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Structural and Empirical Analysis of the Labour Market

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Structural and Empirical Analysis of the Labour Market

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Preface

This dissertation was written in the period from September 2011 to August 2015 while I was enrolled as a PhD student at the Department of Economics and Business Economics at Aarhus University. I am grateful to the department for providing financial support and an excellent research environment. In addition I would like to thank the Research Centre for Labour Market Studies (CAFE) for funding to numerous courses and conferences - activities which have been very helpful and instructive during my PhD.

I would like to thank my two supervisors, Michael Svarer and Torben M. Andersen, for helping and being available despite their numerous other obligations (such as saving various Nordic economies). The friendly, relaxed atmosphere and constructive discussions have suited me well. Michael deserves thanks for encouraging me to enrol as a PhD student, introducing me to the research environment and always encouraging me to study more *now* due to the risk of higher future costs. I also want to thank Rune Vejlin, Henning Bunzel, Lars Skipper and many others for always being willing to discuss research or explain econometric issues. During my studies I visited the economics department at University College London. I would like to thank the department for its hospitality and Jean-Marc Robin for inviting me.

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Finally I would like to thank my family. My parents and grandparents for always encouraging and supporting me to pursue my goals and for showing me the value of books and studies. My family in law for supporting me and taking care of many things while I was finishing. Pernille deserves special thanks for always being there with love and support and showing me the world outside labour economics (e.g. horses, farming and other adventures).

Jonas Maibom
Aarhus, August 2015

UPDATED PREFACE

The pre-defense was held on October 5th, 2015. I would like to thank the members of the assessment committee, Jeremy Lise, Mette Ejrnæs, and Rune Vejlin (chair) for their very constructive and insightful comments. Some of the suggestions have already been incorporated into the dissertation, and more will follow in the near future.

Jonas Maibom
Aarhus, November 2015

CONTENTS

Summary

English Summary

This thesis consists of three independent chapters on (un)employment and active labour market policies. Each chapter addresses important issues related to the design and targeting of labour market policies. Each chapter differs in both research question and analysis approach. While the first two chapters are purely empirical, the third chapter exploits a combination of empirical analysis and economic theory to develop a dynamic structural model of job search and to estimate it using data from a a social experiment.

The first chapter, *Do Business Cycles Have Long-Term Impact for Particular Cohorts?* (co-authored with Torben M. Andersen, Michael Svarer and Allan Sørensen), is an empirical analysis of how employment rates across Danish birth cohorts vary with the business cycle and their stage in the life-cycle. The paper is a contribution to the literature on "lost" generations and the impact of recessionary years early in the life-cycle (see e.g. Oreopoulos et al. (2012) and Bell and Blanchflower (2011)). In the paper we show that it is important for policy, and when discussing the employment consequences of slumps, to make a distinction between the immediate impact and the subsequent adjustment process. In particular we quantify the importance of two sources of persistence in employment rates at the cohort level - namely endogenous and exogenous persistence. Exogenous persistence relates to a decline in employment generated by persistent changes in the economic environment, e.g. a recession for several years, whereas persistent employment losses generated by a single recessionary year are referred to as endogenous persistence. Our empirical set-up allows for a separation of the two sources of persistence and explicitly takes a cohort perspective building on the life-cycle pattern of labour market participation/employment. As an econometric approach we use System GMM in order to differentiate "true" dynamics associated with responses to the economic environment from unobserved heterogeneity and "deterministic" life cycle changes (for instance the age-gradient in employment levels for young workers). The strength of our analysis is our ability to look at different sources of persistence across the life-cycle. The findings suggest that endogenous persistence is stronger for the old than for the young. Thus while our results show that young workers tend to be more

exposed to business cycle fluctuations than old workers, the young also recover faster than old workers where persistence is stronger. The paper can be seen in relation to the discussion about youth unemployment which has also been a part of the Danish popular debate. Our results indicate that youth will always respond more to economic fluctuations (e.g. have higher unemployment rates), but in the longer run "older" workers are just as worse of. In relation to policy, our analysis therefore does not lend support to particular policy measures being directed at avoiding persistent effect or lost generations for the young. Persistent effects of a decline in employment are strongest among the old with a less strong educational background and there are thus arguments in support of specific labour market policies being targeted towards this group.

The second chapter, *Experimental Evidence on the Effects of Early Meetings and Activation* (co-authored with Michael Svarer and Michael Rosholm), is an analysis of a Danish social experiment called "Quickly Back to Work 2". The experiment was conducted in 2008 by the National Labour Market Authorities, and is in line with a new strategy for more evidence based labour market policies. The experiment consists of three sub-experiments involving combinations of early and intensive active labour market policies: in particular individual meetings or group meetings with a caseworker at the job centre or shorter activation programmes. The interventions are therefore different (both in scope and content) from more traditional activation policies (compulsory participation in e.g. workfare or training programmes) which are costly, and often do not help in terms of bringing unemployed workers quickly back into regular employment (see e.g. Heckman et al. (1999) and Card et al. (2010)). The existence of rich administrative Labour Market data allows us to analyse the impacts of the experiments across regions and subgroups for a long horizon (5 years). As the division into treatment and control groups is random, our main econometric strategy is a simple comparison of means over time. In the appendix, we also show some results of a duration analysis which serves as evidence in favour of our interpretation of the empirical results. By exploiting data on the costs of running the different labour market programmes we conduct a thorough cost benefit analysis where we focus on the impacts on the government budget. We also provide estimates of the welfare gain under distortive taxation (and assuming that the value of leisure is negligible - see chapter three). Our results show that individual meetings between newly unemployed workers and caseworkers increase employment rates by around 4 %. The effects persist over the next four and a half years and improve the government budget with close to EURO 4500 per unemployed. Individual meetings largely outperform the other treatments (group meetings and early activation), both in terms of impact on employment and cost effectiveness. Finally we perform a subgroup analysis focusing particularly on the differential effects between men and women, young and older workers, under different cyclical conditions, as well as impacts on unemployment and employment duration.

The third (and final) chapter, *"Assessing Welfare Effects of ALMPs - Combining a Structural Model and Experimental Data"*, uses data from the experiment introduced above. While the second chapter established that an early intensification of ALMPs can improve government budgets, the overall implications on welfare - or whether this program is actually beneficial for society as a whole - are less clear. In particular, an overall assessment of welfare requires more knowledge about how ALMPs actually work. The empirical literature (see Black et al. (2003a) and Hagglund (2011)) has documented the presence of so-called threat effects (ex ante effects) which suggest that individuals view program participation as costly. Costs exist because programmes tax leisure time by removing some of it and replace it with time in the job centre (PES). The existence of costs for participants implies that an evaluation of programme impacts through an analysis of its effect on employment (as in chapter two), is only partial in nature. To perform an actual welfare analysis we need an estimate of how individuals value their time spent in the programmes. Therefore, in the final chapter of the thesis, I develop an approach which delivers exactly such estimates that are among the very first in the literature.

Costs associated with programme participation are generally unobserved and hard to quantify. Instead costs will have to be determined indirectly through behaviour such as job finding rates or wages in future employment, and the size of costs will ultimately depend on a prediction about behaviour in the absence of the programme (an assumption about the counterfactual). In order to quantify costs I develop and estimate a dynamic discrete choice model of job search and estimate it exploiting the experimental data from above. The structural framework provides a mapping from observed behaviour into the determinants of decision making at the individual level while the RCT improves identification of unobserved costs for two reasons. First the RCT generates exogenous variation in programme participation which ensures that differences in behaviour between control and treatment groups can be prescribed to the impact of the programme. Secondly as a RCT, by construction, is a finitely lived and time-varying intervention this generates useful variation to improve identification of the decision parameters in the model once the experiment is properly accounted for in the model. Using the structure of the model, I calculate the compensating variation (the payment individuals would receive to leave them indifferent between belonging to the treatment or control group) associated with the intensification of ALMPs and I analyse some counterfactual policies. I also illustrate and discuss the identification problem described above. My estimates suggest that the cost associated with programme participation is non-negligible - in particular I estimate that unemployed would be willing to pay up to 340 Euros to escape a series of 7 meetings at the job centre. Redoing the CBA of chapter two by taking into account the lost value of leisure I find that while meetings still constitute a worthwhile social investment, the gain is overstated when neglecting the impact of the programme on participants. Taking into account that individuals dislike programme participation is important for future work in designing optimal labour market policies.

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Danish summary

Denne afhandling består af 3 selvstændige kapitler, hvori strukturelle og empiriske metoder anvendes i analyser med fokus på ledighed og aktiv arbejdsmarkedspolitik. Hvert kapitel omhandler vigtige problemstillinger, som er relevante for fremtidig politik og forskning indenfor arbejdsmarkedet. Kapitlerne adskiller sig fra hinanden i både forskningsspørgsmål og tilgang. I de to første kapitler er tilgangen empirisk, mens kapitel 3 anvender en blanding af teori og empirisk analyse.

Kapitel 1 (Do Business Cycles Have Long-Term Impact for Particular Cohorts?) analyserer, hvorledes forskelle i beskæftigelsesandelen mellem forskelige årgange (cohorts) i det danske arbejsmarked kan relateres til nuværende og tidligere perioders økonomiske aktivitet samt årgangens alder. Analysen er vigtig for at kunne identificere forskellige former for omkostninger i forbindelse med f.eks. recessioner - og dermed for at kunne identificere grupper i arbejdsmarkedet, som er i særlig risiko for fremtidige beskæftigelsestab. Der skelnes mellem umiddelbare omkostninger (eksogen persistens) og efterfølgende tilpasningshastighed (endogen persistens) i forbindelse med afvigelser fra "normal" økonomisk aktivitet. Den empiriske analyse finder at den relative betydning af omkostningerne varierer med årgangens alder, og defor er denne opdeling central. Resultaterne viser således, at selvom unge generelt oplever størst umiddelbar beskæftigelsesnedgang i forbindelse med en recession, så genvindes tabt beskæftigelse og dette foregår væsentligt hurtigere end for ældre årgange. De ældre er derfor værre stillet på lang sigt, hvis de samlede omkoskostninger betragtes - især gælder dette for lavt uddannede.

Kapitel 2 og 3 analyserer de bagvedliggende årsager og den overordnede effekt af deltagelse i arbejdsmarkedspolitiske foranstaltninger i forbindelse med ledighed - herunder særligt deltagelse i møder på jobcenteret og kortere aktiveringsprogrammer. Begge kapitler benytter et kontrolleret forsøg, som blev gennemført i 2008-2009 af Styrelsen for Arbejdsmarked og Rekuttering. I kapitel 2 (Experimental Evidence on the Effects of Early Meetings and Activation) evalueres indsatsen og den overordnede effekt af forsøget over tid (op til 5 år), og indenfor forskellige grupper af deltagere (køn, alder og tidspunkt). Eksperimentet består af 3 forskellige indsatser hhv. individuelle møder, gruppemøder eller fremrykket aktivering. Data på omkostingerne forbundet med de forskellige indsatser muliggør en detaljeret cost-benefit analyse, hvor der fokuseres på effekten af indsatserne på de offentlige budgetter. Analysen viser at individuelle møder på jobcenteret er den mest effektive indsats. Individuelle møder er forbundet med en stigning på 4% i beskæftigelse og en budgetgevinst på 4.500 Euro pr. ledig. Resultaterne kan ses som vigtig vejledning for fremtidig design af aktiv arbejdsmarkedsprogrammer, hvor mere traditionelle foranstaltninger som fx. længerevarende aktivering generelt anses som ineffektive indsatser (Heckman et al. (1999)).

I kapitel 3 (*Assessing Welfare Effects of ALMPs - Combining a Structural Model and Experimental Data*) udvikles en strukturel økonomisk model, som efterfølgende estimeres på samme data, der anvendes i kapitel 2. En række empiriske studier (se Hagglund (2011) og Black et al. (2003a)) har vist, at trusselseffekter er en vigtig årsag til, at nogle arbejdsmarkedsprogrammer viser sig at være effektive. Sådanne effekter kan rationaliseres ved, at deltagerne finder det omkostningsfuldt at deltage i programmerne. Eksistensen af sådanne omkostninger kan betyde, at programmerne ikke er velfærdsforberende og dermed en god investering, selvom de får folk hurtigere i arbejde. Den strukturelle model og anvendelsen af data fra et kontrolleret forsøg muliggør en kvantificering af de omkostninger, som deltagergruppen forbinder med deltagelse i de arbejdsmarkedspolitiske foranstaltninger, og dermed kan en egentlig velfærdsanalyse foretages. Det kontrollerede forsøg skaber eksogen variation i værdien af at være ledig og gennem adfærdsmæssige antagelser i den strukturelle model, kan nytteomkostninger estimeres. Analysen viser, at omkostningerne ved deltagelse er væsentlige og dermed understreges vigtigheden af at inkludere sådanne omkostninger i fremtidige evalueringer af programmer i arbejdsmarkedet.

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CHAPTER 1

Do Business Cycles Have Long-Term Impact for Particular Cohorts ?

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Abstract[1]

Will the current employment crisis produce lost generations with permanently lower labour market attachment? Taking an explicit cohort perspective and based on Danish data we do not find strong persistence in employment rates at the cohort level. Younger workers tend to be more exposed to business cycle fluctuations than older workers, but importantly they recover more quickly from such set-backs than older workers for whom persistence is stronger. Moreover, no cohorts have been permanently "scarred" due to a sequence of adverse shocks. Finally, an explicit

[1] We gratefully acknowledge comments and suggestions from Mette Verner, Frank Walsh and participants at CAFE, ESPE, EEA meetings (2013), ESSLE (2014) and the conference on "Labour Markets During Crises", Maynooth 2014. Corresponding author: Jonas Maibom, Department of Economics and Business, Aarhus University, Denmark, maibom@econ.au.dk

account of overlapping cohorts is shown to affect assessments of persistence in aggregate employment rates.

JEL: J6, E32
Keywords: Persistence, lost generations, employment

1.1 Introduction

The financial crisis has caused sharp employment declines in many countries. A particular concern, spurred by the very high youth unemployment rates, is that the current young generation may become a lost generation in the sense that they never fully recover from the present crisis. Such a scenario entails wide social and distributional consequences, and the medium and long run implications of the current slump are thus intensively debated among unions, politicians and economists. It is well known that past crises have produced persistent decreases in aggregate employment. Notably, the crises in the 1970s and 1980s were associated with strong persistence in the labour market, which in turn resulted in a voluminous theoretical and empirical literature on labour market persistence. Given the severeness of the current job-crisis it may be feared that persistence may be even stronger than in the past.

By persistence is understood that a change in the labour market position has long-lasting effects on future labour market positions. The notion of lost generations entails that this effect pertains to specific cohorts. A particularly strong form of persistence would arise if a cohort entering the labour market in a slump (boom) as a result would experience a worse (better) labour market position in their remaining labour market career.

In addressing the question of persistence various interpretations or distinctions are possible. A key issue is the distinction between exogenous versus endogenous persistence.[2] Exogenous persistence arises if the driving forces of economic activity which affects employment (and thus labour demand) itself display persistence[3], while endogenous persistence relates to whether the labour market response tends to produce persistence. In the latter case even a temporary exogenous decline in the activity level has a persistent effect on employment rates. This suggests two interpretations of the notion of lost generations. One is where endogenous mechanisms in the labour market produce persistence in the labour market position. Another is where some cohorts have been exposed to a sequence of adverse (or good) shocks therefore ending up in worse (or better) positions than other cohorts. Distinguishing

[2] In the business cycle literature there is a long-standing debate on the role of endogenous and exogenous sources of persistence. Output measures generally display strong persistence, and the standard real business cycle model can only replicate this by assuming shocks with strong persistence, see e.g. Cogley and Nason (2005). A literature has explored the source of endogenous persistence such that even temporary exogenous impulses cause a persistent response, see e.g. Andersen (2004).

[3] In e.g. Real Business Cycle models the driving force is productivity shocks with a strong degree of persistence.

between these two sources of persistence is important for policymakers as they may call for very different policy responses. In this paper we consider both notions of a lost generations and we extend previous work - which have so far been focusing on the years around labor market entry - to different stages in the lifecycle. This enables us to quantify the costs (in terms of lost employment) from fluctuations in output along the lifecycle and assess at which stage cohorts respond the most and the longest to fluctuations in output.

The theoretical challenge is to explain endogenous persistence; that is, why a temporary exogenous business cycle shock translates into a persistent effect on labour market variables. The literature has focused on human capital, wage formation (insider-outsider models), capital accumulation and labour market policies/institutions (for a survey of the earlier literature see Roed (1997)). Human capital depreciation during non-employment either due to depreciation of acquired skills and of firm-specific knowledge (see e.g. Pissarides (1992) and Lockwood (1991)) or due to social problems caused by the failure to be self-supporting may be an important source of cohort specific persistence and thus the possibility of lost generations. If a particular cohort is exposed to a large adverse employment shock, it follows that this cohort may find it difficult to return to employment when the business cycle normalizes, and thus it may experience a persistent lower employment rate (for other theoretical explanations see the overview in Oreopoulos, Wachter and Heisz (2012)).

The empirical literature on labour market persistence has both a macro and a micro strand. The macro literature is mainly focused on establishing whether aggregate measures, like employment and unemployment rates, display persistence. A strong form of persistence arises if, e.g., the coefficient in an AR(1) specification of the aggregate unemployment rate is close to unity.[4] Related is the literature considering institutional and structural factors and the possibility that they account for labour market persistence (for a survey see Blanchard (2006)).

While the macro literature has yielded important insights, especially of a comparative nature on speeds of processes in the labour market, it suffers from the problem that it is incapable of clarifying whether the burden of persistence rests on particular cohorts. Importantly, this issue cannot be addressed by considering, for instance, the time-series properties of the (un)employment rate of a given age group (see e.g. Bernal-Verdugo et al. (2012) on youth unemployment). There may be a very strong time dependence in the unemployment rate for a particular age group, say 20-25 years, due to institutional factors. But clearly this is not necessarily implying that there is strong cohort specific persistence. The reason being that cohorts age with time, and hence to assess cohort specific persistence one has to analyse cohorts over time, and not age groups over time.[5]

[4]See e.g. Duval et al. (2006) and Guichard and Rusticelli (2010).

[5]This problem also appears in cross-country comparisons. Giuliano and Wachter (2012) show that although youth unemployment is much higher in France than in Germany, the subsequent labour market performance for older age groups is quite similar in the two countries.

The micro literature exploits individual data to identify employment prospects and possible persistence or path-dependencies depending on individual characteristics. In the seminal paper by Heckman and Borjas (1980) a central question is whether unemployment increases the risk of being unemployed in the future, to answer this question one needs to control for unobserved factors affecting both past and future employment status. Some examples of more recent studies where it is found that being exposed to a negative or positive shock to employment may have longer lasting effects on the future labour market status are e.g. Doiron & Gørgens (2008), Hartman et al. (2010), Mroz & Savage (2006), Cockx & Picchio (2013) and Lesner (2014). The micro approach suffers from two shortcomings. First, it does not allow a separation between endogenous and exogenous sources of persistence. Second, persistence at the individual level does not have clear-cut implications for possible persistence at the cohort level. This point has been raised several times in the literature, recently Blundell and Stoker (2007) point out that the aggregation link between individual behaviour, heterogeneity in many dimensions and aggregate dynamics is certainly not trivial.

In this paper we consider employment responses to the business cycle situation and dynamics at the cohort level in order to avoid the problems associated with both the micro and the macro approach. This intermediary approach - the meso level - will compare employment levels for cohorts at similar stages in the lifecycle at different economic activity levels to identify how cohorts are *generally* affected and adjust to fluctuations in employment. The meso level of aggregation complement the micro and macro approach presented above. The macro approach relying on aggregate (un)employment rates suffers by construction from being silent about potential dynamics at the cohort level, although important for understanding the evolution of the aggregate (un)employment rate over time. We underline the latter point by showing that evaluating persistence from an explicit cohort perspective leads to a different assessment than implied by the aggregate time-series approach. The micro approach focussing on persistence at the individual level does not capture the processes applying at the level of cohorts. The individual path may depend on idiosyncratic individual shocks, whereas we are interested in the response to aggregate labour market shocks and to separate endogenous from exogenous sources of persistence. Analysis based on individual data are critically dependent on having the proper control variables capturing individual characteristics and labour market histories, whereas an analysis at the meso or cohort specific level is less vulnerable to these data requirements under the assumption that the distribution of these individual characteristics is (reasonably) constant across cohorts.[6]

Knowledge of dynamics at the cohort level is important from a policy perspective,

[6]The sample is split by different age groups, and by having cohort specific covariates we allow for differences between cohorts, cf. below. Furthermore our approach allows us to use data covering almost 30 years, and thereby we use variation from several recessions and booms to asses the persistence in employment rates. This would naturally be very challenging in an individual level analysis.

especially whether policies should be targeted towards certain cohorts/age groups. This also serves as an important input in the ongoing discussion about youth unemployment and other employment effects from the current crisis. By analysing cohort specific performance we focus on what survives aggregation from the individual level and becomes embedded in the employment record for a particular cohort. This also implies that our approach allows for peer effects (or externalities) among individuals within specific cohorts and our results will therefore capture more than individual scarring effects which is why the paper is complimentary to the micro approach.[7]

This paper is related to a litterature using variation at the meso level to look at consequences from entering the labour market in a recession (see e.g. Genda, Kondo and Otha (2008), Oreopoulos, Wachter and Heisz (2012), Bell and Blanchflower (2011) and Liu, Salvanes and Sørensen (2013)). The general finding is both a short- and a long-term effect on employment or earnings. The effects differ across educational groups, and especially workers with intermediate and low levels of education are in an exposed position in terms of lost employment. For graduating workers with a college degree or more Oreopoulos, Wachter and Heisz (2012) find employment effects up to three years after graduation but also earnings effects which are much more persistent. This part of the literature is therefore closely connected with the idea of lost generations and the focus is on young workers, as the youth have less experience and are potentially less efficient job seekers. The concern is that cohorts graduating in a recession may be in a particularly exposed position. This paper moves beyond the impact of entering the labor market and focuses on the effects of variations in economic activity throughout the whole lifecycle (both booms and recessions). Furthermore, to avoid endogenous selection into experience groups (years since entry into the labor market) we focus on cohorts defined by their birthyear which are obviously predetermined (this is similar to e.g. Jaimovich & Siu (2009)). This also implies that we allow a part of the response to economic activity (for instance a decrease in employment levels) to run via labour force participation (for instance youth staying longer in education due to high unemployment rates).

We take explicit outset in the life-cycle pattern of employment (inverted U-shaped relation between employment rates and age) and consider whether the labour market situation for a cohort at a particular age affects the same cohort's labour market position at a later age, and possibly through their remaining labour market history.[8] By controlling for the impact and duration of business cycle shocks (exogenous persistence) as well as endogenous mechanisms in the labour market, we are able to

[7]A recent paper by Adda, Dustman, Meghir & Robin (2013) shows that recessions not only affect job-finding rates (unemployment) but also wages and labour market careers for those finding a job. A micro analysis needs to incorporate such potential market effects. By focusing our analysis at a higher level of aggregation we can assess the total effect at the cohort level.

[8]Our procedure also makes it easier to control for the typical life-cycle pattern of employment, which has a strong age-gradient not to be interpreted as persistence For instance, younger aged workers are likely to have a lower employment level (and maybe even less stable) simply due to the fact that they have just entered the labour market.

consider both notions of lost generations discussed above.

We use Danish register data for the entire labour force population for the years 1980-2008. Our key variable is the employment rate. We choose the employment rate rather than the unemployment rate, as the latter does not take the labour force participation decision into account[9] and is associated with various measurement problems in the time frame of our study (it is affected by institutional changes). Moreover, we distinguish by gender and educational levels to identify possible differences in exposure to shocks and in persistence across key socioeconomic groups in the labour market.[10]

Our results show that although the adjustment of employment shocks is gradual, there is no evidence of strong endogenous persistence and thus lost generations in this sense. In line with the literature, we find that younger workers tend to be more exposed to business cycle fluctuations than older workers. Importantly, however, we find that younger workers recover more quickly from such set-backs than older workers. For elderly women with low or medium levels of education, employment rates may be permanently affected by business cycle changes. These findings illustrate the very different costs business cycle fluctuations can have over the life-cycle and underlines the importance of the approach taken in the analysis. Finally, we also find that there is not a strong case of certain cohorts becoming lost generations as a result of having been exposed to a sequence of adverse shocks in vulnerable ages.

The paper is organized as follows: Section 2 describes the data and some facts. Section 3 sets up the econometric model, and Section 4 describes the estimation techniques applied. Section 5 contains the estimation results, and Section 6 considers the specific question of lost generations. Finally, Section 7 concludes.

1.2 Employment rates in a life-cycle perspective

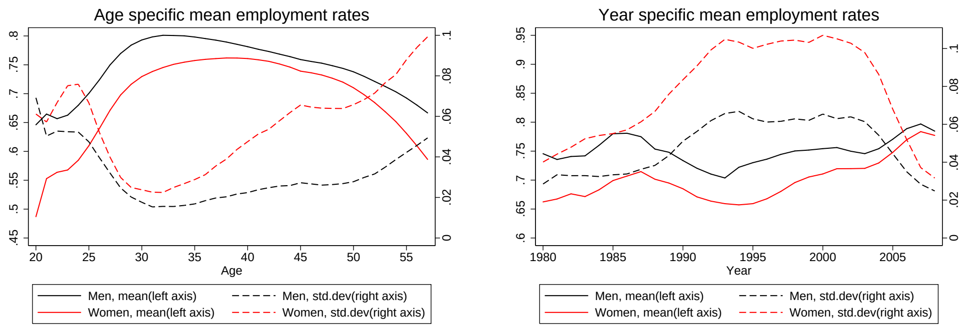
Our empirical analysis is based on a Danish register-based annual matched-employer-employee panel covering 1980-2008. This panel contains the whole of the Danish population. The unit of observation is a given individual in a given year with measurements generally referring to the last week of November. We aggregate across individuals and compute the employment to population ratio (EPR) for each cohort in a given year for ages 20-57. We restrict the analysis to cohorts born from 1935 to 1980.[11]

[9] Clark & Summers (1981) showed that the economic activity level affects both unemployment and labor force participation thus illustrating the need to take both measures into account. Furthermore they showed that there are important differences in how employment fluctuates for different agegroups over the business cycle.

[10] In addition to the employment rate we have considered earnings at the cohort level. However, as the findings were qualitatively similar, we report only the findings for the employment rate.

[11] Note that our sample have truncated spells; i.e. young generations with incomplete spells and older generations where the previous employment spell is not known. Figure A.1 in the appendix gives a visual representation of the data.

FIGURE 1: EMPLOYMENT TO POPULATION RATES BY AGE AND YEAR



Note: For the age specific rates standard deviations are computed from variation across time for a given age group, and for the year specific rates standard deviations are computed from variations in different age groups in a given year.

The sample is split according to gender and education. The educational level of the individual is defined by the highest obtained education around age 30 (or later if not present). Individuals who have education similar to a High School degree or less as their highest education are classified as being low educated, whereas individuals having education similar to a bachelor degree or above are classified as being highly educated (we follow these individuals from age 23 and onwards). The remaining individuals with intermediate levels of education are classified as being medium educated (these will be craftsmen and similar occupations). Note that cohort membership is defined by the birthyear of an individual and therefore we avoid the selection into experience groups determined by year of graduation. Naturally stratifying the sample on levels of education could however induce a similar selection problem but the education groupings suggested here is so broad that selection between groups is unlikely to be a primary concern (see Appendix A,C for an introduction to our data and a statistical assessment of the validity of our sample selection rules).
The basic life-cycle pattern for cohorts is illustrated in the left panel of Figure 1 for males and females, respectively.[12] The evolution of the mean EPR displays the expected inverted U-form with employment rates first increasing and later decreasing with age, reflecting entry and exit into the labour market.[13] The mean EPR for men is higher than for women for all ages, but the inverted U-shape is similar. The standard deviation around the mean follows a U-form attaining its lowest value for the age group 30-35, where the average employment frequency is also close to its peak. The low standard deviation for the middle-aged (prime aged) suggests that there is no strong persistence running through the entire work life for a cohort. That is,

[12] To assess the sensitivity of the figures reported below to the sampling selection scheme (see Figure A.2) we have created similar figures randomly deleting observations to ensure that we have the same number of observations for a given age (year). The findings reported below are robust to such an exercise.

[13] Jaimovich & Siu (2009) show that this empirical regularity (the U-shape) holds for all G7 countries. According to their estimates while the young comprise of 30% of the workforce on average they account for 50% of aggregate employment volatility.

irrespective of the employment trajectory during the early years, all cohorts have approximately the same employment rate when becoming 30-35 years old (the standard deviation is around 2-3 % compared to an average of 5% below age 30). In particular, it is noteworthy that this applies also to cohorts experiencing low employment rate for a number of years when young, cf. below. The low variability in EPR for middle-aged also shows that this age group is not significantly affected by variations in the labour market situation. Later in life the variation around the mean increases again; for men it reaches the same level as in the younger ages (around 25), whereas there is much more variation around the mean for women at later ages.

The right panel of Figure 1 presents the employment rates by year. The Danish economy experienced a large economic crisis through the late 80's and early 90's which caused a sharp and persistent drop in the employment rate (see Figure A.1 in the appendix for output gap during the time period). There is a tendency to convergence in employment rates between genders in the last years included in the sample. The standard deviation (which is now across age groups in a given year) displays a counter-cyclical tendency, suggesting that age groups are differently affected by cyclical variations.[14] The age dependent employment rates are shown for different educational groups in Figure 2. Both for males and females the mean employment rate is increasing in the level of education. Figure 2 also shows that the highly educated, as expected, both enter and leave the labour market later than other educational groups. Moreover, this group has a much lower variation in employment rates above the age of 30. The larger variation before the age of 30 is likely to reflect differences in non-employment timing (e.g. education or unemployment) and length and potentially its relation to the business cycle. Notice the U-form of the standard deviation which again reveals a convergence in cohort specific employment levels at middle ages, whereas variation between cohorts is much more pronounced in early and later ages especially for the low and medium educated. Considering the evolution of employment rates for different educational groups over time (Figure 3) it is seen that we have variation for all educational levels, although for the highly educated group this is likely to be primarily related to the young age groups (see Figure 2). The standard deviation has a weak counter-cyclical pattern for all educational groups.[15]
A simple and intuitive way to consider the possible path dependence in employment rates is to consider the employment trajectories for specific cohorts. Figure 4 plots the age dependent employment rates for those cohorts in the sample that experienced the highest and smallest average EPRs in their 20s. The mean reversion around the ages 30-35 is easily seen from the figure; that is, both cohorts that start above and

[14] Note that the age distribution in the sample is skewed towards the start and the end of the sampling window (see Figure A.2) . However from around 1990-2002 the agedistribution is stable and therefore the increase in the standard deviation is not driven by changes in the age distribution. We have further assessed this by restricting the sample to the ages 20-35 and 45-52 and we still see an increasing standard deviation in response to the economic crisis.

[15] The spike in the standard deviation for the high education group is due to the fact that our sample contains primarily younger aged workers in later years, see Figure A.2.

FIGURE 2: EMPLOYMENT TO POPULATION RATES BY EDUCATION AND AGE

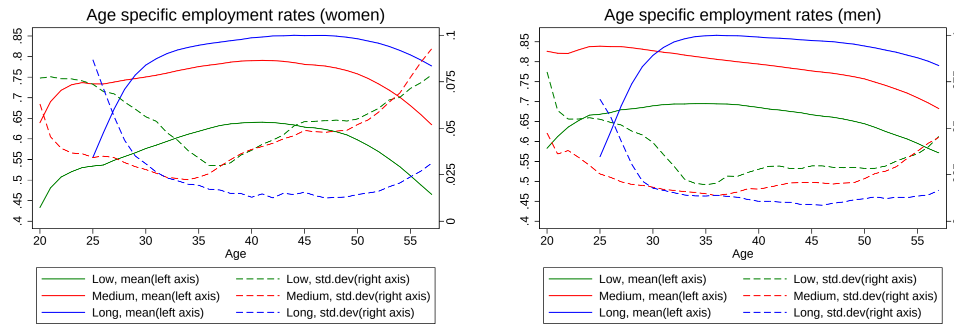
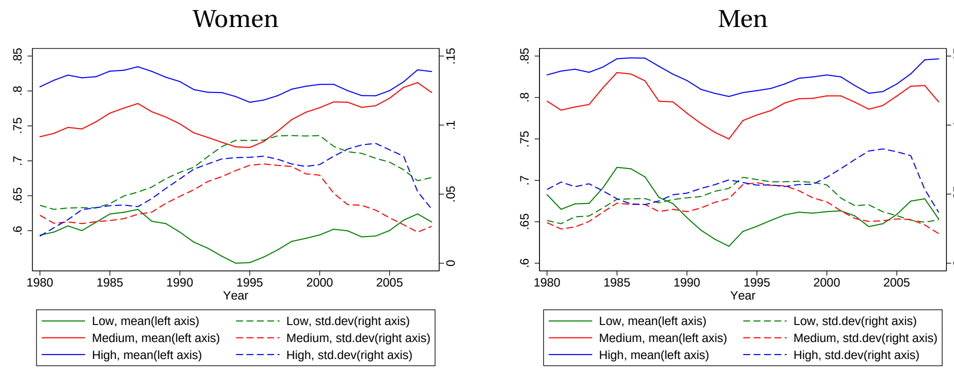


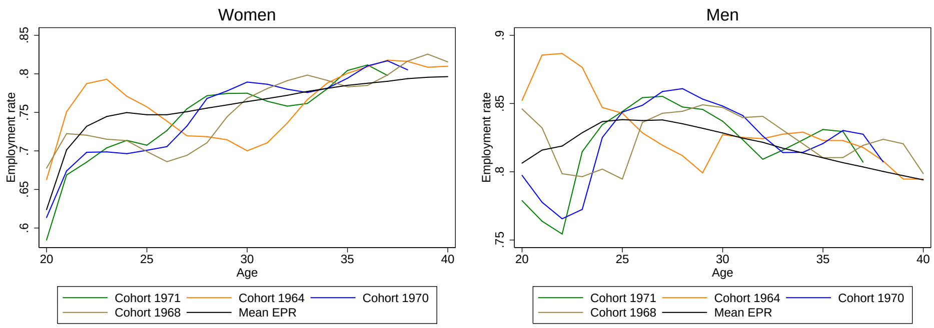
FIGURE 3: EMPLOYMENT TO POPULATION RATES BY EDUCATION AND YEAR



below average eventually adjust towards the mean EPRs. Also note that the ranking in terms of employment rates varies greatly for the selected cohorts over the lifecycle.

Our empirical strategy builds on the findings presented above. We explicitly model the life-cycle pattern in employment rates. Since we do not have complete life-cycle histories in our sample and to accommodate different dynamic patterns across the life-cycle, we split data into sub-samples depending on age: young and old. The young sample consists of cohorts with a minimum of 6 observations in the age range 20-35 (i.e. cohorts 1950-1980), and the old sample consists of cohorts with a minimum of 6 observations in the age range 45-57 (cohorts 1935-1958). The division of the sub-samples can also be seen as a way to explicitly distinguish between the various cohorts and the ages for which we have data. By construction our sample is, in some sense, selected as the observations we have for older ages are represented by older cohorts, and likewise for the youngest – the sample splitting makes this even clearer. To the extent that older cohorts are not representative for older workers in general beyond the intergenerational differences that we allow for in our empirical model (e.g. unobserved heterogeneity), our results would be affected by this selection.

FIGURE 4: EMPLOYMENT TO POPULATION RATES FOR SELECTED COHORTS



One way to phrase the central question regarding selection is whether, for instance, the 1940 generation systematically underperforms because of selection of some kind, or because they are simply the oldest available workers. In this paper we exploit the panel structure of our data to differentiate between "true" dynamics (persistence), time/age effects and unobserved time-invariant heterogenity. This distinction implies that the persistence that we identify is the one revealed through the recovery process (or lack here of) of the cohort and *not* related to for instance a steep age gradient (from e.g. school-to-work transitions). This way the quantification of costs to fluctuations in output (e.g. recession scars) is identified from the magnitude of the response and the duration of the recovery process controlling for other timevarying factors.

1.3 Cohort specific persistence in employment

The model we use in the empirical analysis is setup with the aim to capture the essential life-cycle pattern of employment rates (cf. Figure 1), and to allow for business cycle impacts as well as employment dynamics. As mentioned above, we split the sample by age to account for the possible different performances of different age groups. In addition, we estimate the model separately for the three educational groups. Our strategy is to estimate a generic relation between the employment ratio and age that captures the variations in the time and age domain presented in Figure 1. The EPR, $e_{s,i,t}$, for a given gender s, cohort i (birth year) at a given point in time t depends on both the business cycle situation measured by the output gap (y_t)[16] and the stage in the life-cycle given by the age $a_{i,t}$. The underlying labour market structure and policies are embedded in the relationship, but various specific policy measures may apply for certain age groups in part of the sample period $(z_{i,t})$, cf.

[16] We assume that the response to cyclical variations is symmetric.

below.

$$e_{s,i,t} = \delta e_{s,i,t-1} + \kappa_0 \cdot a_{i,t} + \kappa_1 \cdot a_{i,t}^2 + \theta \cdot I\left(x_2 \geq a_{i,t} \geq x_1\right) \cdot y_t + \gamma \cdot z_{i,t} \tag{1.1}$$

$$+ \beta_0 \cdot y_t + \beta_1 \cdot y_{t-1} + \beta_2 \cdot y_t \cdot s_i + \beta_3 \cdot y_{t-1} \cdot s_i + \psi \cdot s_i + \eta_{s,i} + \epsilon_{s,i,t} \tag{1.2}$$

We allow for the output gap to interact with age (θ) and gender (β_2, β_3). The interaction term is implemented as $I\left(25 \leq a_{it} \leq 30\right) \cdot y_t$ and $I\left(30 \leq a_{it} \leq 35\right) \cdot y_t$ in the young sample and $I\left(50 \leq a_{it} \leq 54\right) \cdot y_t$ and $I\left(55 \leq a_{it} \leq 57\right) \cdot y_t$ in the old sample. $\eta_{s,i}$ represents cohort/gender specific unobserved heterogeneity, and $\epsilon_{s,i,t}$ is the error term. Note that dynamics of the employment rate ($e_{s,i,t}$) driven by aggregate shocks are captured by an ARMA-process embedded in[17] (1.1) ($\delta e_{s,i,t-1} + \beta_0 \cdot y_t + \beta_1 \cdot y_{t-1}$), where the later two terms (the MA part) are the exogenous driving forces, and the first term (the AR part) captures the endogenous adjustment mechanism. For δ close to zero the dynamics of employment rates (around the generic age relationship) are mainly driven by the exogenous impulse, while for δ close to one even temporary exogenous influences may have long lasting effects on the employment rates of a particular cohort (endogenous persistence).[18] Note also that we have tried allowing for further lags and other dynamics in the specification (for instance a AR(2) process for employment rates and a MA(1) process in the error term, see more below). We have also estimated the model separately by gender but the estimates of δ are very similar and therefore we prefer the specification presented above (see Table B.2 in the appendix).

To represent the state of the economy (y_t) we use a measure of the output gap provided by the OECD. The output gap aims to measure the deviation of the level af activity from its structural level. We have tried several other measures for the economic activity, including the output gap from the Danish Economic Council and gaps generated by use of HP-filters. We have not found measures that will alter our results below noticeably. Notice that there are potential simultaneity problems in using the output gap as an explanatory variable for the employment level, therefore we instrument it with lagged levels of the output gap, see below. The business cycle measure is only included with one lag in the reported estimation as we have found that this was sufficient to capture the dynamic process. We will comment further on this restriction below.

It is important to control for unmodelled timeeffects for our empirical strategy to work (estimate δ unbiased), and therefore we include timedummies for some *specific* years that our analysis shows are important, and we also employ a gender specific time trend in our final specification to allow for unmodelled time effects in the employment rates. But since we do not use any cross-sectional variation (for

[17] Here we are neglecting the cross-terms to simplify.

[18] Naturally this depends on β_1. This illustrates that it is important to allow for both adjustment mechanisms. For instance, excluding β_1 would have important implications for the estimated size of both the endogenous and exogenous effects.

instance regional variation) in our measure of economic activity there would also be an identification problem for the output gap if one employs time dummies for the whole of the time period (1981-2008). Therefore we restrict the number of time dummies and we only employ time dummies for specific years where diagnostic plots of our residuals suggest that the measures related to output do not control sufficiently well for the aggregate time effects. In the appendix Table B.3 we compare our preferred estimates to models with a full set of dummies and we show that the estimates are very similar for δ.[19] To the extent that the gender specific time trend, the output gap and time dummies do not control sufficiently well for time effects between individuals our results might be biased although we have not found any such indications in the various robustness checks we present in appendix B.

We have considered various specific policy programmes[20] which have been targeted towards specific age groups during the sample period. A so-called transition scheme for elderly workers was found to be effective at the cohort level. This scheme was in effect in the period 1992-1995 for unemployed individuals in the age group 50-60 years. This scheme allowed the eligible to extend the unemployment benefit period bridging to the statutory early retirement age (60 years). The transition scheme for elderly is found to have a significant negative effect on the EPR (and further analysis shows that this applies mainly for females). This is included in the results reported for the old sample below.

The panel structure of our data allows us to differentiate between "true" dynamics (δ) and factors that vary across, but not within, cohorts over time ($\eta_{s,i}$) even though such factors are unobservable. This distinction also implies that the persistence that we identify is the one revealed through the recovery process of the cohort. Therefore in our model $\eta_{s,i}$ represents life-long differences between cohorts. One way to interpret this coefficient is that it indicates structural differences between cohorts (i.e. a difference in the quality of their education, gender roles, difference in initial conditions before age 20 etc.). Leaving such effects unmodelled will imply an overprediction of persistence as the systematic constant part of the error term will be subscribed to dependence on past values of $e_{s,i,t-1}$ (this way standard OLS will give us an upper bound on the persistence coefficient). The central econometric problem is therefore to distinguish between unobserved heterogeneity and state-dependency captured in $\eta_{s,i}$ and δ, respectively. We solve this problem using estimators suggested by Arrelano & Bond (1991) and Blundell & Bond (1998). We will briefly present our identifying assumptions in the next section (for a further elaboration see Appendix B).

[19]We have tried a sequential approach where we use a full set of time dummies to extract all variation over time and analyze how this affects our estimate of δ. The estimate of δ is generally very stable towards such specifications. Note that year dummies are not included to improve the model fit but are included to improve the validity of our identifying assumptions.

[20]We also considered a youth package introduced in 1996, but did not find it to have significant effects.

1.4 Estimation

The empirical model presented in equation (1.1) suffers from a general identification problem due to the unobserved cohort specific effect $(\eta_{s,i})$, which by construction is correlated with the lagged dependent variable and potentially also other explanatory variables. Traditional Panel Data estimators (fixed effects models) continue by eliminating $\eta_{s,i}$ for instance by taking differences in means, but these procedures have been widely noted to be downward biased (see e.g. Hayasi (2000)) due to the correlation between the demeaned lagged dependent variable and the transformed error term. This correlation arises as any persistent deviation from the true cohort mean will be mischaracterized as a part of the estimated fixed effect in finite samples thus reducing the estimate of δ. Our empirical strategy therefore follows the work of Arrelano & Bond (1991) (Difference GMM) and Blundell & Bond (1998) (System GMM). Our preffered estimates are those produced using the System GMM approach.

The general idea behind both estimators is that we eliminate the fixed effect in equation (1.1) by taking first differences and then we instrument the differenced lagged dependent variable by its lagged levels (Difference GMM). The identifying assumption is that unmodelled time variation is not correlated over time, this implies that internal instruments are valid (the lagged levels of the dependent variable are not correlated with the current error term). This assumption can be relaxed by for instance allowing the error term to follow an MA(1) process (and thereby using later lags of the dependent variables) and it is also testable (see the robustness section in the appendix).

The System GMM estimator extends the set of moment conditions used in estimation by including the original equation in levels now instrumented with the differenced lagged dependent variable to avoid correlation with $\eta_{s,i}$. The added moments often yield more precise estimates but they require additional assumptions about the initial conditions of the process. We asses the validity of this assumption in Appendix (B) by comparing System and Difference GMM estimates, and generally the estimate of δ is similar but the System GMM estimates are preferred as they add extra degress of freedom to estimate the effects of variations in economic actvity.

Our empirical strategy is originally designed for samples where the cross-section dimension is large (we exploit variation across cohorts), and therefore we also discuss the dangers associated with the use of these estimators in limited samples in Appendix B. Finally we also discuss how the preferred specifications were chosen in the estimation process and we present results from tests of the validity of the identifying assumptions (Appendix C). In particular the Hansen test of overidentifying restrictions and a test for serial correlation developed by Arrelano & Bond (1991). We have also tested our estimation results to the sensitivity of allowing the error term to follow an MA(1) process. Our results below are robust to both characterizations of the error term except for the sample of high educated youth, therefore we present results where we use the later specification (i.e. allowing for an MA(1) process in the

TABLE 1: RESULTS FOR THE YOUNG SAMPLE

		Low educ.	Medium educ.	High educ.
δ	Lagged EPR ($e_{s,i,t-1}$)	0.458**	0.484**	0.407**
		(0.0617)	(0.0478)	(0.100)
β_0	Output gap (y_t)	0.468**	0.706**	1.038**
		(0.119)	(0.109)	(0.346)
β_1	Lagged output gap (y_{t-1})	0.147**	0.0839	-0.244
		(0.0553)	(0.0524)	(0.468)
β_2	Output gap×male ($y_t \cdot s_i$)	0.758**	0.506**	-0.0577
		(0.0801)	(0.0896)	(0.488)
β_3	Lagged output gap×male ($y_{t-1} \cdot s_i$)	-0.436**	-0.560**	0.362
		(0.0513)	(0.0553)	(0.577)
θ	Age dummy (25-30)×output gap	-0.0462	-0.0608	-0.467*
		(0.118)	(0.0926)	(0.273)
θ	Age dummy (30-35)×output gap	-0.0803	-0.0335	-0.587**
		(0.121)	(0.0897)	(0.273)
ψ	Male (s_i)	12.13**	6.788**	8.862
		(2.217)	(2.418)	(5.827)

Note: Clustered standard errors in parentheses * p<0.10 ** p<0.05. EPR is multiplied by 100 and age is divided by 10. Estimates produced using the System GMM estimator; internal instruments used are : Predetermined: yeardummies, trend, gender, age; Contemporaneously endogenous: outputgap, employment levels (see appendix)

error term).

1.5 Estimation results

This section presents our estimation results for the *system GMM* estimator.[21] As noted above the sample is split by age and education. The robustness of the results to alternative specification of the dynamic structure of (1.1) is discussed in Appendix B&C, but generally the dynamics and in particular the estimates of δ from these specifications are very similar to those reported below.

Results for the young sample

Our preferred estimates for the young sample are reported in Table 1. Note that the model pools the data for women and men[22], but allows for different responses for the two groups (β_2, β_3), cf. (1.1). The key parameter of interest is δ capturing the autoregressive part of the employment process. For all samples the parameter is in the range 0.4-0.5, This implies that deviations from the generic life-cycle path are only weakly persistent and less than 5 % of the initial impact remains after 6 years.

[21] Extended estimation output with results from FE estimation, OLS and *difference* GMM are available upon request. See also table B.1

[22] We have also estimated the model separately for women and men, but the results are qualitatively similar (see Table B.2) in the appendix.

This is suggestive that the endogenous persistence mechanism is not particularly strong within these samples.Dynamics in the employment rate is driven both by the forcing term (y_t) and the lagged employment rate. To see the dynamic response to a change in the forcing variable we report the impulse response functions below to clarify the dynamic responses.

The estimation captures that men tend to have higher employment rates than women. The difference is declining in the educational level and among highly educated the difference is weak and insignificant. For medium and low educated, males typically experience a larger impact from shocks to output than females, but we also see larger lagged responses from output gaps suggesting that males also recover faster in low and medium samples (the precision is very low in the highly educated sample). From Figure 1 it is seen that the variability of employment is at about the same level for women and men. This suggests that the higher short-run sensitivity to GDP shocks of men is counterbalanced by their faster adjustment. However, the two sources of variation have very different implications for persistence. We consider this issue in more detail below.

Finally, high educated youth are more vulnerable to the cyclical fluctuations in their early years on the labour market (the θ coefficient). For low and medium samples, although insignificant, the estimates imply that the initial impact of fluctuations in output decreases by around 10-20% (4%) for females (males) when individuals are above 25, for high educated individuals the impact is reduced by almost 50% for both males and females. Taking into account the interaction effect of age and economic activity by adding θ, β_0 (and β_1) suggests that education insulates towards fluctuations in output to some degree (this can also be seen in the impulse response functions reported below). This is also found for other countries (see e.g. Bell & Blanchflower (2011) for evidence for OECD countries and Liu, Salvanes and Sørensen (2013) for evidence from Norway).

Results for the old sample

The results for the old sample are reported in Table 2.The autoregressive parameter δ is much higher for the old sample for all three educational groups. Especially in the case of medium education, the coefficient is not significantly different from one, suggesting a strong degree of persistence in the response of employment to business cycle shocks. The significant interaction between gender and the business cycle shock, especially for the low and medium educated, confirms that men are on impact more exposed to business cycle shocks than women, also for the old sample. For highly educated there is generally a smaller (although still significant) difference in the adjustment process for males and females. The effect of the transition scheme is declining in the education level, the accumulated effect is substantial as the scheme reduces the employment level by 1% each period in the low sample. Finally, we see that the employment response to shocks to the economy is generally increasing with

TABLE 2: RESULTS FOR THE OLD SAMPLE

		Low educ.	Medium educ.	High educ.
δ	Lagged EPR ($e_{s,i,t-1}$)	0.874**	0.943**	0.889**
		(0.0624)	(0.0650)	(0.0605)
β_0	Output gap (y_t)	0.269**	0.267**	0.188**
		(0.0920)	(0.0752)	(0.0399)
β_1	Lagged output gap(y_{t-1})	0.0307	0.0172	-0.0713**
		(0.0515)	(0.0469)	(0.0223)
β_2	Output gap×male ($y_t \cdot s_i$)	0.448**	0.353**	0.121**
		(0.0600)	(0.0467)	(0.0432)
β_3	Lagged output gap×male ($y_{t-1} \cdot s_i$)	-0.402**	-0.385**	-0.0886**
		(0.0351)	(0.0454)	(0.0331)
θ	Age dummy (50-54)×output gap	-0.00331	-0.0486	0.0632**
		(0.0606)	(0.0684)	(0.0302)
θ	Age dummy (55-57)×output gap	0.245	0.279*	0.192**
		(0.180)	(0.147)	(0.0694)
ψ	Male (s_i)	-1.063	-1.431	-2.073
		(1.046)	(0.907)	(63.34)
γ	Transition scheme ($z_{i,t}$)	-1.059**	-0.553*	-0.221
		(0.353)	(0.281)	(0.132)

Note: Clustered standard errors in parentheses * p<0.10 ** p<0.05. EPR is multiplied by 100 and age is divided by 10. Estimates produced using the System GMM estimator; internal instruments used are : Predetermined: yeardummies, trend, gender, age; Contemporaneously endogenous: outputgap, employment levels (see appendix)

age within the old sample. Since employment levels are much more sensitive to cyclical fluctuations with age, it follows that variations around the mean increase with age, cf. Figure 2.[23] This finding is robust across educational levels, but is stronger for the low and medium educated.

Comparing the young and old sample it is not obvious who is most affected by recessions. The EPRs of young cohorts are more sensitive to the business cycle situation (especially for the very young), but they recover relatively fast. The EPRs of the older cohorts are less sensitive to the business cycle but highly persistent. The results suggest that young individuals are more exposed to unemployment risks than older individuals during recessions, but that the duration of the average unemployment spell is much longer for the elderly. This could be explained by the fact that non-employment becomes an absorbing state for some old individuals bridging to early retirement (see e.g. Gruber & Wise, 1999). These issues are explored further in relation to the impulse response functions reported below.

[23]Note that the high δ also generates increasing variability around the mean for later ages.

1.6 Are the employment costs of recessions cohort specific?

In this section we assess the dynamic properties of the estimated model presented above. We then consider the implications for lost generations. The notion of lost generations can be interpreted in different ways. We consider two interpretations. The first is that cohorts exposed to a decline in employment at a particular age never fully recover from this set-back, and the employment rate will, as a consequence, be lower permanently. The other interpretation is that particular cohorts have been exposed to a sequence of (large) negative employment shocks, and although they may recover in terms of employment at a later stage, they still have experienced a significant decline in life-time employment. We consider these two notions of lost generations in turn. Finally, we point out that by virtue of the overlapping generations the effect of persistency on the aggregate employment rate is mitigated by the fact that particular generations retire from the labour market.

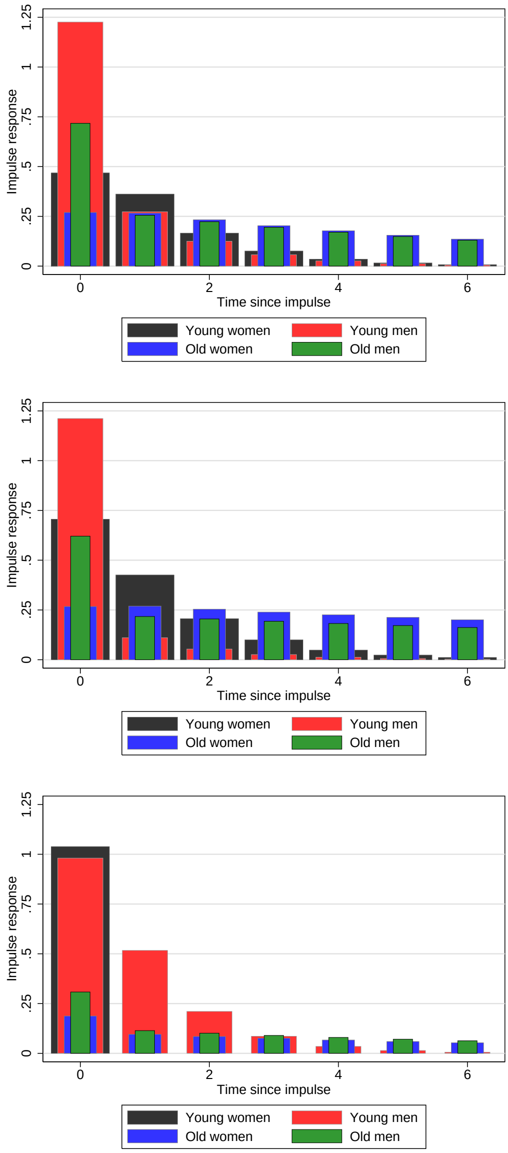
Endogenous persistence

To explore endogenous persistence we consider impulse response functions for employment rates. The impulse response function is generated by exposing employment to a temporary exogenous business cycle shock[24] (a one-time change in the output gap) and evaluating the employment path implied by the dynamics inherent in (1.1). In this sense the response is derived from endogenous mechanisms separate from the properties of the shock (which in itself only justifies a one-time change in employment and subsequent adjustment). The figures below plot the deviations from the underlying counter-factual process where there is no shock, such that the figures show how the employment rate deviates from its standard life-cycle path as a consequence of the temporary shock. The impulse response functions document clear age differences in the response to an output shock. Young males are most affected whereas the initial response of older workers is much lower (recall that this is at the age 45, the prime aged workers) across all education levels, but due to high estimates of δ they only recover partly within the period studied. In fact older workers have the highest response 3 years after the shock in the low and medium samples. Six years after the impulse less than 5 % of the initial shock remains for young women whereas for the older women more than 50 % of the initial shock still remains for the low educated. In fact our estimates suggest that older females with low or medium education never recover fully (before retirement).

Males show higher initial response to an economic shock. In low and medium samples young women recover more slowly such that in period 2 and onwards younger women show larger deviations compared to young males. The impulse response functions show a clear ordering by education where males with low or

[24] The shock is a 1 % output gap in the impact year. In subsequent years the output gap is set to zero.

FIGURE 5: IMPULSE RESPONSE FUNCTIONS FOR AGES 20- AND 45-



Note: The impulse responses for the young have impact at the age of 20 (23 in high sample), and for the old at age 45.The panels only include impulse responses significantly different from zero (10 % level). Standard errors are found by bootstrapping.

TABLE 3: ACCUMULATED EFFECT ON EMPLOYMENT RATES (IN %)

	Low education		Medium education		High education	
Age at impact	Women	Men	Women	Men	Women	Men
20	1.12	1.72	1.52	1.42	1.34	1.85
31	0.98	1.58	1.46	1.36	0.35	0.86
45	1.44	1.84	1.66	1.75	0.62	0.83
54	2.62	3.07	3.32	3.40	1.59	1.80

Note: Calculated as the accumulated deviations within 6 years after a positive business cycle shock of 1 % compared to without any shock

medium education levels are more sensitive to output fluctuations whereas older highly educated workers respond very little.

It is important to note that the employment response for the cohort depends critically on the age at which the shock occurs especially for the old sample. Above we have the shock to appear at the earliest age within the two age groups (age 20 and 45, respectively). In Table 3 accumulated employment changes for different impact ages are reported by the accumulated deviations from mean employment rates 6 years after a positive shock to an output gap of 1 %. This is at the same time an alternative way by which to summarize the effects of the endogenous persistence represented in the impulse responses above. For all educational groups there is a U-pattern in age; that is, the accumulated deviations are largest if the shock appears for the youngest and the oldest. This brings out that the very young and old tend to carry the largest employment consequences of business cycle fluctuations. In fact the effects are largest for old workers in low and medium samples. This finding complements the findings from Figure 5 by underlining that among the young it is the youngest who are most affected by business cycle shocks (and among the old it is the oldest workers).

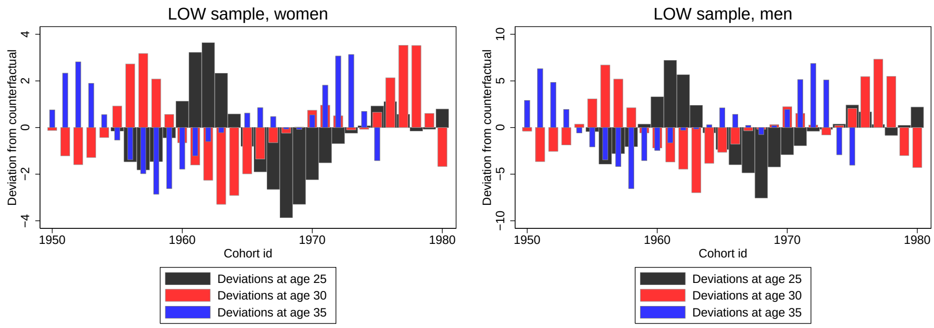
The general insight from Table 3 is that business cycle shocks have important impacts on subsequent labour market performance in the short and medium term, and that this is particularly so for low and medium educated, for older workers and for women. Consistent with the empirical micro economic literature we find that being exposed to a negative or positive shock to employment may have longer lasting effects on the future labour market status (see e.g. Doiron & Gørgens (2008), Hartman et al. (2010), and Lesner (2014)).

Generational effects of economic shocks

Although endogenous persistence is not that strong for most groups (especially the young) it may still be that certain cohorts have been lost or are losing generations as a result of having been exposed to a sequence of adverse shocks during youth. Even though employment rates recover for the cohort, the life-time income or type of employment may still be affected.

To quantify the effects of being exposed to business cycle shocks, realized em-

FIGURE 6: DEVIATION IN EMPLOYMENT RATES FOR DIFFERENT COHORTS, LOW EDUCATED/YOUNG SAMPLE



Note:Computed as deviations from the predicted EPR from the counterfactual EPR where no shocks occur.For each cohort we use the actual series of gaps in output which this particular cohort faced in their life-cycle and compare this to a situation with an output gap of 0.

ployment rates are compared to employment rates in the counterfactual case with no output gaps. Figure 6 shows the accumulated employment deviations at ages 25, 30 and 35 for both men and women with low education across birth years in the young sample.[25] A generation with negative deviations at all three age levels has been affected particularly hard by unfavourable business cycle situations. The lowest employment rate during the observation period was observed in 1993. This is clearly reflected in Figure 6. For, e.g., the cohort from 1964 there is a large negative deviation at age 30. A similar pattern is seen for the other cohorts. It is also clear for, e.g., the 1964 cohort that although they have a much lower employment rate at the age of 30 due to the economic slowdown in the early 1990s, they are back on track at age 35, where they actually have a slightly higher employment rate than the counterfactual situation.

Comparing across cohorts in Figure 6 it is not possible to identify a cohort which is consistently below or above the counterfactual employment level at all three age thresholds considered.[26] In this sense it is not possible to identify a cohort that has carried a disproportionately large burden (gain) of business cycle fluctuations. Figure B.1 in the appendix presents a similar calculation for the old sample, the figure illustrates that the deviations are more persistent here as suggested by our estimates and analysis above.

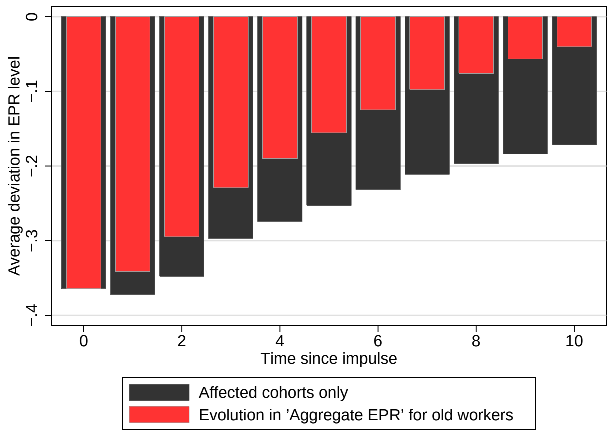
Implication for macro level persistence

The aggregate employment rate at a given point in time is a weighted average of the employment rates for the particular cohorts that are of working age at that point.

[25]We only show the results for low educated as these are generally most affected by business cycle shocks. The results for the other educational groups are of course available from the authors upon request.

[26]This observation is not dependent on the specific ages reported in the figure. The pattern is consistent across different choices of ages.

FIGURE 7: AVERAGE DEVIATIONS FROM COUNTERFACTUAL EPR



Note: The black bars follow the deviations from the predicted EPR from the counterfactual EPR where no shocks occur for affected cohorts only. The red bar follows the evolution of aggregate EPR in the old sample where a new unaffected cohort enters each time period and an affected one exits.

Each year a new cohort enters and an old retires from the labour market. This implies that there is an important distinction to be made between persistence embedded in the employment trajectory of a particular cohort, and the persistence arising at the aggregate level. Even if there is strong persistence at the cohort level, it will be less strong at the aggregate level where account is taken of the fact that cohorts retire. This also points out that one should be careful in interpreting measures of persistence derived from aggregate employment measures. An explicit account of overlapping generations/cohorts may thus give a more detailed and precise assessment of persistence in the labour market than the standard time series approach suppressing this source of dynamics. This is also an argument in favour of the meso approach pursued in this paper.

To illustrate this, Figure 7 displays persistence within the old sample. Consider a temporary shock to output which, in turn, affects employment for all cohorts within the sample in the given year. In the next year a new cohort has entered the old sample and one cohort has left, and so on and so forth. The results presented earlier show that persistence is lower for younger cohorts and as an approximation we assume there is no endogenous persistence for entering cohorts. Eventually, cohorts having been exposed to the shock are no longer present in the sample. The red bars in Figure 7 display the impulse response function for the employment for all in the old age group when account is taken of the overlapping generations dynamics. The black bar is the case where no account is taken of this dynamics. The figure illustrates why analysing the EPR over time, for instance for older workers, tends to misrepresent persistence.

1.7 Concluding remarks

The financial crisis and the steep increase in youth unemployment have received considerable attention. A particular concern is that the costs are embedded in particular cohorts entering the labour market during the slump, and that they therefore are becoming lost generations.

In discussing the employment consequences of slumps it is important to make a distinction between the impact effect and the adjustment process. We develop an empirical set-up which allows an explicit separation between the two and explicitly takes a cohort perspective building on the life-cycle pattern of labour market participation. The empirical analysis is performed on Danish data.

In line with the literature we find that employment of both young and old are particularly sensitive to the business cycle situation. For low and medium educated male individuals a 1 % drop in the output gap implies a 1.2 % drop in the level of employment ratio in the year of the shock. The effects are more pronounced for less educated. However, there are important differences in the adjustment process across groups, which have crucial implications for the discussion of lost generations. In general our estimates suggest that the adjustment process is relatively quick for the young. Male employment rates recover relatively fast (3 years), whereas 40 % of the initial smaller drop in women employment rates remain 3 years after the shock. Although men experience larger initial impact, our estimates suggest that the young women suffer more in terms of accumulated losses due to the slower adjustment process. In general the findings suggests that endogenous persistence is stronger for the old than the young.

The notion of lost generations can be interpreted in two ways. One interpretation is that a temporary (large) employment drop will imply that employment for the cohort is affected for a sequence of years, possibly permanently. This is so-called endogenous persistence where various mechanisms in the labour market imply that a cohort never or only very slowly recovers from set-backs to employment. Another interpretation is that particular cohorts have been exposed to sequences of adverse shocks which over a longer period have implied below normal employment rates, and although employment rates later recover these cohorts have experienced significant losses in life-time income. We do not find strong support for lost generations in either interpretation. Endogenous persistence is in general weak, with the exception of older women with low or medium levels of education. While some cohorts have been exposed to negative shocks over a number of years it tends to even out in the sense that they are later exposed to favourable shocks.

We see the present analysis as complementary to micro and macro studies, and the main finding of the present analysis is that while there is persistent effects at the individual level it does not aggregate to persistence at the cohort level. Hence, the present analysis does not lend support to particular policy measures being directed at avoiding persistent effect or lost generations for the young. Persistent effects of a fall in employment is strongest among the old with a less strong educational background and there are thus arguments in support of specific labour market policies being targeted to his group.

There are two important caveats to our findings in relation to the general discussion about the financial crisis and youth unemployment. The present recession is both deeper and more prolonged compared to the business cycle downturns in our sample. Therefore the potential loss of human capital and the scarring effect due to employment losses may be both stronger and of a different nature than previous crises in our sample. Another important point is that the Danish labour market is rather flexible in a European context. Gross job creation and destruction rates are high in a comparative perspective, and have remained so during the crisis. It may thus be easier for youth to enter the labour market when there always is a relatively high level of job openings. Accordingly the dynamics in the Danish labour market may differ in important respects from other countries, especially in relation to youth. It is an interesting question for future research to explore more carefully how dynamics and persistence mechanisms depend on labour market institutions and policies.

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1.9 Appendix A

Data

The empirical analysis is based on IDA, a Danish register-based annual matched employer-employee panel covering the whole population in the years 1980-2008. The dataset is kept by Statistics Denmark. The data are confidential, but access is not exclusive. The unit of observation is a given individual in a given year with measurements generally referring to the last week of November. From this data set we identify the cohort-relationship of each individual and we determine the educational level of the individual based on his obtained education around age 30 or higher if not present. As explained in the text above, we proceed by constructing 3 samples based on the educational level of the individual (low, medium and high).[27] Table A.1 gives an example of how a cohort is distributed across sub-samples. In each sample

TABLE A.1: SAMPLE SIZE FOR A COHORT (1955) IN THE DATA

Men	Low educ.	Medium educ.	High educ.
Young sample	11524 (31%)	15220 (41%)	10026 (28%)
Old sample	10772 (31%)	14701 (42%)	9526 (27%)

Women	Low educ.	Medium educ.	High educ.
Young sample	12049 (35%)	10306(30%)	12368 (35%)
Old sample	11662 (34%)	10170 (30%)	12123 (36%)

we calculate the employment to population ratio (EPR) for each cohort in a given year separately for males and females. We use the cohorts with individuals born from 1935 to 1980 (this secures a minimum of 9 observations per cohort). Note that our sample has truncated spells, i.e. young generations with incomplete spells and older generations where the previous employment spell is not known (see Figure A.1). The definition of employment relates to the status of the individual in the labour market (i.e. does the person have a job), and furthermore we have an earnings requirement (properly deflated to the given year) such that our employment variable captures something as close as possible to regular employment over the past year.

[27] For a more detailed documentation on IDA see http://www.dst.dk/HomeUK/Guide/documentation/Varedeklarationer/emnegruppe/emne.aspx?sysrid=1013.

FIGURE A.1: OUTPUT GAP SERIES FOR THE SAMPLE PERIOD

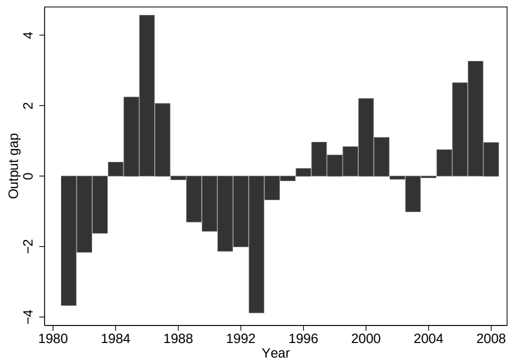


FIGURE A.2: PRESENTATION OF COHORTS IN THE SAMPLE

OLD

Young

Cohort\Year	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008
1935	45	46	47	48	49	50	51	52	53	54	55	56	57																
1936	44	45	46	47	48	49	50	51	52	53	54	55	56	57															
1937	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57														
1938	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57													
1939	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57												
1940	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57											
1941	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57										
1942	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57									
1943	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57								
1944	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57							
1945	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57						
1946	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57					
1947	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57				
1948	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57			
1949	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57		
1950	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	
1951	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57
1952	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56
1953	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55
1954	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54
1955	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53
1956	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52
1957	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51
1958	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50
1959	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49
1960	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48
1961		20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47
1962			20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46
1963				20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45
1964					20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44
1965						20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43
1966							20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42
1967								20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41
1968									20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40
1969										20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39
1970											20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38
1971												20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37
1972													20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36
1973														20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35
1974															20	21	22	23	24	25	26	27	28	29	30	31	32	33	34
1975																20	21	22	23	24	25	26	27	28	29	30	31	32	33
1976																	20	21	22	23	24	25	26	27	28	29	30	31	32
1977																		20	21	22	23	24	25	26	27	28	29	30	31
1978																			20	21	22	23	24	25	26	27	28	29	30
1979																				20	21	22	23	24	25	26	27	28	29
1980																					20	21	22	23	24	25	26	27	28

1.10 Appendix B: Estimators

Difference and System GMM

To address the identification problem mentioned in the main text the diffenrence GMM estimation procedure transforms the empirical model into first differences and then uses various instrumental strategies to identify δ. Consider a simple AR(1) process with unobserved heterogeneity in first differences (we exclude the gender dimension and explanatory variables in the following to economize on space, but results easily generalize):

$$e_{i,t} - e_{i,t-1} = \delta\left(e_{i,t-1} - e_{i,t-2}\right) + \left(\eta_i - \eta_i\right) + \left(\epsilon_{i,t} - \epsilon_{i,t-1}\right) \tag{1.3}$$

$$E\left(\eta_i\right) = E\left(\epsilon_{it}\right) = E\left(\eta_i \epsilon_{it}\right) = 0 \tag{1.4}$$

Note that equation (1.3) still produces biased estimates using least squares routines due to the correlation between $\epsilon_{i,t-1}$ and $e_{i,t-1}$. δ can, however, be consistently estimated using a GMM/IV procedure based on the assumption that

$$E\left(e_{i\tau} \triangle \epsilon_{it}\right) = 0 \text{ for } \tau \leq t-2$$

This procedure follows the work of Anderson & Hsiao (1981) and Arrelano & Bond (1991) and is often referred to as *difference GMM*. The crucial assumption is that the error term is truly idiosyncratic and independent across cohorts and time. It implies that we from $t = 3$ and onwards will have an increasing number of instruments for each subsequent period (where $t = 1$ measures the first observation we have for a given cohort). In period 3 we have e_{i1} as the only instrument for $\triangle e_{i,t-1}$, in period 4 we have e_{i2} and e_{i1}, and so on. This way the number of instruments increases linearly in t. Finally one can also allow for serial correlation in the error term in the form of $\epsilon_{it} = v_{it} + \rho v_{it-1}$ by changing the identifying moment conditions such that $E\left(e_{i\tau} \triangle \epsilon_{it}\right) = 0$ for $\tau \leq t-3$ and this way change the instrument set. Our results in the main text are generally robust to both characterizations of the error term.

Blundell & Bond (1998) show that the procedure above could cause large finite sample biases when using the levels of the dependent variable as an instrument in the differenced equation in models with moderately persistent series and moderately short panels due to a problem of weak instruments. In these cases the lagged dependent variable is simply a bad predictor for the current change in levels, and the estimates become biased in the direction of the within estimator. In line with Arrelano & Bover (1995), Blundell & Bond (1998) show that there are likely to be efficiency gains and bias reductions from incorporating more "informative" moment conditions.

They suggest a *system GMM* estimator that combines both the equation in levels and the equation in first differences. Here $\triangle e_{i\tau}$ is used as an instrument for $e_{i\tau}$ in the level equation and $e_{i\tau-1}$ is used as an instrument for $\triangle e_{i\tau}$ in the difference equation. The moment conditions related to the different equations are estimated jointly. This approach corresponds to the following moment conditions:

$$E\left(e_{i,\tau}\triangle\epsilon_{i,t}\right) = 0 \text{ for } \tau \leq t-2$$
$$E\Big(\triangle e_{it-\tau}\left(\eta_i + \epsilon_{it}\right)\Big) = 0 \text{ for } \tau = 1 \text{ and } t \geq 3$$

The additional level equation $\left(\eta_i + \epsilon_{it}\right)$ implies that we get an extra moment condition for each t compared to *difference GMM* (as later lags of the instrument become redundant when used together with the difference equation). Note that the validity of the moment conditions used in *difference* GMM and the extra set of moment conditions implied by *system GMM* can be phrased as restrictions on the initial condition for the process generating e_{i1}. For *difference GMM* this amounts to the assumption that the initial level of e_{i1} is uncorrelated with $\epsilon_{it}\forall t$, which is fulfilled if the error-term is truly idiosyncratic or MA(1). For *system GMM* the additional requirement is that $\triangle e_{it1}$ is uncorrelated with η_i such that the initial deviations from the long-run mean of the process are not correlated with the long-run mean itself. Arrelano & Bover (1995) link this to a requirement of stationarity of the process of the instrument, such that we require the first moments of the instruments to be time-invariant (conditional on e.g. common year dummies). Relating the assumptions to our model in (1.1) they imply that differences in the changes in the EPR (e) between cohorts are not related to structural differences embedded in η_i.

The procedures presented above easily extend to the case with explanatory variables, which will generally be used as "instruments" for themselves. The moment conditions related to the explanatory variables depend on the assumptions made on the relationship between these and $\epsilon_{it}\forall t$. If the regressors are treated as predetermined (such that present values of ϵ_{it} potentially affect future values of x'_{it}), the conditions are $E\left(x_{i\tau}\triangle\epsilon_{it}\right) = 0$ for $\tau \leq t-1$ when no lags appear in $x_{i\tau}$ for *difference GMM*. In our preferred specification we treat explanatory variables related to the output gap as predetermined and potentially contemporaneously endogenous. Age $\left(a_{it}\right)$, gender $\left(s_i\right)$ and policy programmes $\left(z_{it}\right)$ are treated as strictly exogenous.[28] We therefore use $e_{i,t-2}$ and $e_{i,t-3}$as instruments for $\Delta e_{i,t-1}$ and $y_{i,t-1}, y_{i,t-2}$ and $y_{i,t-3}$for instruments for $\Delta y_{i,t-1}, \Delta y_{i,t}$.

On the basis of the presented moment conditions, we form a generalized metric and solve the minimization problem using the GMM framework. We do not use optimal (two-step) GMM estimators due to the limited sample size in the cross-section dimension that will generally make our estimators worse behaved. This is due to the fact that we are essentially estimating fourth moments of the underlying distribution when determining the weight matrix (see e.g. Hayasi (2000)). Our weight matrix is therefore the Identity Matrix in *difference GMM* and a slightly modified version in *system GMM* (see e.g. Arrelano & Bover (1995)).

[28]We have tested the assumption on z_{it}, and our results are robust to this assumption.

Estimation Procedures

Due to a number of over-identifying restrictions (the number of moment conditions exceeds the number of estimated parameters) in the estimators it is possible to apply standard tests to make inference on the validity of these restrictions. Generally these tests rely on large samples, and they should therefore be used with caution. Specification tests that barely exceed "conventional significance levels" can be misleading as these tests are known to be undersized (the test hardly never rejects the hypothesis) when the instrument count is high. As we have limited data in the cross-section dimension, we will also limit the number of lags of the dependent variable that we include as an instrument. The number of instruments used is generally higher than the number of cohort/gender units in the data (but naturally not observations). A large number of instruments compared to the sample is likely to over-fit endogenous variables and also weaken the Hansen test of over-identifying restrictions.

We follow the practice suggested by Roodman (2009) and test each specification for the sensitivity to reductions in the number of instruments. As a standard we use a maximum of two lags of the (differenced) dependent variable as an instrument for the equation in differences (levels). Furthermore, we try various lags in order to allow for the presence of unaccounted for MA(1) errors in ϵ_{it}. Our results below are robust to these changes. The standard Sargan and difference Sargan tests are inconsistent in the case of heteroskedasticity, and therefore we also use the Hansen test as a guideline (even though they require estimations using a two-step GMM procedure which we generally do not use). Furthermore, we use the test for serial correlation developed by Arrelano & Bond (1991) to determine the validity of our identifying assumptions regarding serial correlation in the error term. The result from these tests and various robustness exercises are reported below.

Our preferred specification is the *system GMM* estimator but generally the estimates are very similar using any of the two estimation procedures, this is documented in Table B.1 which gives estimates of δ using either Difference or System GMM.

Our empirical strategy pools the data for women and men[29], but allows for different responses for the two groups (β_2, β_3), cf. (1.1). In Table B.2 we present estimates on δ in (1.1) when we do the estimation separately on gender to test our assumption that δ does not vary with gender. As can be seen from the table the estimates does not vary significantly across gender.

Lastly in Table B.3 we assess the importance of including timedummies in the estimation. As explained in the main text we do not include dummies for every year in the sample as that would weaken identification of the (β_2, β_3). Instead we add dummies for selected years where diagnostic plots and teststatistics suggest they are important. In the Table we compare 2 different models (a model where we include a

[29]We have also estimated the model separately for women and men, but the results are qualitatively similar (see Table A.3).

TABLE B.1: COMPARING DIFFERENCE AND SYSTEM GMM ESTIMATES ACROSS SAMPLES

Age	Education	Difference GMM	System GMM
Young	Low	0.414 (0.040)	0.458 (0.062)
	Medium	0.433 (0.033)	0.484 (0.047)
	High	0.409 (0.081)	0.407 (0.100)
Old	Low	0.815 (0.043)	0.874 (0.062)
	Medium	0.840 (0.071)	0.943 (0.065)
	High	0.653 (0.115)	0.813 (0.066)

Note: Estimates are produced using the moment conditions presented in the text above.

TABLE B.2: COMPARING SYSTEM GMM ESTIMATES WHEN STRATIFYING THE ESTIMATION ON GENDER

Age	Education	Women	Men
Young	Low	0.560 (0.0444)	0.625 (0.0523)
	Medium	0.547 (0.0323)	0.390 (0.0619)
	High	0.522 (0.100)	0.552 (0.058)
Old	Low	0.934 (0.063)	0.935 (0.046)
	Medium	0.931 (0.0734)	0.956 (0.0625)
	High	0.854 (0.065)	0.788 (0.070)

Note: Estimates are produced stratifying on gender and thereby implicitly testing our assumption of the same δ across gender. Estimates are produced using the moment conditions presented in the text above.

TABLE B.3: EXTENDED ESTIMATION OUTPUT (YEARDUMMIES)

Age	Education	Preferred model*	Fully saturated model**	Reduced model***
Young	Low	0.458 (0.06)	0.439 (0.14)	0.512 (0.04)
	Medium	0.484 (0.05)	0.439 (0.12)	0.497 (0.04)
	High	0.407(0.10)	0.351 (0.35)	0.396 (0.16)
Old	Low	0.874 (0.06)	0.954 (0.10)	0.776 (0.08)
	Medium	0.943 (0.07)	0.964 (0.13)	0.878 (0.07)
	High	0.889 (0.06)	0.713 (0.21)	0.810 (0.06)

* The basic specification used in the main text, ** a model where a full set of year dummies is included in the explanatory variables

(moment conditions remain unchangedcompared to preferred model)

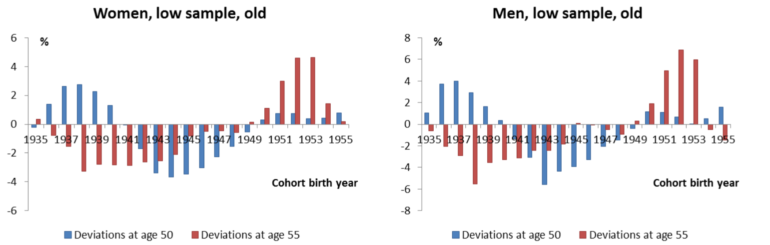
*** a model without yeardummies as explanatory variables (or moment conditions)

full set of dummies and a model where we do not use any year dummies) to our preffered model. As can be seen from the table the estimates are generally very similar.

TABLE A.3: EXTENDED ESTIMATION OUTPUT (YEARDUMMIES)

Education	Low		medium		high	
Age	Young	Old	Young	Old	Young	Old
Trend	-0.119*	0.0872	-0.0103	0.0434	0.0409	-0.0238
	(0.0688)	(0.0760)	(0.0753)	(0.0680)	(0.193)	(0.0193)
Trend*Male	-0.465**	0.0908*	-0.226	0.0928**	-0.207	0.0519**
	(0.120)	(0.0369)	(0.137)	(0.0322)	(0.406)	(0.0173)
Year 2008	-1.036**	-0.192	-0.180	0.00864	-0.311	0.213*
	(0.349)	(0.257)	(0.394)	(0.224)	(0.636)	(0.120)
Year 2001	0.277*	0.461**	0.388**	0.236**	0.286	0.426**
	(0.150)	(0.108)	(0.122)	(0.0945)	(0.394)	(0.0706)
Year 1993	0.982**	0.822**	1.377**	0.655**	1.283	0.754**
	(0.267)	(0.344)	(0.234)	(0.286)	(1.033)	(0.125)
Year 1998	0.852**	0.568**	0.841**	0.782**	0.721**	0.442**
	(0.142)	(0.131)	(0.110)	(0.129)	(0.316)	(0.0785)
Year 1986	-2.239**	-1.824**	-2.605**	-1.363**	-1.697**	-0.831**
	(0.313)	(0.297)	(0.274)	(0.274)	(0.810)	(0.192)
Age ($a_{i,t}$)	2.568	-0.386	-1.091	2.466	152.6**	8.075
	(3.646)	(4.155)	(3.064)	(4.251)	(27.90)	(12.90)
Age squared ($a_{i,t}^2$)	0.242	-0.257	0.256	-0.481	-23.01**	-1.082
	(0.571)	(0.442)	(0.524)	(0.469)	(4.059)	(1.249)
Constant	23.88**	14.09	40.38**	2.829	-206.7**	-4.320
	(4.219)	(11.20)	(3.659)	(9.904)	(41.11)	(5.777)
Obs. number	764	520	764	520	632	520

FIGURE B.1: DEVIATION IN EMPLOYMENT RATES FOR DIFFERENT COHORTS, LOW EDUCATED/OLD SAMPLE



Note:Computed as deviations from the predicted EPR from the counterfactual EPR where no shocks occur.For each cohort we use the actual series of gaps in output which this particular cohort faced in their life-cycle and compare this to a situation with an output gap of 0.

Extended estimation output

Table B.3 presents estimates of variables included in the main specification but not presented in the text (in Table 1 & 2).

TABLE C.1: PREFERRED MODELS

	Young sample			Old sample		
	Low ed.	Medium ed.	High ed.	Low ed.	Medium ed.	High ed.
Hansen Test	40.48	39.39	55.90	31.08	40.72	24.63
AB test AR(2)	-1,23	-1,13	2,66	0,03	-1,26	0.35
Difference Hansen'	0.82	-9.98	1.67	2.14	6.87	0.01
Number of observations	764	764	632	520	520	520
Number of "instruments"	118	116	68	107	107	107

*The difference between the two Hansen tests when we exclude the levels equation and when we include it in the estimation.

TABLE C.2: WHEN NUMBER OF INSTRUMENTS ARE MINIMIZED

	Young sample			Old sample		
	Low ed.	Medium ed.	High ed.	Low ed.	Medium ed.	High ed.
Hansen Test	48.96	46.21	8.28	17.66	23.65	19.57
AB test AR(2)	0.07	0.42	1.30	0.90	0.83	0.27
Difference Hansen*	13.93	12.43	1.26	-0.84	-1.72	3.20
Number of observations	764	764	632	520	520	520
Number of "instruments"	62	62	48	52	52	52

*The difference between the two Hansen tests when we exclude the levels equation and when we include it in the estimation.

1.11 Appendix C: Robustness analysis

Table C.1 reports the value of the Hansen test statistic and the number of observations and "instruments" (moment conditions) used in each sample for our preferred model. Generally we do not reject the identifying assumptions of our model for the overidentifying restrictions. Due to the relatively long sample period (1980-2008) in our data the instrument count becomes large in our preferred model, and this limits the validity of the conventional tests. As already mentioned we have tried to estimate specifications where we limit the number of instruments dramatically in order to analyse the performance of our model in these settings. Table C.2 reports the test results from a specification that uses fewer moment conditions but generates very similar estimates to those reported in the text above. We have also tried specifications that rely on later lags in the instrument set to allow for presence of higher order serial correlation (which the Arrelano Bond test (AB test) indicates for some specifications below). This does not change the main predictions of our model, but it is important to keep in mind that restricting the instrument count increases the variability of our estimates. We have explored various other dynamic specifications of our model in order to determine whether our results are driven by badly specified dynamics. Generally we find this not to be the case. Our estimation approach allows us to distinguish between immediate and adjustment costs from recessions.

Sample selection and initial conditions

As a robustness test we have reestimated our model excluding the oldest of our cohorts to test whether our results are simply due to a selected sample[30]. We still find very similar estimates of δ and thereby a long adjustment process for older workers from shocks to output. As already mentioned, the empirical literature suggests that level of education is likely to be affected by the state of the economy. One standard argument is that the marginal costs of further education are likely to be lower in a recession due to lower employment chances. This implies that the sample selection rule may be endogenously determined, and this could affect our results if it implies that some cohorts will respond differently to variations in output than they would had the economic environment been different in their youth.[31] To get an idea about the magnitude of such entry effects, we have made simple regressions where we regress the ratio of highly educated individuals in a cohort on the values of our business cycle measure around ages 20-25. We find a clear trend in our results (in the sense that younger cohorts obtain more education), and we find a small positive insignificant effect from our BC measure on the ratio of individuals with low education. Therefore, as a further robustness check, we have also estimated our model on data where we do not stratify on educational levels. The estimates and the implied impulse responses are in the same order of magnitude as those we find for low and medium educated. Figure C.1 shows the implied impulse responses that we obtain from this estimation exercise. We therefore conclude that our sample selection on educational level have not biased our conclusions.

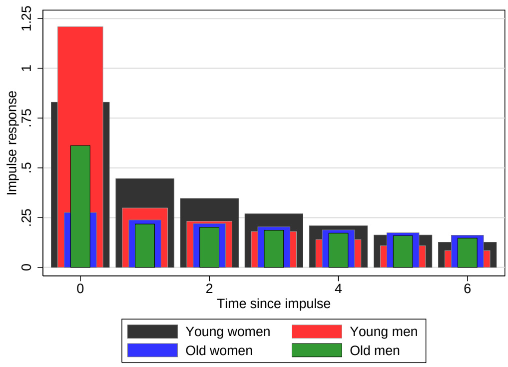
Finally our estimation procedure crucially relies on correctly specified moment conditions which could be interpreted as a restriction on the initial conditions process. Intuitively speaking, if the cohort starts out bad and this affects the rest of its lifecycle such that responses to the business cycle change, our estimates could be biased.

[30] We have also tried restricting the sample in the year domain by deleting observations from the 80's or 00's. Same conclusion applies.

[31] Obviously the time-invariant part of this effect is absorbed in the unobserved heterogeneity term, but time-varying effects could bias our results.

FIGURE C.1: IMPULSE RESPONSES IN RAW DATA BEFORE EDUCATION STRATIFICATION

Note: The impulse responses for the young have impact at the age of 20 and for the old at the age of 45. The panels only include impulse responses significantly different from zero (10 % level). Standard errors are found by bootstrapping.



CHAPTER 2

EXPERIMENTAL EVIDENCE ON THE EFFECTS OF EARLY MEETINGS AND ACTIVATION

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Abstract[1]

We analyse the effects of three Danish labour market experiments involving combinations of early and intensive active labour market policy. We find that frequent meetings between newly unemployed workers have substantial effects on employment rates in both the medium and long run. We perform a subgroup analysis focusing particularly on the differential effects between men and women, young and older workers, under different cyclical conditions, as well as impacts on unemployment and employment duration. In addition, based on detailed information the on costs of running the experiments and on public transfer payments we show that some of the experiments improved government budgets significantly.

[1]Acknowledgement: We are grateful to the Danish Labour Market Board for making data available and for the CAFE grant enabling part of this research, to seminar participants at IAB in Nürnberg, the University of Bergen and SOFI at Stockholm University for valuable comments. Finally we thank Peter Frederiksson and two anonymous referees for very valuable comments and suggestions.

2.1 Introduction

Traditional activation policies (compulsory participation in e.g. workfare or training programmes) are costly, and often do not help in terms of bringing unemployed workers quickly back into regular employment (see e.g. Heckman *et al.* (1999), Kluve (2010), and Card *et al.* (2010). The most effective activation instrument seems to be employment subsidies in the private sector, but unfortunately, private employers are not easily persuaded to participate in such schemes, and it is an often mentioned concern that these schemes displace regular jobs - hence in many countries this instrument cannot be expanded beyond its current limited use. Traditional training programmes (e.g. classroom training) are used much more often, and they sometimes have positive effects when aimed at disadvantaged workers, but the evidence regarding their effectiveness is not compelling, and they are typically very expensive. One of the problems with activation is that initially, it leads to lock-in effects. This is particularly a problem since, ideally, policies should help unemployed as early as possible during unemployment, in order to prevent long-term unemployment. The risk of lock-in and the associated dead-weight losses implies that traditional activation policies are potentially ineffective as preventive measures applied during the early phases of unemployment. In that sense, these policies are remediation measures rather than prevention measures. Hence, the need for effective early active policies is imminent.

In this paper we present results from three recent randomized field experiments involving early and intensive active labour market policies aimed at newly unemployed workers in Denmark with the goal of getting them back into regular employment as soon as possible and thus preventing long-term unemployment. The three experiments we study basically contain a combination of two types of interventions; early and intensive counselling in the form of frequent meetings with case workers in job centres, and so-called 'activation walls'. The later refers to mandatory activation employed fairly early during the unemployment spell with the aim of generating so-called threat effects - the perceived risk of future activation should, according to this line of thought, lead to increased job search prior to participation, see e.g. Rosholm & Svarer (2008) and van den Berg *et al.* (2009). These experiments thus investigate effects of rather novel policy approaches, in both the short and long run, and on different subgroups of workers, enabling us to study how different groups are affected by them. We focus particularly on the differential effects between men and women, young and older workers, under different cyclical conditions, as well as impacts on unemployment and employment duration. We have access to detailed data on the actual implementation of the intended treatment. We show that this is important for understanding and interpreting the results from an experiment, and thus crucial for improving our knowledge of the effectiveness of labor market policies. Finally, we have obtained data on the costs of running the various programmes and the social transfers that participants receive, and we can therefore compare the *real-*

ized programme costs and benefits and how they evolve over time, which is an often neglected part in policy evaluations (Card *et al.,* 2010).[2]

The setting in which we study these policies is the Danish labour market. The Danish labour market model, which is generally referred to as the 'Flexicurity model' and recommended by the EU commission to its member states (European Commission, 2007), is an interesting setting to study these experiments in, as unemployment insurance (UI) is generally very generous (the security component) and the level of employment protection is quite low (the flexibility component). The sustainability of such a system could be challenged by high structural unemployment rates, e.g. due to low incentives for workers to leave unemployment. Active labour market policies (ALMPs) therefore become a pivotal element in ensuring both the availability and the qualification level of the workforce. A recent strand of the literature has focused on optimal design of ALMPs in settings with high UI benefits.[3] Our paper, which is purely empirical, will not be informative on optimal designs, but we can say something about the relative performance of a system with early interventions in the form of meetings or activation walls versus more traditional policies, and thereby we provide some guidance regarding important elements of ALMPs that might be studied in an optimal design context. Our paper thus follows a line of research which is focused on “other aspects” of active policies such as counselling, monitoring and sanctions and other shorter interventions which have a more administrative/institutional character (see e.g. Kluve, 2010) and thus greatly differ from the more traditional activation/training programmes, which have been extensively evaluated.

We find large positive effects of some of the policy components investigated, and there are interesting differences with respect to the type of policy, the age and in particular the gender of the unemployed worker, and the cyclical conditions. Counselling in the form of individual meetings between caseworkers and unemployed workers have significantly positive effects. The effects are especially beneficial for men, where they arise later than for women and seem to arise primarily from longer subsequent employment spells rather than shorter unemployment spells. Activation walls also have a large effect for men, while there is a small temporary negative (lock-in) effect for women. Moreover, the threat effect appears to be present only when labour market conditions are good - we show this point by exploiting that the experiment was conducted right during the tipping point of the business cycle in 2008. Hence, differences in the week of enrolment into the experiment (week of inflow into unemployment) can be used as a proxy for different cyclical conditions. Finally we conduct a cost-benefit (CBA) analysis and find that individual meetings are the most cost effective instrument, as they are much cheaper than activation

[2] Importantly our analysis allows us to take uncertainty from both the benefit side (the estimated employment effect of the experiment) *and* the costs side (the sampling uncertainty in the type of income transfer and ALMPs unemployed individuals participate in)

[3] See e.g. Andersen & Svarer (2007) or Pavoni, Setty & Violante (2013).

walls, and they have slightly larger effects on average.

The rest of the paper is organized as follows: in section 2 we provide a brief overview of the literature on the effects of meetings and threat effects (activation walls). In section 3 we describe the social experiments and the data used for the subsequent analysis. Section 4 contains a presentation of our main results, and we also discuss subgroup effects, more specifically with respect to age, gender, and the business cycle. We also perform a cost-benefit analysis of each intervention. Finally, section 5 contains a conclusion, discussion of policy implications and further research.

2.2 A brief review of related literature

There is an extensive literature on the impacts of 'traditional' activation programmes, see e.g. Heckman *et al.* (1999), Card *et al.* (2010), and Kluve (2010). Policy impacts are typically modest and not always positive.[4] The most favourable effects are found for employment subsidies, while training programmes sometimes have positive effects when aimed at disadvantaged workers. Public job creation shows more negative than positive effects, possibly due to so-called lock-in effects. In this section we will briefly review the literature on meetings and activation walls or threat effects.

Meetings with case workers are a cornerstone of active labour market policies: unemployed workers often register entry into unemployment at meetings, and their eligibility for receiving e.g. UI benefits is assessed. Search effort is monitored at meetings, and if there is non-compliance in the form of no-show, insufficient search or availability, a sanction may be issued. Counselling and job search assistance takes place at meetings. There may be direct referral to vacant jobs, and finally, future participation in ALMPs is discussed and planned at meetings.

Meetings have *ex ante* effects; Hägglund (2011) reports from a randomized experiment conducted in Sweden and shows that, for a broad group of unemployed workers, an invitation to a meeting, aimed at monitoring search activity and assisting with more effective job search, led to an increase in the exit rate into employment by 46% already before the meeting took (or should have taken) place. Black *et al.* (2003) study a profiling tool aimed at identifying workers at risk of long-term unemployment (LTU). Workers with a high estimated LTU-risk were invited to a meeting with the aim of placement in an activation programme. The selection of whom to invite was randomized, and workers reacted to an invitation by increasing job finding rates after receipt of the letter. Unemployment duration was shortened by 2.2 weeks, and the income of invited workers was higher than for the controls during the year after receipt of the letter.

[4] For example, Card *et al.* (2010) find that, in the short term, only 39% of the surveyed studies found significantly positive effects. In the medium term, effects were slightly better, with 50% being significantly positive.

Ex post effects from meetings are generally positive. Meetings with the aim of increased monitoring tend to find positive or zero effects (small and insignificant). Meetings that focus more on the counselling dimension show similar effects (and maybe slightly more favourable results than solely counselling). Van den Berg & van der Klaauw (2006) study a randomized experiment in Rotterdam with monthly meetings involving increased monitoring. They found a switch from informal to formal search channels as a result of the search and documentation requirements, and positive but insignificant effects on the exit rate from unemployment to employment. Keeley & Robins (1985) find something similar for the U.S. using observational data. Gorter & Kalb (1996) study a randomized experiment conducted in the Netherlands, where the time allocated to counselling with caseworkers was increased. They find positive but insignificant effects on the exit rate from unemployment. Hägglund (2009) analyses a social experiment conducted in Sweden, where unemployed youth were offered counselling. He found that, when aimed at all unemployed youth, there were positive effects on the exit rate from unemployment, while this was not the case when the treatment was only aimed at long-term unemployed youth. Crepon *et al.* (2005) analyse a reform implemented in France in 2001, which increased counselling without altering the amount of monitoring. They found a tendency that programmes aimed at 'better' workers increased the exit rate from unemployment, and that all programmes increased subsequent employment duration.

Dolton & O'Neill (1996; 2002) analysed the ReStart program; In England, an offer of meetings every six months for workers with more than six months of unemployment was introduced in 1989. The aim was an improvement of search behaviour (counselling part) and an assessment of the availability for work (monitoring). A randomized experiment was conducted, and the authors showed that this led to a 30% increase in exit rate from unemployment. The effects were long-lasting - five years after entry into the programme, the treatment group still had significantly less unemployment than the controls. Petrongolo (2009) and Manning (2009) both analyse the Job Seekers Allowance programme implemented in the U.K. in 1996 (the former looks at long term impacts while the later looks at short term impacts). This programme involved frequent meetings with a caseworker to document job search activity. They use observational data and exogenous variation in the timing of the treatment relative to the start of unemployment (difference in differences design) and find increasing exit rates out of unemployment. However, this is mainly caused by an increased exit rate into incapacity benefits. For the U.S. Ashenfelter *et al.* (2005) report from a randomized experiment, where search requirements were stricter for the treatment group. The increase in monitoring was only implemented during the first couple of weeks of unemployment. There was no effect of the increased monitoring on unemployment duration or on the costs of unemployment benefits. Klepinger *et al.* (2002) study another U.S. randomized experiment, where unemployed workers are randomized into one of three treatments (and a control group), which involved closer monitoring of different degree and type. Unemployment duration was reduced by 5-7%.[5] Lastly, van den Berg *et al.* (2012) use observational data from Danish administrative registers to study the dynamic effects of meetings. They find that the exit rates peak during the week a meeting is held and then taper off over the next 8 weeks or so. Moreover, the effects of a sequence of meetings tend to be gradual increases in the exit rate from unemployment to employment.

Finally regarding activation walls, there is very little literature. The effects shown by Black *et al.* (2003) and mentioned above could also arise from the perceived risk of activation. Three observational studies based on Danish data and one on German data show that unemployed workers tend to leave unemployment faster when the probability of activation increases (Geerdsen, 2006; Geerdsen & Holm, 2007; Rosholm & Svarer, 2008; van den Berg *et al.*, 2009). This suggests that an early activation wall might have important threat effects, but whether they are large enough to dominate dead-weight losses remains to be seen.

2.3 The Danish labour market and the experiments

This section presents the experimental designs and places them in the context of the Danish labour market. We then discuss the experimental design and proceed by analysing the implementation of the treatment protocols. Finally we look at compliers and non-compliers within the treatment groups.

The Danish labour market is characterized as flexible with less employment protection legislation than most continental European countries and much more labour turnover (see e.g. OECD, 2009). The Danish labour market has a tight social security net with near-universal eligibility for income transfers. Moreover, active labour market policies are among the most intensive in OECD, with around 1.5% of GDP spent per year on active policies. There are two types of benefits for unemployed workers, UI benefits and social assistance. Approximately 80% of the labour force are members of a UI fund and therefore eligible for UI benefits, while the remaining 20% may receive means-tested social assistance. UI benefits are essentially a flat rate. As this paper is only concerned with UI benefit recipients, we shall present the policies that apply to them. The "mutual obligations" principle is a key principle in the current Danish labour market policy. This implies the right of individuals to compensation for the loss of income, but also the obligation to take action to get back into employment. The authorities have an obligation to help the individual improve her situation and also has the right to make certain demands to the individual. Under the current rules, an individual who becomes unemployed and is eligible for UI benefits has to register at the local job centre. She then has the obligation to attend a meeting with a caseworker at least every 3^{rd} month. She has the right and obligation to participate

[5] Johnson & Klepinger (1994) & Meyer (1995) report similar findings from the US and McVicar (2008) from the UK.

TABLE 1: OVERVIEW OF THE 3 EXPERIMENTS

Experiment	Content	Region	Job centres
A	Group meeting each week	Northern Jutland	Frederikshavn, Brønderslev, Hjørring
B	Individual meeting w. caseworkers every other week	Copenhagen & Sealand	Gribskov, Roskilde, Ishøj-Vallensbæk Holbæk, Vordingborg
C	Early activation (after 13 weeks)	Mid-Jutland	Aarhus

in an activation programme after 9 months (6 if below 30 years old) of unemployment and subsequently every 26 weeks. These are the labour market policies that will be faced by individuals in the control groups of the three experiments, who will receive this 'treatment as usual'.

At the meetings, several issues are discussed. First of all, advice on how to conduct effective search is provided. For example, case workers may discuss which search channels are most effective, search requirements, how to construct a CV, preparing for a job interview, and what wage offers to expect when you search as an unemployed. Second, meetings are used to test the availability of unemployed workers for employment, to test if they meet specified job search requirements, and to issue sanctions in case of non-compliance. Third, case workers may also have information on specific job openings, so in some cases they can directly provide this information to an unemployed worker, thus engaging directly in the labour market match-making process. Finally, meetings are used for assessing and discussing the qualifications of the unemployed, and whether they meet the demands in the local labour market. Hence, planning of activation activities can also take place at meetings.

Description of the labour market experiments

The set of randomized experiments analyzed in this paper consists of three separate experiments, each with their own treatment and control group.[6] They were conducted in three different regions in Denmark. The motivation for having three different experiments rather than three treatment arms, which arguably would have led to greater external validity, was a practical concern about the ability of case workers to distinguish three different treatment arms and a control treatment. The experiments are summarized in Table 1. The subjects of the experiments are individuals becoming unemployed during weeks 8-29 in 2008 who are eligible for UI benefits. Once an individual registers as unemployed, she is 'randomized' into the treatment or control

[6]There was a fourth experiment in the region of Southern Denmark, consisting of a combination of weekly group meetings and early activation. However, this experiment was compromised, as one job centre did not implement the intervention, and data have been frequently revised. This experiment and its results are discussed in the Appendix.

group based on her date of birth. Individuals born on the 16^{th}-31^{st} are assigned to the treatment groups, while those born on the 1^{st} to the 15^{th} are assigned to the control groups. No information was given to the unemployed workers on the selection rule. Hence, while this is technically not random assignment, since it is predetermined by date of birth, we will treat it as such. The individuals randomized into the treatment groups then receive a letter, during the first week of unemployment, explaining the new treatment to which they will be exposed.[7] This information letter marks the start of the treatment, since the worker may react to the information on the new regime from the day the letter is read. It was not possible to escape treatment by leaving unemployment for a short while and then re-entering later on. In that case, a worker would re-enter the experimental treatment at the stage where she left it. In all three experiments, the control group receives 'treatment-as-usual', but there may be local variations in the intensity of treatment which will be documented below. The treatment group receives the same treatment as the control group plus the extra elements presented in Table 1.

The experiment labelled 'A' in Table 1 was conducted in the region of Northern Jutland. During the first 13 weeks of unemployment the unemployed worker must attend group meetings each week with a caseworker and a number of other unemployed workers (typically around 10).

The experiment labelled 'B' was conducted in the region of Copenhagen & Sealand, and consisted of individual meetings with a caseworker every other week for the first 13 weeks of unemployment, that is, a total of 6-7 extra meetings during the first 13 weeks of unemployment. Note that, generally, the stated main intention of both group and individual meetings was counselling of the unemployed; no explicit extra monitoring was required to take place by the public authorities. However, the perception of the meetings from the point of view of the unemployed might have been different.

In the experiment labelled 'C', the individual would be required to participate in an activation programme for at least 25 hours per week from week 14 in unemployment at least until week 26. This experiment - the activation wall - was designed specifically to investigate the presence of *ex ante* effects due to the knowledge of having to participate in an activation program, as well as *ex post* effects of actually having participated. The design will therefore only allow us to pick up a mixture of both *ex ante* and *ex post effects* and thereby we can assess overall efficiency in this particular setting. Note that in order to test specifically for the *ex ante* effect, there should have been no actual treatment taking place from week 13 onwards. However, such a setup would not be legal according to the administrative regulations, and ethical concerns could also be present.

[7] The unemployed is not informed that she is participating in a randomized experiment, but rather that she has been chosen to participate in a pilot study.

TABLE 2: NUMBER OF INDIVIDUALS IN TREATMENT AND CONTROL GROUPS

Experiment	Treatment	Control
A (group meetings)	655	705
B (individual meetings)	805	832
C (early activation)	887	836

Data

The data are extracted from administrative registers merged by the National Labour Market Authority into an event history data set, which records and governs the payments of public income transfers, records participation in ALMPs, and has information on periods of employment. The administrative data are used for determining eligibility for UI benefit receipt and for determining whether the job centres meet their obligations in terms of meetings and activation intensities. The information is therefore considered highly reliable.

The event history data set includes detailed weekly information on: labour market status and history (employment, unemployment, in education, on leave, etc.),[8] meeting attendance and programme participation, ethnicity, gender, residence, marital status and UI fund membership. 5528 individuals registered as unemployed in one of the 9 job centres which were part of the experiments, between week 8 and week 29 of 2008, both weeks inclusive.[9] We have removed all immigrants from the sample, due to a concern that immigrants are occasionally assigned an administrative birthday of January 1 when they receive their residence permits. Since the randomization was done by date of birth, this led to an unequal distribution of immigrants across treatment and control groups which could bias the results. This leaves us with a total sample of 4730 individuals in the three experiments. The distribution on treatment and control status in the three experiments can be seen in Table 2.[10]

Each person is followed until the end of January, 2013. Given the evaluation window (week 8-29 in 2008), all individuals can be followed for at least 237 weeks (there are 53 weeks in 2009) and for at most 258 weeks after their entry into unemployment. We can also follow individuals back in time, although the employment information is available only from 2008 and onwards. In table 3 we present summary statistics for the individual characteristics of the members of the treatment and control group

[8] Labour market status is calculated based on information from the register on payments of public income transfers. Data will also tell us whether individuals are employed (in unsubsidised jobs) or not using information from the E-income register, containing information from employers about their employed workers (we do not have information on hours). Finally, there is a residual labour market category, called 'self-sufficient', consisting of the self-employed and individuals that are neither working in the market nor receiving any income transfers (e.g. housewives).

[9] Below "time since experiment start" will be the duration since individuals were assigned to treatment and control groups. We will only use the information about different inflow weeks to construct subgroups in the subgroup analysis (see below).

[10] We have also analyzed the inflow into the experiment and found that the number of individuals entering every week is quite similar in the treatment and control groups.

TABLE 3: SUMMARY STATISTICS FOR EXPERIMENT B: INDIVIDUAL MEETINGS

Characteristics	**Control** Average	**Treatment** Average	P-value
Age (years)	40.13	40.40	0.64
Under 25	0.13	0.11	0.24
25-49	0.60	0.63	0.26
Above 49	0.27	0.26	0.69
Married	0.62	0.60	0.53
Transfer degree	0.26	0.26	0.76
Transfer degree < 0.2 last year	0.63	0.63	0.88
Transfer degree ϵ (0.2;0.5) last year	0.15	0.16	0.65
Transfer degree > 0.5 last year	0.22	0.21	0.82
Share of new unemployed	0.97	0.98	0.67
Transfer degree < 0.2 last 3 years	0.66	0.63	0.20
Transfer degree ϵ (0.2;0.5) last 3 years	0.23	0.25	0.19
Transfer degree > 0.5 last 3 years	0.11	0.11	0.87
Share in UI funds for academics	0.06	0.07	0.42
Share in "Manufacturing" UI fund	0.23	0.20	0.08
Share in Other UI fund	0.14	0.14	0.79
Number of observations	805	832	
P-value from joint test	0.48		

Note: The p-values are the p-values associated with the coefficient on treatment status from a simple linear regression where we regress a given characteristic on treatment status (we use robust standard errors). The joint test is Hotelling's T-squared test of whether the set of means is equal between the two groups.

for experiment B (individual meetings).[11] Similar tables for experiment A and C is presented in Table A1-A3 in the appendix. We test the equality of mean values of characteristics and cannot reject covariate balance (only in 3 cases out of 48 tests the p-value is below 10% and in only 1 case is the p-value below 5%). We have also compared the control groups across the different experiments in order to assess how similar the individuals are. This provides some guidance about whether we can compare the impacts of the experiments across regions. In the appendix (Table A.4) we show tests of equality of means across regions and from these we conclude that the population in experiment C (early activation) differs significantly from the population in the two other experiments. The populations in the experiments with meetings look more similar although there are still some differences. These findings suggest we should be cautious in making to tight comparisons across the experiments.

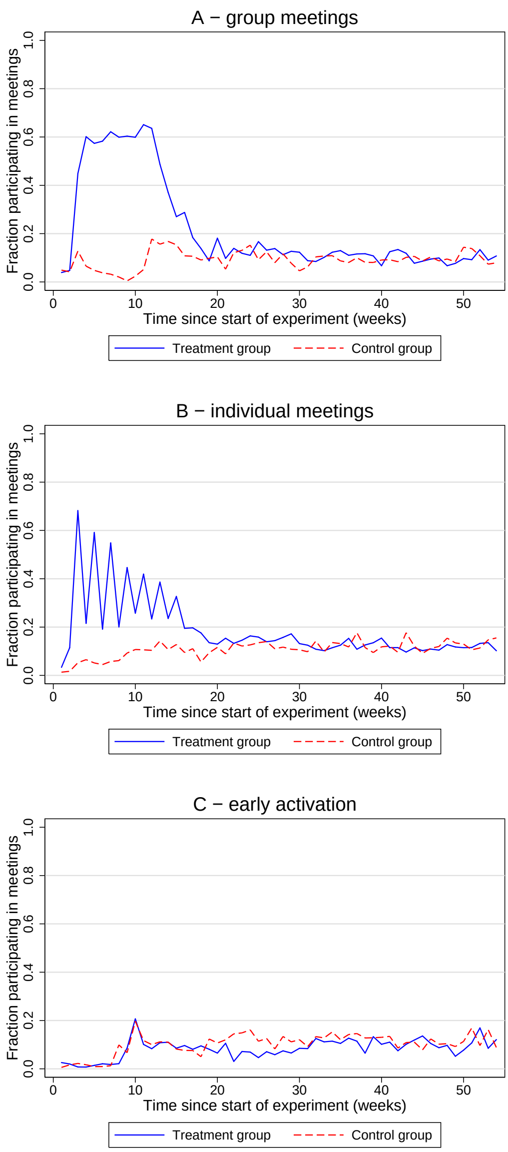
[11] This is the experiment which shows the strongest results below and therefore we have chosen to show the balancing table for this experiment in the main text.

Implementation

In this subsection, we present evidence on the implementation of the three experiments. To show the degree of compliance to the experimental protocol, we show a set of figures on the weekly meeting intensities and activation intensities for unemployed individuals. We have also tabulated these intensities by gender and we have found no remarkable differences in this dimension. The figures should be regarded as lower bounds on the actual implementation in the job centres, as unemployed individuals participating in, for instance, two meetings in a given week, will only be counted once and individuals transiting into employment in a given week are not included. We have also exploited the data on the implementation and the existence of "multiple" control groups and different time profiles of treatment to compare control groups across the experiments to assess whether we can find any evidence on substitution effects due to the experiment (i.e. control groups being treated to a smaller extent).[12] We have not found such differences (Figure A.2 in the appendix shows this comparison). This is also supported by Figure A.1 which shows the Kaplan Meier survival curves for the three control groups. A rank test cannot reject that they are pairwise similar. Figure 1 plots the weekly meeting intensity in the three regions for the treatment and control groups. In Experiment A the treatment group was intended to participate in group meetings on a weekly basis. Only around 60% of the treatment group who were still unemployed in a given week, participated in meetings (during the first 13 weeks). After 52 weeks unemployed treated individuals will on average have participated in roughly 7 meetings more than controls. In Experiment B we observe a saw-tooth pattern reflecting the fortnightly meetings. Summing the meeting intensities for two consecutive weeks, the fortnightly meeting intensity begins around 90% and then falls to about 65% around week 13. After 52 weeks treated unemployed have participated in 5 meetings more on average. In Experiment C there was no intention of extra meetings, and this is also what we observe in the data. Hence, even though participation in meetings does not comply completely with the requirements of the experiment, the treatment groups in the two relevant projects attended significantly more meetings than did the corresponding control groups during the early phases of the unemployment spell. We look more into the characteristics of those that actually receive treatment in the next section. The meeting rate for the treatment and the control groups is the same after the period of the experimental treatment in all regions. Notice, however, that the sequence of intensive meetings continues a few weeks beyond week 13 of the unemployment spell. We interpret this as an implementation lag in the treatment process, as well as a consequence of meetings cancelled earlier in the unemployment spell due to sickness, job search, etc. Figure 2 shows weekly activation intensities. In experiment C with early activation there is a sharp increase in the activation intensity around week 13. Again, not everyone

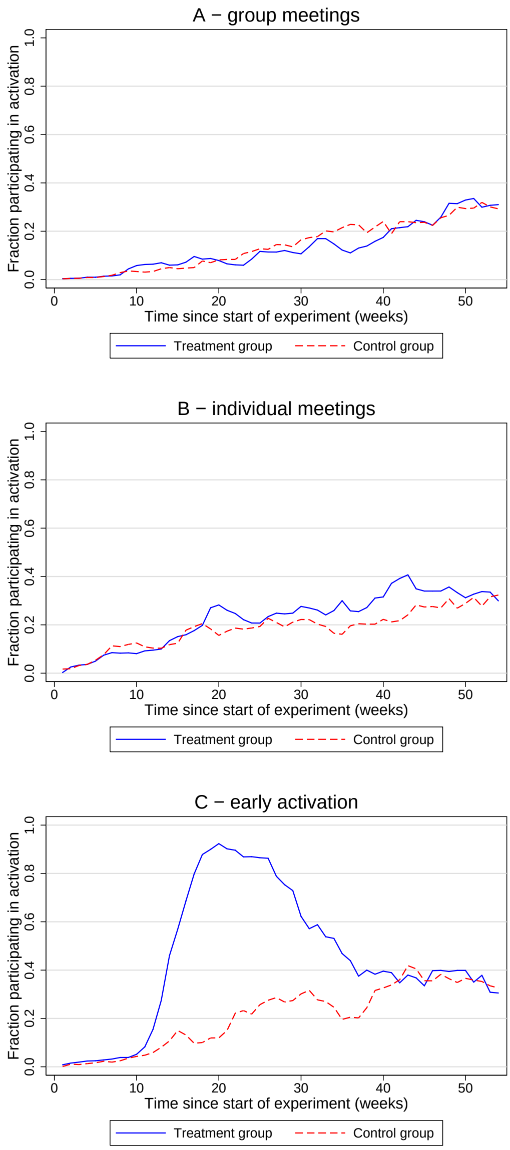
[12] To avoid such effects extra resources were given to the job centres in compensation of the intensified treatment requirement.

FIGURE 1: WEEKLY MEETING INTENSITIES



Note: Meeting intensities for those who are still unemployed in a given week.

FIGURE 2: WEEKLY ACTIVATION INTENSITIES



Note: Activation intensities for those who are still unemployed in a given week.

in the treatment groups was activated between weeks 13 and 26, but the activation intensity is much higher for the treatment group than for the control group. After 52 weeks treated unemployed have participated in 13 weeks of activation more than the control group on average. Further analysis of the type of activation to which the unemployed in the treatment groups were assigned reveals that the unemployed are assigned to programmes with the intention to upgrade and clarify their skills (i.e. educational and training programmes). These are typically programmes with a duration of around 4 weeks. This category of programmes is the most commonly used activation instrument in Denmark (see e.g. Danish Economic Council, 2007). In all regions we observe an increase over time in the activation intensity for those who remain unemployed in the control groups. This follows naturally from the large focus on active labour market policy in the Danish Flexicurity model (see e.g. Andersen & Svarer, 2007). After the end of the experimental treatment period (in week 26), the activation intensities for treatment and control groups converge rather quickly. In Experiment B a larger fraction of the remaining unemployed in the treatment group is activated compared to the control group, this could reflect outcomes from the meetings with caseworkers or alternatively just dynamic selection out of the group (below we show that the treatment group at this point is in employment to a larger extent).

Overall the meetings and activation intensity figures reveal that the treatment groups to a large extent received the intended treatments, and they were treated much more intensively than the control groups in the relevant dimensions. As compliance to the treatment protocol is not 100%, the treatment effects that we will determine below can be regarded as "intention to treat effects". We will not report ATE effects as we believe that the ITT effects are really the policy relevant effects in this setting, since it reflects the 'modus operandi'. Furthermore, compliance is not a static concept in our experiments.[13] The analysis also highlights how important data on actual implementation of the treatment is for our understanding of the effects that we present below. Compliance is never perfect.

In the appendix the issue of non compliance is described in more detail. The main conclusion is that around 90 % of the variation in accumulated compliance status (see the appendix) is due to factors unobserved to us. Thus there are few systematic differences between complier and non-complier based on observed characteristics. From the 10 % of the variation we can explain, we do see some indications that the non-compliers are generally "weaker" unemployed (e.g. having a history of unemployment and sick listing) which could also support that the ITT effects are not directly transferable to ATE effects (if we believe "weaker" unemployed are for instance less likely to benefit from treatments). Naturally, focusing on ITT effects implies that there is no correction for a potential difference in compliance rates

[13] A complier one week is likely to be a non-complier next week. Furthermore scaling by compliance degree would imply that we assume away ex ante effects which have been found earlier in the literature (see above).

between regions, and therefore effects could be driven by "more effort" from certain regions/job centres (that simply treat more). This point serves to motivate why the cost-benefit analysis (CBA), which we present in the final section of the paper, is a crucial element in the evaluation and why data on the actual implementation of policies is important. In the CBA we use the actual costs encountered and thereby take the compliance rates into account in assessing the effectiveness of different treatments.

2.4 Empirical results

In this section we present the effects from each of the three experiments. We report the treatment effect on weeks in employment for each week in a long sample window after the experiment started.[14] The main outcome is the accumulated number of weeks employed from the start of the experiment until week t, and then we let t vary from 1 to 237 weeks**.** The effect of treatment on the accumulated number of weeks employed until week t for individual i (β) is estimated in the following regression:

$$W_{it} = \alpha_t + \beta_t T_i + \gamma_t H_{it} + \varepsilon_{it}$$

where W_{it} is the accumulated number of weeks in employment t weeks after enrolment into the experiment, T_i denotes treatment status and H_{it} is a measure of previous employment history. The treatment effect at time t, β_t, measures the average number of extra weeks spent employed for the treatment group compared to the control group from the beginning of the experiment until t weeks later. We also report the relevant side of the two-sided confidence interval of the effects both at a 5% and 10% level.[15]

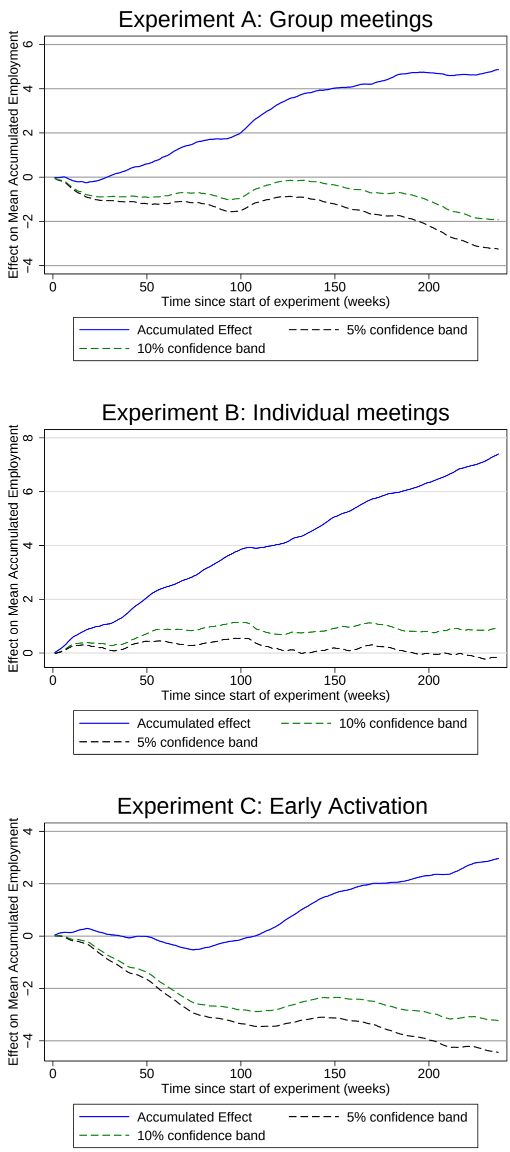
Figure 3 shows the effects of three experiments. The effects are showed as the accumulated difference in employment. In Figure A.3 in the appendix the employment rates for the control group are shown. These can compared to Figure 3 to get an impression of the relative size of the effects (we will also comment on relative size below). For the experiment with group meetings the difference in accumulated employment is close to 5 weeks after 237 weeks, but the effect is not statistically significant. The fact that the effect starts accumulating after a year suggests that the primary channel through which group meetings affect employment is via longer employment duration rather than shorter unemployment duration.[16] For the experiment with individual meetings the effect is significantly positive until around 4 years after the experiment, thereafter it is marginally insignificant at the 5% level. Contrary to the two other experiments the effects starts to accumulate from the week of entry

[14] Since we evaluate the experiment in a dynamic setting, we choose this outcome as a summary statistic of the (potential) differential effects over time. This would also allow for even small differences between the fraction employed in treatment and control groups to accumulate over time.

[15] For instance for positive effects we do not report the upper bound of the confidence interval.

[16] We will investigate the timing of effects in the subsection on heterogeneous effects.

FIGURE 3: THE EMPLOYMENT EFFECT OF THE THREE EXPERIMENTS



Note: The figure shows the accumulated difference in the number of weeks employed between the treatment and control groups. We only show the relevant side of the two-sided confidence bands.

into unemployment. After 237 weeks the difference between the treatment group and the control group is more than 7 weeks in employment. Considering that the mean employment rate of the control group in the sample period is close to 55%, this corresponds to an increase of around 4 % in employment over the period after entry into the programme. For the experiment with early activation there is no significant effect on the difference in accumulated employment, and the effect only becomes positive after around 2 years.

In sum, all three experiments increase the employment rates for the treatment group. The effects are, however, only statistically significant for the experiment involving individual meetings. Lack of statistical significance in the two other experiments can be due to either no real effect, high standard errors due to relative few observations and a "noisy" outcome or both.[17] In the next subsection the effects of the experiments will be confronted with the costs of running the programme.

Effects on government budget

In this section we contrast the costs of running each of the three experiments with the gains from increasing employment rates. Above we have documented differences in the implementation of the treatments across the experiments and our analysis here offers a way to compare the employment gains from different treatments to the realized costs. We will focus on the direct impact on the government budget. The government budget is affected by reduced income transfers, by increased taxes from increased production and by the costs of running the programmes.

The costs are split into costs of income transfers and costs of operating active labour market policies (called programme costs).[18] The costs of income transfers are calculated based on weekly per individual costs of a given income transfer. Programme costs are provided as average costs of operating activation programmes of a given type (cost data are provided by the National Labour Market Authority), individual meetings last between 15 and 30 minutes (the information on the duration of meetings is provided by the participating job centres), and group meetings last 2-3 hours and have 6-30 participants per meeting. The price of a meeting per worker is then calculated by multiplying its duration with the hourly costs of a caseworker and dividing by the number of participants. The *actual* (observed) number of meetings and transfers are used in the calculations and hence the compliance to the treatment protocol is taken into account in assessing the cost effectiveness of the interventions.

Public income transfers represent only a reallocation of income, hence in a traditional cost-benefit calculation they would not be included (see the appendix for an example). They have however a direct effect on the government budget. In Denmark

[17] In appendix we show that the main findings are unaffected by conditioning on more covariates in the regressions presented in Figure 4.

[18] In the appendix (see the section with the cost benefit analysis) we provide more details about the cost components and other components in the CBA.

TABLE 4: EFFECT ON GOVERNMENT BUDGET AFTER 237 WEEKS (PER INDIVIDUAL)

	A Group meetings	B Individual meetings	C Early activation
Saved income transfers[a]	3303	3631	1392
Gain from saved transfers[b]	1558	1713	657
Value of increased production[c]	4263	6508	2607
Gain from increased production[d]	2251	3438	1377
Costs of program[e]	903	47	440
Discounted effect on budget[f]	2539	4457	1392
Confidence intervals	[-1812;6368]	[486;8215]	[-2485;4856]

Note: a : Calculated as the difference in public transfers paid to treatment versus control group in the first 237 weeks. b : Effect of saved transfers when adjusted for direct (37,5%) and indirect (24,5%) taxes. c : Based on annual income of 40.000 Euros. d : The effect from taxes on value of increased production. e : direct programme costs. f : Discounted effect using 3% annual discount rate. Standard errors are found by bootstrapping.

public transfers are subject to income taxes. According to the Ministry of Labour the average tax rate for recipients of unemployment benefits is 37,5%. In addition, their consumption results in further tax payments to the government of 24,5% through value-added taxes, energy taxes etc. In addition, we assume that employed workers are able to obtain work at an annual wage of approximately 40000 Euros (with 46 working weeks)[19]. We assume further that all the gains from increased production accrue to the workers (this implies that we do not have to consider increases in revenues from the taxes of firms etc.). The impact on the government budget is then the saved income transfers after taking into account that paying out social transfers also leads to increases in tax payments from both income taxes and taxes on consumption. To this we add the increase in tax payments and indirect taxes which the increased production generates.

In Table 3 we show the effects on the government budget 237 weeks since entry into the experiment. We use an annual discount rate of 3% [20] and provide 95% confidence intervals based on bootstrapping. For the bootstrap we draw repeated random samples with replacement and in each sub-sample we compute the various components in the CBA. This allows us to take uncertainty from both the benefit side (the estimated employment effect of the experiment) *and* the costs side (the sampling uncertainty in the type of income transfer and ALMPs unemployed individuals participate in) into account.[21] Table 4 shows that individual meetings with caseworkers lead to the largest net gains to government budgets. Experiment B is thus the only experiment, where the net gain is statistically significant. The discounted net gain per unemployment spell is EURO 4457. The net gain to the budget is also positive for the two other interventions, but not significantly so. The net gain from individual meetings is almost twice as large compared to group meetings and the

[19]This is the average annual wage of an unemployed that began a new job in 2008.

[20]We have also tried a discount rate of 2 % and 4 % and our conclusions are not affected.

[21]To our knowledge we are the first to consider both sources of uncertainty.

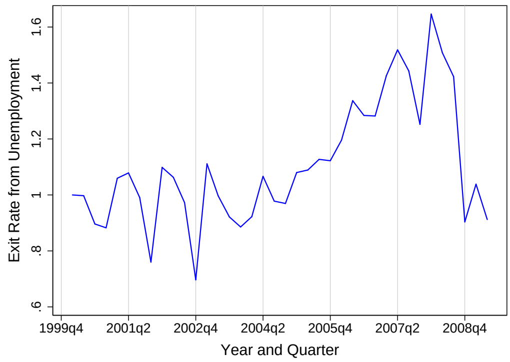
employment effect and low programme costs (also due to the increase in the outflow from unemployment) are the major factors behind this result.

As a supplement to the analysis of the effects of the experiments on government budget, Figure A.7 in the appendix presents the outcome of a more classical cost benefit analysis (welfare gains under distortive taxation). Here the full gain from production is included in the calculation (assuming again that the wage of the worker is a measure of the gain). We assume that the individuals do not value any lost leisure and in addition we assume that the marginal cost of public funds is 20 %, meaning that to finance a given transfer to the unemployed the loss to society is 20%. When reducing transfers the gain to society amounts to 20% of the saved transfers. The saved transfers as such are not included in the CBA as they are simply a transfer internally in society. The costs are the direct costs of running the program corrected for the marginal costs of public funds needed to finance the extra costs.[22] The CBA gives the same message as the analysis on the gain to government budgets. That is, the experiment with individual meetings generates a positive return, whereas the two other experiments do not give a significantly positive return.

Both CBAs ignore general equilibrium effects, and the interpretation should keep that in mind. Also for the later analysis there is some discussion in the literature (e.g. Jabobs (2013) and Kreiner & Verdelin (2012)) on whether marginal costs of public funds should be included. There are different practices in the literature and we do not take a clear stand on whether they should be included or not. The main message will not be altered by assuming marginal costs of public funds to be 0 (welfare gain under lump-sum taxation). It would of course be interesting to know what the full blown effects would be on social welfare. Is there for instance a positive return in a cost benefit analysis that includes effects on welfare for the unemployed and on the functioning of the labour market, where potential effects on wages and total employment are taken into account? Based on the analysis presented in this paper we have no information on these effects and we therefore present the effects on the government budget as the main indicator of the return of running the experiments. Gautier *et al.* (2012) present a cost benefit analysis of another Danish experiment with early activation. They use a search-matching model to assess the effects of welfare and labour market performance and find that the partial equilibrium effects can change substantially when general equilibrium effects are taken into account. The finding relies on rather strong functional assumptions and we will not pursue a similar type of analysis in this paper, but emphasize that the effects of the experiments on government budgets only provide a partial picture of the return of introducing more intensive active labour market policy.

[22] The appendix gives an example of the whole calculation.

FIGURE 4: THE NORMALIZED EXIT RATE FROM UNEMPLOYMENT



Source: Own calculations based on an estimated duration model

Heterogeneous effects

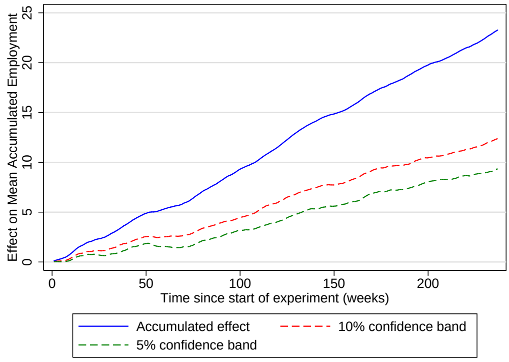
The analysis presented above showed that especially individual meetings had positive direct effects on employment rates. In this subsection results from different subgroups are presented. In the appendix we show the results of the experiments separately for males and females and we also present a series of tables (separately for males and females) with results from a linear regression of accumulated weeks of employment in week 237 on treatment status, age group, business cycle indicator, and their interactions.

The business cycle indicator is potentially interesting because the experiment began just prior to the onset of the financial crisis. Figure 4 depicts the normalized (first quarter of 2000=1) outflow rate from unemployment to employment of individuals entering unemployment from 2000 until 2009 conditional on a wide range of explanatory variables.[23] The figure illustrates the impact of the financial crisis in Denmark. It illustrates that the crisis led to a collapse of outflow rates from unemployment from the beginning of the 3^{rd}quarter of 2008 and onwards. This implies that individuals becoming unemployed in the last part of the inflow period (weeks 16-29) of the experiment will potentially experience worse labour market conditions conditional on elapsed duration, as they become unemployed very close to this dramatic decline in outflow rates.[24] Browsing through the tables in the appendix it is also clear that most of the interaction effects are insignificant and that there are no

[23] This rate is determined by estimating a piecewise constant unemployment duration model on the inflow to unemployment during the period 2000-first half of 2009, including a wide range of explanatory variables, including labour market history and demographics, and specifically time-varying quarterly dummies, which are shown in the graph and capture any variation over time in the outflow rate to employment that is not accounted for by observed variables.

[24] As earlier mentioned the inflow into control and treatment groups is stable, so we can in fact treat the treatment and control groups in "good" or "bad" times as a separate experiment. It is important to keep in mind, that we assume that the intensity and efficiency of treatment is constant wrt. week of inflow below.

FIGURE 5: EFFECTS FOR EXPERIMENT C: EARLY ACTIVATION FOR MEN THAT ENROL EARLY



clear patterns in the results across characteristics. From the figures with the results estimated separately by gender (Figure A.4 in the appendix) it appears that men exhibit larger positive effects from both individual and group meetings than women. In the case of group meetings the effect now becomes statistically significant (although only marginally) and males spent 10 weeks more in employment in the treatment group. When we look at the various interactions in the subsequent tables (Tables A.13 and onwards) we see that in particular married men exhibit large positive effects. For the experiment with early activation there is also a positive (but insignificant) effect for men and from the tables we see that this is particularly true for young males and males that enrol early in the experiment. In Figure 5 this later finding is emphasized by showing the effects for this particular sub-sample. The finding that effects of early activation is particular strong for men is consistent with e.g. Rosholm & Svarer (2008). Here it is also found that men react more to the perceived risk of activation than women. The positive impact from the business cycle situation suggests that labour market prospects which improve job finding rates might be beneficial for the effects of active labour market programs. In Table A.13 in the appendix the findings from a multi-state duration model are presented. The model investigates the impact of being in the treatment group on unemployment and employment duration two years after enrolment in the program separately for males and females. The main findings are that for men the improvement in employment from meetings is caused by a reduced exit from their subsequent employment spells. That is, the duration analysis indicates that the effects from attending meetings for men is not that they find jobs faster, but that they stay employed longer compared to the control group. In addition, the duration results find that there is a locking-in effect for women in experiment C with early activation (see also Figure A.4). This highlights the trade-off by having activation demands early in an unemployment spell. Early activation may motivate unemployed to increase job search, but for those who do not manage to find a job, participation in activation reduces time for job search and can be detrimental to job finding while enrolled in activation.

2.5 Conclusion

This paper presents the labour market effects of three randomized experiments conducted in Denmark in 2008. The experiments entailed different combinations of early and intensive treatment in terms of meetings and activation. The analysis documents differences between the design of the experiment and its actual implementation. As could be expected, treatment intensity is not 100% and since the non-compliers are generally a weaker group of unemployed and the literature has shown important anticipation or ex ante effects, focus is on the ITT effects.

Individual meetings between newly unemployed workers and caseworkers increase employment rates over the next four and a half years by 4 %, and improve the government budget with close to EURO 4500 per unemployed. The positive effect of individual meetings for newly unemployed workers is consistent with the findings in the literature on the effects of meetings between caseworkers and unemployed workers, and indicates that a strong focus on close interaction between case workers and unemployed early in the unemployment period may be a profitable part of active labour market policy. Group meetings, which are clearly cheaper per unit treated than individual meetings, also have a positive impact on employment rates, but the results found in the current study are not statistically significant (except if we look at males only). Early activation provides the least favourable outcome of the three experiments analyzed, and further analysis indicates that although there might be ex-ante effects on exit rates for some unemployed (and especially when job finding possibilities are high) there are also indications of locking-in effects, which highlights the trade-off between introducing intensive and time consuming activities early in the unemployment spell.

The strength of the current analysis is that it is possible to relate the findings on employment with the costs of running the program and hereby the paper contributes with a more elaborate analysis than what is typically seen in the literature on evaluation of active labour market policy. There is however still a big step towards a more comprehensive cost benefit analysis that also considers general equilibrium effects and welfare effects. We see that as a natural path for future research in this area.

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TABLE A.1: SUMMARY STATISTICS FOR EXPERIMENT A: GROUP MEETINGS

Characteristics	**Control group** Average	**Treatment group** Average	P-value
Age (years)	39.97	39.21	0.23
Under 25	0.12	0.15	0.16
25-49	0.60	0.58	0.53
Above 49	0.27	0.26	0.69
Marriage	0.60	0.58	0.36
Transfer degree	0.27	0.28	0.38
Transfer degree < 0.2 last year	0.62	0.62	0.87
Transfer degree ϵ (0.2;0.5) last year	0.16	0.14	0.25
Transfer degree > 0.5 last year	0.22	0.24	0.42
Share of new unemployed	0.99	0.97	0.03
Transfer degree < 0.2 last 3 years	0.61	0.60	0.67
Transfer degree ϵ (0.2;0.5) last 3 years	0.28	0.25	0.36
Transfer degree >0.5 last 3 years	0.11	0.15	0.07
Share in UI funds for academics	0.02	0.03	0.46
Share in "Manufacturing" industry UI fund	0.32	0.29	0.21
Share in Other UI fund	0.10	0.10	0.72
Number of observations	705	655	
P-value from joint test	0.42		

Note: The joint test is Hotelling's T-squared test of whether the set of means is equal between the two groups

2.7 Appendix

Description of individuals in different experiments

To illustrate the composition of unemployed across regions this section provides summary statistics on the composition of the treatment and control groups for each experiment. In addition, Kaplan-Meier survival plots for the control groups in each experiment are compared. Figure A.1 shows the Kaplan-Meier survival rates for control groups in each of the three regions. Log-rank tests do not reject the null hypothesis of equality of survivor functions.

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TABLE A.2: SUMMARY STATISTICS FOR EXPERIMENT B: INDIVIDUAL MEETINGS

Characteristics	**Control group** Average	**Treatment group** Average	P-value
Age (years)	40.13	40.40	0.64
Under 25	0.13	0.11	0.24
25-49	0.60	0.63	0.26
Above 49	0.27	0.26	0.69
Married	0.62	0.60	0.53
Transfer degree	0.26	0.26	0.76
Transfer degree < 0.2 last year	0.63	0.63	0.88
Transfer degree ϵ (0.2;0.5) last year	0.15	0.16	0.65
Transfer degree > 0.5 last year	0.22	0.21	0.82
Share of new unemployed	0.97	0.98	0.67
Transfer degree < 0.2 last 3 years	0.66	0.63	0.20
Transfer degree ϵ (0.2;0.5) last 3 years	0.23	0.25	0.19
Transfer degree > 0.5 last 3 years	0.11	0.11	0.87
Share in UI funds for academics	0.06	0.07	0.42
Share in "Manufacturing" industry UI fund	0.23	0.20	0.08
Share in Other UI fund	0.14	0.14	0.79
Number of observations	805	832	
P-value from joint test	0.48		

Note: The joint test is Hotelling's T-squared test of whether the set of means is equal between the two groups.

TABLE A.3: SUMMARY STATISTICS FOR EXPERIMENT C: EARLY ACTIVATION

Characteristics	**Control group** Average	**Treatment group** Average	P-value
Age (years)	36.21	36.24	0.94
Under 25	0.12	0.13	0.57
25-49	0.72	0.69	0.20
Above 49	0.16	0.18	0.30
Married	0.46	0.50	0.12
Transfer degree	0.44	0.44	0.58
Transfer degree < 0.2 last year	0.46	0.47	0.29
Transfer degree ϵ (0.2;0.5) last year	0.19	0.17	0.20
Transfer degree > 0.5 last year	0.35	0.36	0.79
Share of new unemployed	0.99	0.99	0.37
Transfer degree < 0.2 last 3 years	0.45	0.47	0.43
Transfer degree ϵ (0.2;0.5) last 3 years	0.20	0.17	0.28
Transfer degree > 0.5 last 3 years	0.35	0.36	0.96
Share in UI funds for academics	0.32	0.30	0.39
Share in "Manufacturing" industry UI fund	0.10	0.10	0.94
Share in Other UI fund	0.06	0.06	0.85
Number of observations	836	887	
P-value from joint test	0.63		

Note: The joint test is Hotelling's T-squared test of whether the set of means is equal between the two groups.

FIGURE A.1: KAPLAN-MEIER SURVIVAL ESTIMATES FOR CONTROL GROUPS

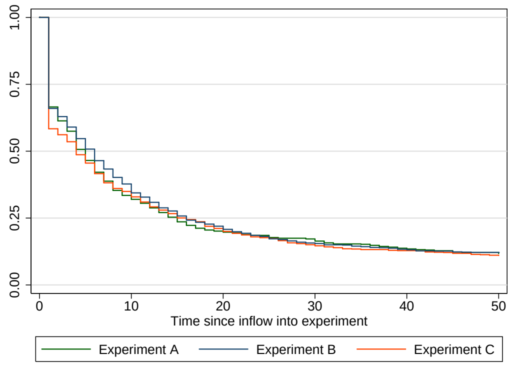


FIGURE A.2: COMPARING TREATMENT INTENSITIES IN THE CONTROL GROUPS

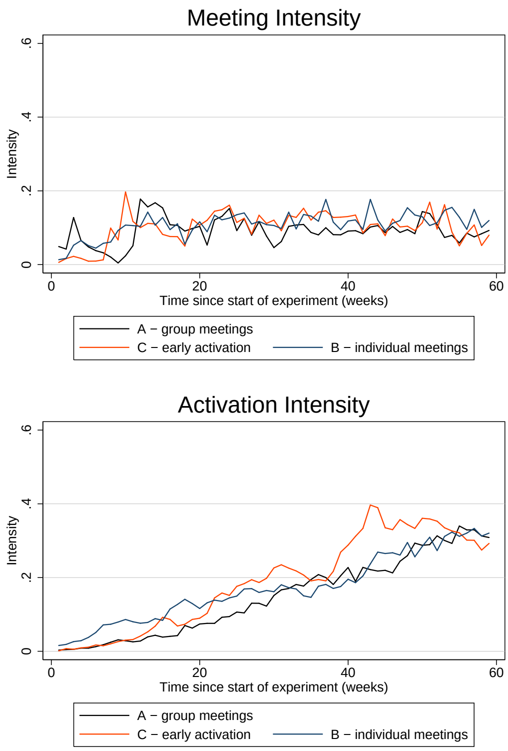


TABLE A.4: COMPARING INDIVIDUALS ACROSS EXPERIMENTS

Full sample	Group - Early*		Individual - Early**		Group - Individual	
	Difference	p-value	Difference	p-value	Difference	p-value
Age (years)	3.38	0.00	-4.04	0.00	0.66	0.12
Under 25	0.02	0.00	0.01	0.60	-0.02	0.08
25-49	-0.11	0.00	0.09	0.00	0.02	0.20
Above 49	0.10	0.00	-0.10	0.00	-0.00	0.92
Married	0.11	0.00	-0.13	0.00	0.02	0.18
Transfer degree (Td)	-0.17	0.00	0.18	0.00	-0.02	0.16
Td < 0.2 last year	0.14	0.00	-0.15	0.00	0.01	0.62
Td ϵ (0.2;0.5) last year	0.05	0.00	-0.05	0.00	0.00	0.80
Td > 0.5 last year	-0.19	0.00	0.21	0.00	-0.01	0.42
Share of new unemployed	-0.01	0.00	0.02	0.00	-0.00	0.77
Td< 0.2 last 3 years	0.14	0.00	-0.19	0.00	0.04	0.01
Td ϵ (0.2;0.5) last 3 years	0.08	0.00	-0.06	0.00	-0.02	0.12
Td > 0.5 last 3 years	-0.22	0.00	2.24	0.00	-0.02	0.09
Share in academics UI fund	-0.28	0.00	0.24	0.00	0.04	0.00
Share in "Manufacturing" UI	0.21	0.00	-0.12	0.00	-0.10	0.00
Share in Other UI fund	0.04	0.00	-0.08	0.00	0.04	0.00

Note: Transfer degree (td) is determined as the fraction of the last year spent on some kind of public support (social assistance, UI, study aid etc.)

* comparing the control in the experiment with Group Meetings to the control group in the experiment with Early Activation.

** comparing the control in the experiment with Individual Meetings to the control group in the experiment with Early Activation

Testing characteristics across regions

Table A.4 reports the coefficient and corresponding P-value from a regression of the respective characteristic on an indicator of e.g. experiment A in a sample containing all observations from experiments A and C. The main finding from Table A.4 is that the population in experiment C with early activation differs from the population in the two other experiments. For most characteristics the populations in the experiments with meetings have the same mean values.

Non-compliance

To analyse the degree of compliance, we construct a variable (*excess treatment*), which adds up the number of meetings/weeks of activation an individual has participated in 5 and 10 weeks after treatment should have started[25] and subtracts the ideal treatment had compliance been 100% (and taking into account that individuals should only be treated when unemployed). This deals with the fact that we see some very short employment spells in the data, which implies that the amount of intended meetings (activation) varies between individuals.[26]

Excess treatment is regressed on a set of explanatory variables, job centre dummies and the local unemployment rate (at the job centre level). In Table A.5, we summarize the mean 'excess treatment', which is negative in Experiments A and B, implying that in week 5 after inflow individuals in Experiment A (group meetings) on average have participated in 2 meetings less than intended. As this difference hardly grows when we look at the excess treatment after 10 weeks, we subscribe a part of this effect to the implementation lag earlier documented. Table A.5 also reports R^2 statistics from our regressions[27]. Generally we have very low explanatory power in our regressions, which implies that the main part of the variation in excess treatment is due to factors unobserved to us. In terms of significant explanatory variables we mainly see job centre differences (significant job centre dummies) but some small differences also prevail. In Experiment A, individuals who have experienced more unemployment during the past 3 years are less likely to participate in the group meetings. In Experiment B, individuals who have earlier been sick listed, or worked in the construction or manufacturing industry are less likely to participate in the individual meetings. In Experiment C individuals aged 50 or above and unemployed who are member of the UI-fund for academics are activated to a larger extent. Interestingly, in Experiment C there also seems to be an inflow effect, such that individuals enrolled into the experiment in the last half of the inflow period are participating in activation to a smaller extent.

Employment rates for control groups

Below we show the evolution in the employment rates for the control groups. These are used as a way to compare our empirical results against.

[25] According to the design protocol i.e. week 1 for Exp. A and B,week 10 for Exp. C

[26] Our accumulated treatment measure, which we call excess treatment, is thereby calculated as the provided treatment subtracted the ideal treatment had there been 100% compliance with the treatment protocol.

[27] Output from regressions are available upon request, we summarize all significant findings from our regressions after 10 weeks below.

TABLE A.5: EXCESS TREATMENT

	Exp. A	Exp. B	Exp. C
Week 5:			
Mean *excess treatment*	-2.01	-0.5	0.43
R^2 of estimated model	0.07	0.07	0.09
Week 10:			
Mean *excess treatment*	-2.35	-0.73	0.30
R^2 of estimated model	0.08	0.11	0.11

FIGURE A.3: EMPLOYMENT RATES FOR THE CONTROL GROUP

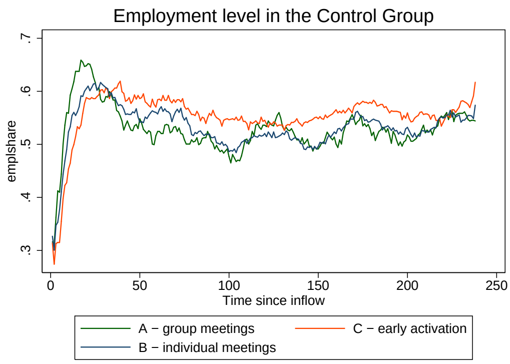


TABLE A.6.1: EFFECT ON ACCUMULATED EMPLOYMENT IN WEEK 104 - DIFFERENT SPECIFICATIONS

	(1)	(2)	(3)	(4)
Exp. A: Group meetings	1.98	2.32	2.30	2.12
	(1.90)	(1.96)	(1.96)	(1.75)
Exp. B: Individual meetings	3.94**	3.93**	3.48*	3.60**
	(1.85)	(1.83)	(1.84)	(1.71)
Exp. C: Early activation	0.01	-0.05	-0.26	-0.65
	(1.72)	(1.75)	(1.72)	(1.68)

Note: Standard errors in parenthesis, **p<0.05, *: p<0.1.
(1): No covariates, (2): Employment history (similar to Figure 3 results), (3): (2) plus job centre and inflow month dummies, (4): (3) + dummies for ethnicity, civil status, age and detailed labour market history.

TABLE A.6.2: EFFECT ON ACCUMULATED EMPLOYMENT IN WEEK 237 - DIFFERENT SPECIFICATIONS

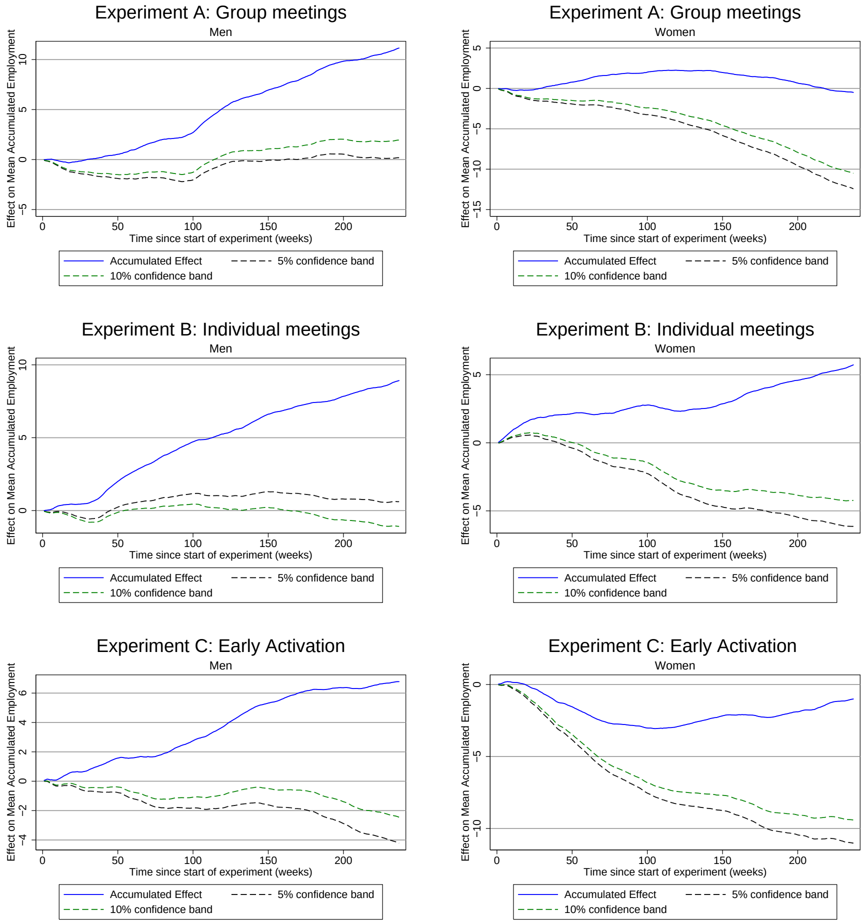
	(1)	(2)	(3)	(4)
Exp. A: Group meetings	4.55	4.81	4.58	4.50
	(4.21)	(4.19)	(3.99)	(3.91)
Exp. B: Individual meetings	6.99*	7.46*	7.44*	6.49*
	(4.06)	(3.89)	(3.90)	(3.66)
Exp. C: Early activation	2.75	2.95	2.59	1.54
	(3.94)	(3.85)	(3.67)	(3.62)

Note: Standard errors in parenthesis, **p<0.05, *: p<0.1. (1): No covariates, (2): Employment history (similar to Figure 3 results), (3): (2) plus job centre and inflow month dummies, (4): (3) + dummies for ethnicity, civil status, age and detailed labour market history.

Conditioning on covariates

In this appendix we show how the results presented in the main text is affected by conditioning on different combinations of covariates. In Figure 3 we present how the difference in accumulated employment develops across the treatment and control group. In Table A.6.1 we focus on the effect in week 104 and in Table A.6.2 we focus on the effect after 237 weeks. The main lesson from Table A.5 is that conditioning on more covariates leads to small improvements in precision of estimates, but does not change the qualitative findings.

FIGURE A.4: EFFECT OF THE EXPERIMENTS STRATIFYING ON GENDER



Heterogeneous effects

In this section, we first present the results stratifying the experiments on gender. Subsequently we present the results by age, civil status and time of inflow into the experiment for the three experiments.

TABLE A.7: EFFECTS FROM EXPERIMENT A: GROUP MEETINGS FOR WOMEN

	(1)	(2)	(3)	(4)	(5)
Treatment	-0.543	0.840	0.137	-12.35	0.737
	(6.024)	(6.602)	(7.398)	(12.14)	(8.391)
History	0.168*	0.168*	0.196*	0.164*	0.164*
	(0.0411)	(0.0405)	(0.0390)	(0.0422)	(0.0410)
Young		-5.915	-14.08	-6.280	-5.136
		(12.85)	(12.90)	(13.14)	(13.12)
Young*Treatment		-9.623	-8.535	1.131	-9.667
		(18.75)	(18.85)	(19.96)	(19.09)
Old			-28.16*		
			(9.715)		
Old*Treatment			1.672		
			(14.85)		
Married				-1.314	
				(9.340)	
Married*Treatment				17.56	
				(13.84)	
Early inflow					8.555
					(8.570)
Early inflow * Treatment					1.441
					(12.59)
Constant	101.2*	101.8*	106.0*	103.2*	98.78*
	(7.200)	(6.931)	(7.121)	(9.582)	(7.768)
N	631	631	631	631	631

Standard errors in parentheses
+ $p < 0.10$, * $p < 0.05$

Note: *History* measures employment history, *young* takes the value 1 if unemployed younger than 26, *old* takes the value 1 if unemployed older than 49, and *early inflow* takes the value one if the unemployed entered the experiment before week 16 in 2008.

TABLE A.8: EFFECTS FROM EXPERIMENT B: INDIVIDUAL MEETINGS FOR WOMEN

	(1)	(2)	(3)	(4)	(5)
Treatment	5.831	6.842	3.682	6.553	4.422
	(6.194)	(6.456)	(6.572)	(11.71)	(7.936)
History	0.162*	0.163*	0.224*	0.157*	0.167*
	(0.0411)	(0.0418)	(0.0411)	(0.0416)	(0.0430)
Young		-12.90	-27.64+	-6.595	-11.06
		(13.84)	(14.24)	(15.04)	(14.24)
Young*Treatment		-14.08	-9.536	-15.95	-16.02
		(19.98)	(20.95)	(21.45)	(20.47)
Old			-55.38*		
			(10.23)		
Old*Treatment			9.603		
			(14.66)		
Married				11.84	
				(9.170)	
Married*Treatment				1.133	
				(13.23)	
Early inflow					-13.24
					(9.076)
Early inflow * Treatment					6.193
					(12.11)
Constant	100.8*	101.8*	106.4*	93.92*	106.1*
	(7.617)	(7.433)	(7.497)	(9.928)	(8.748)
N	737	737	737	737	737

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$

Note: *History* measures employment history, *young* takes the value 1 if unemployed younger than 26, *old* takes the value 1 if unemployed older than 49, and *early inflow* takes the value one if the unemployed entered the experiment before week 16 in 2008.

TABLE A.9: EFFECTS FROM EXPERIMENT C: EARLY ACTIVATION FOR WOMEN

	(1)	(2)	(3)	(4)	(5)
Treatment	-1.041	1.496	1.081	1.525	4.872
	(5.235)	(5.388)	(5.779)	(8.298)	(6.616)
History	-0.0110	-0.00982	0.0125	-0.0185	-0.00826
	(0.0309)	(0.0314)	(0.0308)	(0.0293)	(0.0297)
Young		6.367	3.950	9.242	6.946
		(12.95)	(13.29)	(13.06)	(12.59)
Young*Treatment		-23.73	-23.89	-23.41	-25.02
		(17.36)	(17.71)	(17.76)	(16.60)
Old			-19.92		
			(12.34)		
Old*Treatment			5.644		
			(16.06)		
Married				10.68	
				(7.241)	
Married*Treatment				-0.566	
				(10.31)	
Early inflow					3.524
					(8.345)
Early inflow * Treatment					-9.435
					(11.02)
Constant	133.4*	132.8*	133.3*	127.9*	131.3*
	(4.587)	(4.764)	(4.747)	(6.202)	(5.256)
N	884	884	884	884	884

Standard errors in parentheses
+ $p < 0.10$, * $p < 0.05$

Note: *History* measures employment history, *young* takes the value 1 if unemployed younger than 26, *old* takes the value 1 if unemployed older than 49, and *early inflow* takes the value one if the unemployed entered the experiment before week 16 in 2008.

TABLE A.10: EFFECTS FROM EXPERIMENT A: GROUP MEETINGS FOR MEN

	(1)	(2)	(3)	(4)	(5)
Treatment	11.07+	13.45*	16.77*	-0.0627	12.33
	(5.688)	(6.128)	(7.305)	(9.302)	(7.968)
History	0.311*	0.315*	0.346*	0.314*	0.316*
	(0.0442)	(0.0438)	(0.0426)	(0.0448)	(0.0462)
Young		-0.241	-11.50	-5.995	-0.232
		(9.791)	(10.02)	(10.57)	(10.000)
Young*Treatment		-13.72	-16.96	-3.034	-13.69
		(13.57)	(14.15)	(14.16)	(14.44)
Old			-34.66*		
			(9.145)		
Old*Treatment			-11.85		
			(13.13)		
Married				-13.30	
				(8.329)	
Married*Treatment				23.72*	
				(11.69)	
Early inflow					-3.926
					(7.973)
Early inflow * Treatment					2.867
					(11.41)
Constant	54.64*	53.98*	59.63*	61.74*	55.39*
	(8.721)	(8.733)	(8.804)	(9.513)	(9.603)
N	729	729	729	729	729

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$

Note: *History* measures employment history, *young* takes the value 1 if unemployed younger than 26, *old* takes the value 1 if unemployed older than 49, and *early inflow* takes the value one if the unemployed entered the experiment before week 16 in 2008.

TABLE A.11: EFFECTS FROM EXPERIMENT B: INDIVIDUAL MEETINGS FOR MEN

	(1)	(2)	(3)	(4)	(5)
Treatment	8.925+	9.757+	9.413	-4.428	13.52+
	(5.228)	(5.844)	(6.727)	(9.155)	(7.633)
History	0.295*	0.292*	0.321*	0.275*	0.292*
	(0.0469)	(0.0457)	(0.0452)	(0.0461)	(0.0455)
Young		9.117	-8.217	12.52	9.419
		(10.44)	(10.89)	(11.00)	(10.65)
Young*Treatment		-5.378	-4.526	7.614	-5.495
		(14.83)	(15.77)	(16.20)	(15.92)
Old			-50.19*		
			(8.425)		
Old*Treatment			-1.450		
			(12.22)		
Married				6.974	
				(8.215)	
Married*Treatment				23.38*	
				(11.60)	
Early inflow					4.340
					(7.581)
Early inflow * Treatment					-8.909
					(11.41)
Constant	59.20*	58.43*	69.86*	57.16*	56.52*
	(9.636)	(9.278)	(9.175)	(10.50)	(9.815)
N	900	900	900	900	900

Standard errors in parentheses $+ p < 0.10$, $* p < 0.05$

Note: *History* measures employment history, *young* takes the value 1 if unemployed younger than 26, *old* takes the value 1 if unemployed older than 49, and *early inflow* takes the value one if the unemployed entered the experiment before week 16 in 2008.

TABLE A.12: EFFECTS FROM EXPERIMENT C: EARLY ACTIVATION FOR MEN

	(1)	(2)	(3)	(4)	(5)
Treatment	6.801	1.427	-2.225	-4.618	-9.863
	(5.913)	(6.153)	(6.647)	(8.365)	(7.553)
History	-0.0495+	-0.0487	0.0258	-0.0587*	-0.0525+
	(0.0301)	(0.0317)	(0.0312)	(0.0294)	(0.0317)
Young		-37.89*	-52.17*	-33.11*	-38.61*
		(10.94)	(11.09)	(11.34)	(11.11)
Young*Treatment		35.23*	36.14*	40.06*	34.47*
		(14.86)	(15.56)	(15.50)	(15.74)
Old			-70.97*		
			(10.92)		
Old*Treatment			22.71		
			(15.77)		
Married				11.83	
				(8.622)	
Married*Treatment				10.68	
				(11.84)	
Early inflow					-13.40+
					(7.897)
Early inflow * Treatment					29.26*
					(10.85)
Constant	131.3*	136.9*	141.7*	132.7*	142.4*
	(5.670)	(5.817)	(5.725)	(6.389)	(6.561)
N	839	839	839	839	839

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$

Note: *History* measures employment history, *young* takes the value 1 if unemployed younger than 26, *old* takes the value 1 if unemployed older than 49, and *early inflow* takes the value one if the unemployed entered the experiment before week 16 in 2008.

Duration Analysis

The purpose of this section is to distinguish short and long term effects and to investigate whether the experiment affects individuals' labour market situation through effects on unemployment exits or employment exits The analysis is based on a multi-state duration framework and focus is on unemployment and employment durations. All spells of employment and unemployment experienced during the first two-year period following the start of the experiment are included.[28] To compare outflow rates from unemployment and employment we need to model the selection process as randomization no longer holds. Therefore we estimate a duration model which allows us to account for this selection bias by explicitly modelling the selection process out of the state of interest (for more on this issue see Abbring & van den Berg, 2005 and Ham & LaLonde, 1996). The cost of this is the imposition of distributional assumptions, specifically we assume that the hazard rates can be modelled as mixed proportional hazards.

We use a non-parametric specification for the unobserved heterogeneity distribution, and we do not impose *a priory* a fixed number of mass points in the distribution of the unobserved components. Instead we rely on the Akaike Information Criterion to decide the number of mass points. The baseline hazard is piecewise constant. We control for various explanatory variables and estimate the models separately for men and women, as the above analysis has shown very different behavioural patterns over time. The method of estimation is NPMLE (see e.g. Heckman & Singer, 1983), and we treat individuals moving to other states than employment and unemployment as censored observations. The two hazard rates, from unemployment to employment and from employment to unemployment, are assumed to have a MPH form:

$$\theta_j(t \mid X_j, U_j, D) = \psi_j(t)\exp(X_j'\beta_j)\exp(\delta_j(\tau)D)\exp(U_j) \text{ for } j = ue, eu$$

where $\psi_j(t)$ is the baseline hazard for the transition j. Treatment causes a shift upward or downward in the hazard rates. We allow for time-varying treatment effects; $\delta_j(\tau)$, where τ denotes time since entry into the experiment (for unemployment, $\tau = t$, while for employment spells, τ is equal to the duration of unemployment plus the elapsed employment duration. The time-variation is chosen to capture the change in treatment intensity around week 16 (see the comments above on implementation lags). Formally, this means that we take $\delta_j(t \mid X_j) = \delta_j^1 1(\tau \leq 16) + \delta_j^2 1(\tau > 16)$.

Table A.13 reports the results from the estimation of the model specified above. Explanatory variables are included in the estimations but not reported in the table to save space.

[28]Treatment group assignment has no severe effects on transition rates into other labour market states such as self-sufficiency or other public income transfers. If anything, time spent in such states is reduced. These results are not reported but are available upon request.

TABLE A.13: ESTIMATES FROM THE DURATION MODEL

	Men		Women	
	Coeff	Std.err	Coeff	Std.err
Experiment A (group meetings)				
$\delta_{ue}(1-16)$	-0.066	0.127	-0.004	0.136
$\delta_{ue}(17+)$	-0.016	0.117	0.080	0.122
$\delta_{eu}(1-16)$	-0.073	0.380	0.529	0.490
$\delta_{eu}(17+)$	**-0.318**	0.130	0.029	0.232
Experiment B (individual meetings)				
$\delta_{ue}(1-16)$	0.017	0.108	*0.192*	0.116
$\delta_{ue}(17+)$	0.050	0.104	0.090	0.129
$\delta_{eu}(1-16)$	-0.082	0.404	-0.424	0.494
$\delta_{eu}(17+)$	**-0.283**	0.136	-0.044	0.177
Experiment C (early activation)				
$\delta_{ue}(1-16)$	0.143	0.103	0.036	0.109
$\delta_{ue}(17+)$	-0.039	0.095	**-0.224**	0.112
$\delta_{eu}(1-16)$	-0.084	0.409	0.047	0.407
$\delta_{eu}(17+)$	-0.140	0.134	0.000	0.163

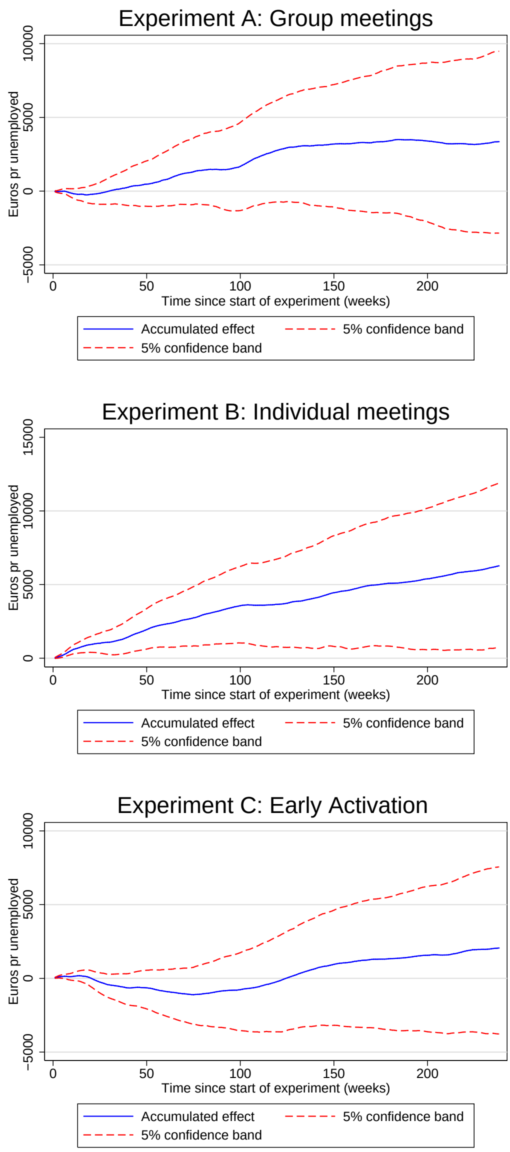
Note: bold (italic) figures indicate significant at the 5% (10%) level.

Cost benefit analysis

As a supplement to the analysis of the effects of the experiments on government budget, Figure A.5 presents the outcome of a more classical cost benefit analysis (welfare under distortive taxation). In the CBA the gains to society of running the experiments are calculated. The gains include the value of increased production and the in addition we assume that the marginal cost of public funds is 20%, meaning that to finance a given transfer to the unemployed the loss to society is 20%. When reducing transfers the gain to society amounts to 20% of the saved transfers. The saved transfers are not included in the CBA as this is simply a transfer internally in society. The costs are the direct costs of running the program and in addition to the marginal costs of public funds needed to finance the extra costs.

The CBA gives the same message as the analysis on the gain to government budgets. That is, the experiment with individual meetings generates a positive return, whereas the two other experiments do not give a significantly positive return. The CBA ignores general equilibrium and welfare effects, and the interpretation should keep that in mind. Also there is some discussion in the literature (e.g. Jabobs (2013) and Kreiner & Verdelin (2012)) on whether marginal costs of public funds should be included. There are different practices in the literature and we do not take a clear stand on whether they should be included or not. The main message will not be altered by assuming marginal costs of public funds to be 0.

FIGURE A.5: CBA OF THE THREE EXPERIMENTS



Further details for the calculations in the CBA:

An example of how we calculate the impact on the government budget is reported below in Table A.14. A similar calculation for the welfare gain under distortive taxation is reported in Table A.15.

Table A.16 report the amounts used to calculate the expenses on income transfers in the sample. In the CBA we calculate everything in Euros (1 Euro = 7.46 DKK) and in nominal 2012 amounts. For some transfer schemes we only have 2010 numbers and here we inflate the numbers assuming a yearly inflation factor of 5%.

Meeting expenses: Here we use the expenses reported by the job centres in 2008 and assume 2% inflation per year.

The 2008 level is around 60DKK with small local differences between job centres:

Activation expenses: We assume that the cost is 1770 Dkr for an individual in any of the states associated with activation in our data.

TABLE A.14: CALCULATING THE IMPACT ON THE GOVERNMENT BUDGET

TAX rate 37,5%		**Gender combined**
(in EURO)	Costs	
Experiment B		
Saved income transfers	3631	3631
1) Corrected for loss in tax revenue	-(0.375)*3631	-1362
2) Corrected for loss in VAT etc.	-3631*(1-0.375)*(0.245)	-556
Saved programme costs	-47	-47
Corrected saved total costs		**1666**
Value of production (empl weeks)	6508 (7.44)	
Increased tax revenue	0.375*6508	2441
Corrected for impact through VAT etc.	(1-0.375)*0.245*6508	997
Corrected acc gain from employment		**3438**
Net result of CBA (in EURO)		*5104*
Discounted net result with se's (2%)		4661 [690;8011]
Discounted Net result with se's (3%)		4457 [486;8215]

TABLE A.15: CALCULATING THE WELFARE GAIN UNDER DISTORTIVE TAXATION

Experiment B		
Saved income transfers	3631	726
Saved programme costs	-47	-57
Saved total costs		669
Acc.gain in employment (weeks)		7.44
Value of increased production		6508
Net result of CBA (in EURO)		7178
Discounted net result (2%)		6556[1010;11599]
Discounted net result (3%)		6269[723;11886]

TABLE A.16: Cost of transfer schemes

Transfer scheme	Amount (DKK)
Passive transfers	2546
Private Work Practice	2444
Public Work Practice	2180
Educational programs	3739
Educational programs (other)	3433
Early retirement	3628
Subsidized employment	3463
Employment or Self-sufficiency	0
Full-time UI	3739
Apprenticeship program	1100
Study-aid	1306
Special temporary transfer (ledighedsydelse)	3585
Employment subsidy (sickness)	3626
Pre-rehabilitation	2546
Sickness benefits	3940
Public Pension Scheme	2378
Specific programme (flexydelse)	3634
Leave schemes	3739

The fourth experiment

The fourth experiment was conducted in the region of Southern Denmark, and it consisted first of weekly group meetings (similar to those in experiment A) during the first 13 weeks of unemployment. After 13 weeks of open unemployment, the unemployed has to participate in an ALMP of at least 25 hours per week for at least 13 weeks. After 6 months of unemployment, the experimental treatment ends. So this is a combination of early counselling and an activation wall.[29] Data from experiment D has been showing a high number of irregularities, and therefore we will primar-

[29]The fourth experiment was partially intended to mimic an earlier complex experimental treatment from 2005-6, see e.g. Graversen & van Ours (2008a; 2008b) and Rosholm (2008).

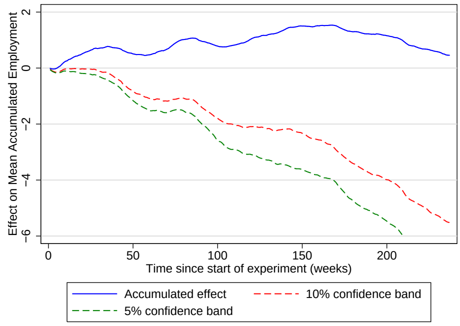
ily focus on the 3 other experiments in our analysis below.[30] The main results from experiment D overall and split by gender are shown below.

Figure A.6 shows the effect of combining group meetings with an activation wall. We observe a positive (and, in fact, significant) effect for women initially, but compared to the case in Experiment A it appears that when we combine meetings with early activation, the effect stops accumulating after less than a year. After two years there is no difference in accumulated employment between the treatment and the control groups, but the effect tends to become more and more negative. For men the effect is close to zero the first year, after which the difference between employment in the two groups favours the control group, although not significantly so. It is interesting that, whereas there was a positive effect for men of early activation in isolation, this is no longer the case when combined with group meetings. If valid, the results could suggest that there is a limit on how intense early ALMPs should be, but as mentioned above we are also uncertain as to the validity of the outcome data. We have tried to exclude the job centre that did not comply with the treatment protocol, but this did not change the results very much.

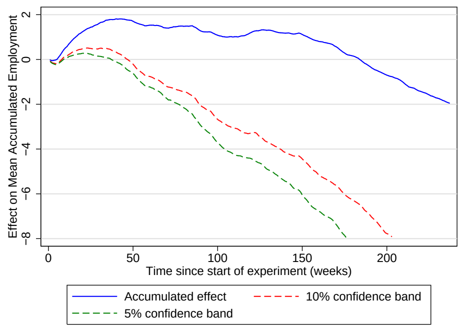
[30] For example, when the administrative data set is updated once per month, this involves obtaining raw data from the job centres. For the two job centres involved in the intervention, these data have been overwritten backwards in time at some point, leading to large changes in the estimated programme effects, depending on the version of the dataset used. Moreover, general compliance problems were reported for the implementation phase, casting some doubts on the validity of the data on meetings and activation as well. Hence, we shall report the results from this experiment, but only for completeness.

FIGURE A.6: THE EMPLOYMENT EFFECT OF EXPERIMENT D (GROUP MEETINGS AND ACTIVATION WALL)

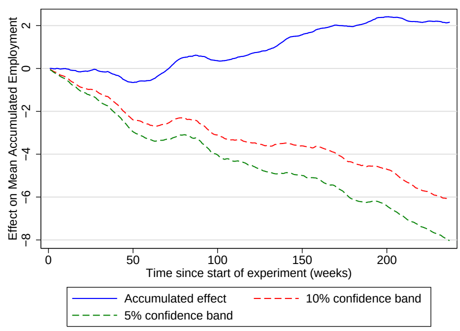
All (pooled sample)



Women



Men



Note: The figure shows the accumulated difference in the employment rate between the treatment and control groups. We report only the relevant side of the two-sided confidence bands which are obtained by bootstrapping.

TABLE A.17: DESCRIPTIVE STATISTICS FOR EXPERIMENT D: MEETINGS AND EARLY ACTIVATION

Southern Denmark (D)	**Control group**		**Treatment group**	
Characteristics	Men	Women	Men	Women
	Average	Average	Average	Average
Age (years)	39.28	39.37	40.81	39.58
Under 30	0.31	0.22	0.24	0.20
30-49	0.43	0.57	0.47	0.60
Above 49	0.26	0.21	0.28	0.20
Marriage	0.49	0.63	0.46	0.61
Danish origin	0.85	0.86	0.87	0.91
Western origin, not Danish	0.07	0.05	0.05	0.02
Non-Western	0.08	0.09	0.08	0.07
Transfer degree < 0.1 last 3 years	0.17	0.06	0.13	0.07
Transfer degree ϵ (0.1;0.5) last 3 years	0.60	0.48	0.63	0.48
Transfer degree > 0.5 last 3 years	0.23	0.46	0.25	0.45
Prior unemployment spell (days)	0.05	0.23	0.19	0.12
Share of new unemployed	0.98	0.97	0.97	0.97
Share in "Construction" industry UI fund	0.10	0.02	0.13	0.01
Share in "Manufacturing" industry UI fund	0.41	0.21	0.37	0.19
Share in Other UI fund	0.10	0.14	0.10	0.13
Number of observations	247	248	247	266

CHAPTER 3

ASSESSING WELFARE EFFECTS OF ALMPS: COMBINING A STRUCTURAL MODEL AND EXPERIMENTAL DATA

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Abstract[1]

Utility costs associated with participation in Active Labour Market Programs (ALMPs) have been suggested in the literature as a way to interpret the well established existence of threat or ex ante effects associated with programme participation. This paper combines data from a randomized experiment with a structural economic model to estimate the utility costs and potential productive effects from programme participation. The model generates a link between observed behaviour such as job finding rates into structural parameters such as utility costs, while the experiment generates exogenous variation in programme participation and ensures that results are not driven by unobserved confounders. The estimates of the model are used to calculate the compensating variation, i.e. the monetary compensation which leaves individuals indifferent between belonging to the treatment or the control group. This

[1]Acknowledgement: This paper benefited from numerous comments and discussions at seminars and conferences. I gratefully acknowledge comments and suggestions from C. Ferrall, J. Lise, T.M. Andersen, J. Bagger, M. Svarer and R. Vejlin. I thank the Danish Labour Market Board for making the data available and the CAFE grant for enabling part of this research. Results were generated using Ox (www.doornik.com) and FiveO (a part of niqlow: jdi.econ.queensu.ca/niqlow).

enables an analysis of whether the programmes represent a worthwhile social investment by comparing the gains to costs including those borne by the participating individuals. Thereby some empirical quantification of a long lasting discussion in the literature that analyses the optimal design of labour market policies is provided. The estimates of the structural model are exploited to analyse the heterogeneity in the compensating variation in relation to future prospects and the timing of treatment in an environment which is characterized by duration dependence in unemployment and rich heterogeneity across individuals. The results suggest that traditional Cost-Benefit calculations which do not take the individual costs into account largely overstate the gain from having these programmes. The costs are substantial and are important to quantify in order to assess whether the current mix between programmes and UI is optimal.

3.1 Introduction

In this paper I estimate how individuals value participation in Active Labour Market Programs (ALMPs) which serve as a conditionality for receiving unemployment insurance (UI). Any potential costs associated with programme participation are a crucial input in an analysis of whether such conditionalities in transfer recipiency constitute a worthwhile social investment or whether potential suboptimal individual behaviour is better controlled by e.g. reducing benefits. Two kinds of ALMPs are analysed: meetings and short activation programmes at the job centre. By combining data from a randomized experiment and a structural economic model, I estimate the utility costs and calculate the resulting compensating variation (CV), i.e. the monetary compensation which equalize expected utility across the treatment and control group at inflow into the experiment. The CV takes potential productive effects from programme participation (increases in job offer rates), the value of alternative choices and future prospects for participants into account. The estimates, and detailed data on other costs and gains from the programmes,[2] allows for an assessment of whether the programmes under investigation constitute a worthwhile social investments. Thereby some empirical evidence on how ALMPs affect individual behaviour and how this should affect our use of these programmes is provided.

While there exists a large literature evaluating the effectiveness, in terms of e.g. job finding, of various kinds of ALMPs (for reviews see Card et al. (2010), Kluve (2010)) there are very few papers focusing on the mechanism behind any generated impact and in particular the importance of utility costs. The empirical literature (see Black et al. (2003a) and Hagglund (2011)) has documented the presence of so-called threat or ex ante effects which suggest that individuals view programme participation as

[2] The costs considered are i) costs associated with running the programmes, ii) the compensating variation associated with the existence of the experiment, iii) costs associated with an increase in production (lost leisure). Gains considered are i) value of increased production, ii) saved income transfers.

costly. As these programmes 'tax' leisure time by replacing it with time in the job centre (public employment service); unpleasant or uninspiring work, increased effort, monitoring or stigma are all potential explanations for the existence of such costs.[3] The existence of costs implies that an evaluation of programme impacts through an analysis of its impact on e.g. employment, is only partial in nature. Impacts arise at a cost, and in order to assess whether a programme is actually beneficial one needs to contrast the benefits generated by the programme with its costs - here both actual programme costs (staff at the job centre) and individual costs. Since information about individual costs are generally not available this ultimately introduces an imbalance between what programme evaluators evaluate as beneficial, and what society (or a social planner) would.[4] The imbalance stems from the fact that the unemployed respond to costs which are not included in the evaluation of the programme. This favours programmes which generate e.g. the largest reductions in unemployment duration regardless of how participants value participation in the programmes. The importance of this imbalance depends on the magnitude of utility costs, and therefore further knowledge is of central interest. The question is also interesting from a theoretical point of view as the magnitude of costs is important for whether conditionalities in transfer recipiency *can* actually constitute a worthwhile social investment (see next section).

Any quantification of costs borne by the individual requires some link to a behavioural model, the surrounding environment and an accurate description of the incentives faced by potential participants over time. This link generates a translation of observed behaviour into decision theoretic parameters such as utility costs. In the case of ALMPs this quantification is further challenged by the fact that participation is (ultimately) a conditionality for receiving UI benefits. Non-participation is therefore associated with a substantial loss of income due to sanctions or suspension from benefits for a period of time. Therefore a direct expression of preferences for the programme through choices of potential participants, choosing whether to participate or not, are not present - unemployed workers will choose to participate although participation is associated with costs that outweigh direct benefits from participation.[5] Costs must therefore be determined indirectly through behaviour such as job finding rates or wages in future employment. This requires a full economic model of

[3]Both pecuniary and non-pecuniary costs are therefore explanations for why the non-market (job centre) wage could differ from the market wage and they are essentially explanations of compensating wage differentials (effort in the job centre is unpleasant and thus the "payment" is higher). In this paper I do not try to distinguish these different explanations, but instead estimate the total impact on agents.

[4]In the words of Heckman et al. (1999): *".. By doing this, however, these evaluations value labour supply in the market sector at the market wage, but value labour supply in the non-market sector at a zero wage. By contrast, individuals value labour supply in the non-market sector at their reservation wage."*

[5]A literature starting with Moffitt (1983) identifies the stigma/utility cost associated with receiving welfare comparing take-ups and non-take-ups (extensive margin). This paper use variation in the intensive margin (the intensity of the conditionality) and compare the behaviour of individuals in intensive regimes with similar individuals in less intensive regimes. Variation in the intensive margin is generated by a social experiment and is thus exogenous, which is useful in the identification of the utility cost.

behaviour and an accurate description of behaviour in the absence of the programme in order to identify the change in behaviour induced by the programme and thus the individual costs.

In order to quantify costs this paper develops a dynamic discrete choice model of job search and estimates it exploiting data from a Danish randomized experiment. The structural framework provides a mapping from observed behaviour into the determinants of decision making at the individual level. The experiment improves identification of unobserved costs for two reasons. First the experiment generates exogenous variation in programme participation, which ensures that differences in behaviour between control and treatment group can be prescribed to the impact of the programme. Secondly as the experiment is a finitely lived and time-varying intervention which generates useful variation in the incentives faced by individuals and improve identification of the central parameters.[6] To exploit the experimental variation the model contains a thorough description of how the treatment changes over time which is particularly important to take into account when estimating the costs of programme participation in a dynamic setting. It allows agents to take into account that incentives for the treatment group change as they progress through the experiment - every week is one week closer to the expiration of the intensified treatment and thus the future cost associated with programme participation declines. The model is used to calculate the compensating variation (CV) associated with the experiment. The CV takes into account that individuals can influence their likelihood of remaining unemployed, and thus their chances of participation in the programmes. The CV is therefore different from utility costs that reflect the immediate cost associated with inevitable programme participation. For instance the CV will be lower for individuals "capable" of leaving unemployment fast compared to individuals with worse employment prospects. Similarly the CV associated with interventions at inflow into unemployment is higher than in the case where programmes start later in the unemployment spell because the former makes future participation more likely. A final important aspect which influences the size of the CV is the presence of risk aversion in the model. This increases the monetary compensation due to decreasing marginal utility of wealth (decreasing "efficiency" of the initial monetary compensation) and the inter-temporal separation between the paid compensation and future programme participation.[7] Naturally a quantification of these aspects - and an as-

[6] From a methodological point of view this paper is therefore a part of a growing literature combining economic models and empirical strategies with high internal validity (here experiments). Two different approaches can roughly be distinguished by whether the experimental variation is used as a source of validation (a test of the behavioural model) or identification of parameters of the model. See Wolpin and Todd (2006), Attanasio et al. (2012), Ferrall (2012) and Lamadon et al. (2004) for examples of different approaches.

[7] There are no asset markets or savings in the model, the existence of these would allow the agents to smooth consumption across states and thus potentially decrease the impact of this later channel. In an environment without risk aversion the accumulated utility costs would represent an upper bound of the welfare costs associated with the experiment, but since risk aversion is an important justification for the existence of UI this is incorporated into the model.

sessment of their relative importance - are important inputs in the discussion and future design of optimal labour market policies. The aim of the model is to generate an environment with several sources of heterogeneity between unemployed agents and in the cost associated with programme participation. The heterogeneity in the environment implies that the impact of ALMPs differs across individuals depending on their current state and future prospects. This way heterogeneous treatment effects are endogenous to the model and the resulting CV - which serves as a crucial input in a subsequent welfare calculation - will also vary across agents.
In the model agents face two discrete choices: while unemployed they choose a level of search intensity and if a job offer is present they choose whether to accept the job offer or not. The social environment is stationary and ergodic. Employed individuals stochastically accumulate skills each period while employed. Their job separation rate depends on their level of skills. If they loose their job their stock of skills may depreciate. Unemployed workers face job offer rates which depend on their search activity and their duration in unemployment. Wages depend on a draw of firm productivity and the level of skills. While unemployed individuals receive UI and in return have to participate in meetings/activation programmes - participation in these programmes is potentially costly but may increase job offer rates.
From a methodological point of view the model follows in the lines of a novel framework developed in Ferrall (2002, 2012).[8] This framework extends the classical work by e.g. Rust (1987) into a setting with unobserved non-IID time-varying state-variables, unanticipated (or zero probability) choices, corrections for endogenous sampling (initial conditions) and the inclusion of a finitely lived experiment. In order to improve the identification of unobserved state variables the framework is extended in this paper. In particular, moments which are only indirectly linked to state variables, and therefore not directly computable from the distribution over states in a given period, are added to the set of moments which are used in estimation. The extension includes introducing an "inner Markov chain" to the solution algorithm outlined in Ferrall (2012), which calculates the distribution of e.g. employment duration over time although employment duration is not a state variable in the model. The modification shows how further moments can be added to the model without increasing the state space or having to simulate the model. The extension improves the estimation of the transition probabilities for unobserved state-variables as it increases the number of predictions of the model which can be compared to corresponding data moments, for instance moments describing the distribution of employment

[8] Ferrall (2012) studies the Self-Sufficiency-Project in a structural model and develops a framework which incorporates the non-stationarities implied by the design of the experiment. He use the model to study how the SSP affect incentives for low wage workers and whether the policy enables them to escape the "poverty trap". The model includes a waiting period and a qualifying period where potential participants must obtain work to qualify for a wage subsidy. The analysis illustrates that these non-stationarities are crucial in interpreting the experimental impact. Furthermore the paper shows how a well-defined structural model which incorporates these non-stationarities substantially improves out of sample predictions, the overall fit of the model and thus any policy recommendations.

durations are informative about the interaction between skills and job separations. The estimates suggest that the cost associated with programme participation is non-negligible, in particular unemployed would be willing to decrease UI benefits in a given week with up to 50 % in order to escape ALMP participation. The size of the utility costs are just below the lowest possible sanction individuals may receive if they do not participate in ALMPs. The average CV associated with the experiment is up to 20 times larger than the monetary costs associated with programme participation. The analysis shows that the CV varies with future prospects, in particular it is smaller for individuals where alternative choices are more valuable - for instance in the case of high skilled versus low skilled workers. The high average CV is partly driven by individuals with low employment prospects who need larger compensation.

Using detailed information on the benefits and costs associated with the experiment under investigation the paper presents a welfare analysis which includes the costs associated with the loss of leisure in relation to both increases in employment rates *and* due to an increase in participation in ALMPs. The size of the compensating variation implies that the gain from the most favourable intervention (meetings at the job centre) is reduced by 50 % while the other intervention (early activation) is associated with only a small increase in welfare. The welfare analysis thereby illustrates the importance of including more aspects that just direct programme costs in an assessment of the optimal level of ALMPs in the labour market.

This paper proceeds as follows: the next section contains some background and a review of the related literature. Next the experiment and the available data are presented. The following section contains some key features of the data which the model will try to incorporate. Then the model and the empirical implementation are presented. The final sections contains results and a conclusion.

3.2 Background and Related Literature

Policy makers have become increasingly focused on adverse selection and moral hazard in relation to UI as the empirical relevance of such phenomena has been documented in the literature (see e.g. review by Chetty and Finkelstein (2013)). Several countries, and especially Northern European countries (see e.g. Andersen and Svarer (2007)), have introduced programmes targeting UI recipients such as meetings, job search assistance and workfare/activation programmes in an attempt to re-align incentives, reduce moral hazard and improve market functioning. By some this is referred to as 'active social insurance' (Roed (2012)) to underline that UI is not only a passive transfer of income, but instead participation in these programmes serve as a conditionality for receiving benefits.[9] ALMPs can have two very different aims:

[9] One example of this is the Danish labour market model were UI is generally generous and the level of employment protection is quite low. The sustainability of such a system could be challenged by high structural unemployment rates, e.g. due to low incentives for workers to leave unemployment. Therefore ALMPs are considered a crucial part of the model and participation in such programmes is considered

i) improve the qualification level of the unemployed through e.g. counselling or training and thus improve future job possibilities, or ii) they serve as mechanisms for ensuring that the unemployed are actually available and searching to get out of unemployment. The latter objective is often mentioned as an important component as the empirical literature has found limited relevance of the first aim - especially in the case of traditional training programmes (see for instance Heckman et al. (1999) and Kluve (2010)).

In this paper ii) is rationalized as a utility cost associated with programme participation while i) enters through an increase in job offer arrival rates immediately after programme participation. The cost might consist of several policy invariant parameters such as stigma or disutility associated with participation (see e.g. Moffitt (1983)), loss of leisure and an increase in effort in order to attend meetings at the job centre.[10] Although ALMPs might be successful in reducing moral hazard in the market by increasing e.g. search activity, any costs associated with programme participation challenges whether these programmes actually make individuals better off - or whether they would instead prefer lower benefits. These costs imply that some individuals are worse off than before the introduction of ALMPs, this is in fact why some search more to leave unemployment before being activated, while at the same time the market is now more efficient. The overall implications for welfare are therefore less clear.

There is very little empirical work trying to quantify costs or assess the welfare implications of programme participation. Greenberg and Robins (2008) provide estimates of the value of lost leisure for participants in the Self-Sufficiency-Project in Canada. This enables them to quantify the gain in consumer surplus instead of the raw income gain associated with the wage subsidy.[11] The authors find that when the loss in non-market time is taken into account, the net benefits from that policy is substantially reduced and sometimes even negative. Their analysis thereby provides further empirical justification for why knowledge of how participants value their time in different settings should be of central interest in the literature and in the evaluation of programmes.

The analysis in Greenberg and Robins (2008) is different in a number of dimensions compared to the current paper. First, as participation is voluntary, participants prefer

both a right and a duty (see e.g. Andersen and Svarer (2007)).

[10] Search activity could also change due to the fear/risk of getting a sanction for non-compliance with the search requirements (see below). In the model presented below search activity will change as a response to utility costs, there is no risk of getting a sanction in the model.

[11] Using a matching procedure they identify the group of compliers in the experiment (the part of the treatment group which enters employment caused by the subsidy). For this group of workers they use the earned wage in employment, w^* (including the subsidy) and the same wage without the subsidy, w^n. The two observations and economic theory can be used to bound the individual labour supply curve. The analysis exploits the fact that the individual reservation wage for starting to work must be above w^n - as the compliers do not work at inflow into the experiment - and thus by adding assumptions about the value of w_R and the curvature of the labour supply curve the authors can calculate the part of the gain in income which is offset by increased effort.

participation and the size of the subsidy is used as a reflection of the value generated by the programme. A similar expression for the value of the programme does not exist for the experiment presented below - here participation is an obligation. The value of the programme will therefore have to be determined indirectly through changes in behaviour such as job finding rates. Second, while Greenberg and Robins (2008) use the size of the subsidy as an expression of the value created by the programme, in the current paper exogenous variation in treatment status is exploited to compare behaviour between treated and non-treated. This source of variation, and the structural model, allows for a quantification of a broader concept of costs including fixed costs associated with actual participation. The model generates a mapping of different channels of behaviour into decision parameters and therefore exploit differences in behaviour along other channels than wages only to learn about the size of costs.
Below other related theoretical and methodological literature is discussed.

Other related theoretical and empirical literature

The theoretical literature has analysed how and whether conditionalities such as workfare *can* in fact improve welfare in a setting where society has a preference for redistribution. In summary there exists normative work on whether, and under which conditions, conditionalities in benefit recipiency are welfare improving. The theoretical literature has studied two different margins of behaviour. Both along the extensive margin,[12] i.e. the selection of individuals into unemployment, and along the intensive margin,[13]i.e. behaviour while in unemployment (e.g. job search), behaviour may change with the introduction of workfare. The literature shows that workfare can be welfare improving in some settings but it depends on the environment, the nature of costs and the margin on which behaviour is studied.
A number of other papers have analysed how labour market programmes affect individual behaviour in the labour market in a theoretical and empirical framework (see also Cohen-Goldner and Eckstein (2010); Albrecht et al. (2009a); Lamadon et al. (2004)). Adda et al. (2007) develop a structural dynamic model of labour supply to study the impacts of the Swedish labour market programmes. The study differs in a

[12]Besley and Coate (1992) show that while conditionalities (costly unproductive activities) improve market functioning and redistribute income to 'needy' individuals, this does not imply that agents are better of in terms of utility. In particular the work requirement implies a cost of leisure which is high enough to offset the increase in benefits. Kreiner and Tranaes (2005) show that in an environment with voluntary and involuntary unemployment, workfare can be an effective screening device for UI and lead to a Pareto improvement in the economy. The main difference to the setting in Besley and Coate (1992) is that the screening problem is now focused on individuals who differ in their preference for leisure and not in terms of productivity. Other papers that analyse settings where conditionalities can be welfare improving are Cuff (2000) and Beaudry et al. (2009).

[13]Andersen and Svarer (2014) focus on the effects of workfare on moral hazard in job search in a search and matching model. To study behaviour along the intensive margin their framework is dynamic, and their analysis shows that the threat of future participation in workfare increases the search effort of the unemployed before actual participation and lowers his reservation wage. Under a utilitarian criterion the authors show that workfare can in fact improve welfare.

number of ways from the current one, most importantly programme participation is voluntary in their setting and without costs. The model is used to solve the self-selection problem into programme participation and analyse programme impacts on earnings and job offers.[14] Van Den Berg and Van Der Klaauw (2006) analyse how counselling and monitoring programmes affect the transition rate into employment in a Dutch setting. They show theoretically that incomplete monitoring of job search can have adverse effects as individuals substitute search towards formal (and measurable) search channels and away from informal search. They compare the predictions to results from a social experiment which includes a survey about search channels and find some evidence of substitution effects. The paper is focused on the impact of closer monitoring on different search channels and the existence of individual costs beyond the costs of searching or the implications for welfare are not analysed. The impacts on employment from the studied intervention are small and the authors explain this by inefficient targeting of the programme and a low intensity of treatment. They argue that a too excessive focus on the monitoring of job search activity is inefficient and that alternative policies such as 'leisure taxes' may be more efficient.

Summary of literature and relation to model

The presentation above have shown that while there exists some empirical and theoretical work on how labour market programmes affects both participants and non-participants there exists very little work focused on the existence of individual costs and their implications for the attractiveness of these programmes. The theoretical literature shows that workfare can be welfare improving in some settings but it depends on the environment, the nature of costs and the margin on which behaviour is studied.

This paper exploits changes in behaviour along the intensive margin to identify the cost associated with programme participation (e.g. changes in unemployment duration). Since the experiment is an unexpected event and does not change the inflow into unemployment and because employment separations are exogenous in the model, there will be no characterization of how selection into unemployment depends on the existence and intensity of ALMPs. It is however perfectly plausible that behaviour on both margins is driven by the the same cost (this requires that we disregard any fixed costs associated with entry into UI which depends on the intensity of future ALMPs), but naturally predictions of behaviour along the extensive margin requires a quantification of all the decision parameters related to this decision.[15]

[14] The type of programmes under investigation are more traditional training or job-experience programmes with longer durations. By participating in the programmes participants renew their eligibility to UI. The authors show that by abolishing the latter rule, welfare can be increased as the efficiency of the market increases (as moral hazard is reduced). In line with previous literature they find limited effects from job training programmes and modest impacts from job experience programmes.

[15] Due to data limitations such a quantification is outside the scope of the current paper, in particular data on the reason for employment separations would be required to model this margin.

Finally, there is no monitoring of search activity in the model presented below. One difference between this paper and earlier work related to sanctions and monitoring (see also Fredriksson and Holmlund (2006)) is therefore that the model below associate a cost to utility to each meeting at the job centre whereas the earlier literature attributes all changes in behaviour to the disutility in the case where individuals are sanctioned. The two formulations generate similar behaviour but the former is directly linked to current periods costs. While in reality both explanations are probably relevant to explain the increases in the job finding rate I report below, it is beyond the scope of this paper to separate the two. Furthermore, as the sanctioning rate is very low in the Danish labour market (see e.g. Svarer (2011)) and the stated intention of the treatments stated below was no intensification of monitoring, this could suggest that the risk of getting caught is maybe the less relevant channel.

3.3 Data, Institutions and the Experiment

This section presents the Danish institutional setting, the social experiment and the data used in the analysis. The Danish labour market is rather flexible and is referred to as an example of the Flexicurity model.[16] It has less employment protection legislation than most continental European countries and much higher labour turnover (see e.g. OECD (2009)). At the same time a tight social security net with near-universal eligibility for income transfers keeps income security high. Finally active labour market policies are seen as an important part of this model.[17] Today ALMPs are among the most intensive in OECD, with around 1.3% of GDP spent per year on active policies and more than 12 billion Dkk on ALMPs alone (see Board (2014)). There are two types of benefits for unemployed workers, UI benefits and social assistance. Approximately 80% of the labour force are members of a UI fund and therefore eligible for UI benefits. The remaining 20% may receive means tested social assistance. The policies that apply to UI recipients are presented below, they constitute the target group of the experiment presented in the next section.
UI benefits are essentially a flat rate due to an upper bound on payments (see e.g. Lentz (2009)) and the duration of benefits in the period under study (2008-2009) is 4 years. A 'right and duty' principle governs labour market policies. Unemployed individuals have the *right* to compensation for the loss of income, but also the *duty* to take action to get back into employment and follow instructions from the job centre (public employment service). Interactions between public authorities and unemployed individuals take place in job centres and activities are mainly contact (meetings) and activation (see Maibom et al. (2014)). At inflow into unemployment a

[16] Before the financial crisis the EU commission recommended this model to its member states, referring to Denmark as a model case (Commission (2007))

[17] The 1980s were characterized by persistently high unemployment rates and a low intensity of ALMPs. As the intensity of ALMPs grew, structural unemployment fell, and therefore observers have seen intensive ALMPs as an important part of the Flexicurity model (see e.g. Andersen and Svarer (2007)).

UI eligible individual has to register at the local job centre. She then has to attend a meeting with a caseworker every 3rd month and to participate in an activation programme after 9 months (6 if below 30 years old) of unemployment and subsequently every 26 weeks. For the experiment outlined below these are the labour market policies that will be faced by individuals in the control groups. Treated individuals are obliged to participate in further activities beyond the activities presented here.
In order to increase the knowledge about the effectiveness of current labour market policies the National Labour Market Authorities have conducted a series of experiments. Evaluations have established that there are potentially favourable gains from earlier and intensive active labour market programmes (ALMPs) in the form of either meetings or activation programmes (see e.g. Graversen and van Ours (2008a); Maibom et al. (2015)). But, importantly the evaluations says nothing about the effect of these interventions on welfare.

Experimental Design

The experiment was conducted in two different regions in Denmark in 2008. Each region had a separate treatment (either an intensification of individual meetings or early activation) and each region also had their own treatment and control group. The experiment is presented and analysed in Maibom et al. (2015) and I refer to their paper for details on the setting beyond what is presented below.[18]
The target population of the experiments were UI eligible individuals who became unemployed during weeks 8-29 in 2008. The assignment to treatment or control groups was based on the date of birth. Individuals born on the $16^{th} - 31^{st}$ were assigned to the treatment groups, while those born on the $1^{st} - 15$ were assigned to the control groups. No information was given to the unemployed workers on the selection rule. Once immigrants are excluded from the sample Maibom et al. (2015) find no deviations from random assignment, and therefore I treat it as such. See also Appendix B Table 3.13, for balance of means tests and descriptives.
At inflow into the experiment treated individuals received a letter explaining the new treatment to which they will be exposed. The information letter marks the start of the treatment, since the worker may react to the information on the new regime. Table 3.1 presents an overview of the activities in the treatment group beyond the regular activities presented above. Individuals in the treatment group from the region around the capital city, Copenhagen (R1), had to participate in individual meetings with a caseworker every other week for the first 13 weeks of unemployment, a total of 6-7 meetings during the first 13 weeks of the experiment. The stated intention of the individual meetings was counselling of the unemployed - no extra monitoring was required to take place, but naturally this says nothing about the perception of

[18]The experiments investigated here were a part of a larger experiment 'Quickly Back to Work 2' which consisted of four separate experiments, each with its own treatment and control group. See Maibom et al. (2015) for details.

the meetings from the point of view of the unemployed nor the actual content. Individuals in the treatment group from the region around the second largest city, Aarhus (R2), were required to participate in an activation programme for at least 25 hours per week from week 14 in unemployment until week 26. This experiment - the activation wall - was designed specifically to investigate the presence of ex ante effects due to the knowledge of having to participate in an activation programme, as well as ex post effects of actually having participated.[19,20]

From the presentation it is clear that the experiment have some important features

Table 3.1. Content of the experiments

Weeks	Meetings (R1)	*	Activation (R2)	*
0-1	Recieve Information Letter	W	Recieve Information Letter	W
1-13	Individual bi-weekly meetings	T		W
14-26		PT	activation programme	T
26-		PT		PT

Note: The table presents the content of activities individuals in the treatment group has to participate in *beyond* any regular activities (see above). R1 denotes the meetings region and R2 the region with activation.
* W: waiting phase, T: treatment phase, PT: post-treatment phase

which should be incorporated into the structural representation to model the incentives faced by unemployed workers accurately - and thus estimate key decision parameters credibly. In particular, the unemployed treated individuals progress through three different phases with different duration (phases are outlined in Table 3.1): i) a waiting phase (W) which starts with the information letter and stops when actual treatment begins (in R1 this constitutes 1-2 weeks and in R2 this will be 13 weeks), ii) the actual treatment phase (T) and iii) ex-post treatment (PT) which marks the end of the experiment. The model presented below is set up to account for the fact that incentives change as individuals progress through the experiment. For instance individuals might be more likely to increase their search effort as T approaches and similarly the incentive to leave unemployment declines as PT approaches and the future intensity of activities declines.

[19]Note that in order to test specifically for the ex ante effect in an experimental setting, there should have been no actual treatment taking place from week 13 onwards. For our analysis the assumptions implied by the model allows us to test for the existence of such effects namely through the presence of a substantial utility cost.

[20]An important advantage of the available data in Maibom et al. (2015) is that it allows evaluators to assess the extent to which the planned treatment was actually implemented. Their analysis documents that the intended treatment was implemented to a large extend. There are also some deviations from perfect compliance as the meetings and activation intensity is not as high as planned (80% versus 100 % by design). While there can be several explanations this issue is ignored below as agents might still react solely to the threat of participation. This corresponds to assuming that non-participation in treatment in a given week is truly exogenous and unexpected (for instance due to administrative changes or other events).

Data and Definitions

The data are extracted from administrative registers merged by the National Labour Market Authority into an event history data set, which records and governs the payments of public income transfers, records participation in ALMPs, and has information on periods of employment. The data includes detailed weekly information on: labour market status and history (employment, unemployment, in education, on leave, etc.). Labour market status is calculated based on information from the register on payments of public income transfers. This data is subsequently merged with two other datasets BFL and IDA[21] in order to obtain further information, in particular monthly wages before taxes, hours and the education level of workers.[22]
The raw sample excluding immigrants consists of 3385 individuals who are either assigned to the treatment or control group. To have a more homogeneous sample I disregard workers below the age of 22 and above the age of 58.[23] This leaves 3099 individuals in the sample. The data is divided into sub-groups depending on the educational level of the individual. There are 3 educational levels: low (individuals with only primary education and less than 12 years of education), medium (individuals with vocational education and 12-14 years of education), high (individuals with further education and above 14 years of education). Table 3.2 shows the division into subgroups defined by region, treatment status and education levels.
The final data identifies individuals in any public support schemes at a given point

Table 3.2. Number of observations

Education Groups	Low	Medium	High
Control (R1)	211	376	137
Treatment (R1)	212	399	141
Control (R2)	102	298	396
Treatment (R2)	92	307	428

R1: meetings region, R2: activation

in time - these will be unemployed in the model presented below. The data used does not allow for a meaningful distinction between individuals in regular employment (where the registers contain wage information etc.) and individuals who are in a residual 'self-sufficient' group where there is no information on either wages or public

[21] IDA: Integrated Database for Labour Market Research. IDA is a matched employer-employee panel containing socio-economic information on the entire Danish population. Both persons and firms can be monitored from 1980 onwards. BFL: Employment Statistics for Employees. BFL contains monthly data on jobs, paid hours of work and total wage to employees throughout the year. BFL is available from 2008 and onwards. Both data sets are available through servers at Statistics Denmark (see dst.dk).

[22] The analysis below uses wages after imputed taxes, assuming a tax rate of 37.5 % for all workers (this corresponds to the average tax rate for individuals on UI in 2008, see Maibom et al. (2015))

[23] The age-restrictions allow me to ignore decisions about retirement and entry into education. I treat entry into educatoin after the age of 22 as any other public support scheme (in the data less than 4 % of workers transit into some kind of education which is supported by the state)

support (this group contains self-employed, black-sector workers and workers out of the labour force). Individuals transitioning to the self-sufficiency state are therefore treated as individuals transitioning into employment as these are individuals who have opted out of any public support scheme (UI eligibility is 4 years at the time of the experiment).[24] Figure 3.16 in Appendix B shows how the fraction in the residual group evolves over time. Unsurprisingly changing the outcome makes the impact of treatment a little larger, but I show below that the important data features are similar regardless of the used employment definition.
In the next section I provide more details on the impact of the experiment in relation to the model developed below.[25]

3.4 Data and model

This section presents features of the data which serve as the motivation for the specification of the model presented in the next section. Table 3.13 in Appendix B shows average characteristics for treated and controls in each region and the p-value associated with a test of equality of means. In general the sample is balanced both in terms of past earnings, demographics and employment history. The descriptives show that while the experiment was directed at newly unemployed workers, 20-30 % of the participants came from other states than employment (e.g. education, unemployed or previously sick-listed). To interpret the generated impacts of the experiment it is therefore important to keep in mind that some of the treated individuals were in fact previously unemployed which could affect the size of impacts. There are also some important regional differences (the distribution across cells in Table 3.2 also differs depending on the region) which implies that comparing impacts across regions requires further assumptions. The estimated structure of the model can be used to analyse the sensitivity of the raw impacts to these differences.

Data patterns

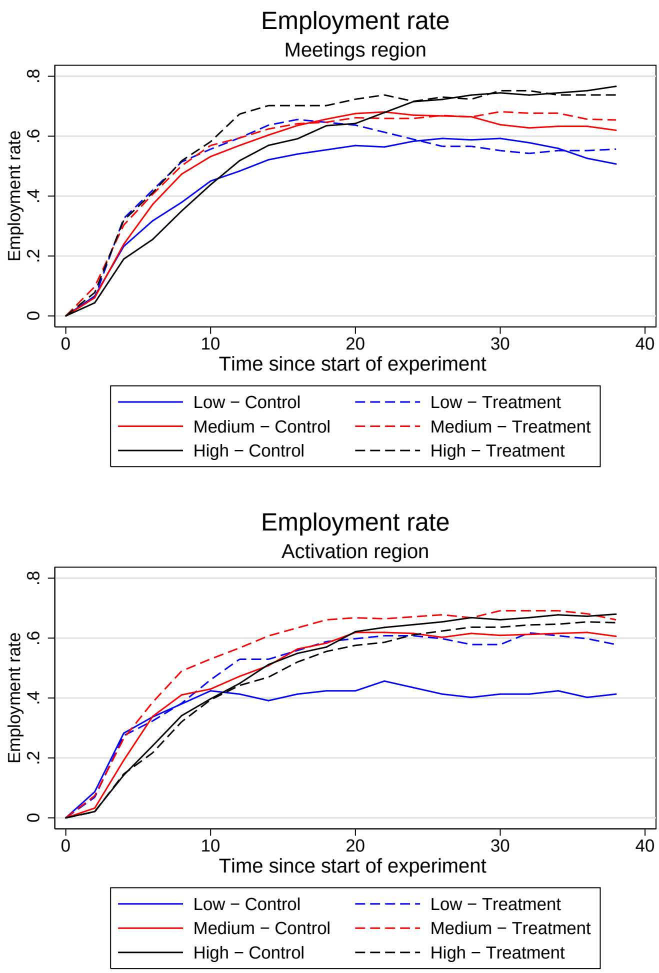
Figure 3.1 shows the employment rates from inflow into the experiment and onwards for the two regions. The figure shows that employment rates increase rapidly within the first 20 weeks hereafter the employment level stabilizes. There are educational

[24]The definition of employment is thereby slightly different from the definition used in Maibom et al. (2015) as the model thus captures the decision of whether to stay in public transfers or not. In Maibom et al. (2015) time spent in the employment state is compared across treatment and control group. Individuals not in employment are either self-sufficient or in public support.

[25]In general, the findings in Maibom et al. (2015) is that meetings lead to a significant increase in employment rates. Furthermore a positive and statistically significant effect on accumulated weeks spent employed remains significant over the whole 5 year horizon studied. The activation wall produces results which are positive but insignificant, but for certain subgroups and especially young workers the impacts are large and statistically significant. Estimates from a duration model suggest both the presence of effects ex ante and subsequent employment duration effects. There are also interesting gender differences where females generally respond faster than males.

differences in the inflow rates and in the "stable" employment levels. In particular there is a clear educational ordering in the employment level after 30 weeks: the employment rate is around 70% for individuals with high education, and slowly increasing, whereas the employment level is around 55% (40 %) for individuals in the medium (low) group and stable or slightly deceasing. The hazard rate out of unemployment for the control groups (see Figure 3.2) is declining with duration in unemployment. This implies that even in relative terms the initial outflow is high compared to the pool at risk.

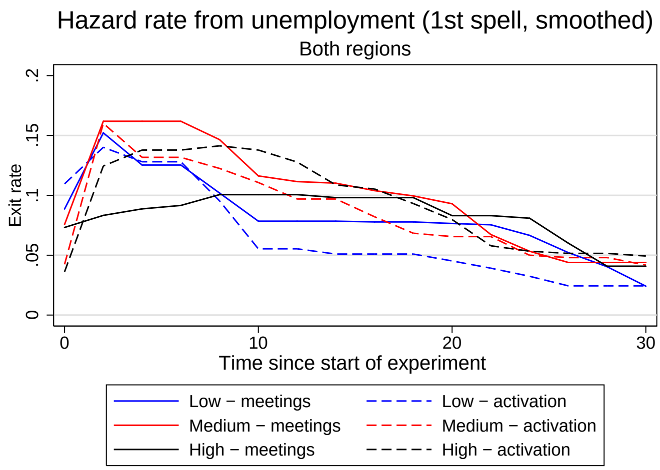
Figure 3.1. Employment rates and inflow (treated and controls)



Note: time since start of experiment is measured in weeks

Figure 3.1 also shows some interesting differences between treatment and control

Figure 3.2. Hazard rate for individuals in the control group



Note: time since start of experiment is measured in weeks

groups. In particular across both regions (with one exception) it appears that treated individuals are in employment to a larger extent. The difference is large initially and then it substantially decreases over time, except for low educated in the activation region. Table 3.3 show the result of a regression of employment status on treatment status for different regions and time periods. The table shows that already after 2(4) weeks in the experiment treated individuals in the meetings region (R1) are significantly more employed. At this point unemployed individuals *may* have participated in 1(2) meeting(s) and therefore the results indicate either a very productive first meeting or the presence of ex ante or threat effects. In the activation region (R2), where treated individuals only start participation in activation after 13 weeks (see Table 3.1), the results are more mixed after 4 weeks. When I run the same regression 10 (14) weeks after inflow into the experiment the results are much larger for treated individuals in R2. The regressions therefore suggest that the timing of the treatment is important and that differences are large in the very early stages of treatment which could be a combination of both threat effects and programme effects. The fact that the effects accumulate this early (and also before treatment starts) indicates that the existence of a utility cost could be an important channel.[26]

Figure 3.3 shows the average hourly wage as a function of duration in employment.

[26] As earlier mentioned the "employment criterion" used here defines anyone who do not receive public support as employed. Table 3.15 in Appendix B performs the same analysis using a stricter employment criterion which was also used in Maibom et al. (2015). The effects are very similar and the main findings and significance remains although some of the effects are smaller in magnitude which suggests that a part of the response to treatment goes through self-sufficiency or self-employment and then later employment (a part of individuals in self-sufficiency could also be employed due to data limitations).

Table 3.3. Employment results

	Meetings			Activation		
After 2 weeks	Low	Medium	High	Low	Medium	High
Treatment indicator	0.0887*	0.0620*	0.123*	0.0256	0.0634+	0.0257
	(0.0410)	*(0.0314)*	*(0.0506)*	*(0.0627)*	*(0.0349)*	*(0.0265)*
After 4 weeks						
Treatment indicator	0.102*	0.0446	0.120*	-0.0156	0.0496	-0.0274
	(0.0468)	*(0.0349)*	*(0.0567)*	*(0.0690)*	*(0.0384)*	*(0.0298)*
After 10 weeks						
Treatment indicator	0.120*	0.00197	0.149*	0.115	0.105*	-0.0408
	(0.0481)	*(0.0356)*	*(0.0589)*	*(0.0717)*	*(0.0405)*	*(0.0343)*
After 14 weeks						
Treatment indicator	0.130*	0.0351	0.133*	0.166*	0.0936*	-0.0547
	(0.0473)	*(0.0348)*	*(0.0577)*	*(0.0712)*	*(0.0399)*	*(0.0348)*
Observations	423	775	278	194	605	824

Note: The results are from separate OLS regressions after 2, 4, 10 and 14 weeks. The dependent variable is employment status. Huber/White standard errors, + $p < 0.10$, * $p < 0.05$

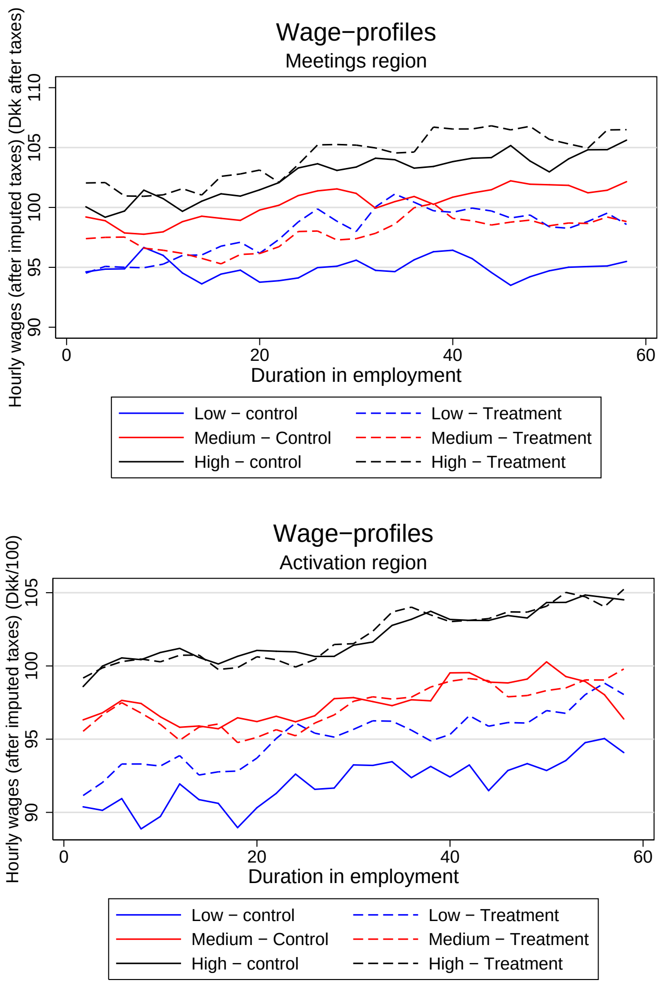
Wages generally increase with employment duration. The level and the growth rate of wages differ by education, and there is also variation within educational groups (the standard deviation is around 20-25% of the mean). Wage-profiles in treatment and control groups are generally similar, but wages seem slightly higher (lower) for low (medium) educated individuals in the treatment group. Table 3.18 in Appendix B shows the results from a regression of wages on treatment status after 10 weeks in employment.[27] The results show that the differences across groups are insignificant except for medium educated individuals in the meetings region.

Summary and relation to model:

The model contains different explanations for decreasing outflow rates and differences across education levels documented in Figures 3.1 and 3.2. These are duration dependence in job offer probabilities, differences in wage offers and differences in preferences (both in terms of observables and unobservables). The estimated parameters will be informative about what drives the declining pattern.
Changes in the average wage of employed workers in the model can be driven by two explanations: dynamic selection out of employment as low wage individuals leave for unemployment or true skill gains which imply higher wages. The model allows for these features through a search sensitive component of wages (different

[27] Differences in wage profiles (or lack of) can also be flawed by selection as treatment status is no longer exogenous in post-unemployment spells. In the presence of any impact or behavioural change associated with the experiment the composition of individuals in employment will differ between control and treatment.

Figure 3.3. Wage-profiles for employed workers



Note: time since start of experiment is measured in weeks

wage offers) and stochastic skill accumulation while employed. The skill level will be unobserved to the econometrician and changing over time. Differences in wages and wage growth will be important for how individuals value employment (and therefore also lead to differences in the compensating variation associated with programme participation).

3.5 Model

This section presents the model in more detail. Each subsection presents different elements: The dimensions of actions and heterogeneity (state variables). The different primitives of the model: the utility function, the wage function and the evolution of skills. The decision rules which determine individual behaviour, and finally the timing of the model. The next section explains how the model is solved and the estimation proceeds.
Individuals in the model, are forward looking and infinitely lived. They maximize the discounted sum of all future pay-offs by making discrete choices in a dynamic environment. The environment is stationary and ergodic (conditional on state variables) and a time period in the model corresponds to 2 weeks in the data.[28] State variables are discrete and the transition probabilities for state variables depend on the characteristics of the agents in ways that will be specified below.[29] The environment is characterized by duration dependent job offer rates, search sensitive wages, stochastic skill accumulation in employment and depreciation at inflow into unemployment. Employed individuals face a probability of a lay-off which is independent of individual choices but depends on their skill level. Unemployed receive UI and participate in ALMPs which consist of two elements: meetings and activation. Participation in a given programme is associated with a potential loss of utility while it can also increase the probability of receiving a job offer. To estimate these components a non-stationary and finitely lived experiment is introduced into this environment (see more below). Differences in technology and preferences generate heterogeneous impacts of the experiment and therefore heterogeneous treatment effects are endogenous to the model.

Choices and State Space

Table 3.5 contains an overview of the parameters to be estimated, this entails preference parameters and parameters which affect the transition of stochastic state variables. Table 3.17 in the appendix provides an overview of other model parameters

[28] To maintain focus on the individual behaviour and utility costs the model is cast in partial equilibrium. The inclusion of GE effects is still seldom in the literature and the interventions considered here are relatively short which could complicate the analysis further as firms simply do not have time to respond to the change in the environment (alternatively they may know that the intervention is temporary). Gautier et al. (2012) consider the GE effects in an earlier Danish experiment where the treatment period and intensity is longer. See also Lamadon et al. (2004) who focus on the Self-sufficiency Project conducted in Canada. They calibrate a search and matching model using data on the control group and use the data on the treatment group to validate their predictions about the equilibrium effects of the SSP.

[29] The model presented below use the same overall framework as in Ferrall (2012, 2002) which also contains the necessary assumptions and requirements to the primitives (e.g. environment, transition functions and utility) enabling the researcher to solve the model and deal with a problem of initial conditions in an environment with unobserved state variables which evolve in a non-iid fashion. While developing model primitives it is therefore ensured that these assumptions are met. Primarily this implies ensuring the existence of an ergodic distribution - the main requirement is that transitional dynamics for each state variable is either ergodic, invariant or dependent.

which are not estimated (e.g. the meetings intensity). The next subsections contains more detail on how primitives of the model depend on parameters and state variables.[30]

Let α contain current actions and let θ contain the value of the state, i.e. the collection of variables which summarize all information about the past needed in the forward-looking optimization problem. The action space consists of two variables: a search activity choice ($ac \in \left\{0, \frac{1}{7}, .., 1\right\}$) and a working status choice ($wc \in \{0, 1\}$) if a job offer arrives. Individuals choose search activity along both the extensive margin and the intensive margin while they only make a choice at the extensive margin of employment if a job offer is available - the intensive margin (e.g. hours worked) is assumed fixed and constant across jobs.[31] When employed there are no choices to be made and any potential wage increase is explained through stochastic skill accumulation (this also includes changes related to job to job transitions in the data). While there could be important effects from job quieting behaviour the data will not allow us to determine the reason for job separations.

The state space (θ) summarizes all relevant information in the environment influencing individuals in their decision making (wages, employment status, wage process). Table 3.4 contains an overview of the elements of the state space which consists of a collection of state variables describing the "normal" social environment and another collection of state variables which describe the experiment, thus $\theta = \left(\theta_{\text{env}}, \theta_{\text{exp}}\right)$.

θ_{env} consists of a time-invariant part and a time-varying part. While the time-invariant part of the state space is unaffected by choices made by individuals, time-varying states may change as a result of choices. Since a part of the time-varying state space evolves stochastically, individuals do not know the future position of the state space θ' with certainty but form rational expectations. The time-invariant state variables divide individuals into experimental, educational, regional and 'preference for leisure' groups. A state variable **g** marks the treatment status of individuals (control, treatment), **e** marks the education level of the individual (low, medium or high skilled) and **r** the region (R1 or R2) - all variables are observed by the econometrician. The environment is also composed on a (finite) number of types (**k**) who differ in how costly searching for a job is (or their value of leisure). Type status is unobserved to the econometrician and the distribution of types differ across educational and regional groups.

Time-varying state variables are variables for unemployment duration (**cu**), meetings or activation participation status (**mp**/**ap**), a potential job offer (**j**), the level of skills/human capital (**hc**) and the employment status of the individual (**em**). Un-

[30] $\underline{\kappa}^e$ implies that κ is a vector with an education specific entry (see Table 3.5). In this case this implies that the cost associated with working is estimated separately for each education group.
#points(hc) gives the number of different values the state variable hc may take. State variables always take values $\{0, 1, ..., points(hc) - 1\}$ unless otherwise stated.

[31] To model the intensive margin of employment further characteristics of the employment situation would be necessary, for instance detailed data on working hours, other benefits and tax schemes.

employment duration (**cu**) counts the duration of the current unemployment spell (since last job loss). The meetings (activation) variable (mp/ap) indicates whether individuals currently participate in one of the programmes. A job offer (j) is a draw of firm productivity which is mapped into an actual wage offer through a wage function. Wages are also a function of the current level of skills (hc).
The experiment is included into the model by adding two state variables (collected in $\theta_{\text{experiment}}$) to the state space. These state variables serve as "accounting variables". They cosists of a treatment phase indicator (**p**) and a counting variable for the time spent in the current phase (**c**). The inclusion of these variables allows the incentives to change as individuals progress through the experiment: for instance the incentive to leave unemployment may increase as the individual progress through the "waiting phase" knowing that in 6 periods an early activation scheme begins.

Utility, Costs and Wages

The current payoff is described as a function of generated income and costs (pecuniary and non-pecuniary):

$$U(\alpha,\theta) = \log\Big(Income(\alpha,\theta) - Cost(\alpha,\theta)\Big)$$

The formulation keeps costs in monetary units while ensuring that agents are risk averse - thereby an insurance motive can exist in the economy.[32] Costs vary with education levels (e) and unobserved type (k). They depend on effort and mandatory programme participation:[33]

$$C\hat{o}st(\alpha,\theta) = \xi^{\underline{\Psi}^k} \cdot ac + \underline{\kappa}^e \cdot wc + \underline{\phi}^e_{ap} \cdot ap + \underline{\phi}^e_{mp} \cdot mp$$

Costs are incurred from exerting effort either through search activity ($ac \neq 0$), working (wc = 1) or participation in programmes. Costs are linearly increasing in the intensity of effort. The education specific cost connected to the participation in ALMPs ($\underline{\phi}^e_i$) depends on the type of programme (i.e. either meetings or activation) as programmes are different in content and scope. The costs associated with working are education specific while the ξ is the same for all individuals. The total cost associated with searching (ξ^{ψ_k}) vary across types and change according to the estimate of $\underline{\psi}^k$ which is estimated separately for each type (k). One type has linear costs as $\underline{\psi}^{k=1}$ is normalized to 1.
Utility is only meaningfully defined when income succeed costs. To avoid taking

[32] The formulation is similar to Shimer and Werning (2008), $u(c_t - v(e))$, if we assume that capital markets do not exist or workers are liquidity constrained such that they consume all income each period. Shimer and Werning (2008) use CARA utility, here I use the log().

[33] Note that in the current version ξ^{ψ_k} would be easier expressed as $\underline{\xi}^k$ (thus just estimate type specific search costs). In a future version costs will be formulated as $cost(\alpha,\theta)^{\psi_k}$, therefore I stick to the separation between ψ_k and ξ below.

Table 3.4. Elements of the state space

Sub-space	State variable	Symbol	Type	Transition	#points()	Data
θ_{env}	Education group	e	Time-invariant	none	3	Observed
θ_{env}	Regional group	r	Time-invariant	none	2	Observed
θ_{env}	Treatment group	g	Time-invariant	none	2	Observed
θ_{env}	Effort Type	k	Time-invariant	none	2	Unobserved
θ_{env}	Unemployment duration	cu	Time-varying	deterministic	10	Observed
θ_{env}	Job offer	j	Time-varying	stochastic	8	Unobserved
θ_{env}	Meetings status	mp	Time-varying	stochastic*	2	Observed
θ_{env}	Activation status	ap	Time-varying	stochastic*	2	Observed
θ_{env}	Skill level	hc	Time-varying	stochastic	6	Unobserved
θ_{env}	Employment status	em	Time-varying	stochastic	2	Observed
θ_{env}	Lost job	l	Time-varying	stochastic	2	Observed
θ_{exp}	Treatment phase	p	Time-varying	deterministic	3	Observed
θ_{exp}	Clock (time in current p)	c	Time-varying	deterministic	6	Observed

* in the treatment phase π_{mp} and π_{ap} are set to 1. #points(hc) gives the number of different values the state variable hc may take. State variables always take values $\{0, 1, ..., points(hc) - 1\}$ unless otherwise stated.

Table 3.5. Estimated parameters:

Preference or wage parameters

Symbol	Model	Note	Dimensions
ξ	Utility	Search cost	1
$\underline{\kappa}^e$	Utility	Work cost	dim(e)
$\underline{\phi}^e_{mp}$	Utility	Meetings cost	dim(e)
$\underline{\phi}^e_{ap}$	Utility	Activation cost	dim(e)
$\underline{\psi}^k$	Utility	Leisure preference	dim(k)
$\underline{\pi}^{r,e}_k$	Type	Fraction of type 2	dim(e*r)
μ	Wages	Wage constant	1
$\underline{\sigma}^e$	Wages	Return to J	dim(e)
η	Wages	Return to hc	1
ρ	Smoothing	Smoothing kernel	1

*dim(k): variable varies with the number of unobserved types (2)

** To ensure the existence of an ergodic distribution this parameter must be strictly larger than 0

Transition functions

Symbol	Model	Note	Dimensions
$\underline{\pi}^r_{w,1}$	Job offers	Duration dependence	dim(r)
$\underline{\pi}^e_{w,2}$	Job offers	Long term job offer**	dim(e)
$\pi_{w,mp}$	Job offers	Productive effect (meeting)	1
$\pi_{w,ap}$	Job offers	Productive effect (activation)	1
$\underline{\pi}^e_{lj,1}$	Job loss	Risk of job loss, hc impact**	dim(e)
$\underline{\pi}^r_{lj,1}$	Job loss	Regional effect**	dim(r)
$\underline{\pi}^e_{hc,1}$	Skill level	Appreciation of hc	dim(e)
$\pi_{hc,2}$	Skill level	Loss of hc**	1

*dim(r): variable varies with the number of regions (2)

** To ensure the existence of an ergodic distribution this parameter must be strictly larger than 0

the log of a negative number and to keep parameters in the relevant area for optimization,[34] costs are expressed as a fraction of maximum attainable earnings for an individual with the highest education level, wage offer and skills. W_{max}[35] therefore does not vary between different types of agents. Total costs are therefore expressed as:

$$Cost(\alpha,\theta) = W_{max} \cdot \hat{Cost}(\alpha,\theta)$$

[34] I.e. in the range where changes in parameters leads to changes in actual behaviour and thus changes in the fit between data and model. Even in the absence of log utility any parametrization of costs exceeding income generates predictions by the model which are indistinguishable. In the optimization process $\xi, \underline{\kappa}, \underline{\phi}_{ap}, \underline{\phi}_{mp}$ are therefore restricted to $[-1,1]$.

[35] $W_{max}(\alpha,\theta) = \exp\left(\mu + \underline{\sigma}^{e=3}1 + \eta \cdot 1\right)$

Income consists of the wage when working and UI when unemployed:

$$Income(\alpha,\theta) = \begin{cases} W(\alpha,\theta) & \text{if wc=1} \\ UI & \text{if wc=0} \end{cases}$$

When unemployed individuals receive UI which is determined as a fixed amount assuming that all individuals qualify for the maximum amount of benefits. Lentz (2009) estimates that around 90% of the unemployed workers in the labour market qualifies for this amount.[36] UI eligibility is not modelled here since enrolled unemployed are newly unemployed (with some deviations as documented above), the study period is relatively short and eligibility is 4 years in this period.
The wage function is similar in some dimensions to Ferrall (2012), and is modelled as:

$$W(\alpha,\theta) = \begin{cases} 0 & \text{if j=0} \\ \exp\left(\mu + \underline{\sigma}^e \Phi^{-1}\left(\frac{j}{\#\text{points}(j)}\right) + \eta \cdot \left(\frac{\text{hc}}{\#\text{points(hc)}}\right)\right) & \text{if j>0} \end{cases}$$

μ is a wage constant and represents the deterministic part of wages, η measures the return to skills and $\underline{\sigma}^e$ measures the importance of the frictional or search sensitive component of wages (a draw of firm productivity). The transformation of values of j into percentiles of the normal cdf ensures that the distribution of wages is not uniform and that the wage dispersion does not depend on the dimension of job offers. The presence of a search sensitive component in wages (through different job offers) implies that individuals form reservation wages as optimal stopping rules. The reservation wage will be revised as unemployment duration increase and therefore an analytical expression is not obtainable as in the more standard case (see e.g. Wolpin (1987)). Differences in wages across educational levels are generated by differences in skill accumulation (presented below) and in the return to search. As $\underline{\sigma}^e$ varies across educational groups it allows the within group variance to be different and this is also an important channel through which experimental impacts can differ as the cost of accepting lower wage offers differs depending on the estimate of $\underline{\sigma}^e$.

Jobs and Skills

At inflow into unemployment individuals have no job offers (j = 0), thereafter a job offers arrive each period with probability π_w. Arrival rates are determined as a function of search activity, unemployment duration and programme participation. Following the literature on endogenous search (see e.g. Mortensen and Pissarides (1999)) job offer rates are proportional to search activity:

$$\pi_w = \text{ac} \cdot \left[\Phi\left(\underline{\pi}^r_{w,1} \cdot \text{uedur}\right) + \underline{\pi}^e_{w,2} + \pi_{w,mp} \cdot \text{mp} + \pi_{w,ap} \cdot \text{ap}\right]$$

[36] The replacement level of a worker earning 150% above average earnings is around 0.6, see Bjoern and Hoej (2014). Therefore UI is set to $0.6 \cdot W_{max}$ in the model.

The probability of receiving an offer consists of a regional specific duration dependent term $\left(\underline{\pi}^r_{w,1}\right)$ and constant terms $\underline{\pi}^r_{w,2}, \pi_{w,mp}$ and $\pi_{w,ap}$. $\pi_{w,mp}$ ($\pi_{w,ap}$) represents a potential increase in job offer arrival rates the period after participation in a meeting (activation). The duration dependent term is similar to Wolpin (1987). If employers use duration in unemployment as a screening device, which has been suggested in the literature (see e.g. Kroft et al. (2013) and Belzil (1995)), $\underline{\pi}^r_{w,1}$ will be negative. In this case the first term goes to 0 as uedur increase and $\underline{\pi}^e_{w,2}$ is then the probability that a long term unemployed receives a job offer. Note that the model also allows for "spurious" negative duration dependence in the form of dynamic selection generated by changes in the composition of unobservable types and the stock of skills across remaining unemployed individuals. The observation that outflow rates are declining with unemployment duration (as documented in Figure 3.2) can thereby also result from the more "able" (high paid or low cost) types leaving unemployment early, while the remaining stock consists of a consecutively weaker group of unemployed.

The concept of skills included in this model can be thought of as a mixture of general and specific skills - sometimes skills are transferable to new jobs, other times skills are specific to past jobs.[37] Skills are included to generate differences in the value of a job (both through payment and stability) across agents which are unobserved and change over time. It is an important channel through which the incentive to leave unemployment differs across both time and individuals. While employed the stock of skills appreciates every period with an education specific probability $\underline{\pi}^e_{hc,1}$ reflecting skill improvements through learning on the job. When separated from a job, skills are lost with probability $\pi_{hc,2}$. This captures that acquired skills have become obsolete in the market and therefore expected future wages will be lower for instance because individuals will have to start in a new job without any prior experience in the specific tasks.

Finally, the level of skills also affects the expected duration of a job. Jobs end with probability π_{lj}:

$$\pi_{lj} = \underline{\pi}^r_{\mathrm{lj},2} \cdot \left[\underline{\pi}^e_{lj,1} \cdot \left(1 - \frac{\mathrm{hc}}{\#\mathrm{points}\left(\mathrm{hc}\right)}\right)\right]$$

The job separation process is allowed to differ between education levels and regions where the region with meetings is set as the reference category ($\pi^{meeting}_{lj,2} = 1$). Job separation probabilities decline (or increase) in how skilled workers are. This generates a source of duration dependence in employment as workers who have been employed for longer periods are also likely to have accumulated more skills and thus less (more) likely to exit to unemployment. The link between skills and job destruction implies

[37] Since I do not focus on human capital accumulation in general, lasting experience or life cycle effects are not included in the model (a time period in the model is 2 weeks). The ergodic distribution of skills is therefore constant over time (while some individuals loose skills and others accumulate skills) although individuals in the sample become older (here 80 weeks). In larger samples workers could potentially be distinguished by age groups to allow for differences in the level of skill.

that a random sample of workers at inflow at a given point in time will be a selected from the underlying ergodic distribution of workers in terms of skills and willingness to work.

Active Labour Market Programs

ALMPs enter the model in two ways. Firstly programme participation is associated with extra effort and lost leisure measured by ϕ_i in the utility function.[38] Secondly there can be productive effects $\left(\pi_{w,mp}, \pi_{w,ap}\right)$ from programme participation through an increase in job offer arrival rates. Individuals have to participate in ALMPs and the only way to escape programme participation is by becoming employed. In both control groups meeting participation is random and happens with probability $\pi_{mp} = 0.15$. The probability of participation in an activation programme is 0 during the first 10 weeks of unemployment, hereafter it increases with unemployment duration until an intensity of 0.35. The parameters are chosen in order to match the meetings and activation intensity in the control group documented in Maibom et al. (2015). The treatment group face the same participation probabilities as the control group in the waiting and post-treatment phase (see Table 3.1). In the treatment phase they participate in programmes with certainty.

Dynamic Program and Choices

The value of a (α, θ) combination at a given point in time is the sum of the current reward and an expected future reward which is affected by current choices and the position in the state space. Individuals have perfect knowledge with regard to the probability distribution from which future realizations will be drawn (each element of α,θ, $U()$ and $P()$ has been presented above):

$$\begin{aligned} \forall \alpha \in A(\theta), \quad v(\alpha,\theta) &= U(\alpha,\theta) + \delta E\left[V(\theta')\right] \\ &= U(\alpha,\theta) + \delta \sum_{\theta'} P\{\theta'|\theta,\alpha\} V(\theta') \end{aligned}$$

At each point in time the individual solves this decision problem choosing the actions that give him the highest value. The value function can be determined as:

$$\forall \theta,\ V(\theta) = \max_{\alpha} v(\alpha,\theta) \tag{3.1}$$

Conditional on a position in the state space θ (and ignoring even cases) the model generates a strong prediction about individual behaviour as one action maximizes the equation above. There are two approaches in the literature to allow "observationally"

[38]Note that in principle ϕ_i could also be negative (and this is allowed for in the estimation) such that programme participation generates utility gains. If this is the case a reverse threat effect may exist for individuals who unexpectedly experience an increase in the intensity of interactions (a so called attraction effect). The empirical literature suggests effects in the opposite direction.

similar agents to make different choices and thus increase the correspondence with real data. One approach adds further dimensions of unobserved heterogeneity to θ while the other introduces uncertainty in the predictions of behaviour ex post. In particular Rust (1987) add a 'taste shifter' - an additive and unobserved continuous state variable to the utility function - while Eckstein and Wolpin (1999) smooth choice probabilities ex post. The procedure followed here is a mixture. Firstly, the existence of discrete unobserved state variables (human capital and wage offers) provides an explanation for why two observationally similar individuals make different choices. Secondly, to allow for zero-probability or unanticipated events choice probabilities are smoothed ex post. Choice probabilities are smoothed using a logistic kernel $(\rho > 0)$:

$$
\begin{aligned}
\tilde{v}(\alpha,\theta) &= \exp\left\{\rho\left[v(\alpha,\theta) - V(\theta)\right]\right\} \\
P\{\alpha|\theta\} &= \frac{\tilde{v}(\alpha,\theta)}{\sum_\alpha \tilde{v}(\alpha,\theta)}
\end{aligned}
\tag{3.2}
$$

where ρ determines the importance of smoothing. The smoothing of choice probabilities implies that if the value associated with an in-optimal choice is close to the value of an optimal choice ($\tilde{v}(\alpha,\theta) \approx 1$) the probability of either choice will be similar. Choice probabilities connected to actions which are far from optimal ($\tilde{v}(\alpha,\theta) \approx 0$) will be close to zero. As ρ increase the probability that agents make unexpected/in-optimal choices decrease, as the distance from optimal values receives higher weight and $\tilde{v}(\alpha,\theta)$ is pushed towards zero.[39] Smoothing ex post introduces a wedge between the decision rule agents *anticipate* to follow and what happens in reality. Basically the current formulation allows agents to make zero probability or unanticipated events.

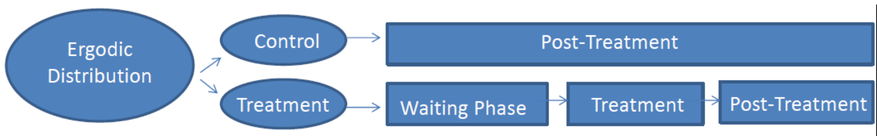
Timing

Figure 3.4 illustrates the timing of the model: from the ergodic distribution the outflow from employment into unemployment in a given period is selected into either control or treatment groups. Due to the design of the experiment the distribution over both observable and unobservable states is identical at inflow into the experiment. Individuals in the control group enter an environment without treatment (post-treatment world) and progresses through unemployment making choices according to the structure laid out above. Conditional on θ_{env} their environment is stationary. For the treatment group this does not hold as the accounting variables

[39] While the expression of choice probabilities above looks almost identical to the one in Rust (1987) there is one fundamental difference. Here smoothing is ex post while the standard Rust model adds a taste shifter to the model such that individuals take the existence of shocks to utility into account when they solve for optimal values. When this taste shifter follows the extreme value distribution an expression of the choice probabilities can be analytically solved for. This leads to a slight modification of the contraction mapping (it now becomes a log sum instead of the sum above) in the calculation of choice probabilities of the model. The main argument for adding the taste shifter is to smooth choice probabilities.

in θ_{exp} variables c and p change over time which affects the likelihood of present or future programme participation. At inflow into the experiment, individuals in the treatment group enter the waiting phase (see Table 3.1). Their future differs from what was expected at outflow from employment: while unemployed they will go through a waiting phase and a treatment phase before they enter the phase without treatment. In the later phase the environment is identical to the control group, but the distribution over states is potentially different due to the impact of the experiment.

Figure 3.4. Timing in the model



3.6 Solution, Estimation and Identification

This section contains a brief presentation of how the model is solved. It is discussed in more detail how previous work is extended with additional calculations that increases the set of predictions from the model which can be compared with data. Next the estimation procedure is presented, and a discussion of identification of central parameters of the model is provided. The model is estimated using the method of moments. Table 3.6 contains a summary of the chosen moments including the mean and standard deviation of the time series of moments. The moments capture employment, unemployment and wage dynamics which are informative about the structural parameters of the dynamic program.

Solution of the model and initial conditions

To generate predictions to compare with data the model is solved in a series of steps which will be briefly commented on below. More details are outlined in Appendix A. The solution procedure consists of 5 steps, similar to the steps presented in Ferrall (2002), and one additional step which will be presented in the last paragraph of this subsection:

i) Solve for $V(\theta)$ in (3.1).

ii) Calculate the policy function $P\left(\alpha|\theta\right)$ as given in (3.2).

iii) Use the transition function for state variables ($P\left(\theta'|\theta,\alpha\right)$) and the policy function (from ii) to solve for how the distribution over states evolve from one period to the next unconditional on choices ($P\left(\theta'|\theta\right)$).

iv) Use the state-to-state transition matrix (determined in iv) to solve for the ergodic distribution across states. [40]

v) From the ergodic distribution and the state-to-state transition matrix create a sample of unemployed workers which matches the data on observables (e.g. unemployment duration) and also takes account of the dynamic selection on unobservables.

Step v) takes into account that the data is not a random sample of workers from the ergodic distribution, but endogenously sampled as to enter the experiment individuals had to become unemployed - and some even had to remain unemployed for a longer period of time (see the Data section above). This makes the sample negatively selected in terms of both observables and unobservables compared to the average worker in the ergodic distribution. Naturally neither of the above invalidates the experimental design but it is important to take into account in an analysis focused on quantifying parameters in the decision process and extrapolating the results into other settings with different individuals or policies. If this initial conditions problem (see e.g. Aguirregabiria and Mira (2010)) is not accounted for, estimates of the decision parameters will be biased as unobservables correlate with observables and decisions in ways which are unaccounted for.[41]

The five steps outlined above can be used to solve for $\Omega\left(\theta|t\right)$, the distribution over states at time t since inflow into the experiment for the selected group of individuals enrolled into the experiment. $\Omega\left(\theta|t\right)$ can be solved for by iterating on the initial distribution across states defined in v) using the state-to-state transition matrix defined in iv).[42]

Finally a further step vi) is added to the solution procedure. In order to increase the number of predictions from the model which can be compared to data, the fraction of individuals which have followed specific paths or spells and thier distribution across spells can be determined by extending the solution procedure outlined above.

vi) Determine the fraction of workers satisfying certain spell requirements and determine their distribution across state variables for each t

This final step is illustrated by example. To economize on state variables there is no state variable which counts employment duration in the model presented above. In order to calculate moments related to employment duration the inflow into employment for each t, and the distribution across states, is determined. This implies that the distribution of employment durations for each period t can be obtained. In practice an inner "reduced" Markov chain is added to the solution of the model which determines $\Omega^{RED}\left(\theta|t\right)$, the distribution across states satisfying certain spell

[40] To solve for the ergodic distribution solve for the fixed point (vector) in $\pi\left(\theta\right) = P\left(\theta'|\theta\right)\pi\left(\theta\right)$. Ferrall (2002) shows the conditions which are required for the existence of an ergodic distribution - the main requirement is that transitional dynamics for each state variable is either ergodic, invariant or dependent.

[41] For example individuals with long unemployment spells at inflow into the experiment, may accept low wages either because they have a low cost of working or low skills. If we do not take into account that this group is a negatively selected group in terms of skills we prescribe all behaviour to the former.

[42] $\Omega\left(\theta|t\right) = P\left(\theta'|\theta\right)^t\omega\left(\theta\right)$ where $\omega\left(\theta\right)$ denotes the initial distribution over states at the start of the experiment.

requirements (here employment for a period of time).[43] These calculations thereby allow me to include moments which are only indirectly linked to a state variable.[44] In relation to the model specified above, these calculations allow the inclusion of moments describing both employment durations and wages conditional on employment durations to the set of moments which is matched on. Adding these moments has the advantage that the unobserved state variable human capital is now more directly linked to data moments which strengthens the identification of the job separation and skills accumulation processes.

Estimation

The parameters of the model are estimated using the method of moments. The estimation proceeds as follows: For a set of parameters the model generates a series of behavioural predictions which are translated into moments and compared to data. The difference between these moments is now minimized by changing the parameters, and resolving the model to generate new predictions, until a minimum is found. To calculate the predictions of the model start by calculating the expected value of a certain outcome (for instance the wage) conditional on a position in the state space θ.

$$E\left(Y|\theta\right) = \sum_{\alpha} P\left(\alpha|\theta\right) Y\left(\alpha,\theta\right)$$

Where $Y\left(\alpha,\theta\right)$ is a given outcome which may vary with both choices and position in the state space. $E\left(Y|\theta\right)$ gives us the expected value of a moment conditional on a position in the state space. The initial distribution over states and the Markovian structure of the problem determine how the conditional moment evolves over time. Next the conditional moments are weighted with the corresponding distribution over states at a given point in time. Predictions are determined conditional on time (t) and also conditional on the time invariant states: unobserved type (k), educational (e), regional (r) and treatment groups (g).[45] This results in a time series of moments:

$$E\left[Y_M|t,e,r,g,k\right] = \sum_{\theta_{|k,e,r,g}} \Omega(\theta_{|k,e,r,g}|t,k,e,r,g)E\left(Y|\theta\right)$$

[43] $\Omega^{RED}\left(\theta|t+k\right) = P^{RED}\left(\theta'|\theta\right)^k \omega_t^{INFLOW}\left(\theta\right)$ where $\omega_t^{INFLOW}\left(\theta\right)$ denotes the inflow into employment in period t. $P^{RED}\left(\theta'|\theta\right)^k$ is a transition matrix which have non-zero entries for transitions which implies that the individual stays employed. $\Omega^{RED}\left(\theta|t+k\right)$ gives the fraction of individuals who has been employed for k periods at time t+k and the distribution across statevariables θ.

[44] The procedure is basically the method of moment equivalent to the *simulated* method of moment estimators where the simulated data enables the researcher to condition on moments not directly linked to the model. The inner chain calculates the distribution of e.g. employment duration over time although employment duration is not a state variable in the model. The modification shows how further moments can be added to the model without increasing the state space or having to simulate the model (but naturally it still affects computational speed). A recent paper by Eisenhauer et al. (2015) documents that the simulation error that exists in models exploiting simulated moments can affect the estimates in non-trivial ways.

[45] The notation $\theta_{|e}$ therefore refers to the set of state variables excluding the state variable e.

Where to calculate moments related to employment duration (#12-#14 in Table 3.6) $\Omega\left(\theta|t,k,e,r,g\right)$ is substituted with $\Omega_{RED}(\theta|t,k,e,r,g)$ which was defined above. Next, moments are weighted with the distribution over unobserved types (k):

$$E\left[Y_M|t,e,r,g\right] = \sum_k \lambda\left(k,e,r\right) E\left[Y_M|t,e,r,g,k\right]$$

where $\lambda\left(k,r,e\right)$ is the proportion of type k individuals within educational group e and region r. Finally model predictions are compared to data predictions:

$$\left(E\left[Y_D|t,e,r,g\right] - E\left[Y_M|t,e,r,g\right]\right)' W \left(E\left[Y_D|t,e,r,g\right] - E\left[Y_M|t,e,r,g\right]\right)$$

The weight matrix is an inverted diagonal matrix with the variance of the data moments in the sample. The method of moments now proceeds by minimizing this objective. Standard errors are calculated for the standard one stage GMM case (see e.g. Cameron and Trivedi (2005)):

$$\text{Var}\left(\Theta\right) = \frac{1}{N \cdot T}\left(\hat{G}'W\hat{G}\right)^{-1}\hat{G}'W\hat{S}W\hat{G}\left(\hat{G}'W\hat{G}\right)^{-1}$$

where Θ denotes the vector of parameters to be estimated, $\hat{G}$ is the Jacobian matrix and $\hat{S}$ the sample variance covariance matrix of the matrix of moments over time.

Identification

The parameters of the model are identified by restrictions (of both the behavioural and functional form) generated by the model on how the moments can vary over time, within and across educational and regional groups. The existence of experimental variation generates exogenous variation in programme participation which is useful in the identification of costs, ϕ_i, and the experiment also generates variation in incentives across time which can be useful to separate $\pi_{w,i}$ and ϕ_i. To assess whether the imposed structure and the selected moments are sufficient to recover the structural parameters a “baby-version” of the model, with the main central mechanisms, have been simulated and subsequently the generated data were used to confirm that the chosen parameter values could be recovered from estimation. Although this is by no means a formal proof of identification nor an actual Monte Carlo exercise it still provides a good indication of whether the model is identified.
Some central issues related to identification can be illustrated by discussing some “exclusion restrictions” in the model (see e.g. Wolpin (2013, 1987)). For instance, the model predicts that unemployed individuals are unemployed *either* because a job offer has been rejected or because no job offer was available. As the data contain no information on whether the unemployed received a job offer nor the size of the wage offered, the two explanations should be distinguished using data on job finding and accepted wages only. This distinction requires either functional form restrictions or

Table 3.6. Included time series of moments

			Meetings			Activation		
			Low	Medium	High	Low	Medium	High
#	Data Moment	Model Moment	Mean (S.D)	Mean (S.D)	Mean (S.D)	Mean (S.D)	Mean (S.D)	Mean (S.D)
1	Job separations	lj	0.026 *(0.08)*	0.027 *(0.09)*	0.022 *(0.09)*	0.026 *(0.09)*	0.027 *(0.10)*	0.025 *(0.11)*
2	Wages Squared	$W(\alpha,\theta)^2$	4322 *(986)*	6150 *(1355)*	7747 *(2156)*	3570 *(689)*	5255 *(1192)*	6833 *(1932)*
3	Wages	$W(\alpha,\theta)$	45.89 *(8.36)*	60.39 *(10.50)*	73.46 *(16.99)*	37.14 *(5.31)*	53.52 *(9.79)*	65.81 *(15.38)*
4	UE dur squared	cu^2	64.74 *(19.90)*	46.30 *(13.81)*	31.27 *(5.64)*	76.00 *(26.00)*	53.32 *(18.15)*	40.36 *(9.06)*
5	E-inflow* UE dur	$wc\cdot(1-\mathrm{e})\cdot\mathrm{cue}^2$	1.68 *(0.86)*	1.56 *(0.76)*	2.09 *(1.75)*	1.35 *(1.11)*	1.50 *(0.62)*	1.73 *(0.76)*
6	U	$(1-\mathrm{em})+wc\cdot\mathrm{lj}$	0.51 *(0.09)*	0.41 *(0.11)*	0.30 *(0.16)*	0.57 *(0.06)*	0.45 *(0.10)*	0.36 *(0.14)*
7	E	em	0.46 *(0.11)*	0.57 *(0.13)*	0.67 *(0.18)*	0.40 *(0.08)*	0.52 *(0.12)*	0.61 *(0.16)*
8	Inflow into E	$wc\cdot(1-\mathrm{em})$	0.02 *(0.03)*	0.02 *(0.03)*	0.02 *(0.03)*	0.02 *(0.03)*	0.02 *(0.03)*	0.02 *(0.02)*
9	Inflow wages (iw)	$wc\cdot(1-\mathrm{em})\cdot W(\alpha,\theta)$	2.26 *(2.68)*	2.49 *(3.32)*	2.44 *(2.80)*	1.99 *(2.83)*	2.24 *(2.72)*	2.21 *(2.41)*
10	iw * UE dur	$wc\cdot(1-\mathrm{em})\cdot W(\alpha,\theta)\cdot\mathrm{cu}$	15.58 *(8.51)*	15.70 *(9.99)*	18.92 *(16.88)*	12.09 *(9.43)*	14.85 *(8.13)*	16.34 *(8.74)*
11	Wages for E only	#3/#7	91.03 *(26.05)*	96.89 *(27.53)*	100.43 *(28.69)*	88.15 *(25.17)*	93.60 *(26.61)*	98.86 *(28.11)*
12	E-Dur	emdur	7.65 *(4.16)*	10.40 *(6.09)*	13.26 *(8.53)*	6.33 *(3.65)*	9.37 *(5.53)*	11.70 *(7.70)*
13	Wages * E-dur	$W(\alpha,\theta)\cdot\mathrm{emdur}$	722 *(390.32)*	1063 *(628)*	1435.93 *(955)*	584.27 *(331)*	918.82 *(551)*	1241 *(839)*
14	Wages * E-dur^2	$W(\alpha,\theta)\cdot\mathrm{emdur}^2$	20267 *(17377)*	31028 *(27807)*	43430 *(41712)*	15607 *(13559)*	26557 *(24281)*	36723 *(35725)*

Note: The table contains the subgroup specific means and standard deviations on the time series of moments exploited in estimation (time series is 80 weeks). Wages are after imputed taxes. UE: unemployment, E: employment, dur: duration, emdur: is not a state variable in the model but calculated for each period the existence of a variable which affects the availability of job offers without directly affecting the decision to accept a job offer or not, and similarly a state variable in the wage function should affect wages without affecting preferences. Here the impact of unemployment duration on job offer rates, the impact of skills on wage offers serve as such restrictions. Furthermore the restrictions across regions, time[46] and educational groups, and the functional form of duration dependence implies that the probability of receiving a job offer and the parameters of the wage function can be separated from preferences. This also serve as motivation for why some of the moments matched on are unemployment duration squared and duration dependent outflow rates from unemployment. If two individuals are identical besides different unemployment duration and they have different inflow rates, this is thus informative about how the probability of a job offer changes with unemployment duration. Similarly information on the level and evolution of skills provides information regarding the parameters of the wage function and thus provides further justification for the importance of step vi) outlined in the solution procedure above. This step ensures that unobserved state variables can be linked to certain patterns in the data which improves identification of the parameters of the process. In particular, the (squared) interaction between employment duration and wages is informative of how the distribution of human capital evolves over time while matching on employment duration and the rate of job loss is informative of the parameters determining job loss.

However, the most important source of variation is generated by the existence of the experiment. The experiment generates exogenous variation in the cost of being unemployed which allows us to distinguish between competing cost structures. The experimental variation allows a separation of different environments such as i) an environment with large costs of programme participation and search intensity from ii) an environment with low costs. These two explanations could generate the same size of impact but will have very different welfare implications: in i) agents incur substantial utility costs as they have to participate in “harmful programmes” without us directly observing it in the data, whereas in ii) the agents utility changes very little. Non-experimental data on individuals participating in ALMPs will not allow us to distinguish these explanations without assumptions that allow us to evaluate what individuals would have done in the absence of treatment. Identifying costs thus requires a comparison of participants with non-participants at a given point in time, and conditional on a limited set of state variables. This requires the assumption that agents are similar in all other dimensions than programme participation.

Furthermore the experimental variation in incentives generates an additional channel of variation which is useful. One way to distinguish between $\underline{\phi_i}$ and $\pi_{w,i}$ (two

[46]Repeated spells of unemployment are also useful in this context since tastes are constant over time in the model. Magnac and Thesmar (2002) show how stationarity of the utility function serve as another exclusion restriction in dynamic models. In particular, as the utility of choices does not change with time itself observing individuals make choices at different points in time (and in different enviroments (e.g. after longer spells of employment or unemployment)) therefore also serves as identifying variation.

different explanations for why programmes work) is by looking at the time profile of individual behaviour. An increase in the inflow to employment in the weeks prior to programme participation or within the first weeks is informative of the size of the utility cost component, whereas outflow rates after programme participation informs us about the qualification effect.

Overall the experiment generates the opportunity to observe identical agents in different settings and from their differential behaviour and, by the imposed structure of the model, to analyse the way that the treatment affects individuals. Contrasting moments from the treatment group with the control group allow us to keep other time varying confounders such as duration dependence and differences in skills fixed and attribute differences in behaviour to the exogenous difference in programme participation. The model and the experimental data therefore allow us to assess the importance of utility costs in an environment which is very rich in terms of heterogeneity between participants: some will face different returns to searching (due to different cu), some will face different wage offers (hc or j differences) or different job stability (differences in hc), some will differ in preferences for leisure etc. This also affects the size and distribution of the compensating variation associated with the experiment or other policies as it introduces differences in the prospects (and thus in the value associated with alternative choices) for future employment across individuals.

3.7 Results

Below some evidence on the fit of the model are presented with a series of figures that compare model predictions with data. Initially focus is on the fit of the model in the control group. Subsequently some key implications of the estimated model are shown and the long term predictions of the model is compared to data. The section proceeds by comparing the impact of the experiment generated by the model with data, and then some key channels through which participation in ALMPs affect job finding are presented. Next the implications of the estimates and the importance of the individual level costs of participating in ALMPs is discussed. The section concludes with a welfare analysis of the experiments under investigation incorporating the costs associated with increased production and participation in ALMPs. To incorporate the later the compensating variation is calculated.

Table 3.7 presents the estimated parameters and associated standard errors. The table shows that wages are generally increasing in the education level of individuals. The return to job offers ($\underline{\sigma}^e$) is higher for high educated individuals which increases both the average wage offer and the dispersion in the wage offer distribution for high educated workers. There are also differences in the cost of working ($\underline{\kappa}^e$) across educational levels - while low educated workers get lower wages their cost associated with working is also lower. There are substantial costs associated with participation in ALMPs, and the gains in terms of an increase in job offers are very small and insignificant (below I discuss the implications in more detail). The estimates also show that the environment is composed of two different types with different search costs determined by ξ^{ψ_k}: T1 with linear costs have the highest costs of searching which amounts to around 25 % of his UI, while T2 incur lower costs ($\xi^{\underline{\Psi}^{k=2}}$) of around 10 % of his UI when searching at the highest intensity.

Model fit

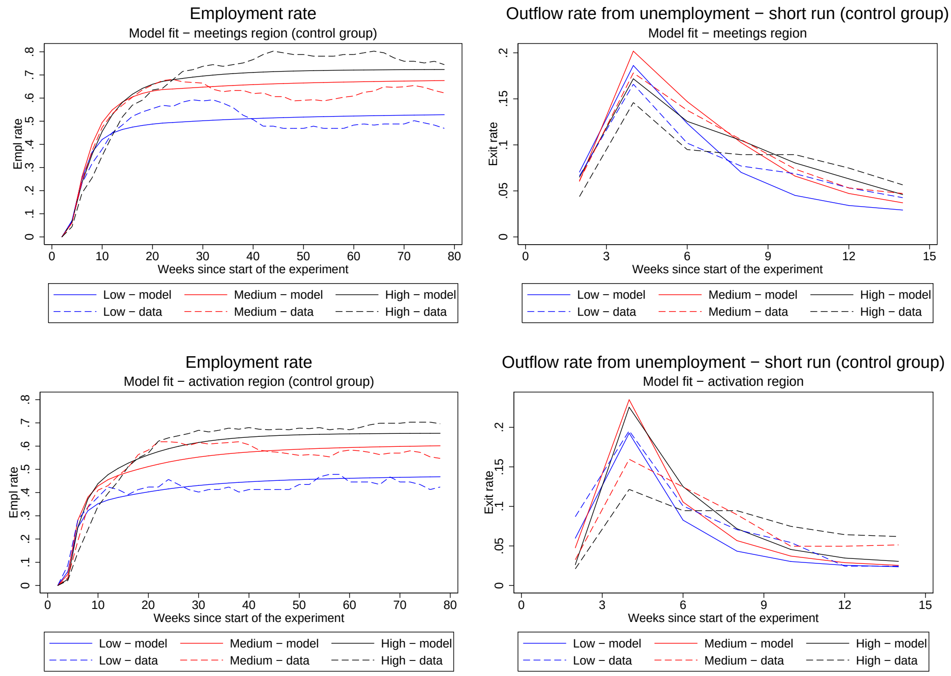
Below some evidence on the fit of the model for the control group is presented. For comparison Appendix C contains the same graphs for the treatment group, the difference between control and treatment predictions will be discussed in the subsection on the impact of the experiment. Figure 3.5 shows the correspondence between predictions of the model and the data for employment rates (see Appendix B Figure 3.19 for more figures on the inflow rate). Generally the fit is reasonable and the figure shows that the model is able to generate both high initial outflow and subsequently decreasing inflow rates. The model fits inflow rates in the long run suggesting that the main predictions in terms of in and outflow to employment are well explained by the model (I show evidence on job separations below). Figure 3.6 shows that the model matches the distribution of unemployment durations reasonably well although beyond 20 weeks the model predicts too high unemployment duration in the region with meetings (this is related to the fact that the model does not fit inflow rates sufficiently well in this region). Fitting unemployment duration is important as the model presents duration dependence as an important characteristic of the labour market. Figure 3.24 shows the fit of the model with the distribution of accepted squared wages and Figure 3.20 in Appendix B shows the fit of the interaction between duration in employment and wages. Generally the model fits both moments well, but wages for highly educated are too high initially. Finally Figure 3.8 shows the fit in terms of job loss. The figure shows that job loss in the data is generally very volatile due to a limited number of observations. The model predictions are smoother but seems to predict the average level of job loss in the data reasonably well. There is a clear educational ordering in terms of the fraction of individuals in the sample who are separated from a job. Since employment levels are also increasing in the education level, and more individuals are therefore at risk of losing their job, the difference in individual probabilities of a job loss across education groups must be substantial.

Table 3.7. Estimated parameters Θ:

Parameter	Model	Note	Region	Low	Medium	High
ξ	Utility	Search cost	Both	0.147 *(0.02)***	-	-
$\underline{\kappa}^e$	Utility	Work cost	Both	0.231 *(0.011)***	0.358 *(0.014)***	0.375 *(0.017)***
$\underline{\phi}^e_{mp}$	Utility	Meetings cost	Both	0.241 *(0.099)***	0.192 *(0.013)***	0.288 *(0.021)***
$\underline{\phi}^e_{ap}$	Utility	Activation cost	Both	0.123 *(0.019)***	0.246 *(0.010)***	0.287 *(0.015)***
$\underline{\psi}^{k=2}$	Utility	Leisure preference	Both	1.665 *(0.155)***	-	-
$\underline{\pi}^{k=2}_k$	Type Proportion	Fraction of type 2	Meetings	0.607 *(0.023)***	0.777 *(0.034)***	0.791 *(0.057)***
-	-	-	Activation	0.584 *(0.035)***	0.730 *(0.051)***	0.718 *(0.048)***
μ	Wages	Wage constant	Both	3.645 *(0.040)***	-	-
$\underline{\sigma}^e$	Wages	Return to J	Both	0.237 *(0.018)***	0.366 *(0.110)***	0.475 *(0.100)***
η	Wages	Return to hc	Both	0.715 *(0.15)***	-	-
ρ	Smoothing	Smoothing kernel	Both	15.554 *(1.228)***	-	-
$\underline{\pi}^r_{w,1}$	Job-offers	Duration dependence	Meetings	-0.160 *(0.16)***	-	-
-	-	-	Activation	-0.376 *(0.09)***	-	-
$\underline{\pi}^e_{w,2}$	Job-offers	Long term job offer	Both	0.089 *(0.009)***	0.102 *(0.009)***	0.155 *(0.012)***
$\pi_{mp,3}$	Job-offers	Productive effect (meeting)	Both	0.002 *(0.012)*	-	-
$\pi_{ap,4}$	Job-offers	Productive effect (activation)	Both	0.014 *(0.029)*	-	-
$\underline{\pi}^e_{lj,1}$	Job-loss	Impact from hc	Both	0.102 *(0.008)***	-0.053 *(0.001)***	-0.071 *(0.001)***
$\underline{\pi}^r_{lj,1}$	Job-loss	Regional effect	Meetings	1	-	-
-	-	-	Activation	0.91358 *(0.069)***	-	-
$\underline{\pi}^e_{hc,1}$	Skills evolution	Appreciation of hc	Both	0.160 *(0.012)***	0.027 *(0.03)*	0.012 *(0.016)*
$\pi_{hc,2}$	Skills evolution	Loss of hc	Both	0.076 *(0.026)***	-	-

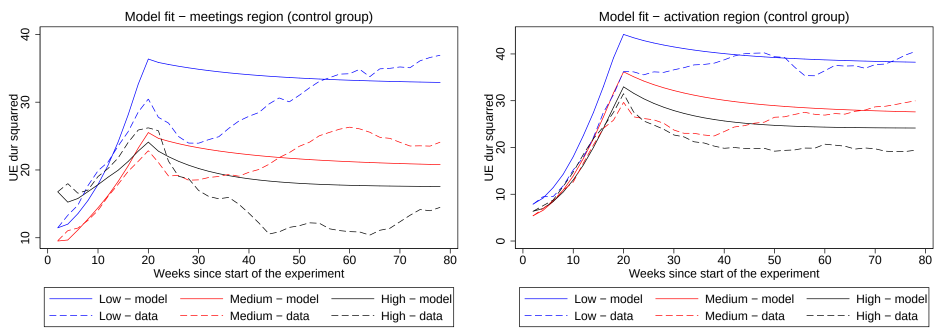
Note: Estimation window is 80 weeks. Standard Error in parenthesis, ** denotes significance at the 5 % level, * denotes significance at the 10 % level

Figure 3.5. Employment (data and model comparison)



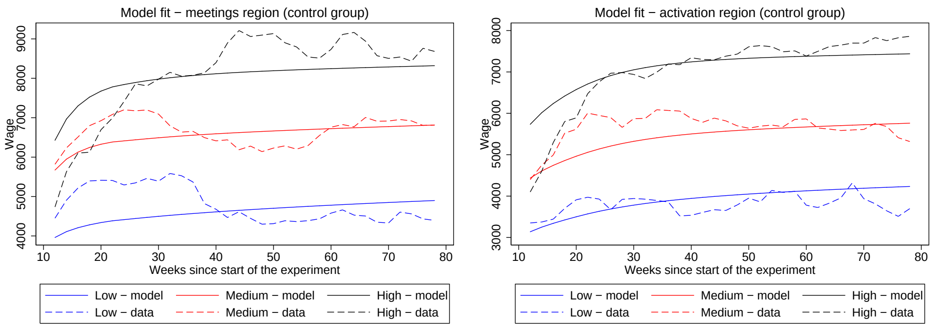
Note: see the appendix for further graphs on inflow rates for later weeks. See Appendix C Figure 3.22 for the same set of figures for the treatment group.

Figure 3.6. Average (squared) unemployment duration (data and model comparison)



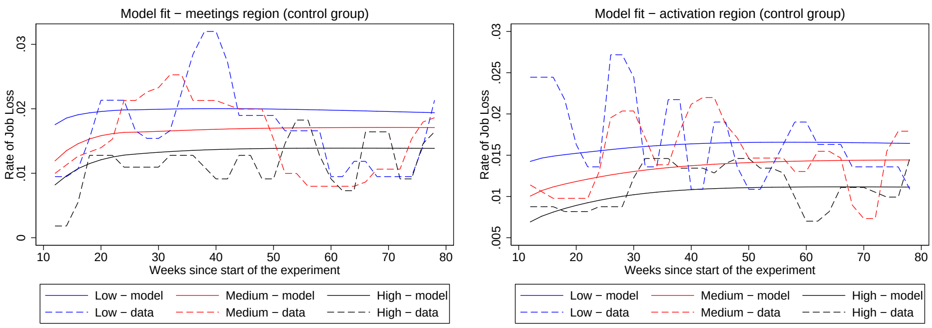
Note: See Appendix C Figure 3.23 for the same set of figures for the treatment group.

Figure 3.7. Squared wages (data and model comparison)



Note: See Appendix C Figure 3.24 for the same set of figures for the treatment group.

Figure 3.8. Job loss (data and model comparison)



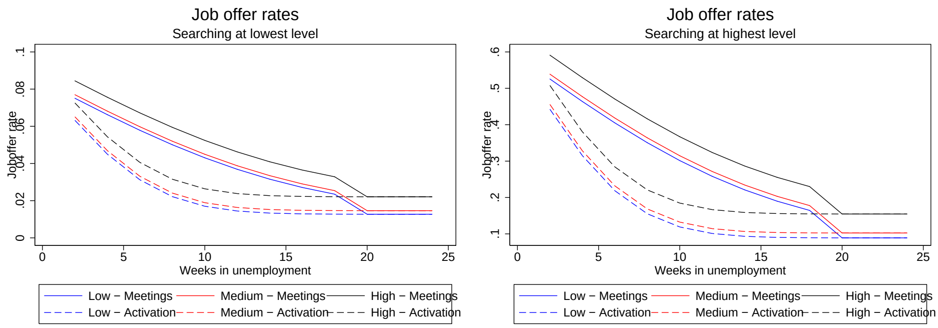
Note: See Appendix C Figure 3.25 for the same set of figures for the treatment group.

Primitives of the model

Figure 3.9 presents how job offer arrival rates varies with the duration in unemployment. The figure displays clear duration dependence in job offer rates - after 20 weeks of unemployment the likelihood that a job offer arrives is around 20 % of the rate at inflow into unemployment. Duration dependence implies that the return to job search substantially decreases over time and therefore individuals are likely to search more in the initial phases of unemployment. Negative duration dependence (see Figure 3.9) is an important part of the explanation for the decrease in outflow rates documented above. Figure 3.21 in Appendix B shows the fit of the model in an environment without duration dependence. This specification is not able to generate the spike initially in outflow rates and subsequently lower rates in the longer run. Either the initial outflow from unemployment is too low or alternatively the model predicts that long run employment levels will be too high. By including duration dependence in job offers the model fits outflow rates in both the short and longer run.
Figure 3.10 shows how wage offers vary as a function of the level of skills. There is a clear difference in wages across education levels and it is growing in the level of skills due to the education specific returns to skills. The level of skills also affects the probability of a job separation, Figure 3.11 presents the rate of job loss as a function of skills. Generally high skilled individuals face around 50 % of the risk of losing their job than low skilled. The risk of a job loss is declining in the level of education. Thus, although Table 3.7 shows that skill accumulation is faster for low educated workers, the higher risk of job separations also makes the risk of losing skills larger. As can be seen from the figures on model fit and model primitives, the environment therefore produces substantial returns to education - highly educated individuals receive higher wages, more stable employment and higher wage growth.
Table 3.8 presents some key moments in the ergodic distribution across education level, regions and types. As mentioned earlier, the environment consists of two very different types of individuals. In particular one type (with linear costs) have high costs of searching and therefore chooses not to search and instead just claim UI. This implies that they have high levels of unemployment duration and low skills. The table documents large differences in employment rates and skills both across and within educational levels. The heterogeneity across individuals is likely to generate heterogeneity in the effectiveness of ALMPs.
Finally the figures and table presented above show that there are some differences across regions (R1 and R2): job offer rates are generally lower in the region with early activation programmes (R2) and duration dependence is more pronounced. The probability of losing a job is also slightly smaller in this region. The environment is therefore less dynamic which is likely to affect the impact of the treatment and suggests that a “raw” comparison of the size of impacts across regions should be done with caution. This concern is further attenuated by the fact that the distribution

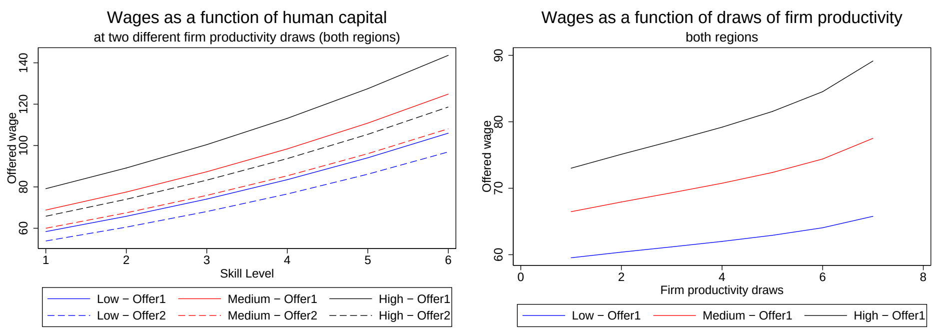
of types differs slightly between the regions.

Figure 3.9. Job offer rates



Note: The figure gives the probability of receiving a job offer next period if searching at the lowest ($ac = \frac{1}{7}$, left), and the highest ($ac = 1$, right) level in the current period

Figure 3.10. Wage offer function

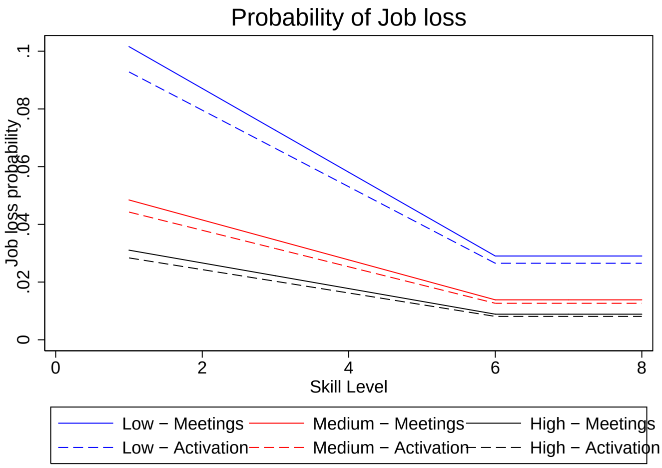


Left: The figure shows the offered wage across education levels as a function of skills (hc) for two different draws of j.
Right: The figure shows the offered wage for different draws of j across education levels.

Out of sample fit

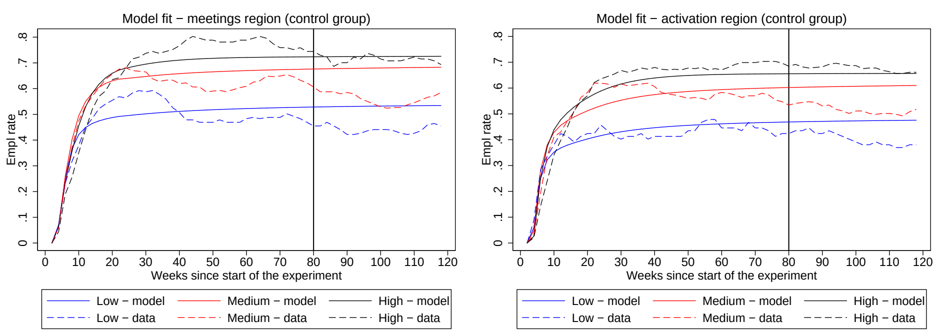
Figure 3.12 illustrates how the model matches the data out of the current sample window. To generate predictions the time series of moments is solved beyond the estimation window of the first 80 weeks. The predictions are then compared to corresponding moments in the data. The figure shows how employment rates continue to decline beyond the estimation window in the data, and the model is not able to capture this decline fully. To the extent that this decline is not driven by changes in

Figure 3.11. Job Loss



the environment after the experiment, this implies that the predictions of the ergodic distribution may be upward biased implying the future employment prospects of individuals are overstated.

Figure 3.12. Out sample predictions



* The vertical bar (at 80 weeks) marks the end of the estimation window

Impact of the experiment

Figure 3.13 compares the impact of the experimental intervention in the model to the data. The figure shows that there are clear regional differences in the response to being enrolled into the experiment in both the model and the data. In the model, individuals in the activation region display smaller responses as participation lies

Table 3.8. Key moments describing the ergodic distribution

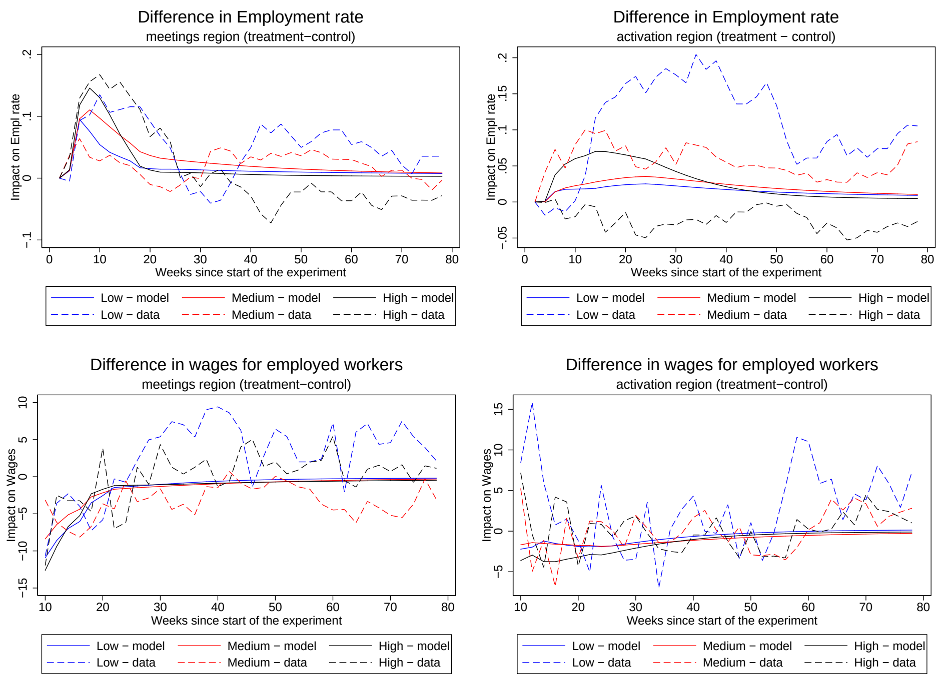
Region	Meetings					
Educational group	Low (R1)		Medium (R1)		High (R1)	
Preference for leisure group	T1	T2	T1	T2	T1	T2
Job loss rate	0.00	0.02	0.01	0.02	0.01	0.01
Employment rate	0.01	0.67	0.39	0.88	0.29	0.89
Unemployment duration***	8.99	2.19	5.22	0.56	6.19	0.56
Average skill level**	0.01	3.48	1.85	4.38	1.07	3.75
Region	Activation					
Educational group	Low (R2)		Medium (R2)		High (R2)	
Preference for leisure group	T1	T2	T1	T2	T1	T2
Job loss rate	0.00	0.02	0.01	0.01	0.01	0.01
Employment rate	0.01	0.45	0.20	0.85	0.25	0.87
Unemployment duration***	8.99	4.39	7.02	0.85	6.59	0.73
Average skill level**	0.01	2.29	0.93	4.18	0.85	3.60

Note: The table describes the ergodic distribution from which the inflow into unemployment is sampled (see estimation-section). R1: meetings region, R2: activation region,
* T1: high preference for leisure types ($\psi_k = 1$), T2: low preference for leisure types ($\psi_k \neq 1$), ** ce grid is $\{0,...5\}$, *** cu grid is $\{0,...,9\}$

further in the future. In both regions the current fit of the model generates too small impacts, especially for low educated individuals. The impacts are largest in the region with meetings where employment increases with up to 15 % within the first 5 weeks. The lower panel in the figure displays the impact on average wages for employed workers. Both in the data and in the model impacts are very small. In the meetings regions average wages are slightly lower initially both in the model and the data - over time the difference disappears.

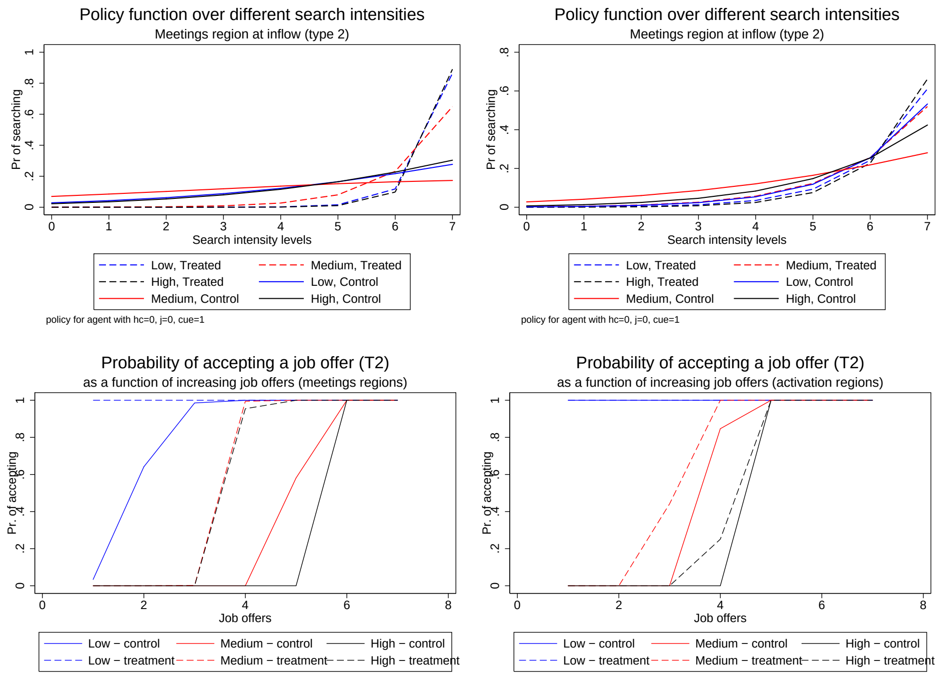
The impact of the experiment - or an increase in ALMP participation - is generated through several behavioural changes in the model. Figure 3.14 shows the policy function (defined in 3.2) over different levels of search activity and wage offers for members in the treatment and control groups at inflow into the experiment in the meetings region. Two things emerge from the figures: individuals in the treatment group change both their reservation wage and search behaviour in response to future participation in programmes which entails utility costs. The policy function over search intensities shows that treated individuals search more, and at higher levels. The likelihood of searching at the highest intensity is around 0.8 for high and low educated treated individuals compared to 0.2 in control group. The lower panel in Figure 3.14 shows that individuals in the treatment group also have lower reservation wages.

Figure 3.13. Impact of the experiment



Note: The upper panel gives the difference in employment rates for treatment and control groups in the model and data. The lower panel gives the difference in wages scaled with the fraction in employment

Figure 3.14. Channels of behaviour



* The figure shows the probability that agents choose either a given search level (upper panel) or to accept a given wage offer (lower panel). The figures show the policy function for individuals of type 2 (non-linear costs) in treatment and control groups. The response from Type 1 is generally smaller as effort is more costly and skills are lower.

Welfare calculations

Table 3.9 reports the monetary estimates of ϕ_{ap}, ϕ_{mp} and the equivalent reduction in UI. The estimates imply that individuals would be willing to reduce the level of UI with up to 50 % in the week of participation in order to escape participation.[47] The costs are increasing in the education level. The cost of participating in a meeting is higher than the corresponding one for activation when it is taken into account that a meeting is a much shorter intervention. Therefore unemployed would prefer an hour in activation compared to an hour in a meeting.

Table 3.9. Estimates of the cost of ALMP participation

Meetings	Low	Medium	High
$W_{max} \cdot \phi_{mp}$	32	30	45
Equivalent reduction in UI	35%	32%	49%
Activation	Low	Medium	High
$W_{max} \cdot \phi_{ap}$	25	45	43
Equivalent reduction in UI	27%	49%	48%

Note: The table reports the monetary value ($W_{max} \cdot \phi_i$) and the corresponding equivalent reduction in UI in the week of participation in ALMPs.

ϕ_{mp} and ϕ_{ap} measure the utility cost associated with *participating* in a programme in a given period. In order to calculate the total impact on treated individuals in the experiment under investigation further calculations are required. Generally two relevant calculations can be performed: i) the impact of the experiment on realized cost and ii) the effects on expected utility. i) only includes costs associated with actual programme participation in the treatment group, whereas ii) use the change in the value functions between treated and controls at inflow into the experiment as an estimate of the total effect on treated individuals. ii) therefore also includes the impact of the threat of future participation although such costs may never be realized because individuals change behaviour and increase their efforts to leave unemployment. The importance of this later channel depends crucially on the value of alternative actions; if for instance the cost associated with leaving unemployment earlier than "planned" is small the compensation needed to equalize the value functions is also smaller.

To calculate the monetary equivalent of the difference in expected utility the compensating variation (CV) is calculated for each value of θ and weighted with the distribution over states at inflow into the experiment. The CV is the monetary com-

[47] For comparison Svarer (2011) reports that the size of sanctions related to failure to meet eligibility criteria (e.g. participating in a meeting) ranges from a loss of benefits for 2-3 days to 3 weeks (in severe cases benefits may be removed until new eligibility through employment has been established). The cost associated with not attending a meeting should be an upper bound on the size of utility costs (else participants should simply not attend).

pensation which leaves individuals in the treatment groups indifferent between belonging to the treatment or control group at inflow into the experiment. It can be defined as:

$$V\left(\theta_{\text{env}|g} + CV\left(\theta_{\text{env}|g}\right)|\text{g=treatment, p=0, c=0}\right) \quad = \quad V\left(\theta_{\text{env}|g}|\text{g=control}\right) \quad (3.3)$$

where the LHS is equal to:

$$\max_{\alpha}\left\{\log\left(Income\left(\alpha,\theta\right) - Cost(\alpha,\theta) + CV\left(\theta\right)\right) + \delta E\left[V\left(\theta'\right)\right]\right\}$$

In practice the state specific parameter (CV) is solved for in an optimization problem where the objective is the difference in value functions between treated and controls. Under a given set of monetary compensation levels the value function for individuals in the treatment group is calculated and compared it to the control group, This continues until the difference is 0.[48] Table 3.10 presents the average CV (within education, region and preference for leisure groups) associated with the experiment under investigation. The table shows that the average CV is substantial and up to 28 times larger than the monetary costs associated with participation in programmes in a given week. The compensation differs across educational groups and types where types with lower costs of searching require lower compensation. Furthermore the compensating variation is larger in the region with meetings as the probability of future participation is higher and closer in time (participation starts immediately after inflow).

Several features of the environment explain the high CV. Most importantly, as individuals are risk averse the utility function displays declining marginal utility of income which implies that the efficiency of compensation at inflow (before programme participation) is lower than giving it at the time of actual programme participation. This implies that the compensation exceeds expected future costs because the outcomes are risky and individuals prefer an environment without risks (the control group). Furthermore the average group specific CV masks substantial heterogeneity within groups. Table 3.18 in the appendix displays how the CV varies for different durations of unemployment (cu) and skills (hc) for low educated individuals in the meetings region. The table documents substantial heterogeneity in the CV, and two findings emerge. First the CV is increasing in unemployment duration and secondly it is declining in the level of skills or the value of future employment. A long term unemployed (cu=9) needs twice as high compensation as a newly unemployed. Similarly a

[48] Since the utility function is non-linear solving for the CV implies that the contraction mapping should be resolved for each guess of compensation. Any compensation may change current actions and thus the expectations about the future. Furthermore, this calculation cannot be performed solving for the value functions in the control group due to the ergodic structure of the problem which implies that individuals eventually end up in the same state as their inflow state and thus receive the compensation again (individuals take this into account calculating the value associated with the given state). To ensure a one time compensation the compensation should therefore be calculated for the treatment group at inflow into the experiment.

high skilled individual only needs 50 % of the compensation given to a low skilled counterpart. The table thereby shows that the high average compensating variation is partly driven by individuals with low employment prospects who needs to be compensated much more.

Table 3.10. Compensating variation associated with the experiment

Meetings Region						
Education groups	Low		Medium		High	
Type	T1	T2	T1	T2	T1	T2
Compensating variation (CV)	912	634	794	434	941	314
CV relative to utility cost	28.5	19.8	26.5	14.5	20.9	6.9
Activation Region						
Education groups	Low		Medium		High	
Type	T1	T2	T1	T2	T1	T2
Compensating variation	227	67	444	181	796	204
CV relative to utility cost	9.1	2.7	9.9	4.0	18.5	4.7

Note: The CV (defined in 3.3) is weighted with the initial distribution across states at inflow into the experiment. T1: high preference for leisure types ($\psi_k = 1$), T2: low preference for leisure types ($\psi_k \neq 1$)

Welfare analysis

The estimates of the CV can be used to analyse the overall impact on welfare associated with the experiment under investigation. The impact on welfare is analysed incorporating the value of lost non-market time both in terms of costs associated with participation in ALMPs and in terms of costs from an increase in production. In the CBA the gains to society of running the experiments are calculated. The gains include the value of increased production and in addition it is assumed that the marginal cost of public funds is either 20% or 0%[49]. The former means that to finance a given transfer to the unemployed the loss to society is 20%. When reducing transfers (by bringing individuals into employment) the gain to society amounts to 20% (0%) of the saved transfers. The saved transfers as such are not included in the CBA as this is simply a transfer internally in society. The costs are the direct costs of running the programme and in addition to the marginal costs of public funds needed to finance the extra costs.[50] The calculations are reported in Table 3.11. The table shows that a traditional CBA substantially overestimates the value of social programmes by assuming that the value of lost leisure is 0. This is especially true in cases where the

[49] There is a discussion in the literature (see e.g. Kreiner and Verdelin (2012)) on whether marginal costs of public funds should be included. Below calculations with 20 % and 0 % are therefore presented

[50] See Maibom et al. (2015) for further details. The same employment definition as reported in their paper is used for determining the increase in production. Maibom et al. (2015) analyse the impact of the experiment on government budgets and a simple welfare analysis (assuming that individuals do not value leisure) is conducted.

programme requires some effort from the individual which he regards as unpleasant, as in such cases the non-market wage is substantially different from 0. In the case of meetings the gain of the programme falls by 50% and in the case of activation the gain is reduced with 80 % and is close to 0.

Table 3.11. Cost Benefit Analysis

in EURO pr. participant	Input	Costs MCPF=20%	Costs MCPF=0
Meetings: *			
Saved income transfers	3631	726	0
Saved programme costs	-47	-57	-47
Saved total costs		669	-47
Acc. gain in employment (weeks)	7.44		
Gain of increased production		6508	6508
Costs from increase in production*		-1130	-1130
Value of increased production		5378	5378
CBA before welfare effects (in €)		6047	5331
Loss in welfare**		3007	3007
Net result of CBA (in €)		**3040**	**2291**

*Costs associated with the increase in production are the average value of $\underline{\kappa}$ averaged over types and education **I use the average cost (compensating variation) obtained by averaging over types and educational groups. *** The time frame is 237 weeks as in Maibom et al. (2015)

Table 3.12. Cost Benefit Analysis

in EURO pr. participant	Input	Costs MCPF=20%	Costs MCPF=0
Activation:			
Saved income transfers	1392	278	0
Saved programme costs	-440	-528	-440
Saved total costs		-250	-440
Acc. gain in employment (weeks)	2.98		
Gain from increased production		2607	2607
Costs from increase in production*		484	484
Value of increased production		2123	2123
CBA before welfare effects (in €)		1873	1683
Loss in welfare**		1482	1482
Net result of CBA (in €)		**391**	**201**

*Costs associated with the increase in production is the average value of $\underline{\kappa}$ averaged over types and education **I use the average cost (compensating variation) obtained by averaging over types and educational groups. *** The time frame is 237 weeks as in Maibom et al. (2015)

3.8 Conclusion

Active Labour Market Programs (ALMPs) such as meetings at the job centre or shorter workfare (activation) programmes have been presented as a way to improve efficiency and reduce moral hazard in the labour market. The empirical literature has documented the existence of so-called threat effects which are consistent with the existence of a costs associated with programme participation. These costs arise because individuals spend a part of their non-market time at the job centre where they have to exert effort and potentially do unpleasant work (and maybe even feel stigmatized). Although costs are an important driver behind generated impacts, the previous literature has mainly focused on the gains of these programmes in terms of increasing job finding rates. However, in order to assess whether such programmes are indeed worthwhile social investments and whether better alternatives exist, gains associated with the programmes should be contrasted to costs including individual level costs. Therefore knowledge about the magnitude and distribution of such costs is needed.

Determining individual level costs is challenged by the fact they are generally unobservable. Furthermore since these programmes often serve as conditionalities for receiving UI the individual valuation is not directly observable from the individual decision of whether to enter the programme or not. Costs therefore have to be determined indirectly from individual behaviour such as job finding rates and accepted wages. In order to generate a link between behaviour and individual level costs an economic model of behaviour and an accurate description of the incentives faced by potential participants must be specified.

In order to quantify how individuals value ALMPs this paper developed a dynamic model with discrete choices capturing key behavioural channels which can be affected through interactions between unemployed and public authorities (the job centre). The model was estimated using data from a Danish randomized experiment which provides exogenous variation in the intensity of interactions. Thereby the costs agents incurs when they have to go into either activation or a meeting at the job centre can be estimated. To analyse effects on welfare, the structure of the model is used to calculate the compensating variation associated with the experimental intervention. The model incorporates several sources of heterogeneity and the analysis shows that the corresponding estimates of the compensating variation varies greatly among states. In particular some individuals require very large compensations at inflow into the experiment.

Overall the results suggest that traditional CBA calculations which do not take the individual loss of non-market time into account overstate the gain from having these programmes. The individual level costs are substantial and amounts to up to 50 % of UI in a given week of participation. The analysis shows that individual costs and associated compensating variation are important to quantify in order to assess whether the current mix between ALMPs and UI is optimal. Ignoring the existence of these costs implies that we put excessive weight on the efficiency of UI systems while overall welfare may be deteriorated.

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3.9 Appendix A: Solution of the model

I solve the model in a series of steps outlined below. The solution procedure is similar to the one presented in Ferrall (2002).

i) Solve for $V(\theta)$ in (3.1) *using the contraction mapping properties.*

The method of successive approximations and error bounds suggested by McQuad and Porteus is used (see Rust (1986)). Ferrall (2002) gives the conditions under which $V(\theta)$ is a contraction mapping. This have also been tested numerically by starting the fixed point equation from a series of different initial conditions, the resulting value function is indistinguishable across iterations.

ii) Calculate the policy function, $P(\alpha|\theta)$ as given in (3.2).

The policy function specifies how agents behave given a position in the state space. Given a distribution across state variables, aggregate behaviour in a given period can be determined (in a later stage this is then compared to data through moments). To determine behaviour across time the policy function and the transition functions presented in the main text can be combined to specify how the distribution over the state space evolves over time.

iii) Solve for the state-to-state transition matrix:

$$P_{sts}\left(\theta'|\theta\right) = \sum_{\alpha} P\left(\alpha|\theta\right) P\left(\theta'|\alpha,\theta\right)$$

The state-to-state transition function allows us to track the evolution of the state space from some t to some $t+k$ exploiting that the model is Markovian (i.e. iterating on a Markov chain). Given an initial distribution over states the distribution of states at a given point in time can be solved for. The remaining challenge is therefore to specify an initial distribution across states. This is further complicated by the fact that some state variables are unobserved and therefore an initial distribution over states is also unobservable. As explained earlier this problem is solved exploiting the existence of an ergodic distribution.

iv) Solve for the ergodic distribution:

$$P_{ergodic}\left(\theta\right) = \sum_{\theta'} P\left(\theta'|\theta\right) P_{ergodic}\left(\theta\right) \tag{3.4}$$

The ergodic distribution specifies how individuals are distributed across states in the economy in steady state. From this distribution the inflow into unemployment can be determined. The ergodic distribution is found by solving for the fixed point in(3.4), see also Judd (1998). The existence and uniqueness of the ergodic distribution have also been tested numerically.

v) Apply sample selection rules to the the ergodic distribution.

This final step creates a sample that matches the data on observable terms (e.g. unemployment duration) but also takes account of the dynamic selection on unobservables since inflow into unemployment. Using the state-to-state transition function and the corrected initial distribution over observable and unobservable states we can now solve for the distribution over states for each time period since t=0.

These 5 steps now enable me to relate the predictions of the model to the actual data and thus learn about the structural parameters. In the main text the content of a final step vi) is presented:

vi) Determine the fraction of workers satisfying certain spell requirements and determine their distribution across state variables for each t.

3.10 Appendix B: Further Figures and Tables

Further data descriptives

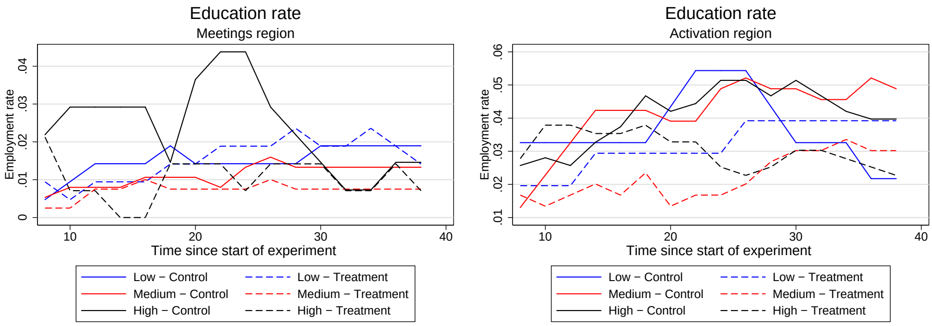
Table 3.13. Descriptives in the Meetings Region

Variable Type	Variable	Treatment	Control	P-value
Demographics	Age	39.18	39.12	0.91
	Below 30	0.21	0.23	0.18
	Above 45	0.27	0.29	0.39
	Fraction Males	0.52	0.55	0.25
	Education length	11.73	11.68	0.79
State before inflow	Newly Non-employed	0.80	0.77	0.18
	Sick-listed	0.12	0.12	0.89
	Education	0.04	0.03	0.30
	Earnings 2007	283890	273857	0.22
	Hourly Wage 2007	192.28	187.50	0.35
Previous Employment	Public sector	0.33	0.33	0.97
	Trade	0.55	0.56	0.90
	Construction	0.12	0.11	0.88
Employment history	Weeks in E (year -1)	40.35	39.70	0.48
	Weeks in E (year -2)	37.82	39.05	0.20
	Weeks in E (year -3)	36.41	36.88	0.64
	Weeks in E (year -4)	35.57	36.46	0.39
	Weeks in E (year -5)	36.30	36.65	0.74
	Weeks in NE (year -1)	11.65	112.30	0.48
	Weeks in NE (year -2)	14.18	12.95	0.20
	Weeks in NE (year -3)	15.59	15.12	0.64
	Weeks in NE (year -4)	16.43	15.54	0.39
	Weeks in NE (year -5)	15.69	15.35	0.74
Observations		752	724	

Table 3.14. Descriptives in the Activation region

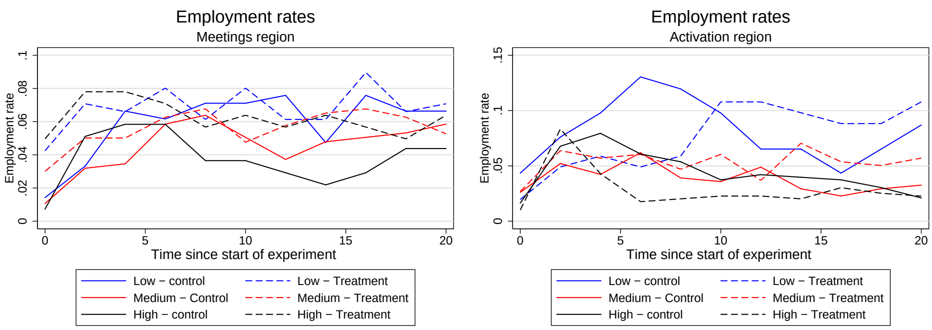
Variable Type	Variable	Treatment	Control	P-value
Demographics	Age	35.64	35.34	0.53
	Below 30	0.38	0.35	0.23
	Above 45	0.20	0.17	0.17
	Fraction Males	0.47	0.48	0.57
	Education length	13.63	13.78	0.26
State before inflow	Newly Non-employed	0.68	0.68	0.83
	Sick-listed	0.06	0.07	0.77
	Education	0.21	0.21	0.74
	Earnings 2007	234322.17	232753.77	0.87
	Hourly Wage 2007	158.30	151.50	0.28
Previous Employment	Public sector	0.42	0.47	0.03
	Trade	0.51	0.45	0.03
	Construction	0.07	0.8	0.87
Employment history	Weeks in E (year -1)	28.70	29.58	0.44
	Weeks in E (year -2)	26.69	26.13	0.63
	Weeks in E (year -3)	25.02	23.49	0.18
	Weeks in E (year -4)	23.10	21.53	0.16
	Weeks in E (year -5)	24.63	23.21	0.24
	Weeks in NE (year -1)	23.30	22.41	0.44
	Weeks in NE (year -2)	25.31	25.87	0.63
	Weeks in NE (year -3)	26.98	28.51	0.18
	Weeks in NE (year -4)	28.91	30.47	0.16
	Weeks in NE (year -5)	27.37	28.68	0.24
Observations		796	827	

Figure 3.15. Fraction of individuals entering education



Note: time since start of experiment is measured in weeks

Figure 3.16. Fraction in self-support



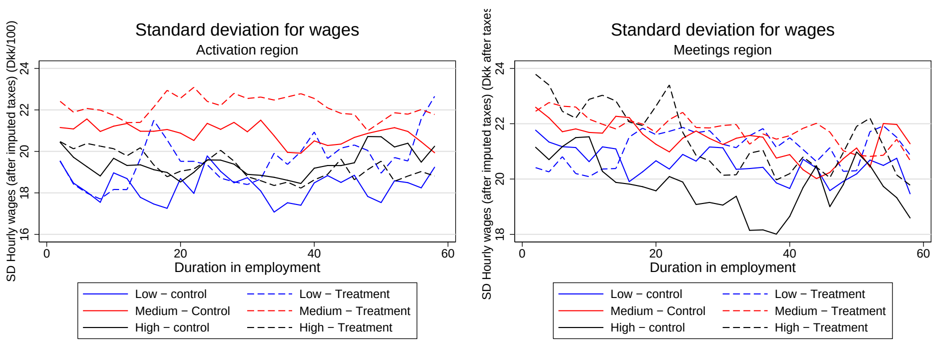
Note: time since start of experiment is measured in weeks

Table 3.15. Replicating Table 3.3 using an alternative employment criterion

Employment status	(1)	(2)	(3)	(4)	(5)	(6)
After 4 weeks	Low, R1	Medium, R1	High, R1	Low, R2	Medium, R2	High, R2
Treatment indicator	0.103*	0.0291	0.1000+	0.0234	0.0349	0.00911
	(0.0445)	(0.0340)	(0.0535)	(0.0643)	(0.0367)	(0.0268)
Constant	0.251*	0.322*	0.226*	0.261*	0.267*	0.175*
	(0.0299)	(0.0241)	(0.0359)	(0.0460)	(0.0253)	(0.0184)
After 14 weeks						
Treatment indicator	0.116*	0.0178	0.0911	0.134+	0.0525	-0.0352
	(0.0482)	(0.0356)	(0.0591)	(0.0702)	(0.0405)	(0.0349)
Constant	0.488*	0.561*	0.540*	0.337*	0.515*	0.502*
	(0.0345)	(0.0256)	(0.0427)	(0.0495)	(0.0286)	(0.0242)
Observations	423	775	278	194	605	824

Note: The results are from separate OLS regressions after 2, 4, 10 and 14 weeks. The dependent variable is employment status (not counting individuals in self sufficiency). Huber/White standard errors, + $p < 0.10$, * $p < 0.05$

Figure 3.17. Standard deviation for wages



Note: time since start of experiment is measured in weeks

Table 3.16. Wage growth

	(1)	(2)	(3)	(4)
Employment duration	20 weeks	40 weeks	60 weeks	80 weeks
Low Education	-0.00284	0.0237*	0.0180	0.0214+
	(0.00963)	(0.0109)	(0.0119)	(0.0130)
Medium Education	-0.00646	0.0215*	0.0188*	0.0371*
	(0.00600)	(0.00670)	(0.00722)	(0.00772)
High Education	0.0226*	0.0470*	0.0648*	0.0830*
	(0.00645)	(0.00714)	(0.00754)	(0.00801)
N	2694	2490	2303	2085

Robust standard errors in parentheses, + $p < 0.10$, * $p < 0.05$. Data is pooled across regions. Wages for employed workers are compared to their own inflow wage after 20,40,60,80 weeks

Figure 3.18. Treatment impact for employed workers

20 weeks in employment	(1) Low, R1	(2) Medium, R1	(3) High, R1	(4) Low, R2	(5) Medium, R2	(6) High, R2
Treatment indicator	2.397	-3.778*	1.830	2.426	-0.255	0.103
	(2.419)	(1.700)	(2.450)	(3.285)	(1.945)	(1.373)
Constant	95.60*	101.2*	103.4*	93.24*	97.84*	101.4*
	(1.703)	(1.209)	(1.691)	(2.478)	(1.320)	(0.957)
N	307	646	256	130	501	754

Robust standard errors in parentheses, + $p < 0.10$, * $p < 0.05$. The sample is employed after 10 weeks of employment

Further details about the model and fit:

Table 3.17. Other parameters in the model (not estimated)

Symbol	Model	Value (Control group)
π_{mp}	Meetings probability	$\pi_{mp} = 0.10$
π_{ap}	Meetings probability	$\pi_{ap} = \min\{0.1 \cdot \text{uedur}, 0.35\}$
δ	Discount rate	0.995
UI	UI level	$0.6 \cdot W_{max}$

* in the treatment phase π_{mp} and π_{ap} are set to 1. ** parameters are set to match features of the data, in particular meetings and activation intensities as documented in Maibom et al. (2015). The replacement level of a worker earning 150% above average earnings is around 0.6, see Bjoern and Hoej (2014). Therefore, in the model UI is set to $0.6 \cdot W_{max}$.

Figure 3.19. Model fit: Inflow rates

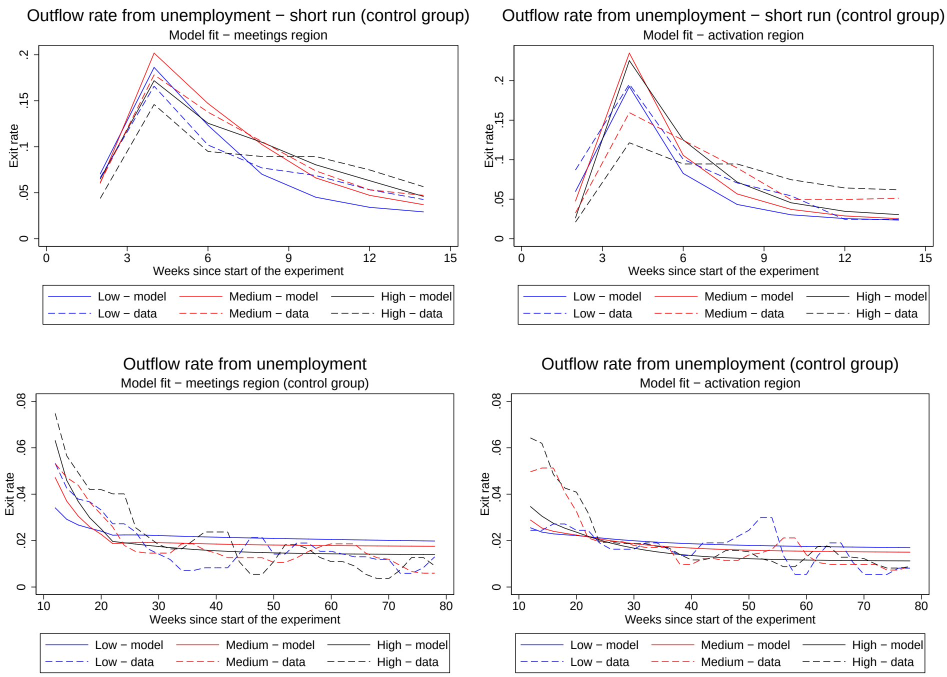
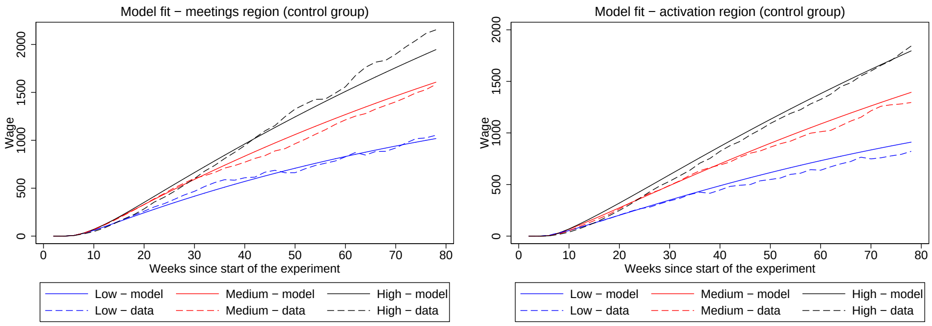
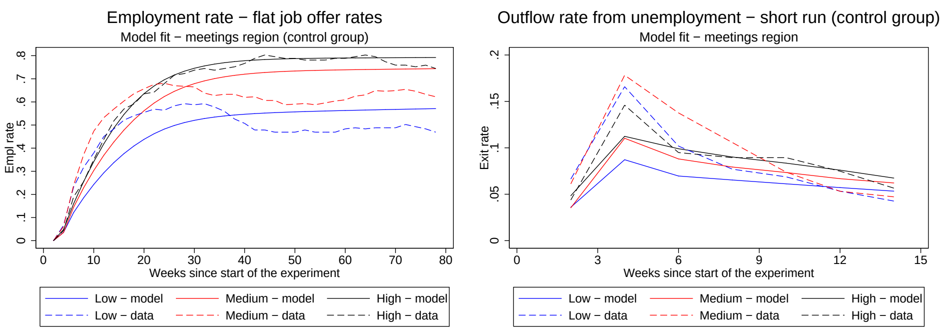


Figure 3.20. Model fit: Employment duration and wages



Note: the figure gives the data and model prediction for moment 13 defined in Table 3.6

Figure 3.21. Eliminating duration dependence



Note: The figure compares data to model predictions in a model where job offer arrival rates do not vary with unemployment duration

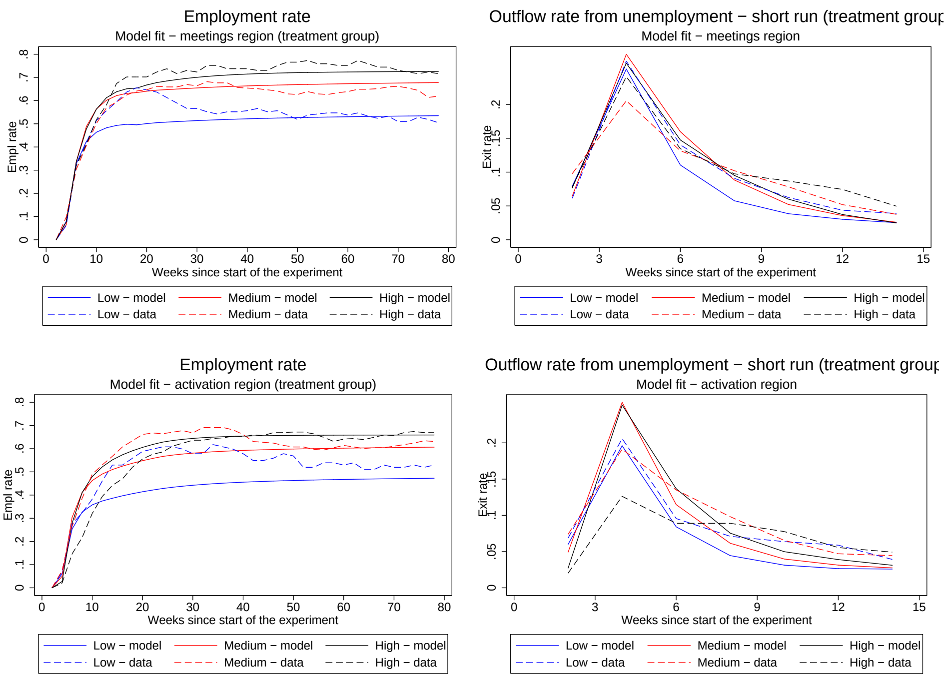
Table 3.18. Heterogeneity in the compensating variation (meetings region)

State variables		Compensating variation	
Unemployment duration (cu)	Skill level (hc)	Low (type 1)	Low (type 2)
0	0	1014	652
1	0	1068	947
2	0	1160	1047
3	0	1297	1121
5	0	1747	1531
7	0	2086	1809
9	0	2138	2025
0	1	914	592
0	2	863	104
0	3	674	111
0	4	469	104
0	5	414	50

Note: The table reports the CV (defined in 3.3) for different unemployment durations and skills for low educated individuals in the meetings region. All other state variables are set to 0.

3.11 Appendix C: Model fit for the treatment group

Figure 3.22. Employment (data and model comparison for the treatment group)



Note: see the appendix for further graphs on inflow rates for later weeks.

Figure 3.23. Average (squared) unemployment duration (data and model comparison for the treatment group)

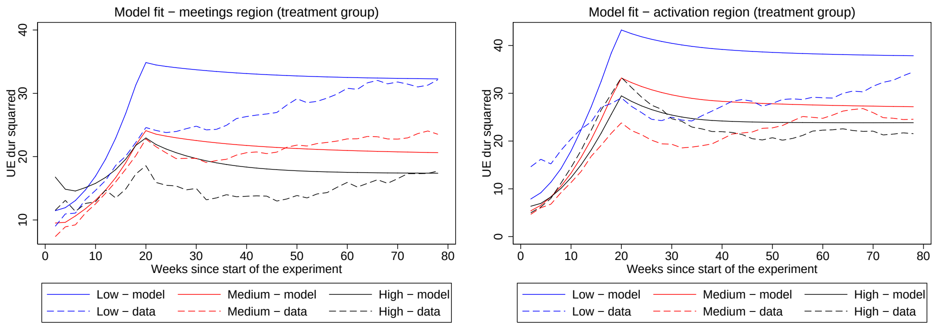


Figure 3.24. Squared wages (data and model comparison for the treatment group)

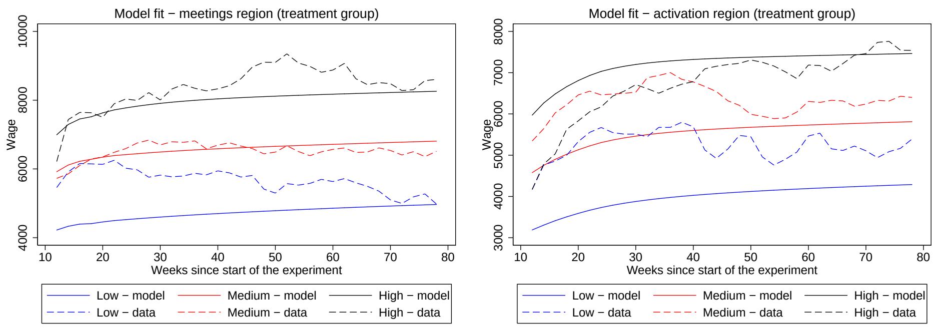
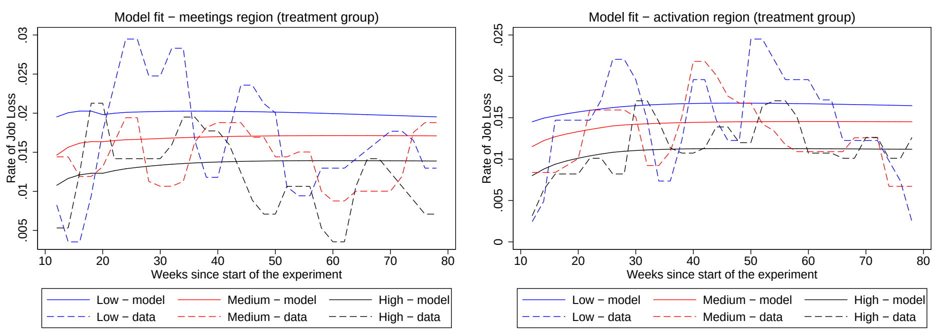


Figure 3.25. Job loss (data and model comparison)



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