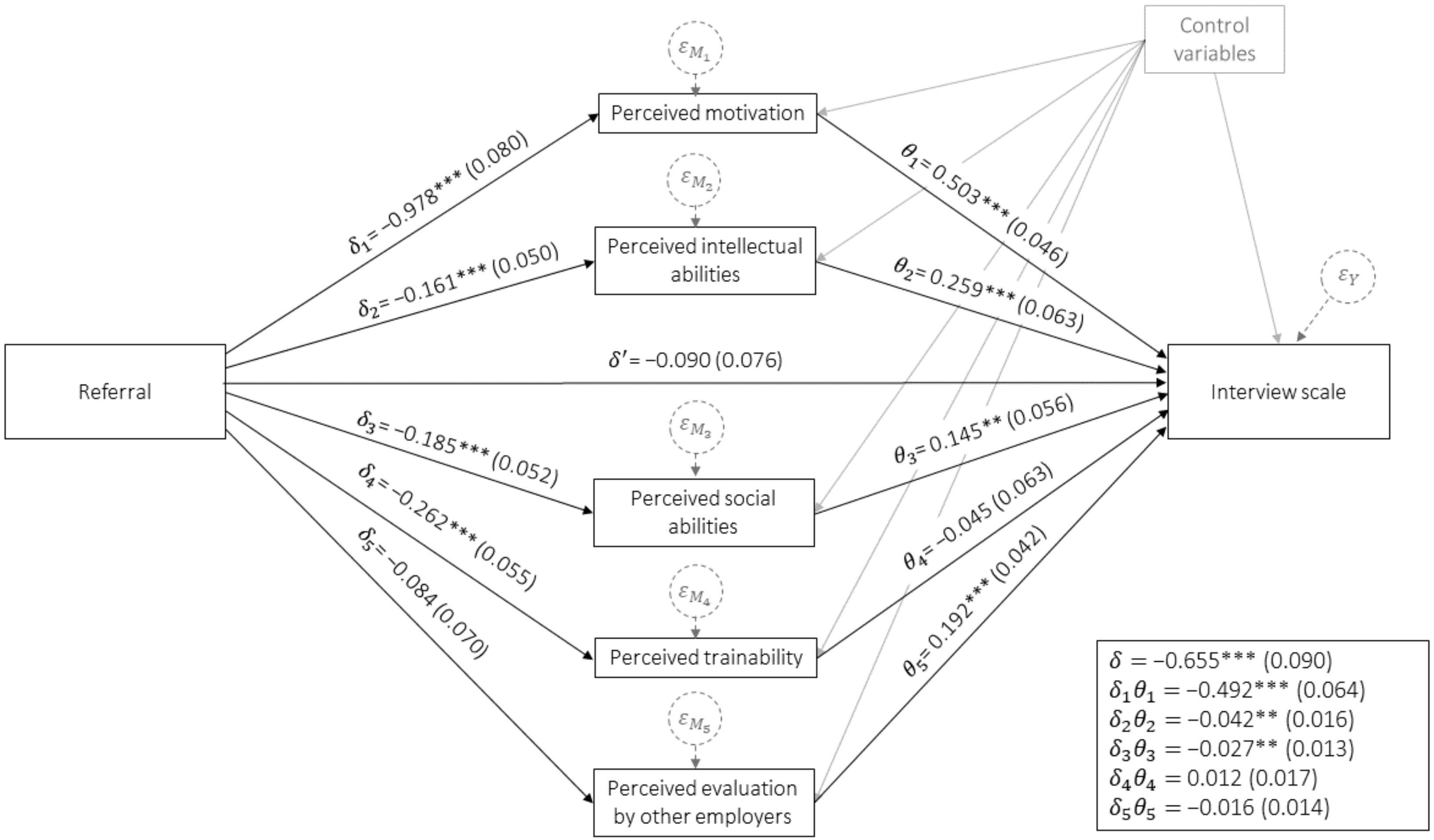
The main results of this analysis with the interview scale (hiring scale) as the outcome are reported in Figure 5.2 (Figure 5.3). The corresponding full estimation results are reported in Table 5.4 (Table 5.8). Just as was the case with the overall signalling effect discussed in Section 5.3.1, the effect of the referral on each of the five perceptions concerning the candidate (i.e. the δ_i in our model) can be interpreted as causal effects as a consequence of the random assignment of a referral to the vignettes. In contrast, the effect of these perceptions on the interview (and hiring) scale (i.e. the θ_i in our model) cannot be given a causal interpretation as the included mediators could correlate with other, unobserved, signals related to applying under a job-vacancy referral system. As a consequence, also the mediation effects $\delta_i\theta_i$ should be seen as associations rather than as causal effects. We return to this point in Section 5.4.

The left-hand side of Figure 5.2 shows the effect of the referral on each of the five mediators. Also after controlling for the other mediators, candidates with a referral are perceived as being less motivated, possessing less intellectual and social abilities, and being less trainable. Again, the impact of a referral on perceived motivation is substantially larger than its impact on any of the other potential signals. Finally, being referred to the vacancy does not have any effect on the perceived evaluation by other employers, and in terms of magnitude.

The right-hand side of Figure 5.2 reports the effect of the different signals on the likelihood of being invited for a job interview. We find that having higher perceived motivation, higher perceived intellectual or social abilities, and a better perceived evaluation by other employers all significantly enhance one's chances of being invited for a job interview. In contrast with what is expected based on queuing theory (Thurow, 1975), we find that perceived trainability does not have any impact on the probability of being invited for a job interview.

By multiplying the left- and right-hand sides of this figure, we can decompose the total effect of a referral on the likelihood of being invited for a job interview ($\delta = -0.655$;

Figure 5.2: Mediation Model with Interview Scale as Outcome



Note. The presented statistics are coefficient estimates (and standard errors in parentheses) for the mediation model outlined in Subsection 5.3.3. δ stands for the total effect, δ' for the direct effect, and $\delta_i\ \theta_i$ for the indirect effects of a referral on the likelihood of an interview invitation passing through mediator M_i. Standard errors are corrected for clustering of the observations at the participant level. The confidence intervals for the mediation effects are based on 10,000 bootstrap samples. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level.

Table 5.4: Multivariate Analysis: Mediation Analysis with Interview Scale as Outcome

Explanatory variables	**Outcome variables**					
	Perceived motivation	Perceived intellectual abilities	Perceived social abilities	Perceived trainability	Perceived evaluation by other employers	Interview scale
A. Candidate characteristics						
Female gender	-0.080 (0.074)	0.157*** (0.054)	0.185*** (0.052)	0.142*** (0.053)	0.076 (0.063)	0.040 (0.058)
Bachelor's degree	0.018 (0.065)	0.991*** (0.066)	0.233*** (0.054)	0.775*** (0.067)	0.215*** (0.065)	0.038 (0.079)
Five years' experience	0.127* (0.066)	0.098* (0.048)	0.070 (0.053)	0.020* (0.053)	-0.037 (0.066)	0.174*** (0.067)
Unemployment duration	-0.029*** (0.003)	-0.019*** (0.003)	-0.019*** (0.003)	-0.025*** (0.003)	-0.049*** (0.004)	-0.023*** (0.004)
Volunteering	0.268*** (0.066)	0.122*** (0.046)	0.949*** (0.068)	0.152*** (0.052)	0.219*** (0.066)	0.017 (0.063)
Referral	-0.978*** (0.080)	-0.161*** (0.050)	-0.185*** (0.052)	-0.262*** (0.055)	-0.084 (0.070)	-0.090 (0.076)
B. Participant characteristics						
Female gender	-0.014 (0.106)	0.082 (0.101)	0.058 (0.077)	0.080 (0.107)	-0.115 (0.138)	0.344** (0.141)
Age	0.012* (0.007)	0.001 (0.005)	-0.001 (0.005)	0.001 (0.006)	0.015* (0.008)	0.018** (0.007)
Highest obtained educational certification						
Secondary education or lower	-0.161 (0.219)	0.173 (0.147)	0.191 (0.137)	0.063 (0.180)	0.184 (0.204)	-0.019 (0.252)
Tertiary education: outside of university	-0.158 (0.103)	0.008 (0.104)	-0.042 (0.078)	0.095 (0.100)	0.288** (0.127)	0.107 (0.122)
Tertiary education: university (reference)						
Frequency of hiring: weekly	-0.123 (0.119)	-0.278*** (0.103)	-0.18** (0.091)	-0.119 (0.106)	-0.146 (0.153)	-0.210 (0.143)
Experience as HR professional: ≥ 10 years	-0.086 (0.134)	0.039 (0.117)	-0.057 (0.093)	0.020 (0.118)	-0.075 (0.164)	-0.182 (0.148)
C. Perceived candidate traits						
Perceived motivation						0.503*** (0.046)
Perceived intellectual abilities						0.259*** (0.063)
Perceived social abilities						0.144** (0.056)
Perceived trainability						-0.045 (0.063)
Perceived evaluation by other employers						0.192*** (0.042)
Observations			1,025			

Note. The presented statistics are coefficient estimates and standard errors in parentheses for the mediation model outlined in Subsection 5.3.3. Standard errors are corrected for clustering of the observations at the participant level. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level.

p = 0.000) found in Subsection 5.3.1 into a remaining direct effect and indirect effects via perceptions concerning the candidate's motivation, intellectual ability, social ability, trainability, and evaluation by other employers. We find a substantial mediation effect related to perceived motivation ($\delta_1\theta_1 = -0.492$; p = 0.000): 75.1% (i.e. – 0.492/– 0.655) of the total effect of a referral on the interview probability is explained by this referral constituting a negative signal of motivation. Additionally, being referred to a vacancy also constitutes a signal of lesser intellectual ($\delta_2\theta_2 = -0.042$; p = 0.010) and social ($\delta_3\theta_3 = -0.027$; p = 0.043) abilities. However, these coefficients are very small compared to the mediation effect related to perceived motivation. The measured direct effect ($\delta' = -0.090$; p = 0.235) is the part of the total effect that remains unexplained by the inclusion of the mediators. Given that this direct effect is not significantly different from zero, we can infer that the signals included in our mediation model fully explain the effect of a referral on interview chances.

These findings corroborate the evidence from qualitative research suggesting that referral is seen as a signal of lower motivation (Bellis et al., 2011; Ingold and Stuart, 2015). They seem to indicate that employers do view the candidates who apply via the PES as candidates who mainly apply in order to continue receiving benefits, which contrasts with the insignificant interaction effect between referral and unemployment duration, as elaborated on in Subsection 5.3.2.

The results of the mediation analysis with the probability of being hired for the position as the outcome variable are largely similar to the results with the probability of a job interview as the outcome variable.

5.4 Conclusion

This article contributed to the literature on the effectiveness of active labour market policies. As argued, the evaluation literature has mainly focused on measuring the overall effectiveness of these programmes, with mixed results. Therefore, in our opinion, the logical next step to take in this literature is to explain why this unsatisfactory effectiveness exists. In this study, we investigated the signalling effect of applying for a job through a vacancy referral scheme. Based on a vignette experiment with HR professionals, we provided first causal evidence for a large negative effect of being referred on one's probability of getting invited to a job interview and finally getting the job. In addition, our experimental design allowed us to explore what exactly is signalled by a job-vacancy referral, testing five potential signals documented in the literature: lower motivation, lower intellectual abilities, lower social abilities, lower trainability, and poor evaluation by other employers. The single most important explanation appeared to be that candidates applying for a position after a PES referral were perceived as significantly less motivated. This corroborates earlier qualitative findings by Bonoli and Hinrichs (2012), Bellis et al. (2011), and Ingold and Stuart (2015).

Besides their academic relevance, our results have substantial policy implications. Our findings are consistent with two possible scenarios. Either the referred candidates are indeed less motivated and the employers act on the basis of earlier experience, or the referred candidates are, in reality, at least on average, not less motivated than general candidates. In the first situation, it is important to know whether those referred are intrinsically less motivated or whether it is the referral which causes the lower motivation. If those referred are intrinsically less motivated, the PES should link the referrals to other policies to increase the benefit recipient's motivation. If it is the referral that is lowering the benefit recipient's motivation, one might question the usefulness of the referral scheme. If, on the other hand, the second scenario is the true scenario, it is important to reverse the negative perception of referred applicants. We see two ways of doing this. One way would be simply not to inform employers of the referral status of applicants. As aforementioned, at the time of Bollens and Cockx's (2016) study, the employer was only informed of a candidate being referred by the PES in approximately 25% of the cases. As they found a large positive effect of the referral on job-finding probabilities, this suggests that not informing employers about a referral could indeed mitigates the negative effect on hiring probabilities. However, if the unemployed person is aware that the employer is not informed about the existence of the referral, and consequently, the PES cannot effectively monitor compliance, this might lower the overall effectiveness of the referral as there will no longer be a threat effect. Another way would entail the PES informing employers better that the referred candidates have gone through an initial screening and should therefore be a better match to the vacancy than other candidates. Overall, our findings suggest that there is room for improvement in the implementation of the vacancy referral scheme in Flanders.

We end this article by acknowledging some limitations inherent to our experimental approach. Firstly, contrary to field experiments, the data collection within a vignette experiment does not take place under real-life circumstances, and participants are aware that they are taking part in an experiment. This creates the risk of participants answering in a socially desirable way. However, we believe this to be less of a concern in our vignette experiment for a number of reasons. An important feature of a vignette experiment is that each participant is shown only a small number of vignettes, and these vary on a number of factors. Therefore, it is very difficult for the participant to ascertain the socially desirable answer (Auspurg, Hinz, C. Sauer, and Liebig, 2014; Liechti et al., 2017; Mutz, 2011). Vignette experiments have been able to identify unequal treatment, even when used to investigate socially sensitive topics such as unequal treatment based on gender and race (Auspurg and Hinz, 2014). Moreover, if we did record some socially desirable answers in our experiment, the results described in this study could be seen as a lower bound of the true effects. To determine whether or not this is the case, it would be interesting to replicate our study by means of a field experiment, although it would not be straightforward to inform employers on the referral status of applicants in a realistic way, and this type of experiment would not allow insight into the specific signals of a job-vacancy referral scheme. Secondly, it is important to recall that we only measured the effect of referral on hiring for individuals with a specific profile applying for a specific position. As a consequence, we cannot say to what extent our results are generalisable to settings with different jobs and candidate profiles. Further research is necessary to ensure the robustness of our results in another setting (see also Van Belle et al., 2017). Thirdly, as mentioned in Section 5.3.3, we cannot give a causal interpretation to our mediation analysis aimed at decomposing the overall effect of referral on hiring chances into a direct effect and five indirect effects via the particular signals theoretically related to a referral. While the treatment of a referral is randomly assigned to the fictitious job candidates within our experiment, the five potential signals related to this referral are not experimentally manipulated. As a consequence, they may correlate with other, unobserved, perceptions.

Nevertheless, we believe that the suggestive evidence for our overall (and causal) negative signalling effect of applying for a vacancy under a referral scheme being, to a large extent, explained by a negative (and causal) impact of applying under this scheme on signalled motivation is a substantial input for further research.

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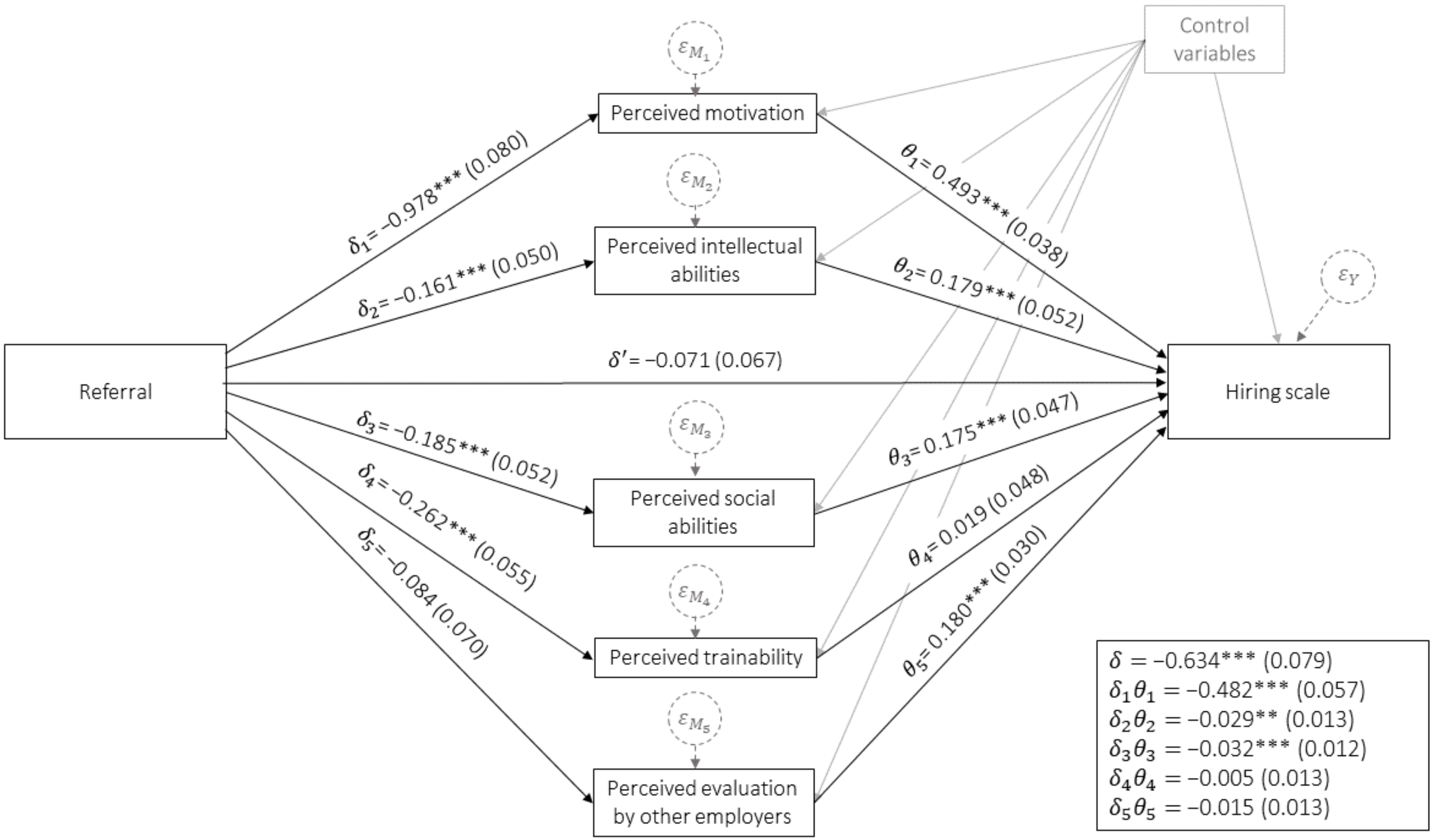
5.5 Appendix A: Additional Figures and Tables

Table 5.5: Correlations between vignette factors

	1	2	3	4	5	6
1 Gender	1.000					
2 Highest obtained educational certification	-0.009	1.000				
3 Previous work experience	-0.081	0.058	1.000			
4 Mentioned social activities	-0.020	0.006	-0.009	1.000		
5 Unemployment duration	-0.007	0.030	0.024	-0.037	1.000	
6 Referral	-0.017	-0.038	0.077	0.011	-0.001	1.000

Note. Cramer's V is reported as all values being categorical. These statistics are based on the full sample of 1,025 observations.

Figure 5.3: Mediation Model with Hiring Scale as Outcome



Note. The presented statistics are coefficient estimates (and standard errors in parentheses) for the mediation model outlined in Subsection 5.3.3 δ stands for the total effect, δ' for the direct effect, and δ_i θ_i for the indirect effects of a referral on the likelihood of hiring passing through mediator M_i. Standard errors are corrected for clustering of the observations at the participant level. The confidence intervals for the mediation effects are based on 10,000 bootstrap samples. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level.

Table 5.6: Signals and Accompanying Statements

Signal	Statement
Perceived motivation	I think this person will be sufficiently motivated to perform properly in this job.
Perceived intellectual abilities	I think this person possesses sufficient intellectual abilities to perform properly in this job.
Perceived social abilities	I think this person possesses sufficient social abilities to perform properly in this job.
Perceived trainability	I think this person will be easy to train.
Perceived evaluation by other employers	I think this person has often been rejected by other employers.

Note. All statements are translated from Dutch. The scale with respect to the perceived evaluation by other employers is reverse scored.

Table 5.7: Multivariate Analysis: Regression Analysis with Hiring Scale as Outcome

Explanatory variables	(1)	(2)	(3)
A. Candidate characteristics			
Female gender	0.144** (0.068)	0.209* (0.108)	0.148** (0.069)
Bachelor's degree	0.325*** (0.074)	0.401*** (0.107)	0.328*** (0.073)
Five years' experience	0.223*** (0.068)	0.303*** (0.103)	0.217*** (0.068)
Unemployment duration	-0.043*** (0.004)	-0.042*** (0.005)	-0.043*** (0.004)
Volunteering	0.298*** (0.066)	0.308*** (0.102)	0.305*** (0.066)
Referral	-0.634*** (0.079)	-0.381* (0.229)	-0.635*** (0.166)
Referral × Female gender		-0.144 (0.163)	
Referral × Bachelor's degree		-0.153 (0.148)	
Referral × Five years' experience		-0.156 (0.168)	
Referral × Unemployment spell		-0.001 (0.007)	
Referral × Volunteering		-0.035 (0.160)	
B. Participant characteristics			
Female gender	0.124 (0.137)	0.128 (0.136)	0.195 (0.151)
Age	0.013** (0.007)	0.013* (0.007)	0.014* (0.008)
Highest obtained educational certification			
Secondary education or lower	-0.177 (0.217)	-0.191 (0.219)	-0.362 (0.268)
Tertiary education: outside university	0.029 (0.130)	0.021 (0.131)	0.123 (0.153)
Tertiary education: university (reference)			
Frequency of hiring: weekly	-0.369** (0.143)	-0.366** (0.143)	-0.462*** (0.163)
Experience as HR professional: ≥ 10 years	-0.199 (0.138)	-0.198 (0.139)	-0.246 (0.160)
Referral × Female gender			-0.142 (0.160)
Referral × Age			-0.002 (0.009)
Referral × Secondary education or lower			0.328 (0.281)
Referral × Tertiary education: outside university			-0.182 (0.165)
Referral × Frequency of hiring: weekly			0.172 (0.167)
Referral × Experience as HR professional: ≥ 10 years			0.090 (0.181)
Observations		1,025	

Note. The presented statistics are coefficient estimates and standard errors in parentheses for the regression model outlined in Subsection 5.3.2. Standard errors are corrected for clustering of the observations at the participant level. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level.

Table 5.8: Multivariate Analysis: Mediation Analysis with Hiring Scale as Outcome

Explanatory variables	**Outcome variables**					
	Perceived motivation	Perceived intellectual abilities	Perceived social abilities	Perceived trainability	Perceived evaluation by other employers	Hiring Scale
A. Candidate characteristics						
Female gender	-0.080 (0.074)	0.157*** (0.054)	0.185*** (0.052)	0.142*** (0.053)	0.076 (0.063)	0.106** (0.051)
Bachelor's degree	0.018 (0.065)	0.991*** (0.066)	0.233*** (0.054)	0.775*** (0.067)	0.215*** (0.065)	0.044 (0.069)
Five years' experience	0.127* (0.066)	0.098** (0.048)	0.070 (0.053)	0.020 (0.053)	-0.037 (0.066)	0.137** (0.054)
Unemployment duration	-0.029*** (0.003)	-0.019*** (0.003)	-0.019*** (0.003)	-0.025*** (0.003)	-0.049*** (0.004)	-0.013*** (0.003)
Volunteering	0.268*** (0.066)	0.122*** (0.046)	0.949*** (0.068)	0.152*** (0.052)	0.219*** (0.066)	-0.065 (0.059)
Referral	-0.978*** (0.080)	-0.161*** (0.050)	-0.185*** (0.052)	-0.262*** (0.055)	-0.084 (0.070)	-0.071 (0.067)
B. Participant characteristics						
Female gender	-0.014 (0.106)	0.082 (0.101)	0.058 (0.077)	0.080 (0.107)	-0.115 (0.138)	0.125 (0.108)
Age	0.012* (0.007)	0.001 (0.005)	-0.001 (0.005)	0.001 (0.006)	0.015* (0.008)	0.005 (0.005)
Highest obtained educational certification						
Secondary education or lower	-0.161 (0.219)	0.173 (0.147)	0.191 (0.137)	0.063 (0.180)	0.184 (0.204)	-0.196 (0.168)
Tertiary education: outside university	-0.158 (0.103)	0.008 (0.104)	-0.042 (0.078)	0.095 (0.100)	0.288** (0.127)	0.059 (0.101)
Tertiary education: university (reference)						
Frequency of hiring: weekly	-0.123 (0.119)	-0.278*** (0.103)	-0.181** (0.091)	-0.119 (0.106)	-0.146 (0.153)	-0.198* (0.111)
Experience as HR professional: ≥ 10 years	-0.086 (0.134)	0.039 (0.117)	-0.057 (0.093)	0.020 (0.118)	-0.075 (0.164)	-0.141 (0.112)
C. Perceived candidate traits						
Perceived motivation						0.493*** (0.038)
Perceived intellectual abilities						0.179*** (0.052)
Perceived social abilities						0.175*** (0.047)
Perceived trainability						0.019 (0.048)
Perceived evaluation by other employers						0.180*** (0.030)
Observations			1,025			

Note. The presented statistics are coefficient estimates and standard errors in parentheses for the mediation model outlined in Subsection 5.3.3. Standard errors are corrected for clustering of the observations at the participant level. *** (**) ((*)) indicates significance at the 1% (5%) ((10%)) significance level.

Chapter 6

General Conclusion to the Dissertation

In this dissertation I have studied unemployment from two perspectives. On the one hand, I have analysed some of the potential causes for an unemployment spell to turn into long-term unemployment, while on the other hand, I have estimated the effectiveness of labour market policies implemented in order to limit the duration of unemployment or to alleviate its negative consequences. Unemployment, and especially long-term unemployment entails large costs for the unemployed individual and for society as a whole, and this is especially true when the unemployment spell occurs at the beginning of ones working life. Furthermore, the incidences of both long-term unemployment and youth unemployment are still at relatively high levels in the current post-crisis economy. Therefore, the topic and the conclusions of this dissertation are particular relevant from an academic point of view as well as from a policy perspective. Indeed, in order to tackle the issue related to persistent unemployment, we first need to gain insight in the reasons why we observe this phenomenon. Likewise, policies developed to improve labour markets, prevent long-term unemployment and temper the negative income effects of temporary unemployment spells should be rigorously evaluated, both from a micro and from a macro point of view.

This general conclusion serves three purposes. Firstly, I will briefly summarize the main findings of the four chapters of this dissertation. Secondly, I will review limitations of the current research and propose a number of avenues for future research. Finally, some take-away messages for policy makers, employers and the long-term unemployed will be provided.

6.1 Summary of the main findings

Given that the four papers in this dissertation approach the same subject–unemployment–from different points of view, it is not straight-forward to draw an overall conclusion from the dissertation. Instead, I will summarize the conclusions from the different chapters and draw some links.

As stated in the introduction of this dissertation, in order to design policies minimising the incidence of long-term unemployment, it is crucial to know what causes this long-term unemployment. This is the aim of the first study of this dissertation. Here, we considered the mechanisms behind the observation that part of the negative duration dependence in unemployment is caused by a negative sorting in hiring (Eriksson and Rooth, 2014; Kroft et al., 2013). Our vignette experiment revealed that employers are indeed reluctant to hire job candidates with a long history of unemployment, providing further evidence for a substantial scarring effect of unemployment. Moreover, our experimental design allowed us to shed light on the reasons underlying this observation. We find that this reluctance to hire long-term unemployed is to a large extent mediated by the perception of unemployment as signalling lower intellectual and social capabilities and, in particular, a lower motivation. In addition, we find no evidence that employers believe the unemployed to have lost their skills over the course of their unemployment spells. These findings have some important consequences for the ideal design of labour market policies, which are at the centre of the other studies of this dissertation.

The second study undeniably shows that–overall–there is a large scope for labour market policies to alleviate unemployment. This study looks at labour market policies from a macroeconomic perspective by means of a panel model using data on expenditures in active and passive policies for 58 advanced and emerging economies. We provide robust evidence that spending in active labour market policies (ALMPs) ameliorates the state of the labour market. The interaction with spending in passive labour market policies is nevertheless crucial. Indeed, additional spending in ALMPs only decreases unemployment under the condition that a sufficient amount is spent in passive labour market policies, which also have a potential positive labour market effect if large enough amounts are spent on ALMPs.

While this second study showed large potential impacts of the interaction between active and passive policies, not all such policies live up to this potential. The third study of this dissertation estimates the effectiveness of a particular Belgian (Flemish) policy–combining both an active and a passive component–using a Regression Discontinuity design. Indeed, an increase in the waiting period for young jobseekers from 9 to 12 months did not significantly increase the transition from unemployment to employment while the youth work plan–an ALMP providing intensified counselling–did only enhance the exit rate out of unemployment after 8 months but did robustly decrease the number of working days, especially for youths living in poorer households.

Finally, given that not all labour market policies are effective in obtaining the desired outcome, an important question is why this is the case. In the fourth study we shed light on one potential explanation, namely the negative signal associated with participation in labour market policies. For the purpose of this study, we focus on one ALMP in particular, a job vacancy referral scheme in Flanders, Belgium. By means of a vignette experiment with HR professionals, we find evidence for a large negative effect of being referred on the probability of getting invited for a job interview and consequently getting the job. As in the first chapter, our experimental design allowed us to unravel the mechanisms behind this crude result. The single most important explanation appears to be that candidates applying for the position after receiving a referral by the public employment service are perceived as significantly less motivated. This study thus confirms the importance of the signalling value of labour market policies as a potential explanation for their limited effectiveness

6.2 Suggestions for future research

Current research in the domain of labour economics and labour market policy evaluation is focussed on measuring the effectiveness of policies. However, what will be central for future research in the domain, in my opinion, will be the “why question”. Indeed, a lot of "why questions" remain. Why do employers consider long term unemployed to be less motivated? Why is the interaction between active and passive labour market policies so important? Why are certain labour market policies less effective than others in decreasing unemployment? Why do employers see participation in certain labour market programs as a negative signal? Finding an answer to these questions and uncovering the mechanisms behind observations is crucial from an academic point of view, but also for policy makers. In order to find the cure for long-term unemployment we have to start with the correct diagnosis.

Secondly, the current research has unveiled the importance of studying these questions using an integrated approach. If we only focus on active labour market policies without taking the interaction with passive policies into account, important implications could be lost. Also, when evaluating specific labour market policies, it seems imperative to take both the demand and the supply side factors influenced by the policy into account. Indeed, looking at both sides of the labour market can provide a valuable insight into why certain policies do not live up to their potential.

Finally, an important question which has up to know remained underexplored is how the research findings generalise to different contexts. It is not because a certain policy appears to work in a specific country for a specific population that it will work when implemented elsewhere. Again it is important to investigate whether these results are generalizable and if not, why this might be the case.

6.3 Take-away messages

Regardless of these remaining questions, this dissertation provides some important policy implications. As an unemployment spell is clearly seen as a negative signal by employers it is crucial for labour market policy to activate the unemployed early in the unemployment spell. In instances where early activation is unsuccessful it is important to attenuate the signal that these unemployed would be less motivated. Another key aspect for effective labour market policy is the interaction between active and passive labour market policies, as both seem to enhance each other in a robust way. This does nevertheless not imply that all policies combining an active and passive component are effective in improving labour market outcomes, as is shown by the third chapter. Moreover, the negative stigma associated with certain labour market policies might lower the effectiveness of certain policies. Policy makers should take these factors into account when designing labour market policies.

The results in this dissertation also have some implications for other labour market actors, namely employers and the unemployed. In the war for talent and with the ageing society in mind, we need all able individuals to contribute their labour. In order to achieve this, recruiters should be trained not to discriminate against certain candidates, including on the basis of their unemployment histories. Finally, the knowledge that employers see a long unemployment spell (or participation in certain programs) as a negative signal of motivation is also important for job seekers who could as a result avoid long unemployment spells or better emphasize their motivation in their job application.

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