Essays on Unemployment, Job Search Behavior and Policy Interventions

Inauguraldissertation
zur Erlangung des akademischen Grades
eines Doktors der Wirtschaftswissenschaften
der Universität Mannheim

vorgelegt von Laura Johanna Pohlan
im April 2018

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Verteidigung: 05.07.2018

Acknowledgments

This thesis would not have been possible in its current form without the support of a number of special people who I would like to thank.
First of all, I would like to thank my first supervisor and co-author Gerard van den Berg for his constant support and the many discussions and suggestions that helped to greatly improve this thesis. I would also like to thank my second supervisor Carsten Trenkler for his valuable comments.
Furthermore, I am very grateful for the fruitful collaboration with my co-authors.
Thank you to Annette Bergemann and Arne Uhlendorff (Chapter 2) for their productive cooperation especially during the revision of our paper for Labour Economics.
Thank you to Maximilian Blömer (Chapter 3), Nicole Gürtzgen (Chapter 3 and 4) and Holger Stichnoth (Chapter 3) for letting me be part of the minimum wage team.
Thank you to André Nolte (Chapter 4) for the pleasant and constructive collaboration.
This thesis was written during my time at the Centre for European Economic Research (ZEW) in Mannheim, to which I am very grateful for providing me with such an inspiring research environment and the opportunity to participate in so many national and international workshops and conferences. I would like to thank all of the participants at the seminars at ZEW and at the University of Mannheim for their comments and discussions which helped to greatly improve the chapters of this thesis. My special thanks goes to Annette Hillerich-Sigg and Boris Ivanov with whom I shared an office, to my fellow PhD students and to my friends.
Moreover, I would like to thank my colleagues in the research department "Labour Markets, Human Resources and Social Policy" at ZEW for the great time we had together both during and outside of office hours.
Above all, I am very grateful to Florian Fetzer and to my parents Sabine and Dirk Pohlan for their constant personal support while writing this thesis.

Mannheim, April 2018 *Laura Johanna Pohlan*

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Introduction

Job loss is a life event with lasting consequences for workers' income and employment trajectories. Empirical studies reveal that periods of unemployment have a persistent negative effect on job finding, earnings and job quality (see e.g. van den Berg and van Ours, 1994 and Burda and Mertens, 2001). In the long run, being out of work adversely affects the human and social capital accumulation, the mental and physical well-being and the hiring prospects of the unemployed (see e.g. Pissarides, 1992 and Clark and Oswald, 1994).

This dissertation explores through which channels unemployment leads to exclusion from society and how policy interventions and technological innovations affect individual job search behavior and are able to bring unemployed persons back into the labor market. This thesis starts with an analysis of how job loss impacts different dimensions of social exclusion. Thereafter, I investigate whether participation in job creation schemes improved the employment prospects of participants in the years following German reunification. The last two chapters of this thesis study the impact of two key macroeconomic events, the introduction of a uniform minimum wage and the emergence of high-speed internet, on search frictions in the German labor market and the resulting effects on employment outcomes.

All four chapters contained in this dissertation are based on large individual-level data sets from Germany. Using administrative data from the German Federal Employment Agency, it is possible to precisely measure the duration of different labor market states and transitions between them. Information on subjective assessments, job search strategies and employment biographies of East Germans from the 1990s are provided by survey data. In all of the studies, the aim is to identify causal relationships by employing different empirical methods, ranging from instrumental variable estimation to structural modeling.

In Chapter 1 of this thesis, I analyze the economic and social consequences of job loss that might lead to marginalization from society. The concept of social exclusion has become increasingly prominent in public debates regarding social vulnerability and disadvantage (Federal Government, 2017). In this context unemployment is considered one of the main risk factors for social marginalization. I investigate the causal impact of job loss on multiple dimensions of social exclusion by combining inverse propensity score weighting with a difference-in-differences approach. In this way, individuals who become unemployed can be compared to workers still in employment. Based on linked survey and administrative data from Germany for the time period 2007–2015, the results suggest that job loss has particularly detrimental effects on the subjective perception of social integration, life satisfaction, the access to economic resources and mental health. Moreover, this chapter shows that becoming unemployed hinders the fulfillment of psychosocial needs that are typically associated with an employment relationship, such as social status and higher self-efficacy. The effects of job loss are long-lasting, growing more profound the longer the duration of unemployment and remaining present even if the individual finds a job again. Looking at effect heterogeneity, I find that having a partner and being highly educated reduces the negative effects of job loss. The chapter shows that unemployment leads to exclusion from society through multiple channels. Social marginalization carries a high risk of individuals ending up in a state from which they will never return to work. Less job search effort and lower chances of being hired due to discouragement, stigmatization, human capital depreciation and living in deprived neighborhoods can lead to long-term unemployment (see e.g. Atkinson and Kintrea, 2001 and Biewen and Steffes, 2010). Following the severe negative effects of job loss documented in Chapter 1, the next chapters of the thesis focus on various instruments that may help to get the unemployed back into work.

There is much debate over the optimal design of policy interventions to effectively reduce the risk of unemployment persistence and to increase the stability of employment relationships. Active labor market policies (ALMPs), such as training programs and job creation schemes (JCSs), aim at advancing the reintegration of unemployed individuals into regular employment.[1] In industrialized countries, such ALMPs are a popular tool for fighting unemployment, especially in times of economic instability. These programs offer workers the opportunity to keep a foot in the labor market and thus prevent depreciation of social and human capital.

In Chapter 2 of this thesis, which is co-authored by Annette Bergemann and Arne Uhlendorff, we analyze the impact of participation in JCSs on job search outcomes in the context of the turbulent East German labor market in the aftermath of German reunification. This was an economic environment characterized by high levels of job destruction. JCSs that offer temporary work opportunities for the unemployed in the public

[1] See e.g. the overview articles of Card et al. (2010) and Card et al. (2017) on the effectiveness of ALMP programs.

and nonprofit sector were implemented extensively in order to cushion this development. These schemes reached their peak in 1992 when, on average, 388,000 individuals were employed through JCSs and public spending from both the German Federal Government and the German Federal Employment Agency amounted to 10.4 billion DM in East Germany. Using survey data from the East German state of Sachsen–Anhalt for the period 1990-1999 and building upon the timing-of-events approach, we estimate multivariate discrete time duration models taking selection based on both observed and unobserved heterogeneity into account. Our results indicate that after initial negative effects during the typical program duration of twelve months the impact on the job finding probability becomes insignificantly positive. These effects are probably driven by reduced job search effort during participation resulting in a rearrangement of the job queue. Additional results, however, suggest that female and highly skilled participants leave unemployment quicker than other groups, which results in highly skilled women benefiting from participation. In general, we find no significant impact on post-unemployment employment stability.

Policies that impact on national systems of wage determination might influence an economy's competitiveness and employment levels through wage restraint and labor unit costs. One of the most important wage-related commitments of the previous legislative period was the introduction of a uniform statutory minimum wage amounting to 8.50 euros an hour. Prior to this, minimum wages had been implemented only in selected industries. Accordingly, most of the existing empirical work on the impact of minimum wages in Germany has been conducted non-structurally for these industry-specific minimum wages (for an overview see e.g. Möller, 2012). Reduced form approaches have significant limitations as they typically rely on the assumption that there are no general equilibrium effects of minimum wages and they are not able to assess the impacts of different minimum wage levels. Modeling both sides of the labor market is important as firms are likely to adjust their number of vacancies and wage offers in response to minimum wage changes.

In Chapter 3 of this thesis, my co-authors Maximilian Blömer, Nicole Gürtzgen, Holger Stichnoth, Gerard van den Berg and I estimate an equilibrium search model and simulate the introduction of a uniform minimum wage based on German administrative data. Equilibrium search models explicitly incorporate firms' adjustments and provide, for instance, theoretical foundations for spillover effects on workers earning more than the minimum wage. Moreover, structural models of the labor market can help to gain a better understanding of the magnitude of search frictions which may create a certain amount of power for employers. The model used in Chapter 3 incorporates worker and firm heterogeneity and allows for different job offer arrival rates for the employed and the unemployed. In this setting, the sign of the employment effects of a minimum wage is not restricted a priori. Our simulations show that unemployment is, except for a small range of very low minimum wages, a monotonically increasing non-linear function of the minimum wage level. We find that medium to high minimum wages have an overall negative employment effect but the results differ strongly across different segments of the labor market defined by region and type of occupation. Our analysis indicates that the heterogeneity in the employment effects is mainly driven by differences in firm productivity rather than by variation in search frictions or the opportunity costs of employment.

There are other ways to reduce search frictions and make the job search process more efficient that might help unemployed job seekers find a job more easily. For instance, the emergence of high-speed internet as a new mass medium during the last two decades has made the accumulation of information about potential job offers in the market less costly.

In Chapter 4 of this thesis, which is co-authored by Nicole Gürtzgen, André Nolte and Gerard van den Berg, we study the effects of the emergence of high-speed internet on the reemployment probabilities of unemployed job seekers on the West German municipality level. We combine data on the share of households who could technically have access to broadband internet via digital subscriber line (DSL) technology with administrative data on individual employment biographies and construct monthly reemployment propensities for an inflow sample into unemployment. To address the endogeneity in broadband internet availability, we follow an instrumental variable approach by exploiting technological peculiarities at the regional level that affected the roll-out of high-speed internet in the early DSL period. Our results suggest that the introduction of high-speed internet has positive effects on reemployment probabilities, with the internet improving the prospects of finding a job especially for male workers after the first four months of unemployment. To further explore the relationship between increased internet availability and individuals' job search behavior, we complement the analysis with individual survey data. Our results reveal that home internet access is indeed causally related to online job search, though there is no evidence that this leads to a higher number of job interviews attended. On a descriptive basis, however, the duration dependent incidence of job interviews indicates that home internet access is associated with more job interviews after three months in unemployment. This finding provides some tentative evidence that being able to search for a job online increases job offer arrival rates with a certain time delay, which appears to match the delay found in the municipality level analysis.

Chapter 1

Unemployment and Social Exclusion*

1.1 Introduction

The negative consequences of unemployment are discussed in many empirical studies. Long periods of unemployment reduce the reemployment probability (see e.g. van den Berg and van Ours, 1994 and Kroft et al., 2013) and coincide with lower reemployment wages (see e.g. Addison and Portugal, 1989 and Burda and Mertens, 2001). Beside the classical economic effects of job loss, the literature documents a negative link between unemployment and health (see e.g. Browning and Heinesen, 2012 and Black et al., 2015), between unemployment and physical and mental well-being (see e.g. Clark and Oswald, 1994 and Kassenboehmer and Haisken-DeNew, 2009) and between unemployment and social ties (see e.g. Eliason, 2012 and Kunze and Suppa, 2017).

There is a growing research and policy interest in the link between labor market integration and social integration. The term ‘social exclusion’ has become increasingly prominent in policy debates regarding poverty and social inequality and often refers to disadvantages in core living conditions that reduce the possibilities of participating in society (European Commission, 2010 and Federal Government, 2017). Social exclusion can be viewed as dynamic multidimensional process where various deficits reinforce each other (Room, 1995). In this context unemployment is considered one of the main risk factors for social exclusion. Exclusion from employment might lead to alienation from society and increase the risk of long-term dependency on social welfare benefits (see e.g. Bhuller et al., 2017), committing suicide (see e.g. Sullivan and von Wachter, 2009), becoming a criminal or victim of a crime (see e.g. Freeman, 1999) and to support

*This chapter has benefited from comments and suggestions by Gerard van den Berg, Clemens Hetschko, Boris Ivanov, André Nolte, Friedhelm Pfeiffer, Carsten Trenkler and Arne Warnke. I would further like to thank the Research Data Centre (FDZ) at the IAB for data access.

extreme parties (see e.g. Falk et al., 2011). Furthermore, social exclusion may not only affect the current generation, it may be passed on to the next generation (Machin, 1998).

The aim of this chapter is to shed light on the causal impact of job loss on social exclusion by discussing in great detail the driving mechanisms behind this association. From an individual's point of view, periods of unemployment can affect the economic and social situation in different ways and to varying degrees. The reduction in disposable income due to job loss creates restrictions on the financial side. Consequently, maintaining a minimum standard of living, but also participating in social and cultural activities, could become more challenging (Jenkins and Cappellari, 2007). Beside the economic strain, unemployment may take away non-pecuniary benefits associated with working such as time structure, the chance to demonstrate competences and skills, an individual's status and social relations (see e.g. Jahoda, 1981). Moreover, sociologists and psychologists emphasize that redundancy could come with stigmatization, the feeling of insecurity and shame. Hence, the loss of a job represents a potential source of stress and can lead to emotional and physical distress, isolation and alienation.[1] These economic and social consequences of unemployment are expected to contribute to or be accompanied by the subjective feeling of social exclusion.

There are some empirical studies that investigate the relationship between labor market integration and an overall subjective evaluation of social integration with the help of survey data. Based on the first five waves of the survey 'Labour Market and Social Security' (PASS), Gundert and Hohendanner (2014) make use of panel data techniques and find that the risk of feeling socially excluded is higher among the unemployed than among employed workers. Furthermore, their results indicate that the degree to which employment contributes to perceived social affiliation is related to the level of job security. The reports conducted by Gallie and Paugam (2003), Böhnke (2004) and Layte et al. (2010) provide a comparative analysis of social exclusion across European countries and point to a positive relationship between unemployment and average levels of perceived social exclusion in a society.

Instead of concentrating on one overall measure of social exclusion as in the studies mentioned above, I define a set of multiple interdependent factors which characterize marginalization from society and might be affected by periods of unemployment. Social exclusion describes an objectively precarious financial situation, but also refers to the feeling of being part of society. This subjective feeling might depend on the individual's emotional stability, social network, relative position in society but also on personality traits which could help to cope with multiple deprivation (Popp and Schels, 2008). In

[1] See e.g. Brand (2015) who provides a literature review on the economic and non-economic impacts of job loss for the United States.

this analysis I study the effects of job loss on several dimensions of social exclusion: the individual perception of social integration, life satisfaction, mental health status, economic resources, social participation, social status and self-efficacy. Gundert and Hohendanner (2014) discuss how different mechanisms meditate the relationship between employment status and social integration. However, the identification of mediation effects relies on strong assumptions that are likely to be violated in my setting (see e.g. Gelman and Hill, 2007 and Imai et al., 2010). For instance, it is not possible to identify effects mitigated by so-called mediators and direct effects of job loss as long as further unobserved mediators are existent. Moreover, it is difficult to distinguish between mediators and outcome variables as the social and economic effects of unemployment can mutually reinforce each other. That is the reason why my study concentrates on the total effects of job loss on the different dimensions defined above. In addition, I provide new insights into the consequences of unemployment by studying heterogeneous effects for subgroups defined by sociodemographic characteristics and by the type of job loss and the duration of the unemployment spell.

I contribute to the literature by analyzing the effects of becoming unemployed based on a combination of survey and administrative data and a method that allows me to account for selection effects due to time-constant unobserved characteristics and reversed causality. This study makes use of the panel data set PASS-ADIAB 7515 which covers about 10,000 households per wave and includes individual information on the areas of employment, education, income, health, social life and housing. Additionally, the rich administrative data set of the Federal Employment Agency provides detailed information on job and firm characteristics and employment histories. In a first step, I estimate the probability of job loss given a large set of control variables reflecting individual and household characteristics as well as the labor market history. In a second step, I apply inverse propensity score weighting combined with a difference-in-differences approach to control for observed and permanent unobserved differences between individuals who become unemployed and those who do not.

My results are in line with previous findings and point in the expected direction. Unemployment has particularly detrimental effects on the subjective perception of social integration, life satisfaction, the access to economic resources as well as on mental health. Looking at psychosocial needs that are typically met by an employment relationship, I find that social participation is not affected by job loss while the social status and the self-efficacy level become lower. Furthermore, I find some evidence for effect heterogeneity across subgroups. Individuals with a partner and high-skilled workers suffer less from unemployment. I also study the effects of job loss depending on the type and the time that passed by since the employment relationship has ended. The main finding is that the effects become more profound the longer the duration of unemployment. The negative consequences of previous unemployment are still present even if the individual finds a job again.

The rest of the chapter is structured as follows. Section 1.2 discusses theories regarding the concept of social exclusion and the consequences of job loss. Section 1.3 describes the data source and the measurement of the outcome variables. Section 1.4 presents the empirical identification strategy. Section 1.5 describes the sample and shows model diagnostics. Section 1.6 presents the results of the empirical analysis and Section 1.7 concludes.

1.2 Theoretical Considerations

1.2.1 The Concept of Social Exclusion

The term 'social exclusion' has its origins in France in the 1970s and referred to persons who were unprotected by social insurance and at risk of permanent detachment from society. A widespread adoption of the term in Europe started in the 1980s, when unemployment rates were high and threatened national modes of social integration (Kronauer, 1998). More recently, the European Union declared 2010 as the European Year for Combating Poverty and Social Exclusion.

Thus far no operationalization of the concept of social exclusion has been established as a standard in the literature. However, sociologists have emphasized some key characteristics of the concept on which the theoretical framework of my analysis is based (see e.g. Room, 1995; Rodgers et al., 1995; Atkinson, 1998 and Sen, 2000). Social exclusion is viewed as a dynamic process, involving deprivation across a range of dimensions which affect individual opportunities to be connected to mainstream society.[2] Exclusion from society can be described as disadvantages in core living conditions, such as housing, income, education, employment and well-being (Andreß, 2003), which reduce the possibility of maintaining an 'appropriate' standard of living and social participation. However, social exclusion is not only determined by an objectively precarious financial situation but also by the individual perception of belonging to society. Criteria and standards that define social integration are to a large degree subjective and are weighted

[2] Social exclusion might depend on several interdependent dimensions of society such as the labor market, economic resources, social participation, educational, health care and social welfare institutions or civic and human rights. As the institutional and political system of western social welfare states like Germany or the Scandinavian states should in principle be accessible for every citizen I do not consider these dimensions in this study.

differently by individuals. In order for an individual to feel part of society and take part in social activities it is of great importance that the individual is able to shape his or her life according to subjective perceptions and aims. Hence, social integration depends on both an individual's capacity to act and an individual's actual actions (Sen, 1985). The subjective feeling of social integration might be influenced by general life satisfaction, mental health status, an individual's close social surrounding, the relative position in society but also on personality traits such as self-efficacy which could help to cope with multiple deprivation (Popp and Schels, 2008).

The above reasoning shows that social exclusion has multiple interdependent dimensions which can reinforce each other. Exclusion from society can also impact, for instance, on social participation or mental health through the lack of perceived integration as well as alienation. Higher perceived alienation is associated with lower well-being and a higher risk of depression (Layte et al., 2010). As it is not clear which variables act as mediators and which ones as outcomes, I concentrate on the total effects of job loss on the following outcome variables which might lead to social exclusion: perceived **social integration**, **well-being and mental health**, **economic resources** and the **psychosocial needs** *social participation*, *social status* and higher *self-efficacy*.

From an economic point of view, social exclusion is strongly related to exclusion from the labor market. Obsolete skills, living in deprived neighborhoods and discouragement effects (see e.g. Atkinson and Kintrea, 2001; Murie and Musterd, 2004 and Clark et al., 2010) might in turn considerably reduce the individual employment prospects and hence lead to long-term dependency on social welfare benefits. However, these channels should be highly related to the outcomes I am looking at, as social capital, emotional stability and personality traits such as self-efficacy are important determinants of reemployment probabilities (see e.g. Darity and Goldsmith, 1996 and Helliwell and Putnam, 2004). In the following I will provide more detailed explanations for potential effects of job loss on the outcome variables under consideration. The empirical identification of the causal effects is discussed in Section 1.4.

1.2.2 The Consequences of Job Loss

Economic resources. Job loss leads to exclusion from the labor market and needs that are associated with an employment relationship. Two main functions of paid employment can be emphasized: the first function is the provision of financial resources, which allow individuals to maintain a minimum standard of living and to shape life according to subjective perceptions and aims. Job loss coincides with earnings losses and hence might constrain the access to economic resources. As a consequence unemployed individuals might have to adjust their lifestyle, for instance by changing their diet, their place of residence or their general spending behavior. Financial constraints could also affect their participation in social and cultural activities (Jenkins and Cappellari, 2007). Poverty researchers usually distinguish two approaches of measuring poverty, a resource-based poverty measure and a measure of deprivation. While the former defines poverty primarily in financial terms (lack of income and consumption), the latter measure concentrates on a direct measure of what individuals are able to be or to do. This approach was suggested by Sen (1992) who defines poverty as the inability of individuals to achieve a minimal level of capabilities to function (such as the inability to be healthy, clothed, sheltered, etc.). The advantage of this approach is that it takes into account the inherent ability of individuals to translate consumption into welfare as well as the impact of public goods on welfare (e.g. public health, education, etc.). In this study I will concentrate on economic deprivation due to job loss which is reflected by the non-availability of basic goods and the non-participation in activities satisfying basic needs.

Psychosocial needs. The second function of employment refers to psychosocial needs that go beyond the need for financial resources. Jahoda (1981) proposed a latent deprivation theory which states that unemployment causes deprivation not only of manifest economic resources, but also of five latent psychosocial needs that are usually met through an employment relationship: the need for a time structure to one's day, the need for social contacts outside of the immediate family, the need to be a part of a collective purpose, the need for status and personal identity and the need for regular activity. According to Jahoda (1981) and others (e.g. Creed and Muller, 2006; Paul and Batinic, 2010 and Gundert and Hohendanner, 2014) the absence of those functions, together with economic strain, might explain why an individual's perception of social integration as well as subjective well-being declines when becoming unemployed. In this study I focus on the following psychosocial needs that can be met more easily through working: *social participation*, *social status* and higher *self-efficacy*.

Social participation. On the one hand, when individuals become unemployed they typically lose their daily social contacts, for example to colleagues or customers. In addition, the literature documents a negative relationship between unemployment and social participation. Social participation might comprise formal participation like activity in a club or organization and informal participation like interaction with friends and relatives (Dieckhoff and Gash, 2015). It has been found that the unemployed engage in social activities less often (see e.g. Kunze and Suppa, 2017) and have less social support from close relations and authority figures compared to employed individuals (see e.g. Jackson, 1999). Moreover, the psychological distress that goes along with being unemployed is compounded by the negative social attitudes towards unemployment which risk further alienating the unemployed from mainstream society (Gallie et al., 2003). As a consequence, the loss of social contacts can lead to lower life satisfaction. Dolan et al. (2008) provide a detailed literature review on the determinants of subjective well-being and find evidence that an important factor which positively influences subjective well-being is social contacts.

On the other hand, the additional leisure time could also be beneficial for social participation of the unemployed. Studies that focus on the time use of employed and unemployed individuals show that unemployed persons spent more time on home production and leisure activities such as socializing than the employed (see e.g. Krueger and Mueller, 2012). Hence, the net effect of job loss on social participation is not clear a priori.

Social status. According to Jahoda (1981) an individual's position in life is in large part defined by one's job. This notion is supported by Paul and Batinic (2010) who state that individuals tend to see themselves in a similar way as others see them and even employed workers with a relatively low occupational status, for example manual workers, feel that they are treated with more respect and recognition than unemployed persons. Job loss might bring a certain stigma as well as feelings of shame and worthlessness. The loss in social prestige may be reflected in the subjective perception of occupying a lower social status. There are studies that focus on the relationship between social norms that are associated with different labor market states and subjective well-being. Findings point to lower life satisfaction due to status and identity effects caused by the event of job loss (see e.g. Clark, 2003; Stutzer and Lalive, 2004 and Hetschko et al., 2014).

Self-efficacy. In social-cognitive theory the construct of self-efficacy deals with the ability of an individual to deal with demanding situations by taking adaptive action (Bandura, 1997). Self-efficacy might be an important individual characteristic in modern labor markets in which more and more responsibility is shifted to the worker. Tisch and Wolff (2015) discuss the link between employment and self-efficacy. Employed workers are likely to be more confident with respect to their problem-solving capabilities due to the feedback received from people outside of their family like colleagues and superiors. Moreover, an employment relationship links individuals to a collective purpose or goal that might lead to increased self-efficacy when such goals are achieved. Regular activity at the workplace might help an individual to learn about and to value his or her own skills.

Hence, Jahoda's latent functions of employment should positively influence self-efficacy. Fryer (1986) states that individuals might differ in their reaction to unemployment which cannot be explained by Jahoda's deprivation theory. He assumes that individuals want to actively control their lives by making plans and pursuing goals. Hence, job loss and the associated loss of the above mentioned functions may lead to a lower level of self-efficacy. In addition, becoming unemployed might be viewed as individual failure (Silver et al., 2005).

Social integration. Labor market integration plays a central role in feeling part of society. Sociologists have provided some empirical evidence that employment is related to a higher level of perceived social integration than unemployment (see e.g. Gundert and Hohendanner, 2014 and Layte et al., 2010). As discussed above job loss might lead to multiple deprivations such as financial constraints, social contacts and social status which could determine the degree to which individuals feel as though they belong to society. In this study I analyze the impact of job loss on an overall subjective evaluation of social integration as defined by those components which are regarded as important from an individual's point of view.

Well-being and mental health. There are a number of existing studies that show the detrimental effects of unemployment on individuals' subjective well-being (see e.g. Frey and Stutzer, 2002 and Helliwell, 2006 who give a literature review on happiness research). Life satisfaction can be viewed as the ultimate result of what resources enable people to do and to be, in other words their ability to convert resources into a good life. Furthermore, being emotionally stable is a central dimension of employability and a basis for regular activity, which can be interpreted as an individual's potential to be part of society. In contrast, job displacement could cause psychosocial and financial stress which might result in unhappiness and mental health problems (see e.g. McKee-Ryan et al., 2005 and Paul and Moser, 2009 for meta-analyses on the mental health effects of unemployment). In the short-run, mental health problems might appear, for instance, in the form of fear, dejection or irritability. Studies showed that in the long-run the unemployed face a higher risk of dying early and are more likely to commit suicide (see e.g. Ruhm, 2000 and Sullivan and von Wachter, 2009) which could be interpreted as the worst form of social exclusion.

However, individuals might also quit their job voluntarily, for instance due to dissatisfaction with working-conditions. In this scenario, the effect of becoming unemployed on subjective well-being is ambiguous from a theoretical point of view.

There might be additional contributing factors to why unemployed individuals are socially excluded. Anxiety due to reduced life-course predictability might also influence whether an individual feels part of society or not. Unemployed individuals are likely to face a lower level of life-course predictability compared to employed workers as their situation might require a change of residence or to get involved with new social groups (Gundert and Hohendanner, 2014). In addition, the trust in institutions and other people might decline due to job loss, which could in turn affect social and mental well-being (Helliwell and Wang, 2011). Furthermore, stigmatization and human capital depreciation might also foster social exclusion. Unfortunately, the PASS-ADIAB 7515 does not contain questions reflecting information on these potential channels.

1.3 Data and Measurement of Outcomes

1.3.1 Data Source

This study is based on individual level data provided by the German Federal Employment Agency. The PASS-ADIAB 7515 data set combines weakly anonymous survey data provided by the household panel study 'Labour Market and Social Security' (PASS) with administrative data from the Integrated Employment Biographies which are based on employers' notifications to the social security authorities.

The PASS is a household panel survey and is designed for research on the living-conditions of low-income households in Germany (Trappmann et al., 2010). The survey is financed by the Ministry of Labour and Social Affairs and has been conducted on yearly basis since December 2006.[3] The PASS-ADIAB 7515 is based on the nine subsequent annual waves of the PASS (2007–2015). In the first wave about 12,500 households and 19,000 individuals were interviewed. The initial sample consists of two subsamples of almost equal size, one of which is drawn from the unemployment registers of the Federal Employment Agency and contains a sample of households with at least one benefit unit on the reference date in July 2006, while the second is a general population sample, oversampling low status households. The initial subsample of benefit recipients is refreshed each year. In the context of panel surveys sample attrition between survey waves plays a crucial role. Attrition might be caused by death, moving abroad or non-

[3] The response rate on the household level in the first wave of 30.5% (Bethmann et al., 2016) is in line with other surveys in comparable populations. For example the LSS 2005 (Meßmann et al., 2008) and the benefit-recipient survey conducted as part of the evaluation of the experimentation clause (ZEW, IAQ and TNS Emnid, 2007) achieve almost equal response rates.

response due to non-contactability or refusal. The attrition rates of the PASS panel range between 18% (Wave 9) and 43% (Wave 2) of households between two consecutive waves. Approximately 20% of the dropouts are only temporary and return in the following wave. In the ninth wave 13,271 individuals living in 8,921 households were interviewed.[4]

The PASS gathers detailed information on individual and household characteristics in the fields of employment, education, income, health, social life and housing. For the purpose of this study, examining the effects of job loss, the PASS has the advantage of including questions on the subjective assessment of well-being, living conditions and individual attitudes. The Integrated Employment Biographies complement the survey data with detailed information on individual employment histories including start and end date of dependent employment, registered unemployment or registered job-search or unemployment benefit receipt periods on a daily basis. In this way, I am able to construct precise durations and numbers of periods in a particular employment state.[5] This administrative data source covers all surveyed persons who have at least one entry in their social security records from 1975 onwards in West Germany and starting from 1992 in East Germany. Periods of self-employment, civil service, and of military service are not included in the data set. Alongside information on different labor market states, the data include individual information on (daily) wage records and on firm characteristics such as industry code, median wage paid or firm size. An individual's past labor market performance should be highly related to unobserved factors like ability and motivation which are likely to influence my outcome variables. Hence, information on individual employment histories may help to identify the causal effects of job loss (Heckman et al., 1997).

1.3.2 Measurement of Outcome Variables

In the following I describe how the outcome variables social integration, well-being and mental health, economic resources and the psychosocial needs social participation, social status and self-efficacy are measured in this study. The PASS questions underlying the outcome variables and a description of their construction is presented in the corresponding section in Appendix 1.B.

Social integration is quantified by the subjective perception of social affiliation ranging from 1 to 10; from feeling excluded (1) to feeling part of society (10) (see

[4] Table 1.A.1 in Appendix 1.A provides a detailed description of the number of interviews in each wave.

[5] Survey data can only be linked to administrative data from the Federal Employment Agency for those who agreed to the linkage. Table 1.A.2 in Appendix 1.A shows that on average 80% of the respondents agree on merging the two data sets in each wave.

Section 1.B.1). In this analysis the impact of job loss on two measures of emotional stability is analyzed (see Section 1.B.2): life satisfaction and mental health status. To quantify life satisfaction I make use of a question which is standard in large-scale surveys like the GSOEP or the BHPS. Individuals are asked to assess on a 0 to 10 scale how satisfied they are currently with their life as a whole, with 0 meaning that the person is completely dissatisfied and 10 meaning completely satisfied. In addition, I use a variable with five categories indicating whether an individual has been "extremely", "quite a bit", "moderately", "a little bit" or "not at all" affected by mental health problems, like fear, dejection or irritability in the last four weeks.

To measure the access to resources enabling a basic standard of living and social participation, I use two variables (see Section 1.B.3). First, I use a deprivation index which is included in the PASS data set. The surveyed households are asked to indicate whether they possess a list of basic goods considered essential for an appropriate standard of living in society. For instance, the household is asked whether it has an apartment with at least as many rooms as persons living there, with a garden or balcony and whether the household possesses a car. Moreover, the household members are asked to indicate whether they participate in activities satisfying basic needs, such as having a hot meal or saving a fixed amount of money, as well as in social activities, such as inviting friends for dinner at home or going to the cinema once in a while. All in all the deprivation index is based on a list of 26 goods or activities. In addition, survey participants are asked whether the household does not possess these goods or does not participate in certain activities due to financial or other reasons. In order to construct the deprivation index properly only items that are missing for financial reasons are counted. In this way, it is ensured that conscious decisions, for instance a household choosing not to own a car or television, are not misinterpreted as a reduced standard of living. As a second measure of economic resources, I use the subjective satisfaction with the standard of living in total on a 0 to 10 scale, ranging from "completely dissatisfied" to "completely satisfied".

I quantify social participation with the help of two different measures (see Section 1.B.4). First, I exploit information on how many close friends (can also include family members outside the household) the individuals have.[6] Moreover, I use information on the activity in organizations or associations. The PASS includes a question on whether the respondent is actively engaged in a union, political party, church community, clubs such as music, sport or culture clubs or another organization. Based on the responses to this question I construct a variable ranging from 0 to 5 indicating how many activities the

[6] Unfortunately, the PASS-ADIAB does not include information on the composition of the social network. The network of unemployed persons might change with a higher proportion of friends being also unemployed and at risk of social marginalization (Gallie and Paugam, 2003).

individual is engaged in. To measure social status the relative ranking matters. I make use of a question asked in PASS, where the respondent should rank himself or herself on a 1 to 10 scale, where 1 means belonging to the bottom of society and 10 to the top (see Section 1.B.5). The self-efficacy index used in this study is introduced and tested by Schwarzer and Jerusalem (2006). It is based on a five item battery where the respondent has to decide whether they "apply completely", "tend to apply", "tend not to apply" or "do not apply at all" (see Section 1.B.6).

1.4 Empirical Identification

The aim of this chapter is to determine the causal effects of job loss on the dimensions of social exclusion defined in Section 1.2. The identification of causal effects relies on a comparison of the outcome levels of workers becoming unemployed to those of otherwise identical but still employed workers. However, in this setting selectivity issues are likely to play a role.

In general, an employment relationship ends either because workers are laid off, their contract expires and is not prolonged or they quit voluntarily. In the empirical analysis I study how the effects depend on the type of job loss. The distinction between voluntary and involuntary unemployment allows me to learn more about the self-selection of employees into unemployment. The PASS-ADIAB does not contain information on mass layoffs which could be used to estimate the effects of involuntary job loss as it is often done in the literature. However, the individual risk of being affected by a mass layoff might also be influenced by selection both on the part of the firm as well as on the employee side.[7] Firms of a different size, sector or workforce composition face different business risks and vary with respect to their employment contract designs. Similarly, employees might self-select, for instance due to family reasons, to work in firms that are less likely to make layoffs. The German Employment Protection Act (*Kündigungsschutzgesetz*) prescribes the requirements for making workers redundant.[8] This law states that termination with notice is only valid if it is based on reasons relating to either the employees' character, conduct, or urgent operational business requirements. The employer has to undertake a social selection of the relevant employees on the basis of length of employment, age, family support obligations and severe disability. However, there might still be a certain scope for an employer to lay off workers with low productivity or bad health. The individual probability of becoming unemployed might be

[7] See e.g. Kletzer, 1998 and Pfann, 2006 who discuss selection of the employees who are affected by mass layoffs.

[8] This law applies only to firms that employ at least ten full-time employees.

influenced by unobservable factors like ability or motivation but also by lower levels of the outcome variables before job loss. For instance, unhappy people or people with few social contacts or mental health problems could be more likely to become unemployed.

The fundamental challenge of causal inference arises because we cannot observe the outcome levels of the same individual simultaneously with and without job loss which makes it impossible to observe causal effects directly (Imbens and Wooldridge, 2009). To address this issue, I apply inverse propensity score weighting (IPW). The basic idea of this approach is to make those workers who do not experience a job loss comparable in their observable characteristics to workers who do lose their job. This is achieved by weighting down the outcome levels for individuals from the comparison group who are over-represented and weighting up those who are under-represented. The weights are determined by the propensity score, the probability of not being employed in the next period ($T = 1$), given observed covariates x:

$$p(x) = \mathbb{P}(T = 1|X = x) \tag{1.1}$$

The difference between the weighted outcome levels of the two groups is then a consistent estimate of the effect of job loss on the different dimensions of social exclusion of unemployed individuals (average treatment effect on the treated (ATT)).

The key assumption for identification of the ATT is the conditional independence assumption, which states that, conditional on the propensity score, potential outcomes are independent of the event of job loss (Rosenbaum and Rubin, 1983). To make the assumption that all selectivity is captured by observables reasonable in my application, I make use of a very large set of determinants of job displacement. For instance, the data provide information on sociodemographic characteristics, subjective indicators, individual health status and household situation. In addition, I have detailed information on individual employment histories and on previous jobs including firm characteristics and whether the position was a permanent position. The selection of the covariates follows screening of control variables used in other empirical studies on the non-pecuniary effects of job loss (see e.g. Kassenboehmer and Haisken-DeNew, 2009 and Marcus, 2013).[9] Moreover, I carefully study the influence of the outcome levels before job loss on the probability of becoming unemployed.

Given concerns over potentially biased results due to unobserved differences between workers who lose their job and their matching partners, I follow Heckman et al.

[9] In the final specification I rely on a set of covariates that might determine job loss from a theoretical point of view and that allows differences between unemployed individuals and those still in employment to disappear for an even larger set of variables.

(1997) who developed a conditional difference-in-differences extension of matching. In this study I combine IPW with a difference-in-differences approach (IPW-DID), as suggested by Abadie (2005), to eliminate permanent differences that are time-invariant and unobserved by the researcher.

In a nutshell, I make use of a two-step procedure to estimate the effect of job loss. In a first step, I estimate the individual probability of job loss between two consecutive waves by means of logit models given a detailed set of observed individual, household, job and firm characteristics. These variables are measured at the first of two consecutive waves such that their levels are not affected by future job loss. Then, I use the fitted values of the propensity scores to calculate the weights. In a second step, I use the inverse-probability weights to compute weighted averages of the changes between the two survey waves of outcome levels for each treatment level. The estimator is given by

$$\hat{\tau}_{ATT} = \frac{\frac{1}{N}\sum_{i=1}^{N}\tilde{p}(x_i^{t_0})\left(\frac{T_i^{t_1}(Y_i^{t_1}-Y_i^{t_0})}{\tilde{p}(x_i^{t_0})} - \frac{(1-T_i^{t_1})(Y_i^{t_1}-Y_i^{t_0})}{1-\tilde{p}(x_i^{t_0})}\right)}{\frac{1}{N}\sum_{i=1}^{N}\tilde{p}(x_i^{t_0})} \tag{1.2}$$

where $T_i^{t_1}$ indicates the event of job loss for individual i, $i = 1,...,N$, in period t_1. $Y_i^{t_0}$ and $Y_i^{t_1}$ denote the observed outcomes of each individual in two consecutive periods t_0 and t_1. The weights are normalized to ensure that the weighted number of control observations sums up to the number of treated: $\tilde{p}(x_i^{t_0}) = \frac{\hat{p}(x_i^{t_0})}{\frac{1}{N}\sum_{i=1}^{N}\hat{p}(x_i^{t_0})}$, where $\hat{p}(x_i^{t_0})$ is the estimated probability of job loss conditional on observed characteristics measured in t_0.

The IPW-DID approach identifies the ATT under the assumption that the average outcomes of unemployed and still employed workers would follow a parallel trend in absence of the event job loss. Hence, this approach assumes that both groups are characterized by similar changes and not by similar levels of the outcome variables in the case of no job displacement. To test for the similarity or divergence, for example due to anticipation of the treatment, I conduct placebo tests by comparing the change in outcomes of both groups in periods before the event of job loss takes place.

1.5 The Sample and Model Diagnostics

1.5.1 Sample Selection

The analysis of the impact of job loss on several dimensions of social exclusion is built on the nine waves of PASS (2007–2015). The sample is restricted to respondents who were interviewed in two consecutive waves t_0 and t_1 and whose administrative records could be identified. Daily information on employment biographies allow me to determine an individual's current employment status at the interview date. Individuals are either part-time or full-time employed and do not receive unemployment benefits in the first of the two consecutive waves (wave t_0). I define two different groups of individuals that can be distinguished by the event of job loss in the second of two consecutive waves (wave t_1). The treatment group consists of individuals who stated that they were employed in wave t_0 and are unemployed and not employed in parallel, for instance via a mini job or an active labor market program in wave t_1.[10] This means that I am analyzing a combination of short-term and medium-term effects of job loss: the duration of the current unemployment spell ranges between one day and one year. Individuals that belong to the control group are continuously employed between two consecutive waves.[11] The sample is restricted to individuals who are between 18 and 64 years old, not in education and for whom no information on observable characteristics and outcome variables that are used in the empirical specification are missing. A detailed description of the variables used in this study can be found in Appendix 1.C (Tables 1.C.1 and 1.C.2).

I end up with a treatment group that consists of 635 cases in which workers are employed in wave t_0 and are unemployed in wave t_1 and a control group that consists of 17,047 cases in which workers are continuously employed between two consecutive survey waves. Table 1.D.1 in Appendix 1.D shows that the same individual might be either in the treatment group, the control group or in both groups several times. Approximately half of the group of workers who lose their jobs and 11% of the control cases are individuals who are considered only once in the analysis.

1.5.2 Descriptive Statistics

Table 1.1 shows selected descriptive statistics of the observable characteristics that are used in the empirical analysis separately for workers that become unemployed and workers that are continuously employed between two consecutive waves. Additional descriptives are reported in Table 1.D.2 in Appendix 1.D. The variables presented in Tables 1.1 and 1.D.2 can be grouped into the following categories: initial levels of outcome variables, sociodemographics, subjective indicators, household and partner characteristics, characteristics of the previous job and the previous firm as well as information on the employment history.

[10] I consider individuals as unemployed in wave t_1 in case they are unemployed according to the Integrated Employment Biographies in t_1 or they have no unemployment entry in the social security records for at most six months but had one before and have an unemployment or employment entry thereafter. In addition, I consider individuals as unemployed in wave t_1 if they have no unemployment entry in t_1 but enter unemployment from employment within three months. I use survey data on the actual labor market state in t_1 in case administrative information are missing.

[11] 94% of the control persons do not change their employer between two consecutive waves t_0 and t_1.

Table 1.1: Selected descriptive statistics

Job loss	Yes	No	Difference	
Initial outcome levels				
Social integration [1-10]	7.39	7.99	-0.60	***
Life satisfaction [0-10]	6.77	7.43	-0.66	***
Mental health status [1-5]	3.85	4.04	-0.18	***
Deprivation index [0-11]	0.82	0.39	0.43	***
Satisfaction with standard of living [0-10]	6.62	7.39	-0.77	***
Number of close friends	1.98	2.19	-0.21	***
Social engagement [0-5]	0.50	0.77	-0.27	***
Social status [1-10]	5.73	6.28	-0.55	***
Self-efficacy [1-4]	3.03	3.08	-0.04	*
Sociodemographics & household characteristics				
Female	0.41	0.53	-0.12	***
Age	40.93	44.18	-3.25	***
Migrant	0.07	0.04	0.03	***
Married	0.39	0.60	-0.20	***
Number of own children	1.24	1.46	-0.22	***
Home owner	0.22	0.47	-0.25	***
Serious health restrictions	0.25	0.18	0.06	***
PQ: no vocational training	0.17	0.09	0.08	***
PQ: vocational training	0.66	0.64	0.02	*
PQ: advanced vocational training	0.05	0.10	-0.05	***
PQ: academic degree	0.13	0.17	-0.05	***
East Germany	0.30	0.26	0.04	**
Previous job characteristics & employment history				
Permanent contract	0.57	0.86	-0.29	***
Tenure	19.82	71.85	-52.03	***
Daily wage	51.90	77.57	-25.67	***
Sector: Agriculture/Production	0.10	0.17	-0.07	***
Sector: Consumption/Food	0.05	0.07	-0.01	
Sector: Construction	0.09	0.05	0.05	***
Sector: Trade	0.12	0.13	-0.01	
Sector: Transportation/Services I	0.29	0.19	0.10	***
Sector: Services II	0.15	0.07	0.08	***
Sector: Education/Health	0.12	0.20	-0.07	***
Sector: Public	0.08	0.12	-0.05	***
Number of employment periods with ssc	7.86	5.52	2.34	***
Employment duration with scc	115.72	183.84	-68.12	***
Number of marginal employment periods	1.63	1.14	0.49	***
Marginal employment duration	12.71	16.77	-4.06	***
Number of unemployment periods	4.53	2.30	2.23	***
Unemployment duration	68.17	30.44	37.74	***
Number of non-employment periods	2.92	2.07	0.85	***
Non-employment duration	41.51	40.69	0.81	
Number of observations	635	17,047		

Notes: PQ: Professional qualification. ssc: social security contributions. Scales of the outcome variables are shown in squared brackets. Differences are statistically significant at the *10%, ** 5% and *** 1% level.
Source: PASS-ADIAB 7515, own computations.

There are substantial differences in the baseline outcome levels between both groups. I find significant lower levels in all dimensions for workers whose employment relationship ends between two waves except for the deprivation index, for which I find a significantly higher level indicating limited access to economic resources. Regarding sociodemographics, I find that men, young workers as well as workers with an immigration background are more likely to become unemployed. Individual unemployment probabilities are higher for low-skilled individuals. Furthermore, workers who lose their job between two consecutive waves are on average less healthy. There are significant differences between both groups with respect to household characteristics. Workers that become unemployed are less often married and are less likely to have children or to be homeowners. Finally, I find a clear pattern when looking at previous job characteristics as well as at the employment history. Individuals that become unemployed have shorter tenures as well as less employment experience and more often suffer from interruptions caused by periods of unemployment or non-employment. Moreover, they are more often employed on a temporary basis, receive on average lower wages and are more likely to work in the production, construction or service industry than individuals who remain employed.

1.5.3 Model Diagnostics

In the baseline specification I apply IPW-DID on the pooled sample based on the nine waves of PASS. In this paragraph I describe balance diagnostics for assessing whether the specification of the propensity score model has been adequately chosen. The results of the propensity score matching can be found in Table 1.D.3 in Appendix 1.D. As shown in the previous subsection, before weighting, individuals that become unemployed and those who remain employed differ with respect to most determinants of job loss as well as the baseline levels of the social exclusion measures. Following Austin (2011) and Guo and Fraser (2015), I examine the standardized differences in means after weighting between individuals who become unemployed and those who do not to test for balance. The standardized difference gives the difference in averages by treatment status, scaled by the square root of the sum of the variances and is formally given by

$$d = \frac{(\bar{x}_{treatment} - \bar{x}_{control})}{\frac{\sqrt{S^2_{treatment} + S^2_{control}}}{2}} \tag{1.3}$$

where $\bar{x}_{treatment}$ and $\bar{x}_{control}$ denote the sample means and $S^2_{treatment}$ and $S^2_{control}$ the sample variances in treatment and control group, respectively. Moreover, I also look at variance ratios. A perfectly balanced covariate has a standardized difference of zero and variance ratio of one. Austin (2011) points out that there exists no universally agreed criterion for how small a standardized difference has to be to provide balance. I follow his rule of thumb according to which a standardized difference of less than 0.1 is taken to indicate a negligible difference in the means of treatment and control group.

The balancing tests of my baseline specification can be found in Table 1.D.4 in Appendix 1.D. This table shows that the standardized differences are close to zero and the variance ratios are close to one for a large set of covariates which is larger than the set of covariates included in the baseline specification.[12]

Table 1.D.6 in Appendix 1.D shows summary statistics of the propensity scores for the unemployed and individuals still in work, which show that there is sufficient overlap between treatment and control group to be able to conduct a proper analysis.

1.6 Empirical Findings

In this section I present the baseline results of the IPW-DID estimates of the effect of job loss on the different dimensions of social exclusion as defined in Section 1.2.2. In a next step I look at heterogeneous effects for subgroups defined by sociodemographic characteristics and by the type of and amount of time since job loss. Finally, I discuss the robustness of the results.

1.6.1 Baseline Results

Table 1.2 presents the estimation results of the baseline specification. The number of observations for individuals that become unemployed between two consecutive waves corresponds to 635 while the number of observations for individuals that remain employed corresponds to 17,047 for all outcomes except for the self-efficacy index. The measure of self-efficacy is not available in wave 5 and 9, which leads to roughly 40% fewer observations. The results show that individuals who become unemployed on average lose out in multiple dimensions. The changes in the outcome variables are

[12]The baseline specification is based on information for 46 covariates. These variables are partly divided into dummy variables which leads to 99 control variables in total. I conduct several robustness checks to examine the sensitivity of the choice of covariates included in the estimations. Table 1.D.5 in Appendix 1.D presents the estimation results for a larger set of covariates, e.g. including the "Big Five" personality traits: extraversion, tolerance, conscientiousness, neuroticism and openness and an aggregation of the control variables to independent factors using factor analysis. The results are comparable to the baseline specification.

standardized in order to allow for better comparability of the estimated effect sizes.[13] My findings provide evidence that job loss is accompanied by a decrease in the overall assessment of social integration and subjective well-being. The estimated short- and medium-term effects on life satisfaction are stronger: job loss leads to a decrease of 0.55 standard deviations (SDs) in life satisfaction compared to 0.33 SDs in social integration. My results suggest further that job loss can be associated with severe mental health problems. Becoming unemployed reduces mental health by 0.31 SDs. Moreover, I find that the deprivation index, which represents a measure of poverty, increases substantially by 0.61 SDs while satisfaction with the standard of living decreases by 0.53 SDs.

Table 1.2: Baseline results for the consequences of job loss

Change in outcomes	Effect of job loss	Standard error	Standard deviation
Social integration	-0.333***	(0.054)	1.602
Well-being and mental health			
Life satisfaction	-0.549***	(0.060)	1.330
Mental health status	-0.309***	(0.046)	1.152
Economic resources			
Deprivation index	0.608***	(0.071)	0.493
Satisfaction with standard of living	-0.529***	(0.057)	1.451
Psychosocial needs			
Social participation			
Number of close friends	-0.049	(0.043)	1.084
Social engagement	-0.048	(0.038)	0.650
Social status	-0.244***	(0.048)	1.354
Self-efficacy	-0.202***	(0.062)	0.379

Notes: Estimates from IPW-DID are based on 635 treated and 17,047 control persons (the estimates for self-efficacy are based on 417 treated and 10,359 control persons). The propensity of job loss is based on a logit model with the control variables reported in Tables 1.C.1 and 1.C.2 in Appendix 1.C. The differences in the outcome variables are standardized. Standard errors are robust and calculated by taking into account that propensity scores are estimated. Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: PASS-ADIAB 7515, own computations.

The psychosocial needs that are typically met by an employment relationship are partly influenced by periods of unemployment; I find no relationship between unemployment and social participation. There is no change in the number of close friends or activities an individual is engaged in due to job loss. The variable social status which measures the position in society decreases by 0.24 SDs if an individual becomes unem-

[13] Figure 1.D.1 in Appendix 1.D presents the distributions of changes in outcome variables between two consecutive waves.

ployed. The results imply negative and significant effects of 0.20 SDs on the self-efficacy index which measures an individual's ability to cope with demanding situations.

To sum it up, the largest negative short- and medium-term effects of job loss can be found with respect to economic resources and life satisfaction. The individual's perception of social integration and mental health status are affected by the same magnitude while the impact on social status and self-efficacy are slightly less strong. Furthermore, I find no effect of becoming unemployed on social participation. The long-term consequences of job loss might be more severe and will be investigated in more detail in Section 1.6.2.

1.6.2 Heterogeneous Treatment Effects

Heterogeneous effects by sociodemographic characteristics. In this subsection I will show estimation results for different subgroups defined by sociodemographic characteristics. I conduct the analysis separately for men and women, low-/medium-skilled and high-skilled workers, individuals who have a partner and those who do not. For each of these subgroups I redo the two-step estimation procedure as described in Section 1.4. In this way, I ensure that observable characteristics are balanced between treated and control individuals for each subgroup. The results are shown in Table 1.3.

It is well known that women react differently to labor market events and shocks compared to men (see e.g. Bergemann and van den Berg, 2008). Moreover, men and women differ with respect to preferences, for instance concerning risk or leisure (Croson and Gneezy, 2009). However, the results in Table 1.3 point to no substantial effect heterogeneity by gender despite for social engagement. I do find that men reduce their social activities significantly, although this effect is comparatively small.

The results in Table 1.3 suggest that low- and medium-skilled individuals feel the effects of unemployment more strongly.[14] The negative effects of job loss are stronger in every dimension except for economic resources, for which the results are comparable. In particular, low- and medium-skilled workers are significantly more dissatisfied with their life (difference of 0.38) than high-skilled. This finding is in line with results from other studies that show that high-skilled workers face a lower risk of becoming long-term unemployed due to higher job search intensity and reemployment success compared to unemployed individuals with lower levels of education (see e.g. Farber, 2005 and Riddell and Song, 2011). Furthermore, being highly educated might help in coping with shocks like job loss (Bonanno, 2004) which is reflected in the fact that I find no impact

[14] Low-skilled individuals are defined as having no professional qualification, medium-skilled as having a vocational training and high-skilled as having an advanced vocational training or an academic degree.

of becoming unemployed on self-efficacy for this group of workers.

With respect to family status, the estimates indicate that individuals who have a partner experience less harsh effects from unemployment than single people do. This is especially true for life satisfaction and financial restrictions. For instance, the effect on the deprivation index is significantly lower than for individuals without a partner. A potential second income source seems to compensate at least partly for the financial loss due to unemployment. Moreover, a supportive partner can compensate for some of the latent negative effects of unemployment like loss of time structure, social contacts and activity.

Table 1.3: Heterogeneous effects by sociodemographic characteristics

Specification	(1) Men	(2) Women	(3) Low-/medium-skilled	(4) High-skilled	(5) Partner	(6) No partner
Change in outcomes			Effect of job loss			
Social integration	-0.320***	-0.338***	-0.352***	-0.114	-0.360***	-0.246***
	(0.073)	(0.076)	(0.059)	(0.151)	(0.066)	(0.094)
Well-being and mental health						
Life satisfaction	-0.472***	-0.594***	**-0.589*****	**-0.205**	-0.415***	-0.663***
	(0.078)	(0.083)	(0.066)	(0.125)	(0.070)	(0.149)
Mental health status	-0.310***	-0.310***	-0.333***	-0.128	-0.353***	-0.247***
	(0.065)	(0.068)	(0.051)	(0.110)	(0.057)	(0.073)
Economic resources						
Deprivation index	0.677***	0.528***	0.613***	0.522***	**0.426*****	**0.709*****
	(0.102)	(0.100)	(0.076)	(0.184)	(0.093)	(0.123)
Satisfaction with standard of living	-0.509***	-0.541***	-0.532***	-0.433***	**-0.412*****	**-0.660*****
	(0.075)	(0.079)	(0.061)	(0.150)	(0.065)	(0.133)
Psychosocial needs						
Social participation						
Number of close friends	-0.020	-0.090	-0.044	-0.121	-0.025	-0.086
	(0.058)	(0.059)	(0.047)	(0.105)	(0.055)	(0.071)
Social engagement	**-0.099****	**0.040**	-0.069*	0.043	-0.049	-0.103
	(0.049)	(0.054)	(0.040)	(0.106)	(0.049)	(0.084)
Social status	-0.226***	-0.247***	-0.276***	0.008	-0.202***	-0.212**
	(0.068)	(0.067)	(0.052)	(0.131)	(0.059)	(0.092)
Self-efficacy	-0.202**	-0.183**	**-0.236*****	**0.080**	-0.177**	-0.203*
	(0.103)	(0.087)	(0.067)	(0.161)	(0.078)	(0.112)

Notes: Estimates from IPW-DID are based on 377 treated and 8,053 control persons in specification (1), on 258 and 8,994 in (2), on 525 and 12,401 in (3), on 110 and 4,646 in (4), on 350 and 12,146 in (5) and on 285 and 4,901 in (6). The propensity of job loss is based on a logit model with the control variables reported in Tables 1.C.1 and 1.C.2 in Appendix 1.C. Robust standard errors are in parentheses. They are calculated by taking into account that propensity scores are estimated. Coefficients are statistically significant at the *10%, ** 5% and *** 1% level. Differences in the effects of job loss between subgroups that are significantly different from zero at the 10% level are indicated by bold numbers. Standard errors of the differences are obtained by bootstrapping (2,500 replications).
Source: PASS-ADIAB 7515, own computations.

Heterogeneous effects by amount of time since and type of job loss. In this subsection I start by empirically testing the hypothesis that the negative consequences of job loss become more severe the longer the duration of unemployment. To do so, I distinguish individuals who have been unemployed for at least six months and less than six months at the interview date after job loss. Furthermore, I consider the change in outcome levels two waves after becoming unemployed in case the individual has still not found a job at this interview date. The results are reported in column (2), (3) and (4) of Table 1.4 and suggest that the negative consequences of job loss become more severe the longer the duration of unemployment, which is in line with recent findings in the literature on subjective well-being (see e.g. Clark et al., 2008). The coefficients in column (2) indicate that this is particularity true with respect to life satisfaction (decrease by 0.20) and economic resources (the deprivation index increases by 0.27 and satisfaction with standard of living decreases by 0.26). The coefficients point in the same direction by looking at the unemployment duration at the first interview date after job loss.

Individuals who experience periods of unemployment between two waves but are employed again in the second of two consecutive waves, are not included in my analysis so far as their outcome levels are measured during periods of employment. However, it would be interesting to study whether the negative effects of job loss are only temporary and vanish as soon as the individual finds a job again. Column (5) in Table 1.4 shows the estimates for treated individuals who are reemployed in t_1. I find that individuals whose employment relationship is interrupted by a period of unemployment have still lower levels in most dimensions. These results suggest that unemployment has long-lasting negative effects even for the currently employed. This finding receives support by Clark et al. (2001) who find that employees with past unemployment experience have lower life satisfaction.

Finally, I show results separately for individuals who are laid off and those who lose their job due to other reasons, e.g. whose contract expired or who quit their job voluntarily (specification (6) and (7) in Table 1.4). In my sample 71% of all workers become unemployed due to dismissal by the employer. I find a stronger effect of being laid off on social status. The other coefficients do not differ much. I also studied the effects of unemployment dependent on previous job characteristics (results are reported in Table 1.D.7 in Appendix 1.D). Interestingly, I find stronger effects of job loss on mental health and self-efficacy for individuals who previously worked in small firms (firms with less than 50 employees). This could be a hint that redundancies in small firms are less anonymous and more often considered as individual failure.

Table 1.4: Heterogeneous effects by time since and type of job loss

Specification	(1) Baseline	(2) Unemployed in t_2	(3) $\geq$ 6 months unemployed	(4) $<$ 6 months unemployed	(5) Reemployed in t_1	(6) Laid off	(7) Other job loss
Change in outcomes				Effect of job loss			
Social integration	-0.333***	**-0.490*****	-0.346***	-0.326***	**-0.060***	-0.361***	-0.296***
	(0.054)	(0.103)	(0.076)	(0.070)	(0.032)	(0.077)	(0.085)
Well-being and mental health							
Life satisfaction	-0.549***	**-0.747*****	-0.568***	-0.535***	**-0.091*****	-0.543***	-0.560***
	(0.060)	(0.106)	(0.085)	(0.070)	(0.034)	(0.074)	(0.098)
Mental health status	-0.309***	-0.417***	-0.258***	-0.327***	**-0.057***	-0.334***	-0.254***
	(0.046)	(0.092)	(0.068)	(0.058)	(0.030)	(0.060)	(0.078)
Economic resources							
Deprivation index	0.608***	**0.879*****	**0.784*****	**0.513*****	**0.134*****	0.575***	0.641***
	(0.071)	(0.146)	(0.110)	(0.088)	(0.042)	(0.088)	(0.132)
Satisfaction with standard of living	-0.529***	**-0.787*****	-0.602***	-0.465***	**-0.195*****	-0.544***	-0.482***
	(0.057)	(0.105)	(0.080)	(0.068)	(0.035)	(0.070)	(0.090)
Psychosocial needs							
Social participation							
Number of close friends	-0.049	-0.088	-0.130**	-0.001	-0.012	-0.053	-0.011
	(0.043)	(0.074)	(0.061)	(0.054)	(0.029)	(0.061)	(0.069)
Social engagement	-0.048	0.083	-0.029	-0.066	-0.015	-0.054	0.010
	(0.038)	(0.062)	(0.053)	(0.046)	(0.027)	(0.043)	(0.075)
Social status	-0.244***	**-0.418*****	-0.220***	-0.259***	**-0.070****	**-0.362*****	**-0.109**
	(0.048)	(0.097)	(0.071)	(0.059)	(0.031)	(0.063)	(0.078)
Self-efficacy	-0.202***	-0.277**	-0.070	-0.293***	**-0.049**	-0.228***	-0.101
	(0.062)	(0.129)	(0.092)	(0.075)	(0.039)	(0.080)	(0.090)

Notes: Estimates from IPW-DID are based on 635 treated and 17,047 control persons in specification (1), (3) - (7), on 187 and 12,885 in (2), on 271 treated in (3), on 363 in (4), on 1,290 in (5), on 415 in (6) and on 172 in (7) (for 48 treated this information is missing). The propensity of job loss is based on a logit model with the control variables reported in Tables 1.C.1 and 1.C.2 in Appendix 1.C. Robust standard errors are in parentheses. They are calculated by taking into account that propensity scores are estimated. Coefficients are statistically significant at the *10%, ** 5% and *** 1% level. Differences in the effects of job loss between subgroups that are significantly different from zero at the 10% level are indicated by bold numbers. Standard errors of the differences are obtained by bootstrapping (2,500 replications).
Source: PASS-ADIAB 7515, own computations.

1.6.3 Sensitivity Analysis

In this subsection I conduct some sensitivity checks to examine the robustness of my findings. The results are reported in Table 1.5.

In a first step, I check the robustness of my results with respect to the model specification. The review article of Imbens and Wooldridge (2009) discusses in great detail the properties of different estimators which are standard in the treatment effects literature. In comparison to simple matching estimators which impute the missing potential outcomes of the treated individuals with outcome levels of nearest neighbors of the comparison group, IPW avoids the requirement of choosing any tuning parameter. Hence, finding an optimal value for the number of nearest neighbors for nearest-neighbor matching, a caliper for radius caliper matching or a bandwidth for kernel matching is not needed. Imbens and Wooldridge (2009) point out that with IPW estimators concerns arise when the covariate distributions of the two treatment groups are substantially different, implying that the propensity score is approaching zero or one. One concern is that in this case the parametric model choice of the propensity score, such as probit vs logit models, becomes more important. To address this issue, specification (2) of Table 1.5 shows the estimation results by applying a probit instead of a logit estimation of the probability of job loss. Moreover, I compare the results of the baseline specification to results obtained by alternative estimators (specification (3) and (4) of Table 1.5): IPW with regression adjustment (see e.g. Wooldridge, 2007) and one-to-five nearest-neighbor matching (see e.g. Abadie and Imbens, 2006). Overall, the estimates are not sensitive to the choice of the model specification.

Table 1.5: Robustness checks: results for the consequences of job loss

Specification	(1) Baseline	(2) Probit	(3) IPW-RA	(4) 5 NN	(5) One person
Change in outcomes			Effect of job loss		
Social integration	-0.333***	-0.332***	-0.335***	-0.324***	-0.348***
	(0.054)	(0.053)	(0.050)	(0.051)	(0.064)
Well-being and mental health					
Life satisfaction	-0.549***	-0.530***	-0.539***	-0.518***	-0.597***
	(0.060)	(0.055)	(0.054)	(0.052)	(0.075)
Mental health status	-0.309***	-0.315***	-0.293***	-0.292***	-0.278***
	(0.046)	(0.045)	(0.045)	(0.045)	(0.059)
Economic resources					
Deprivation index	0.608***	0.617***	0.637***	0.619***	0.538***
	(0.071)	(0.070)	(0.070)	(0.068)	(0.079)
Satisfaction with	-0.529***	-0.510***	-0.495***	-0.510***	-0.543***
standard of living	(0.057)	(0.053)	(0.052)	(0.046)	(0.070)
Psychosocial needs					
Social participation					
Number of close friends	-0.049	-0.049	-0.065	-0.019	-0.061
	(0.043)	(0.042)	(0.040)	(0.041)	(0.056)
Social engagement	-0.048	-0.045	-0.051	-0.026	-0.078
	(0.038)	(0.036)	(0.036)	(0.034)	(0.049)
Social status	-0.244***	-0.234***	-0.254***	-0.240***	-0.272***
	(0.048)	(0.047)	(0.046)	(0.043)	(0.061)
Self-efficacy	-0.202***	-0.193***	-0.189***	-0.156***	-0.221***
	(0.062)	(0.060)	(0.060)	(0.056)	(0.074)

Notes: IPW-RA: Inverse propensity score weighting with regression adjustment, 5 NN: one-to-five nearest-neighbor matching. Estimates are based on 635 treated and 17,047 control persons in specification (1) - (4) and on 412 treated and 5,499 control persons in specification (5). The propensity of job loss is based on a logit model in specification (1), (3) - (5). The variables used in the propensity score estimation are reported in Tables 1.C.1 and 1.C.2 in Appendix 1.C. Robust standard errors are in parentheses. They are calculated by taking into account that propensity scores are estimated. Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: PASS-ADIAB 7515, own computations.

In a last step, I only include the first observation of each individual in the estimation sample (specification (5) of Table 1.5). The number of individuals that become unemployed decreases to 412 and the number of individuals that remain employed between two consecutive waves to 5,499. The estimated coefficients are comparable to the baseline specification.

1.6.4 Placebo Tests

Finally, I test the reliability of my results by conducting placebo tests. In particular, I estimate the effect of job loss on the change in outcomes between wave t_{-1} and t_0. If the outcomes are affected in periods before the job loss occurs that would suggest that either treatment and control group are still systematically different or anticipation effects play a role. The results of the placebo test shown in Table 1.D.8 in Appendix 1.D do not indicate any significant effects. In addition, Figure 1.D.2 in Appendix 1.D presents the mean of the outcome variables in levels in the consecutive waves t_{-3}, t_{-2}, t_{-1}, t_0 and t_1 separately for treated and control individuals before and after inverse propensity score weighting. While there are highly significant differences between treatment and control group before weighting in the time period before job loss, these differences vanish after weighting.[15] All in all the placebo tests indicate that the treatment and control groups are similar with respect to changes in outcomes in earlier periods.

1.7 Discussion and Conclusions

In this chapter I empirically assess the economic and social consequences of job loss. While the number of economic studies on the relationship between unemployment and measures of social integration are quite rare, studies in the field of psychology and sociology point to social exclusion as a result of unemployment (see e.g. Böhnke, 2004; Layte et al., 2010 and Gundert and Hohendanner, 2014). These studies typically rely on survey data only, cannot rule out bias due to unobservables or reversed causality and do not examine the multidimensionality of the consequences of job loss in great detail.

By applying inverse probability weighting combined with differences-in-differences, I study the causal impact of unemployment on different dimensions of the process of social exclusion. I find the strongest negative effects in terms of size on life satisfaction and economic resources, slightly weaker negative effects on perceived social integration,

[15] The number of observations decreases considerably the further I go back in time. The difference in outcome levels between treatment and control group after IPW is insignificant for each outcome variable in the time periods before job loss, except for mental health status in t_{-2} and deprivation index in t_{-1} (significant at 10% level) and number of close friends in t_{-2} (significant at 5% level). However, the reduced number of observations and the three mentioned differences in outcome levels do not lead to diverging trends across both groups.

mental health, social status and self-efficacy and no effect on social participation. Moreover, I find some evidence for effect heterogeneity. The results suggest that high-skilled workers and individuals with a partner experience the effects of unemployment less strongly. In addition, I find larger negative effects of job loss in the long-run. Individuals who are unemployed for more than one year do feel more socially excluded, are unhappier and more financially constrained. The negative effects of job loss are still present even if the individual becomes employed again.

This study shows that the loss of regular work influences social exclusion in various ways. From an economic point of view, social isolation carries a high risk of individuals ending up in a state from which they will never return to work. Discouragement effects, stigmatization, the decay of human capital and living in deprived neighborhoods can lead to both less job search effort and lower chances of being hired, and hence to longer durations of unemployment (see e.g. Pissarides, 1992; Atkinson and Kintrea, 2001 and Biewen and Steffes, 2010).

These considerations could provide new insights into the effectiveness of active labor market policy programs with respect to reducing this risk. While the effects of government sponsored programs on reemployment probabilities are rather mixed (see e.g. Bergemann and van den Berg, 2008 and Card et al., 2017 for an overview), temporary employment, for instance in the form of job creation schemes or wage subsidies might foster the reintegration of the unemployed into society. For instance, the studies of Wulfgramm (2011) and Gundert and Hohendanner (2015) on the effects of the German 'One-Euro-Job' workfare program on social integration and life satisfaction, respectively, emphasize that the unemployed benefit from participation the more the activities resemble regular jobs. Hence, from a policy perspective, it is important to design active labor market policy programs that credibly simulate regular employment in terms of duration, working hours, social and financial benefits. Programs that positively influence the employability of participants as well as boost self-esteem might prevent individuals from feeling rejected by society and thus avoid the onset of a downward spiral ending in long-term unemployment.

1.Appendix

1.A PASS Data Addendum

Table 1.A.1: Number of interviews

Sample	Number of interviews	Refreshment sample
1^{st} wave (2006/07)	18,954 individuals living in 12,794 households	
2^{nd} wave (2007/08)	12,487 individuals living in 8,429 households	1,041 households
3^{rd} wave (2008/09)	13,439 individuals living in 9,535 households	1,186 households
4^{th} wave (2010)	11,768 individuals living in 7,848 households	748 households
5^{th} wave (2011)	15,607 individuals living in 10,235 households	753 households
6^{th} wave (2012)	14,619 individuals living in 9,513 households	961 households
7^{th} wave (2013)	14,449 individuals living in 9,509 households	949 households
8^{th} wave (2014)	13,460 individuals living in 8,998 households	795 households
9^{th} wave (2015)	13,271 individuals living in 8,921 households	900 households

Notes: The panel household sample in wave 5 was supplemented for both recipients of Unemployment Benefit II and the general population sample from new postcode regions in wave 4.
Source: Bethmann et al. (2016).

Table 1.A.2: Agreement on linkage of survey data to administrative data

Sample	Number of interviews with question on linkage	Number of interviews with agreement on linkage	in %
1^{st} wave (2006/07)	17,249	13,766	79.8
2^{nd} wave (2007/08)	3,358	2,560	76.2
3^{rd} wave (2008/09)	2,656	2,128	80.1
4^{th} wave (2010)	2,032	1,774	87.3
5^{th} wave (2011)	5,145	4,414	85.8
6^{th} wave (2012)	2,482	2,002	80.7
7^{th} wave (2013)	1,973	1,613	81.8
8^{th} wave (2014)	1,653	1,327	80,3
9^{th} wave (2015)	1,727	1,471	85.2

Source: Bethmann et al. (2016).

1.B Construction of the Outcome Variables

1.B.1 Social Integration

PASS question on social integration
Some people may feel like they are integrated into normal social life and that they are a proper part of society while others may feel excluded. What about in your case? To what extent do you feel that you are part of society or to what extent do you feel excluded? Please use the numbers from 1 to 10 to rate your opinion. 1 means that you feel excluded from social life. 10 means, that you feel part of it. The numbers from 2 to 9 allow you to grade your assessment.

1.B.2 Well-being and Mental Health

1. *PASS question on life satisfaction*
   How satisfied are you currently with your life as a whole? 0 means that you are "completely dissatisfied", 10 means that you are "completely satisfied". The numbers 1 to 9 allow you to grade your assessment.

2. *PASS question on mental health status*
   How strongly have you been affected by mental health problems, like fear, dejection or irritability in the past four weeks? Please tell me, whether you have been affected "not at all", "a little bit", "moderately", "quite a bit" or "extremely"?

3. *Construction of variable "mental health status"*
   The variable measures the mental health status ranging from 1 "extreme problems" to 5 "no problems".

1.B.3 Economic Resources

1. *PASS question on deprivation*
   If you think of your household, which of the following items do you have? For the items you don't have, is this for financial reasons or for other reasons?

   (a) Do you have an apartment with at least as many rooms as persons living there?
   (b) Do you have an apartment without damp walls or floors?
   (c) Do you have a separate bathroom with a bathtub or shower in your apartment?
   (d) Do you have a toilet inside your apartment?

(e) Do you have central heating, self-contained central heating or district heating? *(not asked after wave 5)*

(f) Do you have a garden, a balcony or a terrace?

(g) Do you have sufficient winter clothing for each member of the household?

(h) Do you have a car?

(i) Do you have a television?

(j) Do you have a video recorder or DVD player?

(k) Do you have a computer with internet access?

(l) Do you have a washing machine?

(m) Do you have an upright freezer, a chest freezer or a refrigerator with a freezer section? *(not asked after wave 5)*

And which of the following things do you or does your household do? For those activities you don't do, is this for financial reasons or for other reasons?

(n) Buy new clothing once in a while for each family member, even if the old clothes are not yet worn out?

(o) Have you a hot meal at least once a day?

(p) Go on a holiday away from home for at least one week a year for each member of the family (this need not be taken jointly)?

(q) Invite friends over for dinner at your home at least once a month?

(r) Eat out at a restaurant with the family at least once a month?

(s) Can each member of the family go to the cinema, the theater or a concert at least once a month?

(t) Save a fixed amount of money at least once a month?

(u) Replace worn but still usable furniture with new furniture?

(v) Pay for unexpected expenses with one's own money, e.g. to replace a broken washing machine?

(w) Receive medical treatment which is not fully covered by your health insurance, such as dentures or glasses if you/one of your family members need them?

(x) Buy over-the-counter drugs such as pain relievers or medication for a cold, if you/someone in the family needs them even if your health insurance does not cover the costs?

(y) Always pay the rent for the apartment and/or the interest on the house or apartment one lives in on time?

(z) Always pay the gas, heating and electricity bill on time? *(not asked after wave 5)*

2. *Construction of deprivation index*

   The deprivation index used in this study is included in PASS and ranges between 0 and 11.08 (see Bethmann et al. (2016) for a detailed description of the construction of the variable). This index is based on how many items are missing and how many activities are not done for financial reasons. Items that are answered with "don't know" or "details refused" are not considered. The index is a weighted index which weights the items according to the share of respondents who considered a particular item as necessary. This procedure is commonly used for the construction of poverty measures (applied for instance by Halleröd, 1995).

3. *PASS question on satisfaction with standard of living*
   How satisfied are you today with your overall standard of living? For your assessment you can use the numbers from 0 to 10. 0 means that you are "completely dissatisfied", 10 means you are "completely satisfied". The numbers 1 to 9 allow you to grade your assessment.

1.B.4 Social Participation

1. *PASS question on number of close friends*
   How many close friends, or family members with whom you have a close relationship, do you have outside your household?

2. *PASS question on social engagement*
   Are you actively engaged in one of the following organizations or associations? (Multiple responses possible.)

   (a) Union
   (b) Political party
   (c) Church community
   (d) Clubs such as music, sport or culture clubs
   (e) Another organization not mentioned here
   (f) No, not actively engaged

3. *Construction of variable "social engagement"*
   This variable indicates the engagement in organizations/associations out of the

five options (a) to (f). This measure ranges from 0 "not actively engaged" to 5 "engaged in all 5 organizations/associations".

1.B.5 Social Status

PASS question on social status
There are groups in our society which tend to be at the top of the social ladder and other groups that tend to be at the bottom. How would you rank yourself using the numbers 1 to 10? 1 means that you are at the very bottom, 10 means that you are positioned at the very top. The numbers from 2 to 9 allow you to grade your assessment.

1.B.6 Self-Efficacy

1. *PASS question on self-efficacy*
   If unexpected difficulties or problems occur, you can deal with them in a number of different ways. Here we have compiled a couple of opinions regarding this topic. Please tell me whether they apply to you "completely", "tend to apply" or "tend not to apply" or "do not apply at all".

   (a) For every problem I have a solution.
   (b) Even if surprising events occur, I believe I can handle them well.
   (c) I have no difficulties in realizing my goals.
   (d) In unexpected situations I always know how to act.
   (e) I always succeed in resolving difficult problems if I make an effort.

2. *Construction of self-efficacy index*
   I take the sum of the four possible outcomes of the five items for each individual and divide by the number of items. If an individual responded only to some of the items, the index is based on the items that are answered. The resulting index ranges from 1 "low self-efficacy" to 4 "high self-efficacy".

1.C Description of Variables

Table 1.C.1: Description of variables based on PASS

Variable	Description
Outcomes measured in waves t_0 and t_1	
Social integration	Categorical variable measuring perceived social affiliation ranging from 1 (feeling excluded) to 10 (feeling affiliated)
Life satisfaction	Categorical variable measuring life satisfaction ranging from 0 (completely dissatisfied) to 10 (completely satisfied)
Mental health status	Categorical variable for assessment of mental health status over the last 4 weeks ranging from from 1 (extreme problems) to 5 (no problems)
Economic resources	Deprivation index based on 26 items (for construction of variable see Appendix 1.B Section 1.B.3) & Categorical variable measuring satisfaction with standard of living ranging from 0 (completely dissatisfied) to 10 (completely satisfied)
Social participation	Number of close friends & Categorical variable measuring social engagement ranging from 0 (not actively engaged) to 5 (engaged in all 5 organizations/associations) (for construction of variable see Appendix 1.B Section 1.B.4)
Social status	Categorical variable measuring assessment of position in society ranging from 1 (belonging to bottom) to 10 (belonging to the top)
Self-efficacy	Index ranging from 1 (low self-efficacy) to 4 (high self-efficacy) (for construction of variable see Appendix 1.B Section 1.B.6)
Job loss measured in wave t_1	Dummy for becoming unemployed between two consecutive waves t_0 and t_1
Control variables measured in wave t_0	
Sociodemographics	
Female	Dummy for being female
Age	Dummies for age groups: 25 - 34 years, 35 - 44 years, 45 - 54 years, > 54 years, reference category is < 25 years
Migrant	Dummy for being an immigrant
Married	Dummy for being married
Religious community	Dummy for belonging to a religious community
Smoker	Dummy for having ever smoked on a regular basis (in 2% of cases the information is missing and is treated as 0)
Serious health restrictions	Dummy for having serious health restrictions (includes officially recognized disabilities)
Hospital visits in last 12 months	Dummy for hospital visits in the last 12 months (in 1% of cases the information is missing and is treated as 0)
Professional qualification	Dummies for highest professional qualification level: vocational training (*Teilfacharbeiter, Lehre, abgeschlossene Berufsfachschule*), advanced vocational training (*Meister, Techniker*), academic degree (*Universität, Fachhochschule*), reference category is no vocational training
East Germany	Dummy for living in East Germany

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Table 1.C.1: Description of variables based on PASS *(continuation)*

Variable	Description
Subjective indicators	
Attitudes to work	Index ranging from 1 (low work attitude) to 4 (high work attitude) (construction similar to the construction of the self-efficacy index)
Health satisfaction	Categorical variable measuring satisfaction with health from 0 (completely dissatisfied) to 10 (completely satisfied)
Household characteristics	
Household income	Dummies for net household income per month in €: 1000 - 1499, 1500 - 1999, 2000 - 2999, $\geq$ 3000, reference category is $\leq$ 999
Couple with children aged $<$ 16 years	Dummy for couple with children younger than 16 years
Number of own children	Dummies for number of own children (living in and outside the household): 1, 2, $>$ 2, reference category is 0
Homeowner	Dummy for being a homeowner
Partner characteristics	
Partner in PASS	Dummy for identification of partner in PASS
Professional qualification of partner	Dummies for highest professional education level: vocational training, advanced vocational training/academic degree, information is missing, reference category is no vocational training
Employment status of partner	Dummies for being employed and information is missing
Employment status	
Permanent contract	Dummies for permanent contract and information is missing
Wave	Dummies indicating the wave of the interview, ranging from wave 2 to 8, reference category is wave 1

Notes: The variables married, professional qualification and number of own children are supplemented by information from the IEB if missing. The variables migrant, religious community, professional qualification, attitudes to work are treated as time-constant and filled with previous or subsequent information if missing. The variables married and number of own children are filled with previous information if missing. In 1% of cases information on household income is missing and filled with previous or subsequent information if the composition of the household does not change.
Source: PASS-ADIAB 7515, own computations.

Table 1.C.2: Description of variables based on IEB

Variable	Description
Control variables measured in wave t_0	
Previous job characteristics	
Employment with ssc	Dummy for being employed with social security contributions (ssc)
Employment full-time	Dummy for being employed full-time
Job classifications	Dummies for 5 job classifications: 1 Farmer/Production/Craftspeople/Technician, 2 White-collar employee, 3 Salesperson, 4 Clerical workers, 5 Service workers, reference category is 1
Tenure	Dummies for employment duration: categories are spitted according to percentiles of distribution: 25 - 50, 50 - 75, $>$ 75, reference category is 0 - 25
Daily wage	Dummies for daily wage in € (2010 prices): categories are spitted according to percentiles of distribution: 25 - 50, 50 - 75, $>$ 75, reference category is 0 - 25
Previous firm characteristics	
Firm size	Dummies for number of employees: 10 - 49, 50 - 249, 250 - 499, $>$ 500, reference category is < 10
Sector of firm	Dummies for 8 sectors: 1 Agriculture/Production, 2 Consumption/Food, 3 Construction, 4 Trade, 5 Transportation/Services I, 6 Services II, 7 Education/Health, 8 Public, reference category is 1
Employment history	
Number of employment periods with ssc	Number of employment periods with social security contributions
Employment duration with ssc	Dummies for employment duration with social security contributions: categories are spitted according to percentiles of distribution: 25 - 50, 50 - 75, $>$ 75, reference category is 0 - 25
Number of marginal employment periods	Number of marginal employment periods
Marginal employment duration	Dummies for marginal employment duration: categories are spitted according to percentiles of distribution: 25 - 50, 50 - 75, $>$ 75, reference category is 0 - 25
Number of unemployment periods	Number of unemployment periods
Unemployment duration	Dummies for unemployment duration: categories are spitted according to percentiles of distribution: 25 - 50, 50 - 75, $>$ 75, reference category is 0 - 25
Number of non-employment periods	Number of non-employment periods
Non-employment duration	Dummies for non-employment duration: categories are spitted according to percentiles of distribution: 25 - 50, 50 - 75, $>$ 75, reference category is 0 - 25
District unemployment rate	District unemployment rate measured at the date of the interview

Notes: IEB: Integrated Employment Biographies, ssc: social security contributions. Periods of self-employment, civil service, and military service are not included in the IEB. Non-employment is defined as periods without entry in the social security records if the period lasts longer than one month. I allow for gaps of one month between periods of employment at the same firm and between two unemployment spells.
Source: PASS-ADIAB 7515, own computations.

1.D Additional Descriptives and Estimation Results

Table 1.D.1: Number of individuals in treatment and control group

How often in	Treatment group only	Control group only	Both groups?
1 time	3**	1,584	0
2 times	.	1,165	96
3 times	0	665	72
4 times	0	647	51
5 times	0	342	.
6 times	0	351	.
7 times	0	220	.
8 times	0	332	.
Total	351	5,306	254

Notes: There are less than 20 individuals who only appear twice in the treatment group as well as more than four times in both the treatment and the control group. Due to data protection rules of the FDZ, these are indicated as missing values.
Source: PASS-ADIAB 7515, own computations.

Table 1.D.2: Additional descriptive statistics

Job loss	Yes	No	Difference	
Sociodemographics				
Religious community	0.50	0.60	-0.10	***
Smoker	0.71	0.60	0.11	***
Hospital visits in last 12 months	0.11	0.08	0.02	*
Subjective indicators				
Work attitude	3.07	3.12	-0.05	
Health satisfaction	6.85	7.30	-0.45	***
Household characteristics				
Household income < 1000 € per month	0.14	0.03	0.10	***
Household income 1000 - 1499 € per month	0.22	0.11	0.11	***
Household income 1500 - 1999 € per month	0.19	0.14	0.05	***
Household income 2000 - 2999 € per month	0.30	0.31	-0.01	
Household income > 2999 € per month	0.16	0.41	-0.25	***
Couple with children aged < 16 years	0.24	0.31	-0.07	***
Female * Couple with children aged < 16 years	0.10	0.14	-0.04	***
Partner characteristics				
Partner in PASS	0.55	0.71	-0.16	***
PQ: no vocational training	0.10	0.07	0.03	***
PQ: vocational training	0.32	0.40	-0.07	***
PQ: advanced vocational training/academic degree	0.07	0.18	-0.11	***

...

Table 1.D.2: Additional descriptive statistics *(continuation)*

Job loss	Yes	No	Difference	
PQ: missing	0.51	0.36	0.16	***
ES: employed	0.31	0.48	-0.17	***
ES: missing	0.56	0.41	0.15	***
Previous job characteristics				
Employment with ssc	0.94	0.92	0.02	***
Employment full-time	0.71	0.65	0.06	***
Job class: Farmer/Production/Craftspeople/Technician	0.32	0.23	0.09	***
Job class: White-collar employee	0.13	0.20	-0.07	***
Job class: Salesperson	0.07	0.07	0.00	
Job class: Clerical workers	0.16	0.22	-0.06	***
Job class: Service workers	0.33	0.28	0.05	
Previous firm characteristics				
Firm size: < 10 employees	0.23	0.15	0.07	***
Firm size: 10 - 49 employees	0.28	0.26	0.02	
Firm size: 50 - 249 employees	0.30	0.29	0.01	
Firm size: 250 - 499 employees	0.09	0.10	0.00	
Firm size: > 499 employees	0.10	0.20	-0.10	***
Wave				
Wave 1	0.10	0.11	-0.01	
Wave 2	0.12	0.11	0.02	
Wave 3	0.15	0.10	0.05	**
Wave 4	0.09	0.11	-0.02	**
Wave 5	0.13	0.14	-0.02	
Wave 6	0.15	0.14	0.00	
Wave 7	0.14	0.14	-0.01	
Wave 8	0.12	0.13	-0.01	
Number of observations	635	17,047		

Notes: PQ: Professional qualification. ssc: social security contributions. Differences are statistically significant at the *10%, ** 5% and *** 1% level.
Source: PASS-ADIAB 7515, own computations.

Table 1.D.3: Logit estimation of probability of job loss

Variable	Coefficient	Standard error
Initial outcome levels		
Social integration	0.003	(0.027)
Life satisfaction: value 6 or 7	-0.352***	(0.133)
Life satisfaction: value 8	-0.425***	(0.156)
Life satisfaction: value 9	-0.729***	(0.217)
Life satisfaction: value 10	-0.781***	(0.272)
Mental health status	-0.064	(0.042)
Deprivation index: 0 - 0.5	0.004	(0.122)
Deprivation index: 0.5 - 1	-0.192	(0.144)
Deprivation index: 1 - 1.5	0.025	(0.169)
Deprivation index: 1.5 - 2.5	-0.116	(0.184)
Deprivation index: > 2.5	0.285	(0.213)
Satisfaction with standard of living	0.070**	(0.035)
Number of close friends	-0.091**	(0.041)
Social engagement	-0.003	(0.070)
Social status	0.051	(0.036)
Self-efficacy: 2.8 - 3.4	0.008	(0.114)
Self-efficacy: > 3.4	0.186	(0.131)
Sociodemographics		
Female	-0.444***	(0.124)
Age group: 25 - 34 years	-0.339*	(0.198)
Age group: 35 - 44 years	-0.523**	(0.235)
Age group: 45 - 54 years	-0.613**	(0.270)
Age group: > 54 years	-0.102	(0.297)
Migrant	0.171	(0.192)
Married	-0.277*	(0.146)
Religious community	-0.187*	(0.103)
Smoker	0.062	(0.101)
Serious health restrictions	0.015	(0.117)
Hospital visits in last 12 months	0.181	(0.154)
PQ: vocational training	-0.127	(0.128)
PQ: advanced vocational training	-0.487**	(0.237)
PQ: academic degree	0.204	(0.193)
East Germany	-0.591***	(0.145)
Subjective indicators		
Work attitude	-0.148**	(0.073)
Health satisfaction	-0.057**	(0.027)
Household characteristics		
Household income 1000 - 1499 € per month	-0.542***	(0.167)
Household income 1500 - 1999 € per month	-0.501***	(0.177)
Household income 2000 - 2999 € per month	-0.689***	(0.179)
Household income > 2999 € per month	-0.775***	(0.216)

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Table 1.D.3: Logit estimation of probability of job loss *(continuation)*

Variable	Coefficient	Standard error
Couple with children aged < 16 years	-0.390**	(0.176)
Female * Couple with children aged < 16 years	0.512**	(0.222)
Number of own children: 1	0.093	(0.141)
Number of own children: 2	0.210	(0.155)
Number of own children: > 2	0.273	(0.179)
Home owner	-0.158	(0.122)
Partner characteristics		
Partner in PASS	0.190	(0.218)
PQ: vocational training	0.113	(0.170)
PQ: advanced vocational training/academic degree	-0.337	(0.233)
PQ: missing	-0.013	(0.303)
ES: employed	-0.234	(0.158)
ES: missing	-0.217	(0.248)
Previous job characteristics		
Employment with ssc	0.822***	(0.244)
Employment full-time	0.352***	(0.121)
Permanent contract	-0.696***	(0.106)
Permanent contract missing	0.788***	(0.251)
Job class: White-collar employee	-0.063	(0.179)
Job class: Salesperson	-0.123	(0.227)
Job class: Clerical workers	-0.110	(0.151)
Job class: Service workers	-0.107	(0.130)
Tenure: 25 - 50 %ile	-0.768***	(0.107)
Tenure: 50 - 75 %ile	-1.417***	(0.178)
Tenure: > 75 %ile	-1.249***	(0.246)
Daily wage: 25 - 50 %ile	-0.335***	(0.121)
Daily wage: 50 - 75 %ile	-0.827***	(0.159)
Daily wage: > 75 %ile	-1.178***	(0.254)
Previous firm characteristics		
Firm size: 10 - 49 employees	-0.301**	(0.131)
Firm size: 50 - 249 employees	-0.325**	(0.134)
Firm size: 250 - 499 employees	-0.166	(0.181)
Firm size: > 499 employees	-0.313*	(0.180)
Sector: Consumption/Food	0.056	(0.230)
Sector: Construction	0.612***	(0.216)
Sector: Trade	0.067	(0.215)
Sector: Transportation/Services I	0.295*	(0.170)
Sector: Services II	0.426**	(0.194)
Sector: Education/Health	-0.145	(0.205)
Sector: Public	-0.179	(0.219)
Employment history		
Number of employment periods with ssc	0.041***	(0.014)
Employment duration with ssc: 25 - 50 %ile	-0.270**	(0.133)

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Table 1.D.3: Logit estimation of probability of job loss *(continuation)*

Variable	Coefficient	Standard error
Employment duration with ssc: 50 - 75 %ile	-0.425**	(0.178)
Employment duration with ssc: > 75 %ile	-0.754***	(0.228)
Numer of marginal employment periods	0.057**	(0.029)
Marginal employment duration: 25 - 50 %ile	-0.162	(0.124)
Marginal employment duration: 50 - 75 %ile	-0.684***	(0.168)
Marginal employment duration: > 75 %ile	-0.999***	(0.227)
Number of unemployment periods	0.031	(0.024)
Unemployment duration: 0 - 25 %ile	-0.159	(0.261)
Unemployment duration: 25 - 50 %ile	0.497**	(0.229)
Unemployment duration: 50 - 75 %ile	0.724***	(0.235)
Unemployment duration: > 75 %ile	0.920***	(0.255)
Number of non-employment periods	0.032	(0.023)
Non-employment duration	-0.001	(0.001)
District unemployment rate	0.030*	(0.016)
Wave		
Wave 2	0.202	(0.199)
Wave 3	0.322	(0.197)
Wave 4	-0.326	(0.217)
Wave 5	-0.302	(0.214)
Wave 6	-0.154	(0.214)
Wave 7	-0.227	(0.214)
Wave 8	-0.406*	(0.218)
Constant	-0.759	(0.634)
Number of observations	17,682	
Pseudo-R^2	0.256	

Notes: PQ: Professional qualification. ES: Employment status. ssc: social security contributions. Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: PASS-ADIAB 7515, own computations.

Table 1.D.4: Covariate balance summary after IPW

	Standardized differences	Variance ratio
Initial outcome levels		
Social integration	-0.009	0.944
Life satisfaction	0.023	0.920
Mental health status	0.021	0.955
Deprivation index	0.011	1.010
Satisfaction with standard of living	0.036	0.909
Number of close friends	-0.020	0.997
Social engagement	-0.004	1.021
Social status	-0.022	1.102
Self-efficacy	0.024	1.051
Sociodemographics		
Female	0.011	1.004
Age	0.014	1.002
Migrant	-0.023	0.929
Married	0.007	1.003
Religious community	-0.009	1.000
Smoker	0.001	0.999
Serious health restrictions	-0.008	0.991
Hospital visits in last 12 months	-0.024	0.942
PQ: no vocational training	0.015	1.027
PQ: vocational training	-0.012	1.008
PQ: advanced vocational training	-0.004	0.982
PQ: academic degree	0.004	1.008
East Germany	0.014	1.012
Subjective indicators		
Work attitude	-0.025	1.009
Health satisfaction	0.004	1.033
Big Five		
Extraversion	0.025	0.962
Extraversion missing	0.045	1.054
Tolerance	-0.010	1.066
Tolerance missing	0.039	1.047
Conscientiousness	-0.062	1.017
Conscientiousness missing	0.053	1.064
Neuroticism	-0.082	0.942
Neuroticism missing	0.040	1.048
Openness	0.003	1.062
Openness missing	0.045	1.053
Household characteristics		
Household income < 1000 € per month	-0.010	0.980
Household income 1000 - 1499 € per month	-0.006	0.992

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Table 1.D.4: Covariate balance summary after IPW *(continuation)*

	Standardized differences	Variance ratio
Household income 1500 - 1999 € per month	-0.010	0.984
Household income 2000 - 2999 € per month	0.021	1.020
Household income > 2999 € per month	0.001	1.001
Couple with children aged < 16 years	0.000	1.000
Female * Couple with children aged < 16 years	-0.003	0.991
Number of own children	-0.003	0.939
Home owner	0.005	1.007
Partner characteristics		
Partner in PASS	0.012	0.998
PQ: no vocational training	0.014	1.041
PQ: vocational training	0.013	1.010
PQ: advanced vocational training/academic degree	0.000	1.001
PQ: missing	-0.021	1.002
ES: employed	0.020	1.017
ES: missing	-0.022	1.006
Previous job characteristics		
Employment with ssc	0.009	0.968
Employment full-time	-0.017	1.015
Permanent contract	0.007	0.998
Permanent contract missing	-0.003	0.989
Job class: Farmer/Production/Craftspeople/Technician	0.025	1.021
Job class: White-collar employee	-0.010	0.979
Job class: Salesperson	-0.018	0.945
Job class: Clerical workers	0.005	1.009
Job class: Service workers	-0.011	0.992
Tenure	-0.026	1.030
Daily wage	-0.068	0.985
Previous firm characteristics		
Firm size: < 10 employees	-0.042	0.949
Firm size: 10 - 49 employees	0.004	1.004
Firm size: 50 - 249 employees	0.016	1.015
Firm size: 250 - 499 employees	0.009	1.025
Firm size: > 499 employees	0.021	1.060
Sector: Agriculture/Production	0.000	1.001
Sector: Consumption/Food	0.000	1.001
Sector: Construction	-0.020	0.946
Sector: Trade	0.000	1.000
Sector: Transportation/Services I	0.013	1.012
Sector: Services II	-0.019	0.964
Sector: Education/Health	0.011	1.025
Sector: Public	0.011	1.036
Employment history		

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Table 1.D.4: Covariate balance summary after IPW *(continuation)*

	Standardized differences	Variance ratio
Number of employment periods with ssc	0.006	1.019
Employment duration with scc	-0.042	1.064
Number of marginal employment periods	0.014	0.998
Marginal employment duration	0.002	1.041
Number of unemployment periods	-0.004	1.106
Unemployment duration	0.056	1.049
Number of non-employment periods	0.013	1.041
Non-employment duration	0.017	1.018
District unemployment rate	0.005	0.993
Wave		
Wave 1	-0.020	0.948
Wave 2	0.007	1.016
Wave 3	0.005	1.010
Wave 4	0.005	1.015
Wave 5	0.014	1.032
Wave 6	-0.002	0.996
Wave 7	-0.010	0.978
Wave 8	0.001	1.001

Notes: IPW: Inverse Probability Weighting. PQ: Professional qualification. ES: Employment status. ssc: social security contributions.
Source: PASS-ADIAB 7515, own computations.

Table 1.D.5: Results for the consequences of job loss based on different sets of covariates

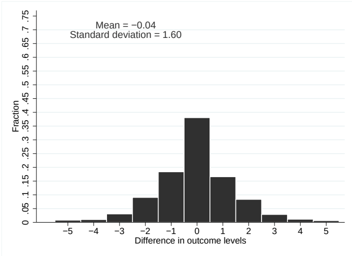
	Baseline	With Big Five	Factor analysis
Change in outcomes	Effect of job loss		
Social integration	-0.333***	-0.330***	-0.354***
	(0.054)	(0.054)	(0.056)
Well-being and mental health			
Life satisfaction	-0.549***	-0.564***	-0.430***
	(0.060)	(0.062)	(0.056)
Mental health status	-0.309***	-0.319***	-0.267***
	(0.046)	(0.045)	(0.049)
Economic resources			
Deprivation index	0.608***	0.608***	0.684***
	(0.071)	(0.072)	(0.072)
Satisfaction with	-0.529***	-0.543***	-0.540***
standard of living	(0.057)	(0.058)	(0.059)
Psychosocial needs			
Social participation			
Number of close friends	-0.049	-0.064	-0.041
	(0.043)	(0.042)	(0.044)
Social engagement	-0.048	-0.053	-0.084**
	(0.038)	(0.038)	(0.037)
Social status	-0.244***	-0.243***	-0.279***
	(0.048)	(0.048)	(0.053)
Self-efficacy	-0.202***	-0.214***	-0.233***
	(0.062)	(0.063)	(0.059)

Notes: Estimates from IPW-DID based on 635 treated and 17,047 control persons (the estimates for self-efficacy are based on 417 treated and 10,359 control persons). The propensity of job loss is based on a logit model with the control variables reported in Tables 1.C.1 and 1.C.2 in Appendix 1.C. Robust standard errors are in parentheses. They are calculated by taking into account that propensity scores are estimated. Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: PASS-ADIAB 7515, own computations.

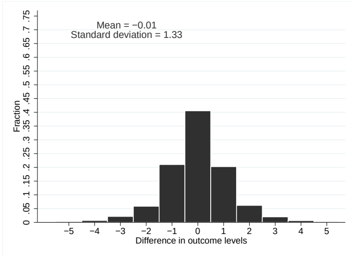
Table 1.D.6: Summary statistics of propensity scores

				Quantiles				
	N	Mean	SD	Min	25%	50%	75%	Max
Treated	635	0.1743	0.1652	0.0005	0.0502	0.1200	0.2495	0.9180
Control	17,047	0.0308	0.0584	0.0002	0.0032	0.0089	0.0301	0.8405

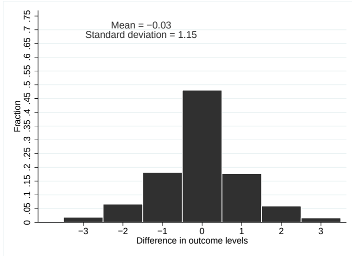
Notes: SD: Standard deviation.
Source: PASS-ADIAB 7515, own computations.



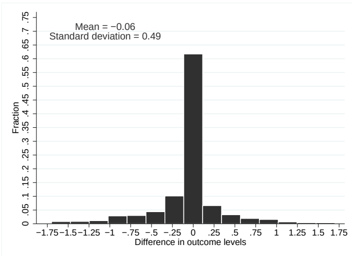
(A) Social integration



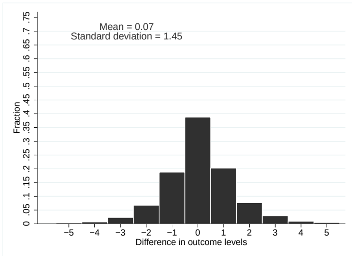
(B) Life satisfaction



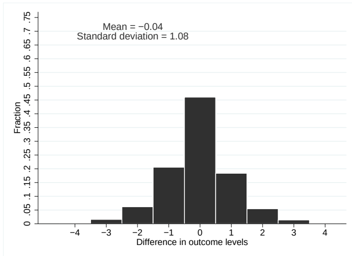
(C) Mental health status



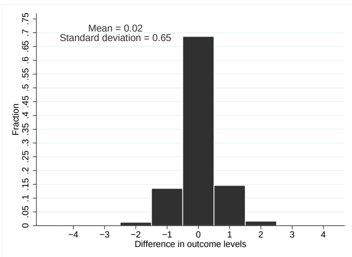
(D) Deprivation index



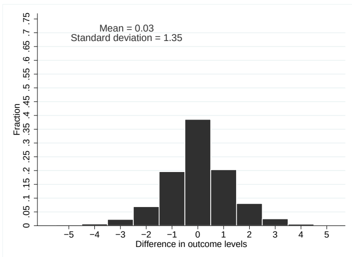
(E) Satisfaction with standard of living



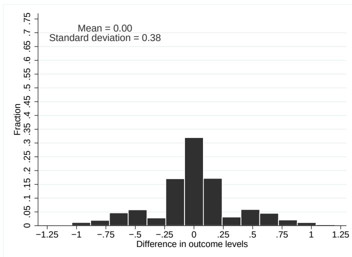
(F) Number of close friends



(G) Social engagement



(H) Social status



(I) Self-efficacy

Notes: Changes in the outcome variables are measured as differences in levels in two consecutive waves t_0 and t_1.
Source: PASS-ADIAB 7515, own computations.

Figure 1.D.1: Distribution of changes in outcome variables

Table 1.D.7: Heterogeneous effects by previous job characteristics

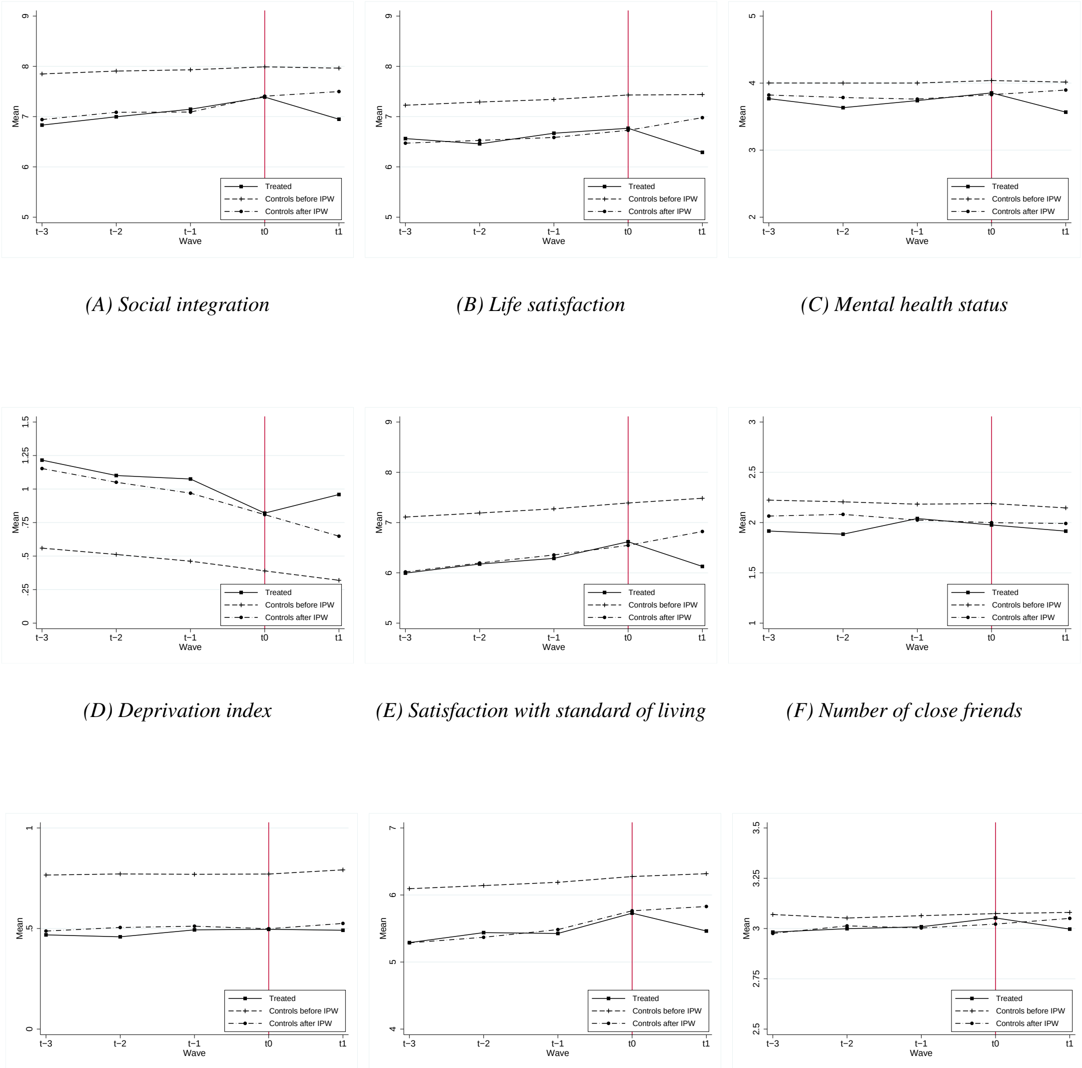
Specification	(1) Firm size: < 50 employees	(2) Firm size: ≥ 50 employees	(3) Job class: Farmer/Production	(4) Job class: White-collar	(5) Full-time	(6) Part-time
Change in outcomes			Effect of job loss			
Social integration	-0.368***	-0.367***	-0.297***	-0.350***	-0.317***	-0.394***
	(0.093)	(0.076)	(0.099)	(0.067)	(0.067)	(0.087)
Well-being and mental health						
Life satisfaction	-0.528***	-0.598***	-0.408***	-0.591***	-0.520***	-0.562***
	(0.085)	(0.078)	(0.100)	(0.076)	(0.069)	(0.103)
Mental health status	**-0.408*****	**-0.220*****	-0.259***	-0.318***	-0.324***	-0.248***
	(0.072)	(0.067)	(0.087)	(0.056)	(0.057)	(0.080)
Economic resources						
Deprivation index	**0.465*****	**0.768*****	0.553***	0.597***	0.605***	0.560***
	(0.102)	(0.110)	(0.125)	(0.090)	(0.083)	(0.130)
Satisfaction with standard of living	-0.471***	-0.612***	-0.371***	-0.604***	-0.551***	-0.485***
	(0.075)	(0.083)	(0.102)	(0.069)	(0.068)	(0.092)
Psychosocial needs						
Social participation						
Number of close friends	-0.028	-0.089	0.009	-0.096*	-0.039	-0.082
	(0.074)	(0.067)	(0.088)	(0.052)	(0.050)	(0.077)
Social engagement	-0.020	-0.052	**-0.207*****	**-0.011**	-0.051	-0.043
	(0.054)	(0.050)	(0.072)	(0.045)	(0.045)	(0.066)
Social status	-0.332***	-0.160**	-0.200**	-0.281***	-0.290***	-0.184**
	(0.071)	(0.073)	(0.085)	(0.058)	(0.058)	(0.085)
Self-efficacy	**-0.376*****	**-0.010**	-0.202	-0.211***	-0.152**	-0.256**
	(0.084)	(0.087)	(0.153)	(0.075)	(0.076)	(0.107)

Notes: Estimates from IPW-DID are based on 324 treated and 7,052 control persons in specification (1), on 311 and 9,995 in (2), on 201 and 3,891 in (3), on 434 and 13,156 in (4), on 448 and 10,996 in (5) and on 187 and 6,051 in (6). The propensity of job loss is based on a logit model with the control variables reported in Tables 1.C.1 and 1.C.2 in Appendix 1.C. Robust standard errors are in parentheses. They are calculated by taking into account that propensity scores are estimated. Coefficients are statistically significant at the *10%, ** 5% and *** 1% level. Differences in the effects of job loss between subgroups that are significantly different from zero at the 10% level are indicated by bold numbers. Standard errors of the differences are obtained by bootstrapping (2,500 replications).
Source: PASS-ADIAB 7515, own computations.

Table 1.D.8: Placebo results for the consequences of job loss

Change in outcomes between t_{-1} and t_0	Effect of job loss	Standard error	Standard deviation
Social integration	-0.055	(0.063)	1.526
Well-being and mental health			
Life satisfaction	-0.064	(0.063)	1.277
Mental health status	0.020	(0.051)	1.136
Economic resources			
Deprivation index	-0.122	(0.089)	0.459
Satisfaction with standard of living	0.026	(0.064)	1.383
Psychosocial needs			
Social participation			
Number of close friends	-0.038	(0.051)	1.054
Social engagement	0.030	(0.040)	0.641
Social status	-0.004	(0.067)	1.293
Self-efficacy	-0.018	(0.084)	0.362

Notes: Estimates from IPW-DID are based on 448 treated and 13,075 control persons (the estimates for self-efficacy are based on 207 treated and 5,319 control persons). The propensity of job loss is based on a logit model with the control variables reported in Tables 1.C.1 and 1.C.2 in Appendix 1.C. The differences in the outcome variables are standardized. Standard errors are robust and calculated by taking into account that propensity scores are estimated. Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: PASS-ADIAB 7515, own computations.



(A) Social integration *(B) Life satisfaction* *(C) Mental health status*

(D) Deprivation index *(E) Satisfaction with standard of living* *(F) Number of close friends*

(G) Social engagement *(H) Social status* *(I) Self-efficacy*

Notes: Means of the outcome variables are measured in levels in the consecutive waves t_{-3}, t_{-2}, t_{-1}, t_0 and t_1 separately for treated and control individuals before and after inverse propensity score weighting (IPW). The difference in outcome levels between treatment and control group before IPW is significant at the 1%-level for each outcome variable in the time periods before job loss. The difference in outcome levels between treatment and control group after IPW is insignificant for each outcome variable in the time periods before job loss, except for mental health status in t_{-2} and deprivation index in t_{-1} (significant at 10%-level) and number of close friends in t_{-2} (significant at 5%-level).
Number of observations: t_{-3}: 190 treated and 7,027 control persons, t_{-2}: 303 treated and 9,690 control persons, t_{-1}: 448 treated and 13,075 control persons, t_0 and t_1: 635 treated and 17,047 control persons.
Source: PASS-ADIAB 7515, own computations.

Figure 1.D.2: Placebo tests on outcome levels

Chapter 2

The Impact of Participation in Job Creation Schemes in Turbulent Times*

2.1 Introduction

This chapter analyzes the impact of participation in job creation schemes (*Arbeitsbeschaffungsmaßnahmen*, JCSs) on job search outcomes in the context of the turbulent East German labor market in the aftermath of the German reunification. The East German economy plunged into a deep recession immediately after the German reunification in 1990. The transition from a centrally planned to a market-based economic system led to plant closures and mass layoffs, leading to a sharp increase in the unemployment rate from virtually zero in 1990 to about 10% in 1991. Active labor market policies (ALMPs) were implemented on a large scale to fight the unemployment crisis. Hereby, JCSs that offer temporary work opportunities for the unemployed in the public and nonprofit sector played a prominent role. These schemes reached an all-time high in 1992 when on average 388,000 individuals were employed in JCSs and expenditures of the German Federal Government and the German Federal Employment Agency amounted to 10.4 billion DM (7.8 billion € in 2015 prices) in East Germany (Spitznagel, 1992). This sum is equivalent to 4.4% of the East German GDP.[1]

*This chapter is joint work with Annette Bergemann (University of Bristol, IFAU, IZA) and Arne Uhlendorff (CREST, CNRS, Unversité Paris-Saclay, IAB, IZA, DIW). A similar version of this chapter is published in Labour Economics 47, 2017, pp. 182-201 which corresponds to the latest discussion paper version (IFAU Working Paper 2017:7, for earlier versions see IZA Discussion Paper No. 10369 and ZEW Discussion Paper No. 17-021). This chapter is based on, but substantially exceeds, my master thesis (Pohlan, 2013) and my dissertation proposal (Pohlan, 2015) submitted at the University of Mannheim in 2013 and 2015, respectively. This chapter has benefited from comments and suggestions by Gerard van den Berg, Bernd Fitzenberger, Stephen Kastoryano and Konrad Stahl.

[1]Federal Employment Agency (1993) and Federal Statistical Office (1993).

The main official objective of providing these JCSs was to improve the employment prospects of the participants. There are at least two potential channels how JCSs might accomplish this. To fix ideas, we are arguing in the framework of a labor market with search frictions (for a more formal analysis see the Supplemental Material of Crépon and van den Berg, 2016). By providing work experience JCSs can increase participants' attachment to the labor market. This stronger bond might motivate the participants to intensify their search effort for a regular job. At the same time JCSs can provide the participants with the ability to signal their positive work attitude. Both, increased search effort and the signaling ability have a positive impact on the job offer arrival rate and the ability to stay on a regular job. The second channel consists of the potential ability of JCSs to shift the wage offer distribution for the participants. Naturally, job seekers become more attractive for employers if their human capital is raised and JCSs offer a number of possibilities to achieve this. Participants acquire cognitive skills by learning–on–the–job, work experience and short training courses, which are sometimes offered in the context of JCSs. Being placed in a work context by way of JCS participation can also foster soft skills.

As Crépon and van den Berg (2016) show both, an increased job offer arrival rate and a shift in the wage offer distribution due to participation in ALMP programs, increases the exit rate from unemployment compared to the situation before participation.

The effectiveness of participation in ALMPs might depend on the state of the economy. In periods with low job destruction rates and relatively high job offer arrival rates the opportunity costs of participating in an ALMP might be relatively high, i.e., searching for a job could be more beneficial because the probability of finding a regular job is high. In contrast to that, in periods with low job offer arrival rates positive effects of ALMPs will carry more weight, as a continued job search is less rewarding. The role of the state of the economy is probably more relevant for longer programs like JCSs and training. They offer the possibility to keep in contact to the labor market as it needs time until the economy is back to a higher level of employment. For evidence of more positive effects of training programs in periods of high unemployment rates see for example Lechner and Wunsch (2009a) and Heinrich and Mueser (2014). In line with this, Forslund et al. (2011) argue that programs with strong locking-in effects should be rather used in an economic downturn, because the cost of forgoing search time is lower than in an economic upturn.

These considerations are particularly relevant if the economy is not only characterized by high job destruction rates but also undergoes a major structural change. Structural change often involves depreciation of human capital. For the unemployed who are not able to use, for example, learning-on-the-job to stop the depreciation, JCSs might be a particularly helpful instrument. Consequently, JCSs have quite some leverage to improve the situation of the participants. This leverage might depend on the level of the human capital. Dynamic complementarities in human capital on which the recent literature focuses (see for example Almlund et al., 2011) mean in this context that high skilled individuals might be particularly affected by depreciation and in turn might be able to benefit most from participation in JCSs.

There exist a number of empirical studies evaluating the employment effects of JCSs for stable, rather matured market economies.[2] The general notion is that JCSs do not have positive effects. However, there are some signs for effect heterogeneity. Some papers conclude that long-term unemployed gain from participation in JCSs, whereas others not (see for example Caliendo et al., 2008a vs. Hujer and Thomsen, 2010). Quite stable results exist with respect to positive effects for hard-to-place[3] women in West Germany (Caliendo et al., 2008b and Hohmeyer and Wolff, 2012). Note that only a small number of these studies investigate whether there is effect heterogeneity with respect to educational level. Those who do distinguish by education level do not find significant differences between different educational levels (see for example Caliendo et al., 2008b).

The state of evidence is different for economies that underwent a major shock, as it was the case during the transformation process. There exist only few studies that evaluate the employment effects of JCSs and those come to rather diverse results. Concerning the impact of JCSs in East Germany for the period after the reunification, Hübler (1997) and Kraus et al. (2000) conclude that JCSs have a rather negative impact on the employment probability of the participants, while Eichler and Lechner (2002) find a substantial decline in the unemployment probability due to participation in JCSs in the period after program end.

The evidence is similarly scarce when considering other transformation countries. One exception is Lubyova and van Ours (1999), who evaluate JCSs for the time from 1991 to 1996 in Slovakia. They find positive effects on the job finding probability for JCSs in the public sector, while JCSs in the private sector that typically had a longer duration seem to reduce the exit rate to regular work. In a related paper, van Ours (2004) finds evidence that part of the difference in the effects are driven by locking-in effects of JCSs, and that those are stronger for men than for women. Kluve et al. (1999) study

[2] See for example Cockx and Ridder (2001) for Belgium, Bonnal et al. (1997) for France, Lalive et al. (2008) for Switzerland and for Germany Hohmeyer and Wolff (2012) and Lechner and Wunsch (2009b) as well as the series of papers using an administrative sample of unemployed in 2000 (see Hujer et al., 2004; Caliendo et al., 2006; Hujer and Zeiss, 2007; Caliendo et al., 2008a; Caliendo et al., 2008b and Hujer and Thomsen, 2010). For overview studies see for example Bergemann and van den Berg (2008) and Card et al. (2010).

[3] Measured for example by a high number of unsuccessful placement propositions or dependency on welfare benefits.

the effects of different ALMPs in the period from 1992 to 1996 in Poland and they find evidence for reduced employment rates mainly among male participants in JCSs.[4] None of these studies investigates whether the effects differ by level of human capital.

A remarkable result of many studies on JCSs, independent of whether JCSs are taking place in stable or turbulent economies, is that women benefit more than men. This seems to be particularly the case in countries with a high female labor force participation. In their overview article, Bergemann and van den Berg (2008) argue that participation in JCSs might help to overcome statistical discrimination.

As mentioned, our study, in line with the focus of the existing literature, concentrates on the main official objective of JCSs namely to evaluate whether participation increases the employment prospects of its participants. It should however be pointed out that with the provision of JCSs additional objectives were pursued. Firstly, JCSs should also provide immediate employment opportunities and income for the unemployed individuals during this period of massive job destruction and a persistent deficit of productive employment opportunities. In this way it was used as a tool for raising the overall number of jobs in the economy. A further claim of at least part of the JCSs in East Germany was to invest in the East German infrastructure. Therewith, the program might have stabilized the income of a large number of individuals and their families during the transformation process, while participants carried out work which is potentially valuable for the society. An exhaustive evaluation of this kind of programs should take these aspects into account.

Our analysis is based on the Labor Market Monitor Sachsen–Anhalt (LMM–SA), which is a survey on the working age population of the East German state of Sachsen–Anhalt. We use the last three waves (1997, 1998, 1999) of the survey, which include retrospective monthly calendars on the complete labor market history, including participation in ALMP programs since the reunification. This calendar offers unique possibilities for the empirical analysis of program participation in the years after the German reunification. Our observation period starts in 1990, shortly before the reunification, and ends in 1999.

The program was in place in all regions in the state of Sachsen–Anhalt, and the data does not contain instrumental variables which could be used to identify causal effects. We therefore estimate discrete time duration models following the timing-of-events approach (Abbring and van den Berg, 2003). This approach allows to control for dynamic

[4] Based on Polish data Puhani (2002) presents similar findings applying matching estimators. His findings based on duration models indicate significantly negative effects for men and women. However, the estimated specifications are very restrictive. For example, he does not control for selection into the treatment and the models assume a homogenous treatment effect over time spent in unemployment.

selection into the treatment based on both observed and unobserved characteristics. We estimate the impact of JCSs on the probability of finding a job and on the probability of retaining employment. This approach has two major advantages in particular in view of evaluating a program in an unstable economy. Firstly, the way we allow for unobserved heterogeneity does not require controlling for past employment outcomes or using past employment outcomes in order to estimate differences-in-differences (as e.g. Caliendo et al., 2008a or Eichler and Lechner, 2002). Besides a lack of availability of detailed data on employment histories before 1990, this type of data might contain relatively little information for the prediction of future outcomes in our observation period. The unemployment rate in the socialistic German Democratic Republic (GDR) was close to zero and a large share of the human capital lost its value during the transformation process. Therefore, our application using the timing-of-events approach delivers new insights into the effectiveness of JCSs.

The second major advantage of our approach is the focus on transition rates. This takes automatically into account that the program does not take place in a stationary environment. Bergemann et al. (2009) show for the case of training in East Germany that using transition rates as success indicator is more appropriate in such a nonstationary environment as compared to the use of unconditional employment rates as it is often done in the literature. Furthermore, estimating the effects on transition rates is more informative because they deliver detailed information about the functioning of JCSs; notably, whether the program helps participants to find a regular job and whether the program helps to stay in a regular job. This is particularly interesting for the German case, as the regulatory framework sets down that JCSs should help to improve the employment situation notably in these two dimensions.

The studies closest to ours are van Ours (2004) and Eichler and Lechner (2002). Eichler and Lechner (2002) evaluate the effectiveness of JCSs in Sachsen-Anhalt for the time period 1992 to 1996 based, as the present analysis, on data of the LMM-SA. The authors do not exploit the monthly retrospective calendar but the panel structure of the data using the waves from 1992 to 1997. In this way they can only identify labor market states at the time of the interviews. Eichler and Lechner (2002) apply a conditional difference–in–differences approach with the unemployment probability as the outcome variable of interest. The estimator is aligned on the labor market state observed directly before the participation. This can affect their estimates if the employment situation is characterized by a temporary (random) deterioration (a phenomenon that is also captured under the heading Ashenfelter's Dip following Ashenfelter, 1978); this randomness in the employment situation is automatically captured in our transition rate model. Moreover, our timing-of-events approach is able to take into account that individuals might enter the programs at a later point in time. An additional innovation constitutes our focus on effect heterogeneity beyond gender, which is able to deliver new insides on the functioning of JCSs.

The evaluation of van Ours (2004) also builds upon the timing-of-events approach, but he solely focuses on the transition rate to work. Moreover, he investigates effect heterogeneity only with respect to gender. We investigate effect heterogeneity with respect to selected further characteristics like education, and estimate models allowing for effect heterogeneity with respect to unobserved characteristics following Richardson and van den Berg (2013). Additionally, we estimate specifications controlling for endogenous participation in training programs and investigate the effects of multiple treatments.

Our results suggest strong negative locking-in effects during program participation. These effects are in line with the intention to provide job opportunities for the unemployed, and might at least partly reflect a rearrangement of the job queue. In a model with homogeneous treatment effects, the negative treatment effect vanishes one year after the program start. Furthermore, we show that women and highly skilled participants leave unemployment quicker than other groups, which results in highly skilled women benefiting from participation. Additional results suggest that JCSs do not influence employment stability.

The rest of the chapter is structured as follows. Section 2.2 describes the East German labor market situation and the institutional settings of JCSs. Section 2.3 presents the data and descriptive statistics. Section 2.4 specifies the empirical model and discusses the underlying assumptions. Section 2.5 presents the results of the empirical analysis and Section 2.6 concludes.

2.2 Institutional Background

2.2.1 Economic Development in East Germany

On the eve of the German reunification in 1990, the economic situation in East Germany was quite desolate. The centrally planned economy of the GDR was characterized by inefficient production processes, obsolete technologies and over-staffing. Following a policy of full employment, the GDR had a labor force of about 10 million in 1989 and unemployment was almost nonexistent. In contrast, the modern market-oriented economy of the Federal Republic of Germany had a labor force of about 28 million and a rate of registered unemployment of 7.9% in 1989 (Federal Employment Agency, 1990).[5]

In this new environment existing East German firms faced enormous difficulties to compete. They could rarely cover their variable costs at the prevailing market prizes. In addition, their former home market broke away as East Germans diverted their spending towards West German products. Production in 1991 only reached two-thirds of its 1989 level. Four years of high growth rates followed in East Germany. However, since 1996 the economy was basically stagnating again.

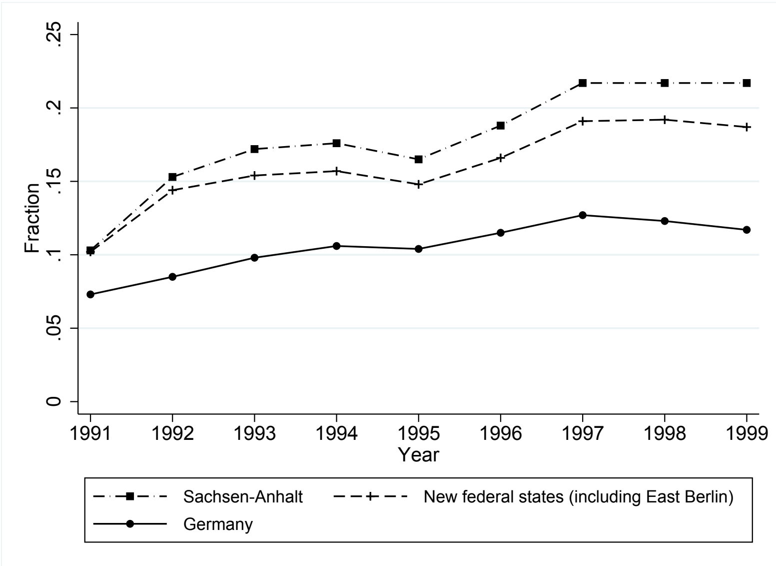
The government reacted with setting up large labor market programs in order to cushion the effects of economic restructuring. Shortly after the reunification the main emphasis was put on instruments that were easy to implement. Short-time work and early retirement schemes were predominant. However, already in 1991, a substantial part of the East German labor force participated in active labor market programs to keep the official unemployment rate - which does not include program participants - from skyrocketing. By correcting the number of unemployed by the number of participants in ALMP programs, adjusted unemployment rates (also called underemployment rates) in East Germany amounted to 25.3% in 1991, peaked at 35.3% in 1992 and decreased to a value of 23.7% in 1999 (see Table 2.1). In 1991, 209,000 individuals participated in JCSs and 280,000 in training programs. Participation in ALMP measures peaked in 1992 with over 800,000 individuals participating on average in full-time programs. From 1993 onwards, the number of participants began to shrink due to policy changes and financial restrictions. However, training and JCSs remain important components of policy interventions in East Germany until the early 2000s.

Despite the heavy use of ALMPs, unemployment increased drastically. During the period 1990-1992, regular employment was reduced from a yearly average of over 9 million jobs down to just under 6 million jobs and the unemployment rate rose from virtually zero in 1990 to more than 10% in 1991. From 1991 onwards, it exceeded the average unemployment rate for Germany as a whole (see Figure 2.1).

Our analysis is based on data gathered in the new federal state of Sachen-Anhalt. In 1999, 2.7 million individuals lived in Sachsen-Anhalt which corresponds to 3% of the population in Germany and to 22% of the population of the new federal states without Berlin (Federal Statistical Office, 2015). Figure 2.1 shows that the unemployment rate in Sachsen-Anhalt exceeded the average of East Germany over the whole observation period. These figures were mainly driven by the high concentration of sectors like agriculture, electrical industry, trade, mining and chemical industry. After reunification, many companies in these fields had to close down due to the loss of trading partners in

[5]For more detailed information on the economic development of East Germany see von Hagen et al. (2002), Burda and Hunt (2001) and Wunsch (2005).

the East and inefficient production processes.



Source: Federal Employment Agency (2017).

Figure 2.1: Unemployment rate in Germany 1991–1999

A factor that should not be neglected when discussing the economic situation in East Germany is the emigration that set in after the fall of the wall. In 1989 and 1990 almost 400,000 individuals, which is about 2% of the East German population, migrated from East to West Germany each year (Kröhnert and Skipper, 2010). The threat of mass emigration was a popular argument for a quick catch-up of East German wages and for an implementation of large ALMP programs among both politicians and union leaders. Indeed, the migration situation changed after 1990. Emigration reduced substantially and was increasingly matched by immigrating West Germans. In 1997, East-West migration reached a minimum with 13,000 individuals. Since then, emigration from the new federal states has increased again.

2.2.2 Background and Aims of JCSs

When the West German Employment Promotion Act (*Arbeitsförderungsgesetz*, AFG) was transferred to East Germany a number of additional regulations were introduced to take into account the special situation of the East German economy. Those exceptions meant, among others, less restrictive rules for participation in ALMP programs shortly after reunification. In the following section, we describe the eligibility rules and some important implementation details of the two different job creation programs which were realized in East Germany. We focus on the institutional regulations in force during the time before the Employment Promotion Act was replaced by the new Social Law Book III in 1998. This time period covers the main part of our observation period.[6]

In this study, the phrase *job creation scheme* includes two different types of programs which were realized in East Germany after the reunification: traditional JCSs (*Allgemeine Maßnahmen zur Arbeitsbeschaffung*, see §§91-96 AFG) and Productive Wage Subsidies East (*Produktive Lohnkostenzuschüsse Ost*, see §249h AFG).[7] The latter were introduced in January 1993 and offered temporary employment opportunities in activity areas like social services or environmental redevelopment. Both job creation programs intended to create additional temporary jobs primarily in the public or nonprofit sector for the time of the subsidy and were similarly handled by the labor offices. They differed, however, with respect to the level of the subsidy, the program duration and the activity areas.

The government pursued several objectives by implementing JCSs in East Germany in the period after reunification. First, in the course of the transformation process, JCSs should simply provide jobs and income for unemployed individuals and those who were at risk of becoming unemployed. In this way the threat of social hardship could be reduced and the official unemployment rate could be lowered. Second, they were used as means to invest in the East German industrial infrastructure. Especially in the time of 1993–1996, this aim was emphasized by the large scale provision of ordinary productive wage subsidies with their restricted activity areas. The third objective, which constituted the main official aim (Wolfinger and Brinkmann, 1996), was in accordance with the traditional role of ALMP measures: the employment subsidies should help the participants to find regular jobs. Indeed, over time the focus nearly completely shifted towards this goal. In addition, the AFG emphasized that especially those JCSs should be supported which help creating *stable* employment relationships. This chapter evaluates whether JCSs help to find and retain regular employment. Hereby, traditional JCSs and ordinary subsidies will be jointly evaluated. Unfortunately, data limitations make it impossible to distinguish between these two different program types (see Section 2.3.1).

2.2.3 Institutional Provisions of JCSs

The implementation of the two types of JCSs involved the following steps. A project organizing institution, which could be a firm, a public authority or a charity, had to create at least one job within a project. This project needed to be beneficial for the community

[6] Further information and data on JCSs in East Germany in the early 1990s can be found for example in Brinkmann and Völkel (1992) and Spitznagel (1992). Only few additional changes concerning JCSs took place with the introduction of SGB III, see for example Wunsch (2005).

[7] In 1998 these subsidies were renamed to Structural Adjustment Measures (*Strukturanpassungsmaßnahmen*).

and had to be additional in the sense that it would not be carried out without the subsidy. Formally, after approval of a project, the local labor office should choose the participants. Surveys in labor offices showed that the time elapsing between the application of a project organizing institution and the actual program start was on average three months (Völkel, 1994). In East Germany so-called "Societies for Employment Promotion and Structural Development" (*ABS-Gesellschaften*) often acted as large scale organizers of JCSs. In the early 90s these societies had a significant influence on the selection of participants. They had a preference for young educated men (Brinkmann and Völkel, 1992).

Participation in a JCS was often financially attractive for unemployed individuals. The wage paid during program participation had to be equal to the wage set by collective wage agreements between the unions and employers organizations for similar but unsubsidized work (*Tariflohn*). The subsidy given to the employer covered part of (or fully) the wage costs. Participants received a fixed-term work contract, which induced regular social security contributions. As a consequence the participant renewed or prolonged his or her eligibility period for unemployment benefits. During participation the local labor office and the participant should continue their search for a regular job. The program ended in case a regular job or a suitable training program was found. More recently, JCSs can be used to test the willingness to work of unemployed jobseekers in Germany, see for example Hohmeyer and Wolff (2012), who study the impact of JCSs on German welfare recipients based on a sample from 2005. In our context, with high job destruction rates and low job offer arrival rates, the test of willingness to work was, however, neither an explicit nor implicit goal of JCSs (see for example Spitznagel (1992)). Moreover, the increased income compared to unemployment benefit payments made participation in JCSs for most of the unemployed job seekers attractive.

The length of traditional JCSs was typically 12 months. In some cases extensions of up to 24 months or even of up to 36 months were possible if a permanent job was offered subsequently by the organizer of JCSs. Productive Wage Subsidies East could be granted even longer. It can be granted up to 48 months for employees that are older than 50, handicapped people or in case a permanent job was offered by the program-supporting employer. The subsidy is not transferable to other employers. It is exclusively paid to the employer who set up the JCS.

The implementation details depended on the type of the subsidy program and the point in time it took place. Formally, participation in traditional JCSs required that the person was unemployed and entitled to some kind of unemployment payment just prior to participation. In addition, a person needed to have been unemployed for at least 6 months within the last 12 months. The eligibility criteria for Productive Wage Subsidies East were less strict. Besides being eligible for some kind of allowance, a participant needed to have been unemployed for 3 months, or needed to have had finished a traditional JCS, or enter from short-time work.

The local labor offices could depart from the above mentioned participation criteria. In particular, the rules with respect to the previous unemployment duration have not been applied strictly in East Germany. This is especially true for the period directly after the unification. Also shortly after the unification, it was common practice after plant's closure to collectively put the work force of the plant into a so-called Mega-JCS. This program involved, for example, closing down the obsolete plant or cleaning-up the environmental damage produced by the plant. We do not consider participation in Mega-JCSs in our main specification as these programs are not primarily aiming at the integration into regular employment.

This practice and the influence of the large scale ABS-Societies on the selection of participants were the main reasons for the deviations from the target group of traditional JCSs. Unemployed older than 50 or younger than 25 and without professional education, long-term unemployed and, as a special regulation for East Germany, also women belonged to the target group. It should be mentioned that for older participants an additional small scale job creation program existed. Albeit being similar to traditional JCSs this program solely intended to bridge the time until retirement (*Maßnahmen zur Arbeitsbeschaffung für ältere Arbeitslose* §§97-99 AFG). In order to avoid the analysis of pre-retirement effects, we will exclude elderly from our analysis (see Section 2.3.1). In the mid 90s, the allocation of JCSs became more in line with the predefined target groups.

In April 1997 an additional productive wage subsidies program was implemented: Productive Wage Subsidy for Business Enterprises (*Lohnkostenzuschüsse Ost für Wirtschaftsunternehmen*, see §249h AFG) which was designed to subsidize temporarily regular jobs. This program of ALMP will not be considered here as it might have qualitatively different effects from JCSs.

2.2.4 Participation and Costs of JCSs

Table 2.1 shows that the number of program participants peaked in 1992 when 388,000 individuals were employed in traditional JCSs in the new federal states (NFS). In this time period high participation rates were mainly realized by Mega-JCSs, where the workforce of closing firms were collectively put into a job creation program. Thereafter, policy changes and financial restrictions led to decreasing yearly stocks. Between 1993 and 1997 the stock of participants in traditional JCSs fluctuated around 200,000 while the stock of participants in Productive Wage Subsidies East fluctuated around 90,000 per year in East Germany. From 1998 onwards, the number of jobs created via traditional JCSs was lower than the number created via Productive Wage Subsidies East. This development was mainly driven by the introduction of the Productive Wage Subsidies for Business Enterprises in April 1997.

Table 2.1: Participants in JCSs (in thousands), 1991-1999

Year	Traditional JCSs			Productive Wage Subsidies East			Underemployment rates in %	
	NFS	SA	$\frac{SA}{NFS}(in\%)$	NFS	SA	$\frac{SA}{NFS}(in\%)$	NFS	SA
1991	208.7	35.7	17.1	.	.	.	25.3	24.8
1992	388.1	88.0	22.7	.	.	.	35.3	37.3
1993	237.5	56.4	23.7	22.5	.	.	32.3	34.4
1994	192.5	40.0	20.8	87.7	21.0	24.0	29.7	30.3
1995	205.8	41.0	19.9	106.5	23.2	21.8	26.4	27.6
1996	191.5	40.0	20.9	86.2	17.6	20.4	24.2	27.2
1997	154.5	33.0	21.4	80.1	17.1	21.4	24.0	27.0
1998	151.8	27.0	17.8	162.4	29.5	18.2	24.1	26.7
1999	168.0	30.0	17.9	180.0	29.0	16.1	23.7	26.6

Notes: JCS: Job creation scheme. SA: Sachsen-Anhalt. NFS: New federal states including East Berlin. Underemployment = unemployed + full-time equivalents of short-time work + training participants + JCS participants + early retirement participants. Underemployment rate = underemployment in % of labor force (employed and unemployed) + training participants + early retirement participants (see the official definitions of the underemployment rate of the Federal Employment Agency (1992-2000)).
Source: Federal Employment Agency (1992-2000), Institute for Employment Research (IAB) (2003).

The relatively high unemployment rate in Sachsen-Anhalt compared to the other new federal states (see Figure 2.1) did not result in a higher share of participants in JCSs in the 90s. The number of participants in JCSs in Sachsen-Anhalt amounted to 20% of the total number of participants in all new federal states in the time period considered for both kinds of job creation measures.

Table 2.2 shows that the expenditures on JCSs by the German Federal Employment Agency for both kinds of programs fluctuate around 5 billion DM (3.7 billion € in 2015 prices) and reached an all-time high in 1996 when costs amounted to more than 8 billion DM (6.0 billion € in 2015 prices) in East Germany. In total, JCSs counted more than 2.5 million participants and produced expenditures of more than 52 billion DM (39.0 billion € in 2015 prices) in the period 1991-1999 in East Germany.

Table 2.2: Expenditures on JCSs by the German Federal Employment Agency (in million DM), 1991-1999

Year	NFS	SA	$\frac{SA}{NFS}(in\%)$
1991	3075	612	20
1992	5083	1664	33
1993	6905	1388	20
1994	4722	1680	36
1995	7109	1734	24
1996	8156	1701	21
1997	6703	1422	21
1998	5453	1054	19
1999	5681	1117	20

Notes: JCS: Job creation scheme. SA: Sachsen-Anhalt. NFS: New federal states including East Berlin.
Source: Federal Employment Agency (1992-2000).

2.3 Data and Descriptive Statistics

2.3.1 Data Set and Sample Selection

The data used stem from the last three years (1997-1999) of the Labor Market Monitor Sachsen-Anhalt (LMM-SA). The LMM-SA is a survey of the working-age population living in the new federal state of Sachsen-Anhalt with around 6.000 survey participants each year.[8]

Similar to other surveys, the LMM-SA provides individual information on socio-economic characteristics like age and professional education. As an important innovation, the LMM–SA introduced in the years 1997-1999 a retrospective monthly employment calendar that goes back until 1990, enabling us to analyze JCSs over a long time period after the reunification.

Recall data over such a long time span can suffer from recall errors. Paull (2002) documents the international evidence. Due to recall error she notes a tendency to report too many labor market transitions and at the same time to underreport short unemployment spells. However, Bachmann and Schaffner (2009) find on the basis of retrospective surveys going back at most two years that this is less of a problem for the German survey GSOEP.

Although our survey covers a longer time span, we argue that the set-up of the

[8]The response rate of 32% (Ketzmerik and Wiener, 1999) is in line with other innovative surveys. For example the response rate of the German Internet Panel amounts to 19% (Blom et al., 2015), and the response rate of the GSOEP innovation refreshment sample F to 51% (Däubler, 2002).

questionnaire is such that our data also suffers less from recall error compared to other data recording similarly long time periods. Firstly, the survey participants are asked to remember their employment history starting with the historic year of 1990, in which the political and economic system of East Germany changed drastically. The accuracy of recall can be strongly improved by such a combination of biographic and historic events (Loftus and Marburger, 1983). Second, the data is collected in a chronological order, which is regarded as the best technique for collecting life history data in a single survey (Sudman and Bradburn, 1973). Furthermore, we try to use broadly defined labor market states, such that recall errors can cancel out. This is particularly relevant for the definition of the 'out of the labor market' state and 'unemployment'. Paull (2002) for example finds that women tend to redefine unemployment to being 'out of the labor force'. In our main analysis we aggregate these two states to being 'unemployed'. The aggregation is supported by the observation that being 'out of the labor force' is a rare event in East Germany for prime aged individuals. Labor force participation is historically very high in East Germany. However, we also present a sensitivity analysis where the 'out of the labor force' state is treated as a censoring event. One potential problem with our data could be that some JCSs are reported as regular employment. This is comparable to the tendency that private training programs are reported as ALMP training programs, see Ketzmerik (2001). However, we do not have a benchmark data set with which we can investigate this issue. As long as this is not systematically related to the success of the program this should not bias our results.

Our data source allows us to distinguish the following combined categories of the labor market status on a monthly basis: *in education*, *employed* (including full-time employed, part-time employed and self-employed), *unemployed* (combined with out of the labor force), *in training*, *in JCS*, *in maternal leave* and *in retirement*.[9]

Our sample focuses on individuals that are between 25 and 50 years old in January 1990 and that had been employed before the Monetary, Economic and Social Union went into effect in June 1990. This allows us to analyze the effect of JCSs for individuals who belonged to the active labor force of the GDR and who were hence fully affected by the transformation process and subsequent introduction of ALMP programs.[10] At the same time, this sampling criteria allows us to exclude individuals who are close to retirement and might use ALMP programs as a bridge to retirement.

[9] For more details on the data set and its use see for example Bergemann et al. (2009) and Ketzmerik (2001) as well as Eichler and Lechner (2002) on earlier waves.

[10] See Table 2.A.1 in Appendix 2.A for the number of observations dropped by each sample selection step. Note that the data collecting institute provided us with a retrospective questionnaire data set, where already only those were selected who gave a full account of their employment history; these were about 95% of all interviewees, see Ketzmerik and Wiener (1999).

Table 2.A.2 in Appendix 2.A presents an overview of the variables used in this study. Based on these data we construct a sample of inflows into unemployment based on individuals whose labor market history is observable until at least September 1997 without interruption. We consider unemployment spells starting in January 1991 or later only if there exists a prior employment spell of at least one month.[11]

This analysis exploits information on 2,235 individuals who experience at least one unemployment spell between January 1991 and the end of the observation period, which can be September 1997, October 1998 or December 1999.[12] In total, the data include 3,864 unemployment spells. Thus, several individuals experience multiple spells and the average number of spells per individual amounts to 1.7. Transitions to other destinations than to employment are treated as right censored. Thus, if an individual enters an alternative ALMP program like training before finding regular employment, the spell is also considered as censored at the point in time the individual enters the alternative program. As a sensitivity analysis, we estimate a model with endogenous right-censoring. In this specification, we model transitions to other states than employment as a competing risk. We additionally estimate specifications in which we define periods of training participation as unemployment and models in which we consider participation in training programs as an alternative treatment. For these specifications, unemployment continues during training participation. Moreover, unemployment spells are right censored in case the observation period ends before an exit out of unemployment can be observed.

In case of treatment, we observe the exact moment of the entry into the program and the actual program duration. However, we do not have any information on the planned participation duration in a JCS. In our model specifications, the time spent in a JCS is assumed to contribute to the unemployment duration. Although program participants may search for a job with reduced effort, they still do search, hence they should be treated as unemployed. In our main specification, an unemployment spell is defined as right censored at the moment a second entry into JCSs is observed. We additionally estimate specifications which model the selection into the second treatment and which estimate a corresponding additional treatment effect.

The phrase *job creation schemes* (JCSs) includes all variants of public employment programs, although they are partly conceptually different, as mentioned in Section 2.2.2. As it is unclear whether programs starting after April 1997 are JCSs or productive wage subsidies for regular jobs, we will only use information on program participation that

[11] Due to data restrictions on the local unemployment rates that are included as controls in the analysis, we have to exclude unemployment spells starting before January 1991.

[12] We also consider persons as unemployed if they indicated to be in a training program for at most 1 month.

started before April 1997 and treat entries after April 1997 as right censored. The baseline specification of the analysis excludes participants in Mega-JCSs, identified as those individuals who enter the program directly after employment. We find a high concentration of Mega-JCSs in Sachsen-Anhalt in the early 90s. In 193 cases a direct transition from employment to a JCS can be observed. In a sensitivity analysis we are going to investigate the effects of both traditional and Mega-JCSs.

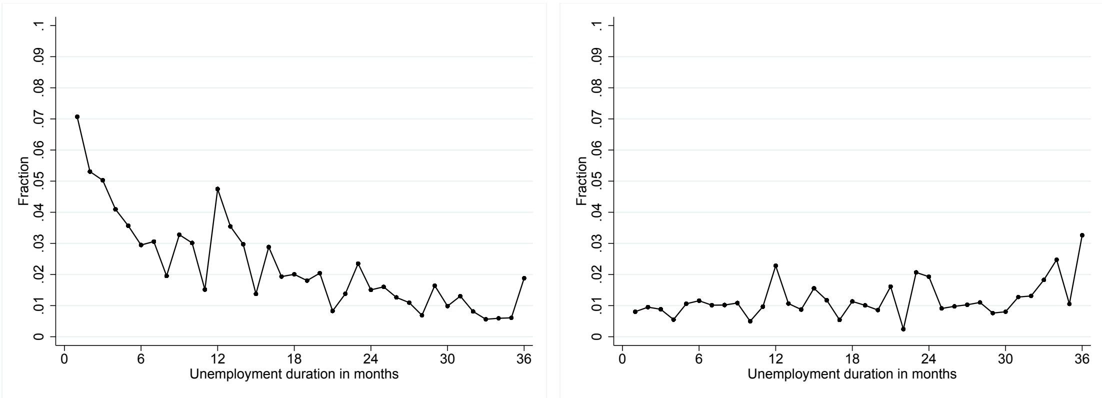
2.3.2 Labor Market Transitions and Durations

We observe that around 11% of the unemployment spells include a period of participation in a JCS (see Table 2.A.3 in Appendix 2.A). About half of the spells without treatment and 26% of the spells with treatment end in a transition into regular employment. 23% of the unemployment spells that are observed to include participation in a JCS are followed by a period of participation in a training program and 16% are followed by a second participation in JCSs. For 23% no transition can be observed within the observation period.

Figures 2.2 and 2.3 present non-parametric Kaplan-Meier estimates of the hazard rates based on information of the first unemployment spell. Figure 2.2 Panel (A) contains the empirical exit rate from unemployment into regular employment which is highest at the beginning of the unemployment spell and then starts to decline. After a second peak at an unemployment duration of 12 months which could be caused by the expiration of unemployment benefits, the exit rate circulates around 1.5%. The conditional probability of entering a JCS (Panel (B)) increases to a level of around 2.3% after one year of unemployment. In the subsequent period the hazard rate has a slight positive trend. Figure 2.3 shows the empirical exit rate from the program to unemployment (Panel (A)) and employment (Panel (B)), respectively. Both plots reveal strong peaks at 12 and 24 months indicating that a substantial share of participants re-enters unemployment and some participants enter employment directly after the program has expired.[13] Moreover, Panel (A) in Figure 2.3 shows that the transition rate to employment during program participation is - especially during the first 11 months - rather low, which suggests that a vast majority stays in the JCS until the end of the contract.

A JCS typically lasts 12 months. Panel (A) in Figure 2.4 shows that around 40% of all JCSs end after one year and only a few last longer than 24 months. The peaks at 12 and 24 months indicate that many individuals exploit the program to the full extent

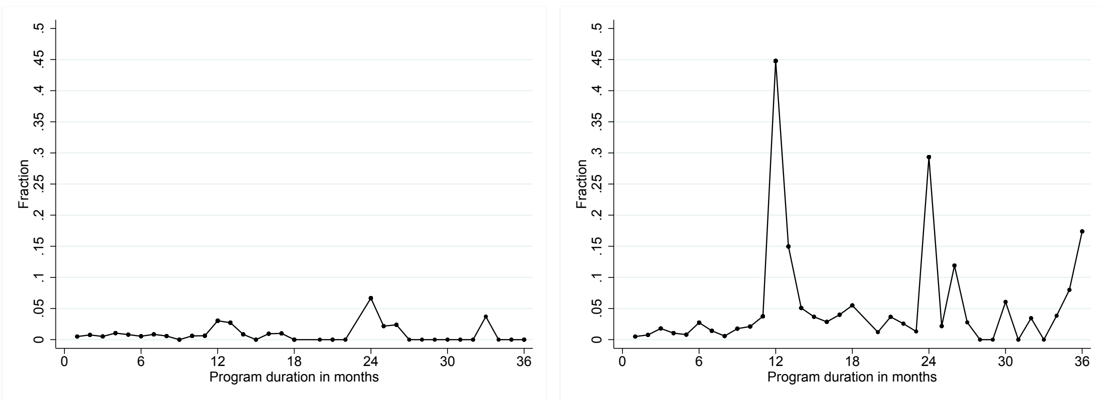
[13]Table 2.A.4 in Appendix 2.A presents summary statistics of the duration of unemployment, of subsequent employment and of JCSs separately for spells that end with a transition to employment, spells that end with a transition to another labor market state and right censored spells due to the end of the observation period.



(A) Transition from unemployment to employment *(B) Transition from unemployment to JCSs*

Notes: JCS: Job creation scheme. Empirical exit rates are based on the first unemployment spell.
Source: LMM-SA, 1997-1999, own computations.

Figure 2.2: Transition from unemployment to employment and to JCSs

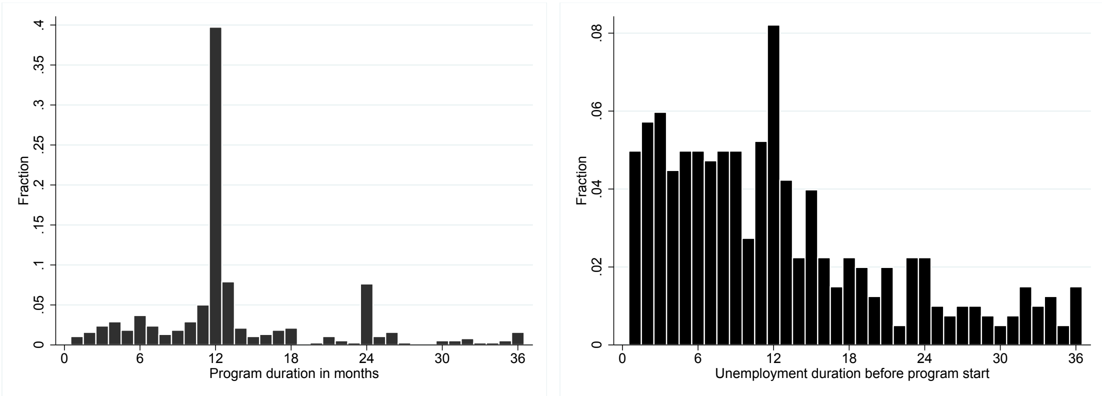


(A) Transition from JCS to employment *(B) Transition from JCS to unemployment*

Notes: JCS: Job creation scheme. Empirical exit rates are based on the first unemployment spell.
Source: LMM-SA, 1997-1999, own computations.

Figure 2.3: Transition from JCS to (un)employment

which can be interpreted as a sign of locking-in effects. Panel (B) in Figure 2.4 displays the distribution of the moment of the program start in the sample of treated individuals. While the largest share of individuals enters the program after exactly 12 months in unemployment (around 8% of all program participants), we have rather equal shares of entries ranging from 3% to 6% in the first 11 months. The distribution of program timing and the transition probabilities from unemployment to JCSs over time shown in Figure 2 underline that we face a dynamic treatment setting with possible entries over the full unemployment spell.



(A) Distribution of program duration *(B) Distribution of unemployment duration before program start*

Notes: The distribution of program durations (Panel (A)) is based on treated individuals for whom the duration is completely observable.
Source: LMM-SA, 1997-1999, own computations.

Figure 2.4: Distribution of program duration and unemployment duration before program start

2.3.3 Descriptive Statistics of Observable Characteristics

Table 2.3 shows descriptive statistics of the observed covariates for all individuals and separately for program participants and nonparticipants. The values of these variables are constant over the observation period. They are measured at the date of the interview with the exception of age which refers to the year 1990.

About half of the unemployed in our sample are women. 60% of the unemployed that participate in a JCS during their first unemployment spell are female. In total, the largest fraction of unemployed individuals is between 45 and 50 years old in 1990. One third of the program participants are 45 to 50 years old.

Furthermore, we include a set of dummy variables indicating the professional education of the unemployed. The comparatively small number of individuals without

Table 2.3: Descriptive statistics of covariates in %

	All	Participants	Nonparticipants
Ages 25-29	18.1	11.1	19.1
Ages 30-34	19.8	20.6	19.7
Ages 35-39	21.5	18.1	22.0
Ages 40-44	15.7	17.1	15.5
Ages 45-50	24.9	33.1	23.7
Female	52.3	59.6	51.2
Male	47.7	40.4	48.8
No Vocational Training	1.7	4.2	1.3
Partly vocational training	2.2	3.1	2.1
Vocational training	50.9	49.8	51.1
Advanced vocational training	7.3	4.9	7.6
Technical college	15.7	16.0	15.7
University degree	22.2	22.0	22.2
Total	**2,235**	**287**	**1,948**

Notes: Descriptive statistics are based on the first unemployment spell. The highest professional education level is measured at the interview date. Age is measured in January 1990.
Source: LMM-SA, 1997-1999, own computations.

or with partly vocational training arises from the obligation to perform a vocational training (*Berufsbildungspflicht*) in the former GDR. Table 2.3 shows that about half of the unemployed individuals have achieved a vocational training and one-fifth exhibit a university degree. The share of unemployed with a high professional education is similar for participants and for nonparticipants.[14]

In addition, the list of covariates included in our estimations contains year and quarter dummies, regional dummies, monthly unemployment rates by labor market district, and a time-varying variable capturing the distance from the expiration date of unemployment benefit claims.[15]

[14]The highest professional education level is measured at the day of the interview. For 14 individuals in our sample we observe an unemployment spell before individuals reenter the educational system. For those individuals we adjust the educational level measured at the time of the interview by the time spent in the educational system since the corresponding unemployment spell. In a robustness check we exclude those individuals from the estimation sample. The results do not change.

[15]The unemployment rates are corrected for the number of participants in ALMP programs and hence are larger than the official numbers.

2.4 Empirical Model

We are interested in the causal impact of entering a JCS on the unemployment duration and subsequent employment stability. Individuals are defined to be treated if they enter a JCS in month t of the unemployment spell, from the corresponding month t onwards. In this section we start with the presentation of a bivariate duration model for the duration until leaving unemployment for a job and the duration until the treatment following the timing-of-events approach (Abbring and van den Berg, 2003).[16] We have monthly information about different employment states and estimate discrete time duration models. Abbring and van den Berg (2003) provide a proof for continuous time models (for identification in dynamic discrete models see Heckman and Navarro, 2007). In a second step we investigate the subsequent employment stability of program participants and nonparticipants by introducing a third transition process similar to van den Berg and Vikström (2014). In addition, we estimate models allowing for a random treatment effect following Richardson and van den Berg (2013) and models with two treatments (JCSs and training), whereby we allow the probability of entering one treatment to depend on the participation in another treatment. None of these model extensions leads to different results. Therefore, in the following we focus on the description of our main econometric model.

Our data set contains multiple unemployment spells for some individuals, which facilitates identification and estimation of the joint distribution of the unobserved heterogeneity variables (Honoré, 1993). Moreover, our data set includes time-varying variables such as the local unemployment rate which provide a more robust source of identification than time-invariant covariates (Gaure et al., 2007). Lagged time-varying variables act as exclusion restrictions. They have an impact on the survival probability until a time t, but conditional on observed and unobserved characteristics lagged values do not have any impact on the transition probabilities in t. Intuitively, individuals with the same observed characteristics x in period t but different values of lagged time-varying variables should only have a different transition probability if the composition with respect to unobserved heterogeneity is different. This has been pointed out earlier by for example Eberwein et al. (1997), and Brinch (2007) provides a theoretical discussion of identification. Brinch (2007) shows that in the presence of time-varying covariates mixed hazard rate models are identified without the proportional hazard rate assumption.

[16] For a detailed discussion of the advantages and disadvantages of the approach, see van den Berg and Vikström (2014).

2.4.1 Durations until Employment and until Treatment

The transition probability of leaving unemployment for a job $\theta_u(t)$ and the probability of entering a JCS $\theta_p(t)$ are assumed to vary with observed characteristics x_t, the unobserved heterogeneity terms v_u and v_p, respectively and the elapsed unemployment duration t. Additionally, the probability of leaving unemployment depends on the treatment status in period t. We assume that the unobserved heterogeneity is constant over time, i.e. across repeated spells of unemployed individuals, and uncorrelated with observed characteristics. $\theta_u(t)$ and $\theta_p(t)$ can be expressed by complementary log log specifications:

$$\theta_u(t|x_t', v_u, t_p) = 1 - \exp(-\exp(\lambda_u(t) + x_t'\beta_u + \mathbf{1}(t \geq t_p)\delta_u + v_u) \quad (2.1)$$

$$\theta_p(t|x_t', v_p) = 1 - \exp(-\exp(\lambda_p(t) + x_t'\beta_p + v_p)) \quad (2.2)$$

$\lambda_u(t)$ and $\lambda_p(t)$ capture the duration dependencies and the vectors β_u and β_p capture the influence of observed covariates. δ_u corresponds to the effect of being treated on the probability of finding a job. The treatment effect might vary depending on the time since the treatment. In our baseline model, we allow for a time-varying treatment effect by specifying two intervals following the start of the treatment in period t_p: $(t_p \leq t \leq t_p + c_1)$ and $(t > t_p + c_1)$. The hazard rate is shifted by δ_{u_1} in the first c_1 months after program start. After a program duration of c_1 months, the hazard rate is shifted by δ_{u_2}. We additionally estimate models with more than two time intervals for the treatment effect, models allowing for effect heterogeneity with respect to selected observed characteristics, and models with treatment effects depending on the point in time the treatment starts.

As described in Subsection 2.2.3, eligibility for program participation requires in principle at least 3-6 months of unemployment experience. These rules have not been applied strictly in East Germany, and, consequently, we observe in our data quite some transitions into the JCS although individuals have not been eligible with respect to the criterion based on the previous unemployment duration (53 cases). Nonetheless, these rules might introduce shifts in the hazard rates depending on the eligibility. Therefore, we include two additional dummy variables indicating the non-eligibility according to these rules. The first dummy variable is relevant for the years 1991 and 1992. From 1993 onwards, depending on the program scheme, individuals should have been at least 6 months unemployed in the previous 12 months or their current unemployment spell should have lasted for at least 3 months. We include a second dummy variable for this period which is one if the stricter criterion of 6 months is not fulfilled.[17]

[17]We additionally estimate models with two dummies for the period from 1993 onwards, to capture the differential rules for the two programs, and we allow for some observed characteristics to have differential effects on the transition probability to JCSs if individuals are not eligible based on the unemployment

For identification – similar to alternative micro-econometric approaches like matching – it is important that the unemployed job seekers do not anticipate the exact moment a JCS starts. This no-anticipation assumption implies that the future realization of the moment of entry into treatment does not affect the probability of leaving unemployment for a job before the moment the treatment starts.[18] It is likely that this assumption holds in our context. As discussed in Subsection 2.2.3, the case worker decides about participation. He or she has to place his candidates as early as possible and has to check potential alternative job offers. Moreover, the gap between program admission and actual start of the program is rather small. Surveys among caseworkers indicate that the time span between the application of an employer for funding of the JCS and the actual program start was on average three months (Völkel, 1994). Hence, the time span between the point in time the individuals are informed about program start and actual program start should be on average less than three months. The no-anticipation assumption implies that individuals do not adjust their behavior once they received this information. Richardson and van den Berg (2013) argue that in a context such as ours where the potential anticipation span is relatively short compared to the duration of the program (or if the anticipation effects are small) the estimates are relatively insensitive to this assumption. Note that our framework allows for ex-ante effects that is ex ante knowledge of the existence of the program and ex ante knowledge of the individual distribution of time to treatment. One important determinant of the program participation might be the expiration date of benefit entitlements, since benefit claims can be prolonged by participation in a JCS. We are able to account for this mechanism by constructing a variable capturing the distance until the individual expiration date. Moreover, our approach takes into account that the treatment could be given at a later point in time.

It should be pointed out that the treatment effect is defined within an environment where the ALMP is present. Consequently, as a common shortcoming of microeconometric ALMP evaluation studies, we cannot capture equilibrium effects (see Crépon and van den Berg, 2016), i.e. we do not focus on how nonparticipants are affected by the program (for example spillover effects) and how the effect of participants is influenced by the existence of the program. This partial approach is motivated by the idea that only if we find positive long-run effects in this framework, the program can potentially pass a

criterion. The latter specification captures potential non-proportionalities due to the eligibility rules in a flexible way. These alternative specifications do not change our main results (see Tables 2.C.3 and 2.C.4 in Appendix 2.C).

[18]It is important to note that the no-anticipation assumption does not exclude that individuals know the probability distribution of future events conditional on observable and unobservable characteristics. Individuals may change their optimal behavior to determinants of the treatment process, but not to the realizations of future treatments.

cost-benefit analysis where also equilibrium effects are considered.

2.4.2 Employment Stability

We additionally investigate the impact of the treatment on the subsequent employment stability. The transition probability from employment to unemployment $\theta_e(t)$ can be expressed by:

$$\theta_e(t|x_t', v_e, t_u, t_p) = 1 - \exp(-\exp(\lambda_e(t) + x_t'\beta_e + \mathbf{1}(t_u \geq t_p)\delta_e + \gamma_1 t_u + \gamma_2 t_u^2 + v_e)) \quad (2.3)$$

$\lambda_e(t)$ captures the duration dependence in employment. The probability of reentering unemployment depends on observed characteristics x_t, unobserved heterogeneity v_e, and on the realized unemployment duration t_u. The unobserved characteristics are allowed to be correlated with the unobserved factors v_p and v_u. The treatment effect δ_e captures the impact of program participation during the previous unemployment spell.

2.4.3 Distribution of Unobserved Heterogeneity and Likelihood Function

We specify the joint distribution G of the unobserved heterogeneity terms v_u, v_p and v_e to be discrete with M support points. The associated probabilities are given by:

$$Pr(v_u = v_u^m, v_p = v_p^m, v_e = v_e^m) = p_m,\ for\ m = 1, ..., M. \quad (2.4)$$

To force the class probabilities to be between zero and one and to sum up to one, we use a multinomial logit parameterization of the class probabilities

$$p_m = \frac{\exp(\omega_m)}{\exp(\sum_{m=1}^{M}\omega_m)}\ with\ \omega_1 = 0,\ for\ m = 1, ..., M. \quad (2.5)$$

For a model with $M = 2$, G would be described by 4 parameters, for $M = 3$ we estimate 8 parameters, etc. This approach allows for a flexible covariance matrix for the unobserved heterogeneity. For similar strategies for modeling the unobserved heterogeneity, see for example Murray (1999), Crépon et al. (2010) and Caliendo et al. (2016). Our model selection with respect to the number of mass points is based on the bivariate duration model. We increase the number of mass points until we cannot improve the model fit anymore. The evaluation of the model fit is based on the Akaike Criterion (AIC). The likelihood contribution of individual i for given v_u^m, v_p^m, v_e^m in period t can be expressed by $l_{it}(x_{it}, v_u^m, v_p^m, v_e^m)$ and the Log-likelihood for the sample with N individuals is given by:[19]

$$\ln L = \sum_{i=1}^{N} \ln\left(\sum_{m=1}^{M} p_m \prod_{t=1}^{T} [l_{it} | x_{it}, v_u^m, v_p^m, v_e^m] \right) \tag{2.6}$$

2.5 Results

We start with presenting the results based on a bivariate duration model consisting of the duration until entry into a JCS and the duration until entry into employment. In the baseline specification, we specify a treatment effect for the first 11 months after the start of the JCS and for the period from 12 months onwards after the start of the JCS. The choice of this cut-off value is motivated by the typical program duration of 12 months and allows us to investigate potential locking-in effects.[20]

In a second step we introduce effect heterogeneity with respect to the point in time the treatment starts, the elapsed treatment duration and selected observed characteristics. In a third step we present results for a model with three equations: the transition rate from unemployment into the program, from unemployment into employment and from employment back into unemployment. Fourth, we estimate models with a second treatment, the participation in training programs. Next, we allow for effect heterogeneity with respect to unobserved heterogeneity (random treatment effects) and investigate the sensitivity of our results with respect to different choices about the definition of the sample, the unemployment state and about the way we deal with repeated spells of unemployment and shared unobserved heterogeneity. Finally, we investigate whether a model allowing for endogenous right-censoring leads to different results.

2.5.1 Baseline Results

We start with a discussion of the model selection in terms of the number of mass points for the heterogeneity components. This selection is based on a comparison of the model fit. We increase successively the number of mass points until we cannot

[19] For the models allowing for random treatment effects we introduce an additional unobserved term v_{jcs}^m. For models taking a second treatment (training participation) into account the model is extended by an additional transition rate from unemployment to training, which depends on the unobserved term v_t^m.

[20] This choice is linked to the construction of our dependent variable. The dependent variable for a transition from unemployment to employment equals one in the last month of the unemployment spell if an individual starts working in the next month. In this way, we are able to estimate the probability for a transition from unemployment to employment in every month of the unemployment spell. This means if a direct transition to regular employment occurs after 12 months of participation in a JCS, the dependent variable equals one in month 12 after program start. Hence, the specification of the treatment effect for the first 11 months and after 11 months after program start captures the potential locking-in effects of a JCS with a typical duration of 12 months.

improve the model fit, evaluated on basis of the Akaike Information Criterion (AIC) and the Bayesian Information Criterion (BIC), anymore. In our application, the smallest value of the AIC and the BIC is reached in the specification with three unobserved mass points (see Table 2.B.1 in Appendix 2.B). In comparison to the model without unobserved heterogeneity, we estimate in this specification six additional parameters for the distribution of unobserved heterogeneity.[21] For the different transition processes we choose a flexible specification of the duration dependence based on eight time intervals.

In Table 2.4 we present the parameter estimates for the baseline model. The correlation between the unobserved heterogeneity terms v_p and v_u is -0.43. This negative correlation implies that we have a negative selection into treatment based on unobservables: individuals that are more likely to participate in a JCS, are in general less likely to find a job. However, with a standard error of 0.49 this negative correlation is not statistically significant from zero.

The main parameters of interest are the treatment effects of entering a JCS depending on the time since program start. In the first 11 months since start of the program, the estimated coefficient is with -1.23 negative and significant. This effect states that the transition rate to employment is reduced by 71% $(\exp(-1.23)-1)$ in the first 11 months after start of participation. From month 12 after program start onwards, the treatment effect vanishes. This result indicates that locking-in effects seem to be important. During program participation, the job finding probability is significantly reduced and after a typical program duration of 12 months the effect becomes positive but insignificant. This positive but insignificant effect after 12 months might capture both the post-program effect and the negative locking-in effect for those whose contract has not been expired yet. It points into the direction that JCSs can be beneficial for the participants.

As laid out in Section 2.4.1 our approach does not take into account equilibrium effects. Recent papers have shown that spillover effects can for example be important in the context of job search and training programs, see Crépon et al. (2013), Ferraci et al. (2014) and Gautier et al. (2017). Helping some job seekers in their search for jobs through specific programs might decrease the probabilities of those unemployed who do not participate in the program. Crépon et al. (2013) provide evidence that these spillover effects are larger in weak labor markets with high unemployment rates. Given the scale of the JCSs and the state of the economy during our observation period, the estimated effects might at least partly reflect spillover effects. This might be especially relevant

[21] Figure 2.B.1 in Appendix 2.B presents the empirical exit rate from unemployment to work during the first unemployment spell jointly for program participants and nonparticipants and additionally the predicted monthly transition rates based on the estimated parameters. The predicted hazard rate fits well with the average of the empirical hazard rate and does a good job of describing the duration dependence.

Table 2.4: Baseline estimation results

	Transition $U \rightarrow E$		Transition $U \rightarrow JCS$	
Effect of JCS in months 1 - 11 after program start	-1.23***	(0.21)		
Effect of JCS in months > 11 after program start	0.23	(0.23)		
Not eligible for JCS 1991-1992			0.36	(0.28)
Not eligible for JCS 1993-1999			-0.65**	(0.26)
Unobserved heterogeneity:				
v_1	-4.81***	(0.62)	-7.46***	(1.16)
$v_2 - v_1$	1.67***	(0.19)	-0.44	(0.30)
$v_3 - v_1$	3.26***	(0.26)	-0.12	(0.37)
ω_2	0.34	(0.30)		
ω_3	-0.62**	(0.27)		
p_2	0.48			
p_3	0.18			
corr(v_u,v_p)	-0.43	(0.49)		
Duration dependence:				
4-6 months	0.03	(0.08)	0.18	(0.20)
7-9 months	-0.04	(0.10)	0.39	(0.25)
10-12 months	-0.03	(0.12)	0.69***	(0.25)
13-18 months	-0.08	(0.13)	0.43	(0.27)
19-24 months	-0.39**	(0.16)	0.31	(0.31)
25-36 months	-0.52***	(0.20)	0.24	(0.31)
> 36 months	-0.66***	(0.23)	-0.70*	(0.38)
Individual characteristics:				
Ages 30-34	0.07	(0.16)	0.20	(0.32)
Ages 35-39	0.09	(0.17)	0.50	(0.31)
Ages 40-44	-0.14	(0.18)	0.44	(0.31)
Ages 45-50	-0.40**	(0.19)	0.67**	(0.32)
Ages > 50	-1.16***	(0.20)	0.89***	(0.34)
Female	-1.10***	(0.08)	-0.08	(0.11)
Partly vocational training	-0.41	(0.39)	-0.23	(0.47)
Vocational training	1.01***	(0.30)	-0.02	(0.35)
Advanced vocational training	1.00***	(0.34)	-0.13	(0.42)
Technical college	0.97***	(0.32)	0.12	(0.37)
University degree	1.17***	(0.31)	0.37	(0.36)
Dummy for remaining unemployment benefit claims	-0.23**	(0.09)	-0.66***	(0.17)
Remaining unemployment benefit claims	-0.03***	(0.01)	0.01	(0.01)
Unemployment rate	-0.01	(0.01)	-0.02	(0.03)
N		2,235		

Notes: JCS: Job creation scheme, U: Unemployment, E: Employment. Standard errors are in parentheses. Current year, quarter dummies and regional dummies are not reported. Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: LMM-SA, 1997-1999, own computations.

during participation in the JCSs, i.e. when the locking-in effects appear. In contrast to the spillover effects of job search programs, the spillover effects during participation in JCSs are positive for the nonparticipants. As mentioned in Section 2.2.3, participation in JCSs was attractive, not only due to the human capital accumulation but also due to the high wages. This attractiveness leads to a reduction in the job search effort. The resulting reduced job finding probability during program participation might then go along with increasing employment prospects on the regular labor market for nonparticipants. These potential effects are not captured by our econometric approach. But we have to keep in mind that the locking-in effects can at least partly reflect a rearrangement of the job queue. As a consequence these looking-in effects do not necessarily reflect a negative effect of JCSs.

2.5.2 Heterogeneous Treatment Effects

We now consider a more flexible time-varying specification of the treatment effect. In line with the typical program durations of 12 and 24 months and inspired by the peaks in the transition rate from the program participation into employment (see Figure 2.3), we allow for five different treatment effects dependent on the elapsed treatment duration. δ_{u_1} measures the effect of JCSs for the period $t_p \leq t \leq t_p + 11$, δ_{u_2} for the period $t_p + 12 \leq t \leq t_p + 13$, δ_{u_3} for the period $t_p + 14 \leq t \leq t_p + 23$, δ_{u_4} for the period $t_p + 24 \leq t \leq t_p + 25$ and finally δ_{u_5} for the period $t > t_p + 25$.

Very similar to the baseline model, the estimated treatment effects indicate that the hazard rate is significantly lower by 70% in the first 11 months after start of participation (Panel A in Table 2.5). In month 12 and 13, the point estimate is positive but insignificant, followed by an estimated effect close to zero in months 14 to 23 after program start. The hazard rate increases significantly by 271% 24 to 25 months after entering a JCS. After 25 months, the effect is still positive but not statistically significant. These results confirm the presence of locking-in effects: individuals have a significantly reduced job finding probability during participation. The treatment effect becomes positive and partly significant after the JCS has finished, which is typically after 12 or 24 months. However, this positive impact on the transition rate to work is not long-lasting.

In an alternative specification, we estimate treatment effects depending on whether program entry occurs in the years 1991-1992 or in 1993-1997. The results in Table 2.5 Panel B show that the effects of participating in a JCS are stable over time.

We additionally allow the treatment effect to depend on the elapsed unemployment duration at the moment of the program entry. We distinguish between program start in the first 12 months of unemployment and after 12 months of unemployment. Table 2.5

Panel C presents the corresponding treatment effects. The point estimates indicate strong locking-in effects independent of the elapsed unemployment duration. These effects are stronger for participants who enter a JCS after month 12 of their unemployment spell. Unemployed who start participating after one year of unemployment seem to benefit from participation: after a treatment duration of 12 months, they are significantly more likely to find a job compared to nonparticipants.

A potential explanation for this finding is that especially long-term unemployed workers are affected by human capital depreciation and that for them being (re-)attached to the labor market has a relatively large value, while for individuals who just entered unemployment these channels are less relevant, yet.

Table 2.5: Time dependent effect of JCS

	Coefficient	Standard error
Panel A. Effect of JCS dependent on time since program start		
Effect of JCS in months 1 - 11 after program start	-1.22***	(0.21)
Effect of JCS in months 12 - 13 after program start	0.34	(0.32)
Effect of JCS in months 14 - 23 after program start	-0.06	(0.27)
Effect of JCS in months 24 - 25 after program start	1.31***	(0.38)
Effect of JCS in months > 25 after program start	0.47	(0.36)
Panel B. Effect of JCS dependent on year of program start		
Start occurs in year 1991 - 1992		
Effect of JCS in months 1 - 11 after program start	-1.16***	(0.34)
Effect of JCS in months > 11 after program start	0.32	(0.32)
Start occurs in year 1993 - 1997		
Effect of JCS in months 1 - 11 after program start	-1.25***	(0.26)
Effect of JCS in months > 11 after program start	0.18	(0.25)
Panel C. Effect of JCS dependent on elapsed unemployment duration at time of program start		
Start occurs in months 1 - 12 of unemployment		
Effect of JCS in months 1 - 11 after program start	-1.17***	(0.25)
Effect of JCS in months > 11 after program start	-0.01	(0.27)
Start occurs in months > 12 of unemployment		
Effect of JCS in months 1 - 11 after program start	-1.48***	(0.40)
Effect of JCS in months > 11 after program start	0.59**	(0.28)

Notes: JCS: Job creation scheme. This specification includes the same list of covariates as the baseline specification (see Table 2.4) and includes three unobserved mass points (M=3). The number of units making a transition from JCS to employment in the corresponding specification can be found in Table 2.C.1 and 2.C.2 in Appendix 2.C. Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: LMM-SA, 1997-1999, own computations.

In a next step we investigate effect heterogeneity with respect to selected observed characteristics. We estimate the treatment effect in the first 11 months after start of the JCS and the subsequent period and allow for a common shift of both treatment effects depending on the age, the gender and the skill level. For women and high skilled participants we find a significantly positive shift in the treatment effect indicating that these individuals seem to suffer less from locking-in effects and are more likely to find a job after a typical program duration of 12 months than male and low/medium skilled participants (see Table 2.6), whereby the effect for females is statistically significant only at the 10% level. We find no evidence for effect heterogeneity with respect to the age of the participants. A joint test suggests that the effect of JCSs in the first 11 months is significantly negative while the effect 11 months after the start of the program becomes significantly positive for high skilled women. We additionally estimate a model allowing for an interaction effect of being high skilled and being a female. The results are reported in Table 2.B.2 in Appendix 2.B and joint tests based on this specification lead to similar conclusions. High skilled women seem to benefit from the participation in JCSs, while a JCS increases the unemployment duration especially for low- and medium-skilled men. We additionally estimate a model in which we interact the observed characteristics with both time-varying treatment indicators. The results are reported in Table 2.B.3 in Appendix 2.B. For none of the observed characteristics we find significantly different coefficients for the two periods. In line with that, a comparison of the Log-likelihood values suggests that the restricted model is the preferred specification. For females, both coefficients are very similar (0.56 and 0.51). For the high skilled group we find a relatively large and significant coefficient for the first 11 months after program start (1.10). The second coefficient for this group is still positive (0.49), but with a p-value of 0.11 not statistically significant anymore. Similar to the restricted model, a joint test still suggests that the effect of a JCS is significantly positive for high skilled women 11 months after the start of the program.

These results are in line with the theoretical idea that high skilled individuals are particularly affected by depreciation and in turn are able to benefit most from participation in JCSs. Moreover, the results confirm the previous finding in the literature that women might benefit more from participation in JCSs than men.

2.5.3 Subsequent Employment Stability

In this section we report estimation results based on a model with three transition rates: the transition rate from unemployment to employment, from unemployment into the program and from employment back to unemployment. Table 2.7 presents the estimation results for the baseline specification. The estimated treatment effects on the exit rate to

Table 2.6: Effect of JCS dependent on observed characteristics

		Coefficient	Standard error
Effect of JCS in months 1 - 11 after program start		-1.80***	(0.34)
Effect of JCS in months > 11 after program start		-0.31	(0.35)
Effect of JCS	× Female	0.52*	(0.27)
Effect of JCS	× Age > 45	0.01	(0.26)
Effect of JCS	× High skilled	0.68***	(0.26)

Notes: JCS: Job creation scheme. This specification includes the same list of covariates as the baseline specification (see Table 2.4) and includes three unobserved mass points (M=3). The number of units making a transition from JCS to employment in the corresponding specification can be found in Table 2.C.1 and 2.C.2 in Appendix 2.C. Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: LMM-SA, 1997-1999, own computations.

work are quite similar to the results we obtain with the baseline specification with two transition rates. We do not find any evidence for an impact on the employment stability. We also do not find evidence for effect heterogeneity.[22] However, it is important to note that some subgroups become rather small because only around 50% of the spells end in employment. Therefore, the results with respect to the effect heterogeneity have to be interpreted with caution.

Table 2.7: Effect of JCS for model with subsequent employment stability

	Transition $U \rightarrow E$		Transition $E \rightarrow U$	
	Coef.	SE	Coef.	SE
Effect of JCS in months 1 - 11 after program start	-1.20***	(0.21)	0.28	(0.25)
Effect of JCS in months > 11 after program start	0.17	(0.21)	0.00	(0.27)

Notes: JCS: Job creation scheme, U: Unemployment, E: Employment, Coef.: Coefficient, SE: Standard error. This specification includes the same list of covariates as the baseline specification (see Table 2.4) and includes three unobserved mass points (M=3). Additionally, we control for the previous unemployment duration and the previous unemployment duration squared for the transition rate from employment to unemployment. Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: LMM-SA, 1997-1999, own computations.

[22] Results of the models with effect heterogeneity are reported in Appendix 2.C (see Tables 2.C.5 and 2.C.6).

2.5.4 Multiple Treatment Effects

In this subsection we present estimation results of a model specification which allows for multiple treatment effects: we investigate the impacts of two treatments, participation in a JCS and participation in training. To be precise, we estimate three transition rates: the transition rate from unemployment to employment, the transition rate from unemployment to a JCS and the transition rate from unemployment to training. We allow for correlations between these three transition processes. With this specification we are able to test whether our previous results change when we take into account that some unemployed might participate in a training program before or after participation in a JCS. Our data suggests that 8% of all unemployment spells include treatment only in terms of participation in a JCS and 19% only in terms of participation in training. For 2% of all unemployment spells we observe a participation in a JCS followed by a period of training and 3% had a period of training before entering a JCS.

Table 2.8 presents the estimation results for this model. Panel A shows the results for a specification where we only allow for a direct treatment effect of training and JCSs, separately in the first 11 months and in more than 11 months after start of participation, for the transition rate from unemployment to employment. Panel B presents the results for a specification where we allow for a direct treatment effect of training (JCSs), separately in the first 11 months and in more than 11 months after start of participation, for the transition rate from unemployment to employment and for the transition rate from unemployment into JCSs (training). Our findings indicate that our estimated effects of participating in a JCS do not change when taking participation in training into account. For both programs we observe a reduced impact on the probability of entering the other program during the first 11 months after program start. For the training program, we observe an increased probability for entering the JCS after this period. Moreover, the training program seems to have first a negative and after some time a significantly positive impact on the transition rate to work.[23]

In our main specification, we treat an unemployment spell as right censored at the moment a second participation in a JCS is observed. We additionally estimate a specification where we model the selection into a second JCS and allow for an impact of a second JCS within the same unemployment spell. The estimated treatment effects are not affected by taking multiple treatments into account (see Table 2.C.7 in Appendix 2.C).

[23] This result is in accordance with the results found by Bergemann et al. (2009).

Table 2.8: Multiple treatment effects

	Transition $U \rightarrow E$		Transition $U \rightarrow JCS$		Transition $U \rightarrow training$	
	Coef.	SE	Coef.	SE	Coef.	SE
Panel A. Specification 1						
Effect of JCS in months 1 - 11 after program start	-1.10***	(0.20)				
Effect of JCS in months > 11 after program start	0.19	(0.19)				
Effect of training in months 1 - 11 after program start	-0.71***	(0.14)				
Effect of training in months > 11 after program start	0.89***	(0.15)				
Panel B. Specification 2						
Effect of JCS in months 1 - 11 after program start	-1.22***	(0.21)			-2.02***	(0.37)
Effect of JCS in months > 11 after program start	0.05	(0.21)			0.31	(0.20)
Effect of training in months 1 - 11 after program start	-0.83***	(0.15)	-1.88***	(0.36)		
Effect of training in months > 11 after program start	0.68***	(0.16)	0.47**	(0.20)		

Notes: JCS: Job creation scheme, U: Unemployment, E: Employment, Coef.: Coefficient, SE: Standard error. These specifications include the same list of covariates as the baseline specification (see Table 2.4). The first specification (Panel A) includes four unobserved mass points (M=4) and the second (Panel B) three (M=3). Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: LMM-SA, 1997-1999, own computations.

2.5.5 Sensitivity Analysis

In a first sensitivity analysis we extend on the analysis with respect to the heterogeneity of treatment effects. We additionally allow for a random coefficient for the treatment effect similar to Richardson and van den Berg (2013). In this model, we allow both treatment effects to vary with respect to unobserved heterogeneity. For simplicity, we assume that the random coefficient is the same for the two treatment effects. The random component is allowed to be correlated with the unobserved heterogeneity in the transition rates from unemployment to employment and from unemployment into the treatment. For the model with M=3 we estimate two additional parameters, i.e., we estimate treatment effects which are specific for each of the three latent groups. It turns out that the model fit, evaluated on the basis of the AIC and the BIC, does not improve compared to a model with a homogenous treatment effect. This implies that the model not allowing for random treatment effects is the preferred specification.[24]

We perform several modifications of the unemployment and treatment definition to check the robustness of our results. An overview of the estimated treatment effects for the different specifications can be found in Table 2.9. All these sensitivity analyses are conducted for the bivariate duration model consisting of the duration until treatment and duration until transition to work.

First, we include participants in Mega-JCSs in our analysis. This type of JCS is described in Section 2.2.3. These cases are defined by a direct transition from employment into the program. The data reveal 193 participations in Mega-JCSs. We estimate two different specifications: first, we include an "artificial" month of unemployment only for Mega-JCS participants and second, we extend each unemployment spell by an "artificial" month of unemployment. In both cases, the optimal model specification is based on four mass points and we find a significant negative treatment effect in the first 11 months after program entry. After 11 months after program entry, the effect is still negative but insignificant in the first specification and negative and significant at the 10% level in the second specification. The negative point estimate might stem from longer program durations of Mega-JCSs compared to other types of JCSs: the mean program duration amounts to 25 months and the median to 22 months.

In a further specification we treat unemployment spells that end in a transition into nonemployment as right censored. We observe 17 transitions to nonemployment in our observation period. Again, our results are robust. Moreover, we define periods in training with previous and subsequent unemployment as periods in unemployment.

[24]Results of the model with a random coefficient for the treatment effect are reported in Appendix 2.B (see Table 2.B.4). The Log-likelihood of this model specification amounts to -9,161.2.

As a consequence the number of unemployment spells decreases and the length of unemployment spells increases. The estimated effect of participating in a JCS are very similar to our main specification.

Table 2.9: Sensitivity analysis

	Coefficient	Standard error
Panel A. Inclusion of Mega-JCS specification 1		
Effect of JCS in months 1 - 11 after program start	-1.00***	(0.17)
Effect of JCS in months > 11 after program start	-0.12	(0.20)
Panel B. Inclusion of Mega-JCS specification 2		
Effect of JCS in months 1 - 11 after program start	-1.09***	(0.17)
Effect of JCS in months > 11 after program start	-0.37*	(0.20)
Panel C. Transition to nonemployment treated as right censored		
Effect of JCS in months 1 - 11 after program start	-1.25***	(0.21)
Effect of JCS in months > 11 after program start	0.23	(0.23)
Panel D. Periods in training defined as periods in unemployment		
Effect of JCS in months 1 - 11 after program start	-1.07***	(0.21)
Effect of JCS in months > 11 after program start	0.14	(0.22)

Notes: JCS: Job creation scheme. These specifications include the same list of covariates as the baseline specification (see Table 2.4). The first and the second specification (Panel A and B) include four unobserved mass points (M=4) and the third and the fourth (Panel C and D) three (M=3). Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: LMM-SA, 1997-1999, own computations.

Next, we investigate how the results change if we control for previous unemployment experience and if we assume that the unobserved heterogeneity is not constant over the different unemployment spells. For this we allow for an impact of the previous number of unemployment spells and the cumulated lagged unemployment duration in a flexible way. We include two dummies for the number of previous unemployment spells, one if we observe one previous unemployment spell and another dummy variable indicating whether we observe more than one previous unemployment spell. Additionally, we include four dummy variables for the cumulated lagged time spent in unemployment. In the first model where we assume independent unobserved heterogeneity the model with two discrete groups (M=2) is the preferred specification (see Table 2.C.8 in Appendix 2.C). While we get a slightly more positive view of the treatment effects based on this specification, the overall picture is rather stable. Once we allow for common unobserved heterogeneity across different spells of the same individuals, all dummy variables capturing previous unemployment experience are much smaller and not statistically significant anymore (see Table 2.C.9 in Appendix 2.C). Moreover, the treatment effects are very similar compared to the main specification. Comparing the model fit with the model assuming independent unobserved heterogeneity clearly suggests that the model using multiple unemployment spells is the preferred specification.

The final sensitivity analysis deals with potentially endogenous right-censoring. See for example Cockx and Picchio (2012) for a transition model taking endogenous right-censoring into account. Our main specification is based on the assumption that right-censoring is – conditional on observed characteristics – random. We investigate whether a model allowing for endogenous right-censoring leads to different results. In this specification, we define a residual and absorbing state which includes transitions to training, JCS after April 1997 and to a second JCS in the same unemployment spell, and transitions to education, maternity leave and retirement. We jointly estimate our main specification with the transition process to this residual state. The results are reported in Table 2.B.5 in Appendix 2.B. The estimated treatment effects are stable. Moreover, we do not find evidence that the unobserved heterogeneity terms in the transition process from unemployment to the absorbing state are statistically significant. This suggests that endogenous right-censoring is not a problem for our specification.

2.6 Conclusions

JCSs are used in many countries in order to fight high unemployment. This chapter focuses on JCSs in East Germany during very turbulent economic times, notably the aftermath of the German reunification in 1990. We provide a comprehensive empirical analysis of the employment prospects of participants in JCSs based on data of an inflow sample of unemployed workers in one East German state (Sachsen-Anhalt). We use the timing-of-events approach that is very well suited, particularly given the economic environment of our evaluation. Firstly, we do not need to rely on the informational content of the employment history of individuals in order to be able to control of unobserved heterogeneity and secondly, the focus on transition rates might be more appropriate given the economy is not in a stable equilibrium.

We analyze whether participation in JCSs has an impact on both the probability of finding a job and the subsequent employment stability. The econometric analysis is based on multivariate duration models. We estimate bivariate models based on the transition rates from unemployment to JCSs and from unemployment to work. We also estimate models with three transition processes taking additionally into account the transition rate from employment back to unemployment. Our approach allows to control for selection into treatment based on observable and on unobservable characteristics.

In this study, we focus on individual employment outcomes under the assumption of existence of the program. This is a common approach in the literature on evaluating ALMPs. In the context of JCSs it is important to keep in mind that this approach does not capture the potential value of the work carried out within JCSs. Moreover, this implies that we ignore potential spillover effects on the employment probabilities of the workers who are not participating in JCSs. However, especially in times of massive job destruction and high unemployment rates, the locking-in effects of JCSs might at least partly reflect a rearrangement of the job queue. If participation of some job-seekers has an impact on the job queue in the short run and increases or stabilizes the human capital of the participants in the medium run, negative locking-in effects have to be interpreted with caution. Analyzing these potential spillover effects in combination with locking-in effects is an interesting topic for future research.

In parts our findings are in accordance with results in more stable economies. During the typical participation period of 12 months we find the expected negative effect on the probability of finding a regular job. This effect vanishes thereafter and becomes insignificantly positive. However, in contrast to findings in more stable economies our results suggest that female and highly skilled participants leave unemployment quicker than other groups. This is in line with the idea that high skilled workers might be more strongly affected by human capital depreciation and therefore participation in JCSs has a stronger impact on them. Moreover, it confirms previous findings that women seem to benefit more from JCSs. Another important result which is new to the literature concerns the stability of jobs after participation. We do not find that the job retention rates are influenced by JCSs; not on average and also not if effect heterogeneity is taken into account. Additionally, we find weak evidence that long-term unemployed also gain from participation in JCSs.

These results add to the so far scarce evidence on the effectiveness of JCSs in transformation economies. The findings show that it is important to not transfer the negative evaluation results on JCSs that are found for stable and rather matured economies to situations that are more turbulent. In situations with high job destruction rates other and/or additional labor market groups might benefit from participation in JCSs. It seems likely that this conclusion transfers to other economic crises than the transformation process.

2.Appendix

2.A Data and Descriptive Statistics

Table 2.A.1: Sample selection

Selection criteria	Resulting number of observations
Fully observed labor market history and year of birth	10,715
Aged between 25 and 50 years in January 1990	6,088
Employed in June 1990	5,529
No missings in education variable	5,466
Final sample on individual level:	
Having at least one unemployment spell starting since 1991	2,235

Source: LMM-SA, 1997-1999, own computations.

Table 2.A.2: Description of variables

Variable	Description	Time-varying?
Effect of JCS in months 1 - 11 after program start	Dummy for participating in a JCS in months 1 - 11 after program start	Yes
Effect of JCS in months > 11 after program start	Dummy for participating in a JCS in months > 11 after program start	Yes
Not eligible for JCS 1991-1992	Dummy for not being eligible for participating in a JCS in 1991-1992 according to the Employment Promotion Act (less than 6 months unemployed in last 12 months)	Yes
Not eligible for JCS 1993-1997	Dummy for not being eligible for participating in a JCS in 1993-1997 according to the Employment Promotion Act (less than 6 months unemployed in last 12 months)	Yes
Individual characteristics:		
Age groups	Dummies for being in corresponding age group during observation period	Yes
Ages 25-29	Aged between 25 and 29	Yes
Ages 30-34	Aged between 30 and 34	Yes
Ages 35-39	Aged between 35 and 39	Yes
Ages 40-44	Aged between 40 and 44	Yes

...

Table 2.A.2: Description of variables *(continuation)*

Variable	Definition	Time-varying?
Ages 45-50	Aged between 45 and 50	Yes
Ages >50	Aged 50 and older	Yes
Female	Dummy for being female	No
Professional education	Dummies for highest professional education level	No
No vocational training	No vocational training	No
Partly vocational training	Partly vocational training (*Teilfacherbeiter*)	No
Vocational training	Vocational training (*Facharbeiter*)	No
Advanced vocational training	Advanced vocational training (*Meister, Techniker*)	No
Technical college	Technical college (*Fachschule*)	No
University degree	University degree (*Universität, Fachhochschule*)	No
Regions:	Dummies for living in one of eight labor market districts of Sachsen-Anhalt	No
Dessau	Dessau	No
Halberstadt	Halberstadt	No
Halle	Halle	No
Magdeburg	Magdeburg	No
Merseburg	Merseburg	No
Sangerhausen	Sangerhausen	No
Stendal	Stendal	No
Wittenberg	Wittenberg	No
Year dummies	Dummies indicating the current year, ranging from 1991-1998	Yes
Quarter dummies	Dummies indicating the current quarter of the year	Yes
Unemployment rate	Monthly unemployment rates by labor market districts	Yes
Dummy for remaining unemployment benefit claims	Dummy for months of remaining unemployment benefit claims	Yes
Remaining unemployment benefit claims	Months of remaining unemployment benefit claims	Yes

Notes: JCS: Job Creation Scheme. The time constant explanatory variables are measured at the date of the interview.
Source: LMM-SA, 1997-1999, own computations.

Table 2.A.3: Labor market transitions in %

Stage 1		Stage 2			Stage 3	
3,864 unemployment spells	⇒	JCS	**10.7** (415)	⇒	Employment	**26.3** (109)
					2nd JCS	**15.9** (66)
					Training	**22.9** (95)
					JCS after April 1997	**9.2** (38)
					Education	**0.5** (2)
					Maternal leave	**1.2** (5)
					Retirement	**0.7** (3)
					RC (September 1997)	**13.7** (57)
					RC (October 1998)	**3.9** (16)
					RC (December 1999)	**5.8** (24)
	⇒	Employment	**47.6** (1,643)			
		Training	**23.9** (825)			
		JCS after April 1997	**3.7** (128)			
		Education	**0.3** (12)			
		Maternal leave	**0.3** (11)			
		Retirement	**1.0** (36)			
		RC (September 1997)	**7.1** (245)			
		RC (October 1998)	**3.6** (124)			
		RC (December 1999)	**12.3** (425)			

Notes: JCS: Job creation scheme. RC: Right-censoring due to end of observation period which can be September 1997, October 1998 or December 1999 depending on the wave of the survey. Absolute values are in parentheses. Transitions to training and to JCSs that started after April 1997 are treated as right censored.
Source: LMM-SA, 1997-1999, own computations.

Table 2.A.4: Summary statistics of unemployment spells

				Quantiles				
	N	Mean	SD	Min	25%	50%	75%	Max
No. of spells per individual	2,235	1.7	1.0	1	1	1	2	7
Transition U $\rightarrow$ E								
Unemployment duration	1,752	9.1	10.3	1	2	5	12	85
JCS duration	109	13	9	1	7	12	13	60
Subsequent employment duration (complete spell)	863	17	17	1	5	12	23	104
Subsequent employment duration (right censored spell)	889	38	28	1	13	33	60	107
Transition U $\rightarrow$ Other LMS								
Unemployment duration	1,221	16	15	1	5	12	22	95
JCS duration	209	15	9	2	12	12	16	59
RC due to end of observation period								
Unemployment duration	891	22	23	1	6	14	31	107
JCS duration	97	17	13	1	12	12	22	60

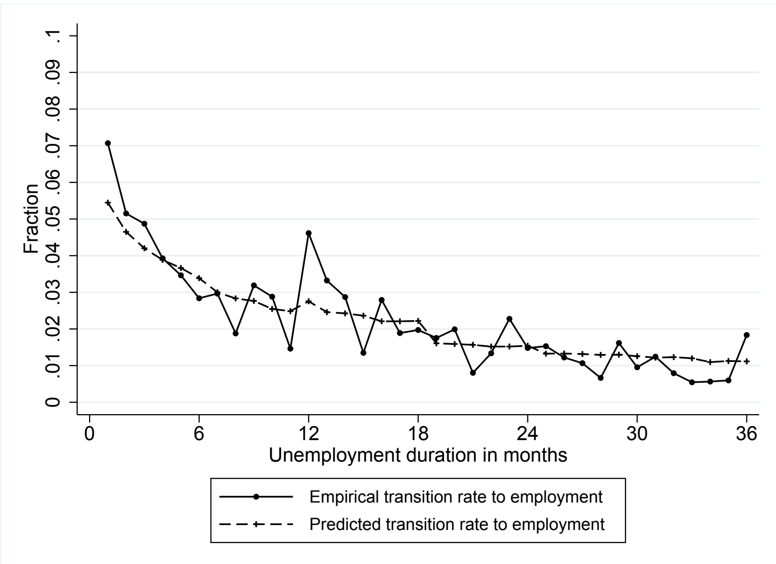
Notes: JCS: Job creation scheme, U: Unemployment, E: Employment, LMS: Labor Market State. RC: Right-censoring. SD: Standard deviation. Complete employment spells end with a transition to another labor market state within the observation period.
Source: LMM-SA, 1997-1999, own computations.

2.B Estimation Results

Table 2.B.1: Model fit depending on the specification of the unobserved heterogeneity

Unobserved heterogeneity (UH)	No UH	2 mass points	3 mass points	4 mass points
Log-likelihood	-9,300.4	-9,178.8	-9,161.3	-9,160.2
AIC	18,764.9	18,527.5	18,498.6	18,502.3
BIC	19,233.3	19,013.0	19,001.3	19,022.1

Source: LMM-SA, 1997-1999, own computations.



Notes: Empirical exit rates are based on the first unemployment spell.
Source: LMM-SA, 1997-1999, own computations.

Figure 2.B.1: Empirical and predicted transition rate to employment

Table 2.B.2: Effect of JCS for high skilled women

			Coefficient	Standard error
Effect of JCS in months 1 - 11 after program start			-1.80***	(0.37)
Effect of JCS in months $>$ 11 after program start			-0.33	(0.36)
Effect of JCS	$\times$ Female		0.63	(0.39)
Effect of JCS	$\times$ High skilled		0.81**	(0.41)
Effect of JCS	$\times$ Female	$\times$ High skilled	-0.34	(0.52)

Notes: JCS: Job creation scheme. This specification includes the same list of covariates as the baseline specification (see Table 2.4) and includes three unobserved mass points (M=3). Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: LMM-SA, 1997-1999, own computations.

Table 2.B.3: Effect of JCS dependent on observed characteristics

		Coef.	SE
Effect of JCS in months 1 - 11 after program start		-2.20***	(0.49)
Effect of JCS in months $>$ 11 after program start		-0.19	(0.37)
Effect of JCS in months 1 - 11 after program start	$\times$ Female	0.56	(0.44)
Effect of JCS in months $>$ 11 after program start	$\times$ Female	0.51	(0.32)
Effect of JCS in months 1 - 11 after program start	$\times$ Age $>$ 45	0.36	(0.44)
Effect of JCS in months $>$ 11 after program start	$\times$ Age $>$ 45	-0.12	(0.31)
Effect of JCS in months 1 - 11 after program start	$\times$ High skilled	1.10***	(0.42)
Effect of JCS in months $>$ 11 after program start	$\times$ High skilled	0.49	(0.30)

Notes: JCS: Job creation scheme, Coef.: Coefficient, SE: Standard error. This specification includes the same list of covariates as the baseline specification (see Table 2.4) and includes three unobserved mass points (M=3). Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: LMM-SA, 1997-1999, own computations.

Table 2.B.4: Effect of JCS dependent on unobserved characteristics

	Coefficient	Standard error
Effect of JCS in months $>$ 11 after program start	1.44***	(0.28)
Effect of JCS	-1.03*	(0.53)
Unobserved heterogeneity in treatment effect:		
$v_2 - v_1$	0.32	(0.30)
$v_3 - v_1$	-0.64**	(0.27)

Notes: JCS: Job creation scheme. This specification includes the same list of covariates as the baseline specification (see Table 2.4) and includes three unobserved mass points (M=3). Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: LMM-SA, 1997-1999, own computations.

Table 2.B.5: Effect of JCS for model with endogenous right-censoring

	Transition $U \rightarrow E$		Transition $U \rightarrow other\ LMS$	
	Coef.	SE	Coef.	SE
Effect of JCS in months 1 - 11 after program start	-1.16***	(0.21)	-1.42***	(0.23)
Effect of JCS in months > 11 after program start	0.30	(0.22)	0.72***	(0.13)

Notes: JCS: Job creation scheme, U: Unemployment, LMS: Labor market state, Coef.: Coefficient, SE: Standard error. This specification includes the same list of covariates as the baseline specification (see Table 2.4) and includes three unobserved mass points (M=3). Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: LMM-SA, 1997-1999, own computations.

2.C Additional Descriptives and Estimation Results

Table 2.C.1: Transitions to employment of JCS participants

	In months 1 - 11 after program start	In months > 11 after program start
All JCS participants	32	77
Female	19	47
Age > 45	13	32
High skilled	20	31
Dependent on year of program start		
Start occurs in year 1991 - 1992	11	29
Start occurs in year 1993 - 1997	21	48
Dependent on elapsed unemployment duration at time of program start		
Start occurs in months 1 - 12 of unemployment	24	44
Start occurs in months > 12 of unemployment	8	33

Notes: JCS: Job creation scheme.
Source: LMM-SA, 1997-1999, own computations.

Table 2.C.2: Transitions to employment of JCS participants dependent on time since program start

In months 1 - 11 after program start	32
In months 12 - 13 after program start	15
In months 14 - 23 after program start	30
In months 24 - 25 after program start	12
In months > 25 after program start	20

Notes: JCS: Job creation scheme.
Source: LMM-SA, 1997-1999, own computations.

Table 2.C.3: Effect of JCS with three not eligible dummies

	Transition $U \rightarrow E$		Transition $U \rightarrow JCS$	
	Coef.	SE	Coef.	SE
Effect of JCS in months 1 - 11 after program start	-1.24***	(0.21)		
Effect of JCS in months > 11 after program start	0.21	(0.23)		
Not eligible for JCS period 1991-1992 (strict criteria)			0.29	(0.28)
Not eligible for JCS period 1993-1999 (loose criteria)			-1.02**	(0.48)
Not eligible for JCS period 1993-1999 (strict criteria)			-0.38	(0.28)

Notes: JCS: Job creation scheme, Coef.: Coefficient, SE: Standard error. This specification includes the same list of covariates as the baseline specification (see Table 4) and includes three unobserved mass points (M=3). The strict eligibility criteria is defined as being unemployed for at least 6 months within the last 12 months. The loose eligibility criteria is defined as being unemployed for at least 3 months. Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: LMM-SA, 1997-1999, own computations.

Table 2.C.4: Effect of JCS with not eligible dummies dependent on observed characteristics

	Transition $U \rightarrow E$		Transition $U \rightarrow JCS$	
	Coef.	SE	Coef.	SE
Effect of JCS in months 1 - 11 after program start	-1.22***	(0.21)		
Effect of JCS in months > 11 after program start	0.24	(0.23)		
Not eligible for JCS 1991-1992			0.53	(0.35)
Not eligible for JCS 1993-1999			-0.44	(0.37)
Not eligible for JCS $\times$ Female			-0.08	(0.29)
Not eligible for JCS $\times$ High skilled			-0.14	(0.30)
Not eligible for JCS $\times$ Age > 45			-0.22	(0.33)

Notes: JCS: Job creation scheme, Coef.: Coefficient, SE: Standard error. This specification includes the same list of covariates as the baseline specification (see Table 4) and includes three unobserved mass points (M=3). Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: LMM-SA, 1997-1999, own computations.

Table 2.C.5: Time dependent effect of JCS for model with subsequent employment stability

	Transition $U \rightarrow E$		Transition $E \rightarrow U$	
	Coef.	SE	Coef.	SE
Panel A. Effect of JCS dependent on time since program start				
Effect of JCS in months 1 - 11 after program start	-1.19***	(0.21)	0.28	(0.25)
Effect of JCS in months 12 - 13 after program start	0.29	(0.31)	-0.82	(0.74)
Effect of JCS in months 14 - 23 after program start	-0.12	(0.25)	0.24	(0.34)
Effect of JCS in months 24 - 25 after program start	1.22***	(0.38)	-0.19	(0.65)
Effect of JCS in months > 25 after program start	0.39	(0.35)	0.13	(0.48)
Panel B. Effect of JCS dependent on year of program start				
Start occurs in year 1991 - 1992				
Effect of JCS in months 1 - 11 after program start	-1.13***	(0.34)	-0.10	(0.43)
Effect of JCS in months > 11 after program start	0.25	(0.32)	0.16	(0.35)
Start occurs in year 1993 - 1997				
Effect of JCS in months 1 - 11 after program start	-1.22***	(0.26)	0.51*	(0.30)
Effect of JCS in months > 11 after program start	0.13	(0.23)	-0.16	(0.35)
Panel C. Effect of JCS dependent on elapsed unemployment duration at time of program start				
Start occurs in months 1 - 12 of unemployment				
Effect of JCS in months 1 - 11 after program start	-1.13***	(0.24)	0.22	(0.30)
Effect of JCS in months > 11 after program start	-0.06	(0.26)	0.00	(0.33)
Start occurs in months > 12 of unemployment				
Effect of JCS in months 1 - 11 after program start	-1.45***	(0.41)	0.48	(0.49)
Effect of JCS in months > 11 after program start	0.54**	(0.27)	-0.04	(0.38)

Notes: JCS: Job creation scheme, U: Unemployment, E: Employment, Coef.: Coefficient, SE: Standard error. These specifications include the same list of covariates as the baseline specification (see Table 4) and include three unobserved mass points (M=3). Additionally, we control for the previous unemployment duration and the previous unemployment duration squared for the transition rate from employment to unemployment. Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: LMM-SA, 1997-1999, own computations.

Table 2.C.6: Effect of JCS dependent on observed characteristics for model with subsequent employment stability

	Transition $U \rightarrow E$		Transition $E \rightarrow U$	
	Coef.	SE	Coef.	SE
Effect of JCS in months 1 - 11 after program start	-1.77***	(0.34)	0.37	(0.43)
Effect of JCS in months > 11 after program start	-0.39	(0.34)	0.05	(0.38)
Effect of JCS × Female	0.51*	(0.27)	0.03	(0.36)
Effect of JCS × Age > 45	0.05	(0.26)	-0.08	(0.35)
Effect of JCS × High skilled	0.60**	(0.26)	-0.11	(0.38)

Notes: JCS: Job creation scheme, U: Unemployment, E: Employment, Coef.: Coefficient, SE: Standard error. This specification includes the same list of covariates as the baseline specification (see Table 4) and includes three unobserved mass points (M=3). Additionally, we control for the previous unemployment duration and the previous unemployment duration squared for the transition rate from employment to unemployment. Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: LMM-SA, 1997-1999, own computations.

Table 2.C.7: Effect of JCS with 2^{nd} participation in JCS

	Transition $U \rightarrow E$		Transition $U \rightarrow JCS$	
	Coef.	SE	Coef.	SE
Effect of JCS in months 1 - 11 after program start	-1.22***	(0.21)		
Effect of JCS in months > 11 after program start	0.25	(0.22)		
2^{nd} participation in JCS	-0.52	(0.45)		
In JCS before			0.91***	(0.19)

Notes: JCS: Job creation scheme, Coef.: Coefficient, SE: Standard error. This specification includes the same list of covariates as the baseline specification (see Table 4) and includes three unobserved mass points (M=3). Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: LMM-SA, 1997-1999, own computations.

Table 2.C.8: Effect of JCS controlling for unemployment history (independent unobserved heterogeneity)

	Coefficient	Standard error
Effect of JCS in months 1 - 11 after program start	-0.89***	(0.29)
Effect of JCS in months > 11 after program start	0.52*	(0.27)
Number of previous unemployment spells:		
2^{nd} unemployment spell	0.22***	(0.04)
3^{rd} - 7^{th} unemployment spell	0.33***	(0.04)
Cumulated lagged unemployment duration:		
13-24 months	-0.41***	(0.10)
25-36 months	-0.81***	(0.14)
37-48 months	-1.16***	(0.19)
> 48 months	-1.63***	(0.24)

Notes: JCS: Job creation scheme. This specification includes the same list of covariates as the baseline specification (see Table 4) and includes two unobserved mass points (M=2). Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: LMM-SA, 1997-1999, own computations.

Table 2.C.9: Effect of JCS controlling for unemployment history (dependent unobserved heterogeneity)

	Coefficient	Standard error
Effect of JCS in months 1 - 11 after program start	-1.23***	(0.22)
Effect of JCS in months > 11 after program start	0.22	(0.23)
Number of previous unemployment spells:		
2^{nd} unemployment spell	0.06	(0.05)
3^{rd} - 7^{th} unemployment spell	0.03	(0.05)
Cumulated lagged unemployment duration:		
13-24 months	0.02	(0.11)
25-36 months	-0.03	(0.17)
37-48 months	-0.02	(0.21)
> 48 months	-0.26	(0.28)

Notes: JCS: Job creation scheme. This specification includes the same list of covariates as the baseline specification (see Table 4) and includes three unobserved mass points (M=3). Coefficients are statistically significant at the *10%, ** 5% and *** 1% level.
Source: LMM-SA, 1997-1999, own computations.

Chapter 3

Unemployment Effects of the German Minimum Wage in an Equilibrium Job Search Model*

3.1 Introduction

Equilibrium job search models have long been used to study labor markets in the presence of search frictions. These frictions create a certain amount of market power for employers, giving them some influence over wages. Understanding the empirical relevance of search frictions therefore provides important insights into which of the two fundamentally different frameworks, the neoclassical or the monopsonistic case, represent the more appropriate view of the labor market. From a policy perspective, knowledge about the "true" labor market structure is of key importance, as the predicted consequences of certain economic policies may substantially differ from those derived from the neoclassical competitive case (Manning, 2003).

In this chapter, we estimate an equilibrium job search model for the German labor market. The German labor market is particularly interesting as it saw the introduction of a statutory uniform minimum wage of 8.50 euros an hour in 2015.[1] Before this

*This chapter is joint work with Maximilian Blömer (ifo), Nicole Gürtzgen (IAB and University of Regensburg), Holger Stichnoth (ZEW) and Gerard van den Berg (University of Bristol, IFAU, IZA, ZEW and CEPR). We thank Andrew Shepard for sharing part of his programming routines with us. This chapter has benefited from comments and suggestions by Richard Blundell, Philipp Dörrenberg, Bernd Fitzenberger, Mario Meier, Fabien Postel-Vinay and Carsten Trenkler. Financial support from the State of Baden-Württemberg (SEEK project 2014) and access to administrative data provided by the IAB are also gratefully acknowledged. Finally, we would like to thank Paul König, Julius Koll, and Pierre Poulon for their excellent research assistance.

[1]While a number of transitional measures respected existing collective agreements and those signed in the meantime, the uniform minimum wage applied to all industries by 2017 at the latest. A further

date, minimum wages had been implemented only in selected industries. Even though monopsonistic labor market structures have frequently been invoked by policy-makers to justify a wage floor, there is surprisingly little empirical evidence on the relevance of search frictions. Given the importance of frictions for the size and even the sign of the (un)employment effects of a minimum wage, this clearly constitutes a major research gap that our study aims to address. By estimating an equilibrium job search model, our analysis not only seeks to gain a better understanding about the relevance of search frictions, but also aims at quantifying the expected labor market effects of a uniform minimum wage.

To assess search frictions and the unemployment effects of a minimum wage, we estimate the wage-posting model by Bontemps et al. (1999), extended to allow for different job offer arrival rates for the employed and the unemployed. The model is well-suited for our purposes: by accounting for heterogeneity in both firms' productivity and workers' reservation wages, it does not restrict the sign of unemployment effects a priori.[2]

Our estimation uses a large administrative data set, the IAB Sample of Integrated Employment Biographies (SIAB). The SIAB is a two per cent random sample of individuals subject to social security contributions during the time period 1975 to 2010. To guarantee a consistent coverage of unemployment spells, we focus on the period 2007–2010. The SIAB data provide an ideal basis for estimating a structural equilibrium search model for several reasons. First and most importantly, the data allow us to precisely measure the duration of different labor market states and transitions between them, notably job-to-job as well as employment-to-unemployment transitions. These transitions are crucial to the identification of the model's central parameters, such as job arrival and destruction rates. Second, as the data are based on employers' notifications to the social security authorities, they are less prone to measurement error than comparable information from survey data. Additional advantages over survey data include the larger sample size and a much lower level of panel attrition. We focus on low- and medium-skilled individuals, as for these groups the assumption of wage posting is most convincing.

In addressing the unemployment market effects of the recent introduction of a uniform minimum wage, our study contributes to the empirical literature on the labor market

transitory exemption was given to those industries where industry-specific minimum wages had already been introduced prior to 2015 via the Posting of Workers Act (*Arbeitnehmerentsendegesetz*). The bargaining parties in an industry subject to this legislation may request that the Federal Ministry of Labour declares its (minimum wage) agreement to be generally binding for the entire industry.

[2] Engbom and Moser (2017) use a similiar wage-posting model, extended for heterogeneity in workers' ability, to study the role of the minimum wage in the decline of earnings inequality in Brazil. A closely related literature assumes wage bargaining instead of wage posting; applications of these models to analyze minimum wage effects include Flinn (2006) and Breda et al. (2016).

effects of minimum wages in Germany. Most studies so far have evaluated the industry-specific minimum wages, typically based on difference-in-differences designs. In what is probably the first quasi-experimental study for Germany, König and Möller (2009) analyze the introduction of a minimum wage in the construction industry. The authors find no significant employment effects in West Germany and small negative effects in the East, where the minimum wage has greater bite. In 2011, the German Federal Ministry of Labour commissioned an evaluation of minimum wages in several industries. In general, these studies also tend to find limited employment effects (e.g., Boockmann et al., 2013 and Frings, 2013), with the exception of the roofing industry (Aretz et al., 2013). While these reduced-form approaches provide valuable insights into the labor market effects of industry-specific policies, they are neither informative about the underlying transmission mechanisms nor are they able to assess the economic impacts of different minimum wage levels. The few available structural studies for Germany have relied on estimates of labor demand functions under the assumption of perfect competition (Ragnitz and Thum, 2008; Bauer et al., 2009 and Knabe and Schöb, 2009). In this framework, the effects of a minimum wage can by construction only be zero (if the minimum wage is not binding) or negative (see the critique by Fitzenberger, 2009). The strong negative effects reported by some of these studies appear at odds with the quasi-experimental evidence, which underscores the need for a richer structural model that allows for a wide range of employment effects.

Our own simulations show that unemployment is a non-monotonic and non-linear function of the minimum wage level. A minimum wage equivalent in real terms to the one actually introduced in 2015 raises the unemployment rate by two percentage points, a sizeable increase of 18% in relation to the old steady-state value. We also document significant heterogeneity in search frictions and in reservation wages across labor markets differentiated by region and type of occupation.

The remainder of the chapter is structured as follows: Sections 3.2 and 3.3 start by giving a brief overview of the model. Section 3.4 provides a description of the data set and the construction of our main variables of interest, and Section 3.5 presents descriptive statistics. Section 3.6 outlines the estimation procedure. Section 3.7 presents the estimation results and graphical representations of the key steady-state relationships. Section 3.8 shows simulation results for the counterfactual introduction of different minimum wage levels. Section 3.9 concludes.

3.2 Theoretical Overview

Equilibrium job search models provide a framework in which the wage offer distribution that workers face in their search emerges as the equilibrium of a non-cooperative wage search and wage posting game between workers and employers. A minimum wage policy alters the wage offer distribution, thereby affecting the number of firms that continue to operate in the market and increasing the average wage offer that an unemployed person can expect to receive. A number of studies have estimated different variants of equilibrium job search models building on the Burdett-Mortensen framework with on-the-job search (e.g. Bowlus et al., 1995; Bowlus et al., 2001 and van den Berg and Ridder, 1998).

A drawback of the Burdett-Mortensen model is that it generates a strictly increasing wage offer density. This has led researchers to shift the emphasis towards models incorporating heterogeneity in firm productivity. Firm heterogeneity has been shown to improve the fit of the wage distribution and has been modelled in different ways in the literature. While Eckstein and Wolpin (1990) assume a log-normal distribution, Bowlus et al. (1995), Bowlus et al. (2001) and Bunzel et al. (2001) allow for a discrete number of firm types. Bontemps et al. (2000) allow for a continuous distribution but estimate it non-parametrically. In the context of a minimum wage policy, heterogeneity in firm productivity is of key importance, as in the case of homogeneous firms a minimum wage would create a "knife-edge" impact on employment, with all firms either leaving or staying in the market. In addition to incorporating heterogeneity in firm productivity, the model by Bontemps et al. (1999) also allows for heterogeneity in workers' reservation wages. This creates more flexibility in terms of the predicted employment effects. In particular, it implies that the minimum wage can, in principle, even have a positive effect on employment. This may be driven by a higher acceptance rate of job offers because the minimum wage precludes low wage offers, it draws more unemployed workers with high reservation wages into the market. In the absence of a minimum wage, these workers have to wait longer for a wage offer that is acceptable to them, as firms by assumption cannot make wage offers conditional on individuals' reservation wages.

3.3 Model Description

In this section, we provide a brief description of the model by Bontemps et al. (1999), which we will extend by allowing the job offer arrival rate to differ across employed and unemployed individuals, as in Shephard (2017) and Engbom and Moser (2017). We start by describing firms' and individuals' strategies. Individuals maximize their expected steady-state discounted future income. They are characterized by heterogeneous opportunity costs of employment denoted by b, which may include search costs and unemployment benefits. The distribution of b is denoted by H, assumed to be continuous over its support $[\underline{b}, \overline{b}]$. Job offers arrive at constant rate $\lambda_0 > 0$ ($\lambda_1 > 0$) for the unemployed (employed) and are characterized by a draw from a wage offer distribution F with support $[\underline{w}, \overline{w}]$. Layoffs arrive at constant rate δ. Unemployed individuals searching for a job face an optimal stopping problem, the solution to which consists in accepting any wage offer w such that $w > \phi$. Employed individuals, in contrast, accept any wage offers strictly greater than their present wage contract. As in Bontemps et al. (2000), the reservation wage is implicitly defined as

$$\phi = b + (\kappa_0 - \kappa_1) \int_{\phi}^{\overline{w}} \frac{\overline{F}(x)}{1 + \frac{\rho}{\delta} + \kappa_1 \overline{F}(x)} dx, \tag{3.1}$$

where ρ denotes individuals' discount rate, $\overline{F}(x) = 1 - F(x)$, and $\kappa_i = \frac{\lambda_i}{\delta}$, $i = 0, 1$. The distribution of reservation wages, A, is then given by

$$A(\phi) = H\left(\phi - (\kappa_0 - \kappa_1) \int_{\phi}^{\overline{w}} \frac{\overline{F}(x)}{1 + \frac{\rho}{\delta} + \kappa_1 \overline{F}(x)} dx\right). \tag{3.2}$$

Equating equilibrium flows into and out of unemployment, the fraction of unemployed with a reservation wage no larger than ϕ for $\phi \leq \underline{w}$ is represented by

$$uA_u(\phi) = \frac{1}{1 + \kappa_0} A(\underline{w}). \tag{3.3}$$

For $\phi > \underline{w}$, the fraction is given by

$$uA_u(\phi) = \frac{1}{1 + \kappa_0} A(\underline{w}) + \int_{\underline{w}}^{\phi} \frac{dA(x)}{(1 + \kappa_0 \overline{F}(x))}. \tag{3.4}$$

From this, one can derive the steady-state equilibrium unemployment rate as

$$u = \underbrace{\frac{1}{1 + \kappa_0} A(\underline{w})}_{\text{(1) UE who accept any job offer}} + \underbrace{\int_{\underline{w}}^{\overline{w}} \frac{dA(b)}{(1 + \kappa_0 \overline{F}(b))}}_{\text{(2) UE who accept/reject offers}} + \underbrace{(1 - A(\overline{w}))}_{\text{(3) UE who accept no offer}} \tag{3.5}$$

Moreover, similar to Bontemps et al. (1999) one can show that in steady-state there exists a unique relationship between the unobserved offer and the observed earnings distribution functions. Equating the flow of layoffs and upgraded wages of those with a wage lower than or equal to w and the flow of unemployed individuals accepting w, the distribution of earnings $G(w)$ is derived as

$$G(w) = \frac{A(w) - \left[1+\kappa_0\overline{F}(w)\right]\left[\frac{1}{1+\kappa_0}A(\underline{w}) + \int\limits_{\underline{w}}^{w} \frac{1}{1+\kappa_0\overline{F}(x)}dA(x)\right]}{\left[1+\kappa_1\overline{F}(w)\right](1-u)}. \tag{3.6}$$

Each firm offers only one wage and incurs a flow p of marginal revenue per worker. A firm seeks to maximize its steady-state profit flow, $\pi(p,w) = (p-w)\cdot l(w)$, with $l(w)$ denoting the size of a firm's labor force. The number of workers, l, attracted by a firm that offers wage w solves

$$l(w) = \frac{d(1-u)G(w)}{dF(w)},$$

and therefore

$$l(w) = \frac{\kappa_1 A(w)}{\left(1+\kappa_1\overline{F}(w)\right)^2} + \frac{\kappa_0-\kappa_1}{\left(1+\kappa_1\overline{F}(w)\right)^2}\left[\frac{1}{1+\kappa_0}A(\underline{w}) + \int\limits_{\underline{w}}^{w}\frac{1}{1+\kappa_0\overline{F}(x)}dA(x)\right]. \tag{3.7}$$

It can be shown that $l(w)$ is an increasing function of the offered wage. Note that the last term distinguishes $l(w)$ from the original model by Bontemps et al. (1999), where $\lambda_0 = \lambda_1$. The term reflects that if $\lambda_0 \neq \lambda_1$, the number of employed and unemployed individuals that are attracted by the firm at a wage w may differ from each other.

Firms are heterogeneous in their productivity p. The distribution of p across active firms is denoted by $\Gamma(p)$, and is assumed to be continuous over its support $[\underline{p},\overline{p}]$. With $w = K(p)$ denoting the function that maps the support of the productivity distribution Γ into the support of the wage offer distribution F, we have $F(w) = \Gamma(K^{-1}(w))$. The solution to the optimal wage setting problem of a p-type firm is represented by

$$K(p) = p - \left\{\frac{\kappa_0(\underline{p}-\underline{w})}{(1+\kappa_0)(1+\kappa_1)}A(\underline{w}) + \int\limits_{\underline{p}}^{p} l(K(x))dx\right\}\frac{1}{l(K(p))}, \tag{3.8}$$

which completes the steady-state solution of the model.

3.4 Data

Our empirical analysis uses German register data, the IAB Sample of Integrated Employment Biographies (SIAB). This administrative data set, which is described in more detail by vom Berge et al. (2013), is a two per cent random sample of all individuals who have at least one entry in their social security records between 1975 and 2010 in West Germany and between 1992 and 2010 in East Germany, respectively. The SIAB data cover approximately 80 per cent of the German workforce, providing longitudinal information on the employment biographies of 1,594,466 individuals. Self-employed workers, civil servants, and individuals doing military service are not included in the SIAB.

The data provide an ideal basis for estimating a structural equilibrium search model for several reasons. First and most importantly, the data contain daily information on employment records subject to social security contributions, unemployment records of benefit recipients as well as of registered job seekers. This permits us to precisely measure the duration of different labor market states and the transitions between them, notably job-to-job transitions as well as transitions between employment and unemployment (while receiving or not receiving benefits). Second, due to their administrative nature the data are less prone to measurement error than comparable information from survey data. Additional advantages over survey data include the larger sample size and a much more limited degree of panel attrition.

Sample selection proceeds in several steps. Before restricting the sample to a specific time span and population, we fill in missing values using all the information available in the full dataset (see Appendix 3.A.1). We then disregard employment spells where individuals receive Hartz IV benefits while working, because for this group the wage alone is not a useful metric for work incentives.

We construct a stock sample by keeping only those employment and unemployment spells including the set date 1 January 2007. Restricting the sample to the period 2007 to 2010 has the advantage that it permits us to include unemployment spells for individuals receiving means-tested welfare benefits, which were not recorded in the data prior to 2007. While this comes at the cost of including left-censored unemployment spells, it enables us to adopt a consistent definition of unemployment throughout the sample period.[3] This leads to a sample of 682,581 individuals.

From this sample we select only individuals who are part of the workforce. The data do not make it possible to distinguish between involuntarily unemployed individuals not receiving benefits and individuals who voluntarily left the labor force or who became

[3] Details on the definition of the different labor market states are given in Appendix 3.A.2.

self-employed or civil servants. To distinguish more precisely between voluntary and involuntary unemployment, we follow the assumptions proposed by Sokbae and Wilke (2009) (see Appendix 3.A.2). To focus on individuals in the workforce, we restrict the sample to individuals who are at least 20 years old and younger than 63 years. The sample is further restricted to low- and medium-skilled individuals. We exclude highly skilled individuals because this group is less likely to be in a labor market that is characterized by a wage-posting mechanism. We then drop individuals who still have missing values in the relevant observables, such as daily wages, the educational and occupational status as well as the regional affiliation. This leads to a new sample size of 370,104 individuals.

The SIAB data do not include information on hours worked. We therefore focus on full-time employment spells and disregard individuals who are employed part-time during the time period under consideration.

To calculate hourly wages for full-time employment spells, we impute the number of hours worked based on information from the German Microcensus. The imputation is done separately by region, sex, sector, job classification, and educational degree. For details, see Appendix 3.A.3.

In the model, each job is characterized by a single, time-invariant wage. For individuals who were employed on 1 January 2007, we compute this wage as the weighted average of the wages earned over the past year in the same job, where the weights are given by the length of time over which a particular wage was received. Likewise, the wage after an unemployment-to-employment spell is based on the weighted average over the first year after the transition.[4] To reduce the influence of outliers, we discount observations with implausibly low hourly wages (wages below 3 euros or below the existing sectoral minimum wages). The resulting final sample contains information on 235,706 individuals.

The wage information in the IAB data is censored since there is an upper contribution limit in the social security system. We do not inlude observations with censored wages.[5]

The model assumes that worker productivity is homogenous. Following Bontemps et al. (1999), we therefore estimate the model separately for different labor market segments. We assume that both the employed and the unemployed receive job offers only within their segment. We define the segments based on six job classifications (occupation types, see Appendix 3.A.6) and two regions (East Germany and West Germany including Berlin). These two dimensions allow us to define segmented labor markets fairly well (though not perfectly). As Table 3.A.1 shows, 95.9% of employment-to-employment

[4] For details, see Appendix 3.A.4.

[5] For details, see Appendix 3.A.5. In a robustness check, we address this issue by replacing censored observations with imputed wages, following Gartner (2005).

transitions remain in the same region, 83.4% remain in the same job classification, and 80.1% remain in the same region and job classification, i.e. the same labor market according to our definition. As for unemployment-to-employment transitions, 95.3% occur within the same region, 73.7% within the same job classification, and 70.7% within both the same region and the same job classification (see Table 3.A.2).

3.5 Descriptives

3.5.1 Transitions

Tables 3.B.1 and 3.B.2 in Appendix 3.B report the type, number, and share of transitions for our stock sample of individuals who were either unemployed (7.6%) or employed (92.4%) on 1 January 2007. Of the 217,733 individuals who were employed on this date, 68% stayed in their job for the next four years while 20% moved to another job and 13% became unemployed. Transitions in the other direction are much more frequent in relative terms: 45% of the 17,973 unemployment spells ended with a transition into regular employment during the four-year period after 1 January 2007. At the same time, 55% of individuals who were unemployed on this date remained without a job over the entire period. Left-censoring is relatively frequent for the unemployment spells (22%) because in some of the data sources for unemployment benefit histories, recording starts at a fixed date which does not necessarily coincide with the beginning of the unemployment spell (see Appendix 3.A.2).

The table also breaks down these statistics by labor market, as defined by region and job classification. About 84% of the individuals in the sample worked or searched for a job in West Germany (including Berlin), the remaining 16% in East Germany. On 1 January 2007, the unemployment rate was higher in East Germany (11%) than in West Germany (7%). However, the fraction of unemployed individuals finding a new job over the four-year observation window was almost identical in East and West Germany (44%). Looking at transitions of employed individuals, we find that most individuals stayed in their current job, while around 20% of the employed individuals in West Germany and 19% in East Germany changed their employer within the four years. The relative frequency of transitions into unemployment was higher in East Germany (17%) than in West Germany (12%).

As for the six job classifications, note the large number of observations for "production, craft" sectors which are still fairly important in Germany and especially in our sample of low- and medium-skilled individuals. The unemployment rate on 1 January 2007 varied between 5% in white-collar jobs and 20% in agriculture, partially reflecting the varying importance of seasonal unemployment. Consistent with this seasonal influence, the share of unemployment-to-employment transitions was particularly high in agriculture (65%). At the other end of the spectrum, only 31% of the unemployed individuals in sales found work within the next four years.

In our administrative data set, the number of observations is large even when interacting job classifications with regions; sample sizes range from 1,225 (agriculture, East Germany) to 76,723 (production/craft, West Germany). Both the unemployment rates and the transitions reflect the differences already discussed; i.e., unemployment rates are lower in the West for all job classifications, and the differences across classifications also hold within the two regions.

3.5.2 Durations

Figure 3.B.1 in Appendix 3.B shows non-parametric Kaplan-Meier estimates of the survival function for remaining in the initial state (employment or unemployment) for the whole sample. The survival functions are also shown for the twelve different sub-samples defined by region and job classification (Figures 3.B.2 to 3.B.7 in Appendix 3.B). In our data, the maximum duration of an unemployment spell is six years.[6] Employment spells can in principle last over the whole observation period: 35 years in West Germany (1975–2010) and 19 years in East Germany (1991–2010).[7]

Transitions out of unemployment. The chance of transitioning into employment is particularly high within the first year – only about 60% of the unemployed were still without a job after twelve months (cf. panel (a) in Figure 3.B.1). By the third year, about 40% of the unemployed had not found employment, and after the third year the survival function flattens out. As can be seen in panel (a) of Figures 3.B.2 to 3.B.7, the pattern is very similar for East and West Germany, but there is substantial variation across job classifications. Most notably, jobs in agriculture (Figure 3.B.2) as well as

[6] "Unemployment benefit I" (ALG I), a non means-tested transfer which is part of the unemployment insurance system, is typically paid for only one year (two years for older workers). Once ALG I runs out, the unemployed are entitled to the much lower and means-tested "unemployment benefit II" (ALG II), which was introduced on 1 January 2005. Before 2005, ALG I was followed by "Arbeitslosenhilfe" instead of ALG II. This means that individuals receiving "Arbeitslosenhilfe" before 2005 were entitled to ALG II afterwards. However, spells of receiving ALG II are only recorded in the data from 1 January 2007 onwards. This makes 1 January 2005 the earliest starting point for unemployment spells in our data. These spells refer to those individuals who received ALG I benefits during 2005 and 2006 and who were entitled to ALG II afterwards (starting from 2007). As our sample covers the period 2007–2010, the maximum duration of an unemployment spell is six years.

[7] 1.29% of the employment spells are left-censored which means employment without interruption at the same firm since 1 January 1975 in West Germany or since 1991 in East Germany.

production/craft jobs (Figure 3.B.3) are characterized by short unemployment durations. Around 75% of the unemployment spells in agriculture were shorter than one year (the share is even higher in West Germany and lower in East Germany). In production/craft jobs (the largest group), more than half of the unemployment spells were shorter than one year. At the other end of the spectrum, unemployed individuals who formerly had sales and service jobs tend to have long unemployment durations (Figures 3.B.5 and 3.B.7).

Transitions out of employment. For individuals who were initially employed, transitions can be either into another job (panel(b) in Figures 3.B.1 to 3.B.7) or into unemployment (panel (c)). The durations of employment spells that end because of unemployment are in general longer than employment spells that end in a job-to-job transition. With regard to employment-to-employment transitions, the probability of still being employed at the current employer is typically around 75% after fifteen years. This holds for both East and West Germany. However, employment spells that end into unemployment tend to be longer in the West than in the East.

Regarding job classifications, sales jobs stand out both for transitions into other jobs and for transitions into unemployment; at each point in time, the share of the employed who have left their initial job for one of these destinations is particularly high. Agriculture is inconspicuous for employment-to-employment transitions, but features a high share of transitions into short durations of unemployment especially in East Germany. White-collar employees tend to have the longest employment durations, with a large fraction of employment spells being right-censored.

3.5.3 Wage Distributions

Figure 3.B.8 in Appendix 3.B shows the distribution of wages before and after a labor market transition for the whole sample. As part of the descriptives, we include all three types of transitions (e $\rightarrow$ e, e $\rightarrow$ u, u $\rightarrow$ e) and also document the wage distributions for right- and left-censored spells. In the estimation, only the wages in the initial employment spell or after a transition from unemployment to employment will be used.

As expected, wages of individuals who change their job tend to be higher than wages before a transition into unemployment. Comparing wages before and after a job-to-job transition, we find that wages earned in the new job are on average slightly higher than the wages earned in the old position. Also in line with expectations, wages after an unemployment-to-employment transition tend to be relatively low. A sizeable fraction of the unemployed move to jobs paying less than 8.50 euros an hour, the statutory minimum wage introduced in 2015, i.e. after our sampling period 2007–2010. This also holds within the different labor market segments defined by region and job classification (see Figures 3.B.9 to 3.B.14 in Appendix 3.B).

These figures confirm the well-known fact that wages tend to be lower in East Germany and also document variation in hourly wages across job classifications. In both East and West Germany, we find that on average wages are higher for white-collar employees (Figure 3.B.11) as well as for office workers (Figure 3.B.13) while wages are lower in agricultural professions (Figure 3.B.9), among service workers (Figure 3.B.14), and for individuals working in sales (Figure 3.B.12).

3.6 Estimation

We begin this section by deriving the likelihood contributions of unemployed and employed workers, taking into account stock sampling as well as left- and right-censoring. We then outline the estimation procedure, which combines the likelihood function with a non-parametric estimate of the wage distribution.

Likelihood – unemployed workers. As seen in Equation (3.5), the steady-state unemployment rate has three components. For individuals with low enough opportunity costs of employment, unemployment is purely frictional. In a second group, unemployment is driven by both search frictions and the opportunity cost of employment; these individuals will accept some job offers, but reject others. Finally, there is a third group for whom unemployment is permanent given the wage offer distribution F, as any wage offer is below their reservation wage. As a result, the likelihood contribution of an individual who is initially unemployed is a mixture distribution:

$$\lambda_0^{2-d_b-d_f} \cdot e^{-\lambda_0(t_{0b}+t_{0f})} \cdot \frac{A(\underline{w})}{1+\kappa_0} \cdot f(w_0)^{1-d_f} + \int_{\underline{w}}^{d_f\overline{w}+(1-d_f)w_0} \big(\lambda_0\overline{F}(x)\big)^{2-d_b-d_f} \cdot e^{-\lambda_0\cdot\overline{F}(x)\cdot(t_{0b}+t_{0f})} \cdot \frac{f(w_0)}{\overline{F}(x)}^{1-d_f} \cdot \frac{dA(x)}{1+\kappa_0\overline{F}(x)} + [1-A(\overline{w})]^{d_b\cdot d_f}. \quad (3.9)$$

The first term of the sum corresponds to purely frictional unemployment. As job offers arrive with Poisson rate λ_0, unemployment durations are exponentially distributed. In a flow sample, where the elapsed (“backward") duration t_{0b} is zero by definition, the density of the residual (“forward") duration t_{0f} is given as $h(t_{0f}) = \lambda_0 \exp(-\lambda_0 t_{0f})$. In a stock sample, we need to consider the total duration $t_{0b}+t_{0f}$, conditional on the elapsed duration t_{0b}. The latter has the density $h(t_{0b}) = \lambda_0 \exp(-\lambda_0 t_{0b})$. It can be shown (e.g. Lancaster, 1990) that the conditional density $h(t_{0f}|t_{0b})$ is given as $\lambda_0 \exp(-\lambda_0 t_{0f})$. For the joint density we then obtain $h(t_{0b},t_{0f}) = h(t_{0f}|t_{0b})h(t_{0b}) = \lambda_0^2 \exp\left(-\lambda_0(t_{0b}+t_{0f})\right)$, which is the term that figures in the likelihood expression above. The term in front of the exponential function is adjusted if either the elapsed or the residual duration is censored ($d_b = 1$ or $d_f = 1$). $f(w_0)$ is the density function of wage offers evaluated at the offer that we observe as the initially unobserved person transits into employment. If the unemployment duration is right-censored ($d_{0f} = 1$), this term drops out of the likelihood function.

The second term of the sum has the same basic structure, but with some adjustments for the fact that individuals in this group are sometimes faced with wage offers that are below their reservation wage. The unemployment spell hazard rate is therefore given not by λ_0, but by the product $\lambda_0\overline{F}(b)$. The second adjustment concerns the wage offer density, which is now truncated at b, so we have $f(w_0)/\overline{F}(b)$.[8]

Finally, the third term applied to individuals who, given F, are permanently unemployed. This implies that the observed unemployment spell must be both left- and right-censored, hence the $d_b \cdot d_f$ in the exponent.

Likelihood – employed workers. For individuals who are initially employed, the likelihood contribution is

$$\frac{\kappa_0}{1+\kappa_0} \cdot g(w_1) \cdot \left[\delta + \lambda_1 \overline{F}(w_1)\right]^{1-d_{1b}} \cdot e^{-\left[\delta+\lambda_1\overline{F}(w_1)\right](t_{1b}+t_{1f})} \left[\delta^{\nu}\left(\lambda_1\overline{F}(w_1)\right)^{1-\nu}\right]^{1-d_{1f}}. \tag{3.10}$$

In steady state, a fraction $\kappa_0/(1+\kappa_0)$ of all individuals is employed. g is the density of wages in the initial job. Unlike for the unemployed, the reservation wage of a worker is observed and equals his or her current wage, so there is no mixing distribution for the durations. However, there are now two competing reasons for why a spell may end: layoff (at rate δ) or a better job offer (at rate $\lambda_1\overline{F}(w)$). The indicator ν equals 1 in the first case and 0 in the second. t_{1b} denotes the elapsed duration, and t_{1f} the residual duration of the current job. d_{1b} equals 1 if the elapsed duration is left-censored, while $d_{1f} = 1$ means that the residual duration is right-censored, i.e. the individual does not change his or her job during the observation period.

[8]Note that as $\overline{F}(b) = 1$ for $b < \underline{w}$, the first term of the sum could be integrated into the second term. We choose to present them separately here to better reflect the conceptual difference between the three components behind the unemployment rate.

Estimation procedure. Maximum likelihood estimation of the model requires functional form assumptions for H and Γ. The estimation is numerically cumbersome as f, g, and $\overline{F}$ are highly non-linear functions of Γ. In particular, optimization involves the numerical computation of the inverse K^{-1}, further complicated by the fact that K contains an integral that has to be evaluated numerically as well. Beyond these numerical concerns, there is the issue that most distributions for Γ imply wage distributions that do not fit the data well.

As an alternative, Bontemps et al. (2000) therefore propose a three-step procedure in which the wage distribution is estimated non-parametrically:

1. In a first step, we estimate G and g (the cdf and pdf of the wage distribution) using a kernel density estimator, and estimate $\underline{w}$ and $\overline{w}$ as the sample minimum and maximum of the wages of workers who are employed on 1 January 2007. Based on these non-parametric estimates and a parametric assumption for the opportunity cost distribution, namely $H \sim \mathcal{N}(\mu_b, \sigma_b^2)$, we obtain consistent estimates for $\overline{F}$ and f (conditional on $\mu_b, \sigma_b, \lambda_0, \lambda_1, \delta$ and the assumption that $\rho = 0.004$) by numerically solving the following expressions (recall that u is a function of $\overline{F}$):
$$\widehat{\overline{F}}(w) = \frac{A(w) - uA_u(w) - (1-u)\widehat{G}(w)}{\kappa_1 \cdot \widehat{G}(w) \cdot (1-u) + \kappa_0 \cdot u \cdot A_u(w)} \tag{3.11}$$
and
$$\widehat{f}(w) = \frac{(1-u) \cdot \widehat{g}(w) \cdot (1 + \kappa_1 \widehat{\overline{F}}(w))}{\kappa_0 \cdot u \cdot A_u(w) + \kappa_1 \cdot (1-u) \cdot \widehat{G}(w)}. \tag{3.12}$$

2. The estimates from Step 1 are plugged into the likelihood function, which is then maximized with respect to μ_b, σ_b, λ_0, λ_1, and δ.

3. Once these parameters are known, the productivity of a firm can be inferred from the wage that it offers:
$$\begin{aligned} p &= K^{-1}(w) \\ &= w + \left(\frac{\kappa_0 \cdot A'(w) \cdot (1 + \kappa_1 \cdot \widehat{\overline{F}}(w))}{(1 + \kappa_0 \cdot \widehat{\overline{F}}(w)) \cdot (\kappa_1 A(w) + (\kappa_0 - \kappa_1)) \cdot u \cdot A_u(w)} + \frac{2 \cdot \kappa_1 \cdot \widehat{f}(w)}{1 + \kappa_1 \cdot \widehat{\overline{F}}(w))} \right)^{-1} \end{aligned} \tag{3.13}$$

Standard errors are obtained by bootstrapping the three-step procedure with 100 replications.

3.7 Estimation Results

3.7.1 Parameter Estimates

Table 3.1 reports the estimated parameters and the associated bootstrap standard errors. The large sample size of almost a quarter of a million observations allows for fairly precise estimates.

For the whole sample, we estimate a monthly job destruction rate δ of 0.0063. Looking at twelve sub-samples defined by region and job classification, we find that job destruction rates are higher in East Germany than in the West, and higher for agricultural and sales jobs than for white-collar employees or office workers. Our estimates range from 0.0054 (white-collar, West) to 0.0118 (agriculture, East). These orders of magnitude are similar to existing studies. For France in the 1990s, Bontemps et al. (1999) find a δ between 0.0032 and 0.0069, depending on the sector. Using SIAB data for an earlier period (1995–2000), Nanos and Schluter (2014) estimate the monthly layoff rate to be between 0.0032 and 0.0243 in Germany. Holzner and Launov (2010), who use data from the German Socio-Economic Panel 1984–2001, estimate a δ of 0.0047.

The estimated κ, i.e., the ratio of the job arrival over the job destruction rate, is greater for the unemployed than for the employed. We find κ_0 to be 13.72 and κ_1 to be 8.18. Holzner and Launov find a κ_1 of 2.2, while their three values of κ for the unemployed (they assume that individuals search on skill-specific labor markets) range between 5.6 and 17.1. In their study for France, Bontemps et al. 2000 also estimate a much higher job arrival rate for the unemployed than for the employed. In all cases, this reflects that continental European labor markets are characterized by relatively little job-to-job mobility compared with the United States.

The differences between regions and job classifications are potentially relevant for the design of the new statutory minimum wage in Germany. After a transition period, the minimum wage became uniform for all workers by 2017 at the latest. Our results suggest that the uniform rate applies to labor market segments that differ in the extent of search frictions and thus in firms' monopsony power on the labor market.

According to our estimates, the distribution of the opportunity costs of employment has a mean μ_b close to 0 euros per hour, both in the whole sample and for the different subsamples. The standard deviation σ_b is estimated to be 1.85 for the whole sample.

However, unlike in the model of Bontemps et al. (1999), the reservation wages are not identical to the opportunity costs of employment. This is because job offer arrival rates are higher when unemployed, so it is optimal for the unemployed to reject certain wage offers in the hope of getting a higher offer in the future (cf. Equation (3.1)). Based

Table 3.1: Estimation results

	N	δ	κ_1	κ_0	μ_ϕ	μ_b	σ_b	β	u
Whole sample	235706	0.0063	8.18	13.72	4.74	0.00	1.85	0.63	0.1081
		(0.0000)	(0.09)	(0.17)	(0.03)	(0.00)	(0.02)	(0.00)	(0.0004)
West Germany									
Agriculture	2685	0.0087	7.50	14.48	4.69	0.00	2.34	0.46	0.1303
		(0.0002)	(0.58)	(1.23)	(0.19)	(0.00)	(0.20)	(0.01)	(0.0040)
Production, Craft	76723	0.0058	12.06	22.71	6.17	0.00	3.11	0.68	0.0856
		(0.0000)	(0.39)	(0.83)	(0.06)	(0.00)	(0.07)	(0.00)	(0.0007)
White-collar	16943	0.0054	10.77	17.84	5.91	0.00	2.97	0.74	0.0964
		(0.0000)	(0.56)	(1.05)	(0.12)	(0.00)	(0.10)	(0.01)	(0.0014)
Sales	10080	0.0079	4.79	8.47	4.15	0.00	1.52	0.50	0.1557
		(0.0001)	(0.22)	(0.42)	(0.09)	(0.00)	(0.07)	(0.01)	(0.0021)
Office	46978	0.0057	8.04	12.54	4.68	0.00	2.06	0.70	0.1101
		(0.0000)	(0.13)	(0.24)	(0.06)	(0.00)	(0.05)	(0.00)	(0.0009)
Service	45250	0.0069	7.36	12.56	4.28	0.00	1.56	0.58	0.1104
		(0.0000)	(0.15)	(0.36)	(0.09)	(0.02)	(0.11)	(0.00)	(0.0010)
East Germany									
Agriculture	1225	0.0118	3.55	14.06	5.32	0.00	3.13	0.34	0.1893
		(0.0016)	(0.42)	(2.58)	(0.27)	(0.00)	(0.24)	(0.02)	(0.0119)
Production, Craft	15099	0.0090	7.16	18.73	5.73	0.00	2.67	0.45	0.1256
		(0.0023)	(0.90)	(2.39)	(0.51)	(0.00)	(0.12)	(0.04)	(0.0179)
White-collar	3081	0.0063	10.57	19.74	5.32	0.00	2.18	0.63	0.1002
		(0.0001)	(3.60)	(7.29)	(0.19)	(0.00)	(0.18)	(0.01)	(0.0033)
Sales	1932	0.0091	4.90	10.70	3.85	0.00	1.09	0.44	0.1704
		(0.0002)	(0.64)	(1.64)	(0.14)	(0.08)	(0.26)	(0.01)	(0.0148)
Office	6655	0.0061	7.89	13.67	4.47	0.00	1.42	0.66	0.1103
		(0.0001)	(0.70)	(1.19)	(0.12)	(0.01)	(0.10)	(0.01)	(0.0023)
Service	9055	0.0075	6.20	12.03	3.95	0.00	1.29	0.54	0.1263
		(0.0001)	(1.44)	(2.45)	(0.09)	(0.01)	(0.15)	(0.01)	(0.0122)

Notes: Calibrated parameters: $\rho = 0.004$. Bootstrapped standard errors in parentheses (100 runs).
Source: SIAB 7510, own computations.

on the estimated parameters, we find that the distribution of reservation wages is centered around a μ_ϕ of about 4.74 euros per hour. The reservation wages tend to be higher and more widely dispersed in the West, with the sole exception of agricultural jobs. Among job classifications, sales jobs stand out for having both the smallest mean (3.85 euros in East Germany and 4.15 euros in West Germany) and the smallest standard deviation of the reservation wage. White-collar workers and workers in production and craft have the highest reservation wages when unemployed.

The differences in μ_ϕ between the subsamples are almost exclusively driven not by inherent differences in opportunity costs (recall that μ_b is close to zero everywhere), but by differences in the frictional parameters. For instance, the difference between κ_0 and κ_1 is particularly large for production/craft jobs in the West and particularly small for sales jobs in the East, which is reflected in a much higher μ_ϕ in the first case. Differences in ϕ also arise because of different layoff rates. The higher the layoff rate, the smaller the expression $\beta \equiv \rho/\delta$ in Equation (3.1), and thus the larger the incentive for the unemployed to be picky when accepting a wage offer – after all, accepting a job already means giving up a higher job arrival rate, and if, in addition, the job has a higher probability of ending, there is less of an incentive to accept it.

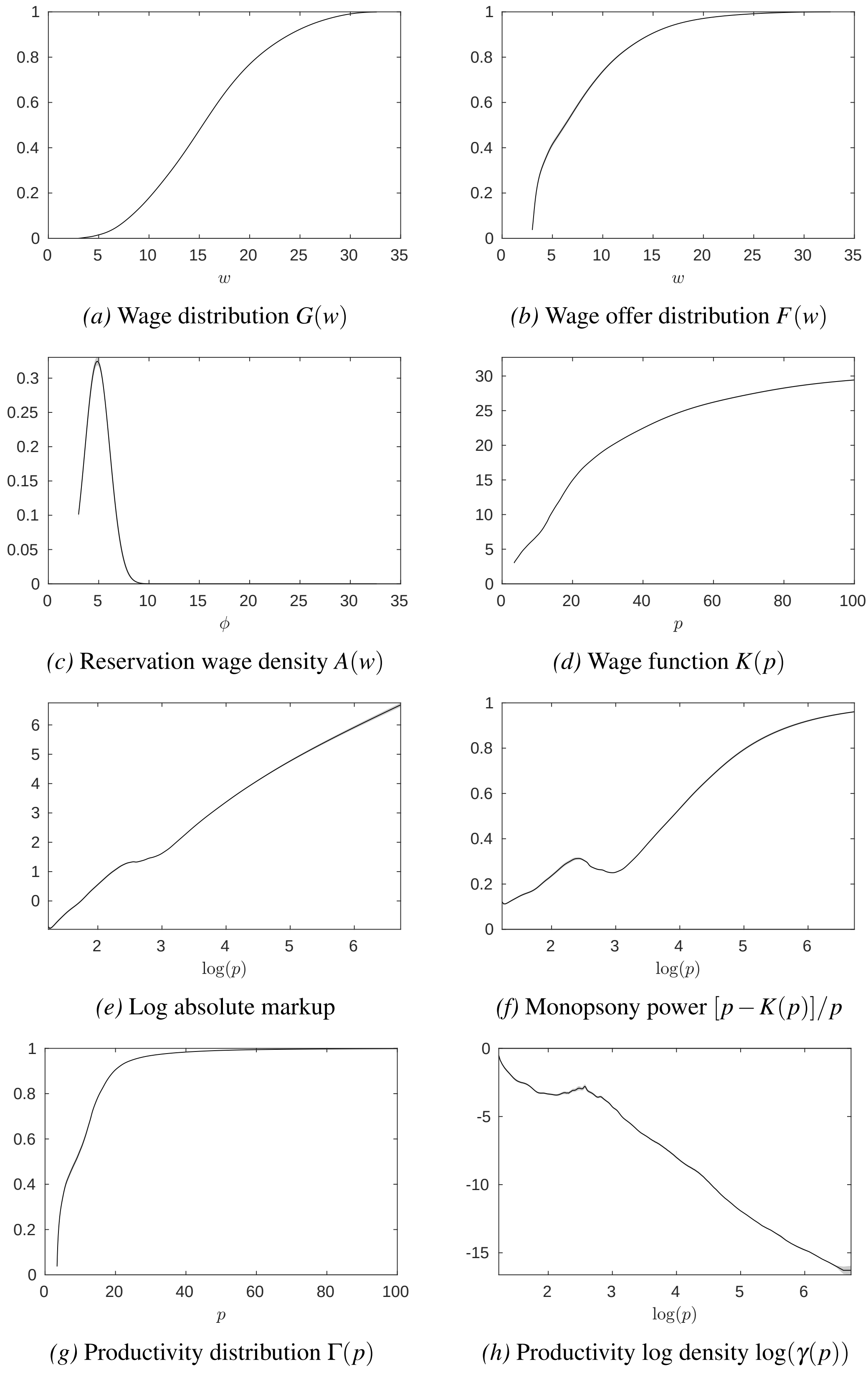
Finally, based on Equation (3.5) we can compute the steady-state unemployed rate u implied by our estimates. For the entire sample, we find a rate of 10.85%, which is higher than the rate of 7.6% observed in our stock sample. The variation across regions and job classifications is in line with the patterns documented for the sample, i.e. steady-state unemployment is higher in the East and in agricultural and sales jobs.

3.7.2 Distribution of Wages, Opportunity Costs, Markups, Productivity

Figure 3.1 shows key plots for the whole sample summarizing the steady-state equilibrium. Panel (a) depicts our non-parametric estimate for G, the cdf of the wage distribution. The pdf g, which is not shown here, is similarly estimated using a kernel density estimator.

To find the wage offer distribution F (panel (b)), the estimate for G is combined with the maximum likelihood estimates for the frictional parameters and the opportunity cost distribution, as outlined in Section 3.6 above. Note that the location and the shape of the wage offer distributions differ from the wage distribution. For instance, more than 70% of the wage offers but only 20% of observed wages are below 10 euros.

Panel (c) shows the estimated distribution of reservation wages. This is a normal distribution centered around $\mu_\phi = 4.74$ euros and truncated at 3 euros, the lowest admis-



(a) Wage distribution $G(w)$

(b) Wage offer distribution $F(w)$

(c) Reservation wage density $A(w)$

(d) Wage function $K(p)$

(e) Log absolute markup

(f) Monopsony power $[p - K(p)]/p$

(g) Productivity distribution $\Gamma(p)$

(h) Productivity log density $\log(\gamma(p))$

Notes: Grey area indicates 95% confidence bands. *Source:* SIAB 7510, own computations.

Figure 3.1: Main equilibrium functions (whole sample)

sible hourly wage. Note that there is hardly any mass left beyond 10 euros. This means that the positive effect of higher minimum wages operating through a lower rate of job offer rejections will be mostly limited to minimum wage levels below 10 euros.

Panel (d) presents the optimal wage offer as a function of firm productivity p. For example, a firm with a value product of 20 euros per hour will optimally set a wage of about 15 euros per hour. The absolute markup, which is shown in a log-log-scale in panel (e), grows monotonically and at a roughly constant rate with a firm's productivity. Expressed as a percentage of productivity (panel (f)), the relationship becomes non-monotonic, although the pattern of a general increase is preserved; while the lowest-productivity firm has a markup of about 15%, the markup is over 80% for the firm with the highest productivity. Put differently, workers obtain less than 20% of the value product in these high-productivity firms. However, as the estimate of the productivity distribution Γ in panel (g) makes clear, such cases are fairly rare, with the bulk of firms having a value product of less than 20 euros per hour. Finally, panel (h) shows that our three-stage estimate of firm productivity results in a (non-parametric) distribution that is not too dissimilar from a Pareto distribution in that the density γ is a straight line in log-log-coordinates over a wide range of p.

The main equilibrium functions for the twelve different labor markets defined by region and job classification can be found in Appendix 3.C.1.

3.7.3 Robustness Checks

Table 3.C.1 in Appendix 3.C.2 reports results from a number of robustness checks for the whole sample. First, instead of disregarding individuals with wages right-censored at the upper limit for social security contributions (SSC), we use a Tobit regression to impute wages above this limit. Second, we replace the imputation of working hours with the assumption that all full-time employees work 40 hours per week. Third, we experimented with different ways of assigning a single wage to employment spells that last over several years, during which time individuals typically experience wage increases. In the theoretical model, this cannot happen as each job is characterized by a single, time-invariant wage. In our main specification, we use the average wage in the same job over the past year. In a robustness check, we use the last observed wage only. The two measures differ to the extent that individuals experience wage changes within the last year. Fourth, we truncated the wage distribution at different levels. In our main specification, wages below 3.00 euros per hour are discounted. We changed this threshold to 2.00 euros and 4.00 euros, respectively. Moreover, when replacing the right-censoring at the upper limit for SSC with an imputation procedure, we tried two variants in which

we truncated the imputed wages at the 95th or 99th percentile. Finally, we set ρ, which is assumed to be 0.004 in our main specification, to alternative values (0.002 or 0.006). We also combined the robustness checks along the different dimensions.

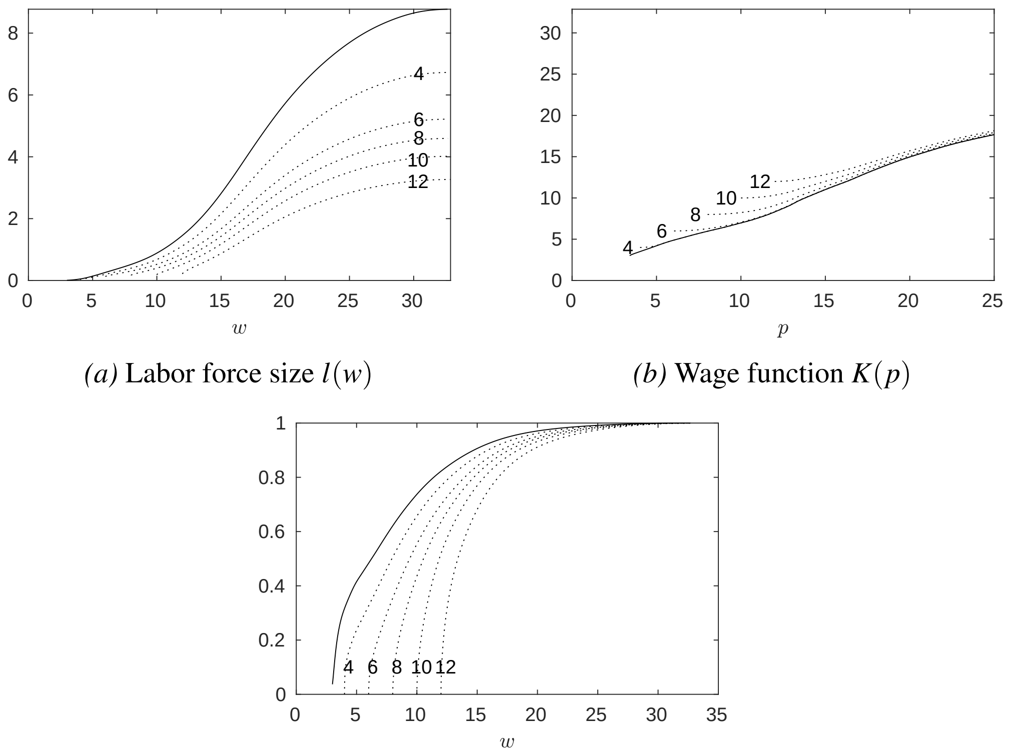
The estimation results are fairly unaffected by these variations. The same is true for the comparative statics results (not shown here for the robustness checks). Only for the different truncation levels (2.00 euros/4.00 euros instead of 3.00 euros) is there a slightly stronger reaction of some of the parameters, though the comparative statics results remain qualitatively very similar.

3.8 Unemployment Effects of Different Minimum Wage Levels

Due to the heterogeneity in both the opportunity cost of employment and firms' productivity, the unemployment rate is a non-linear and non-monotonic function of the minimum wage level.

Decomposition of the unemployment rate. Unemployment can be of three types, as shown by the decomposition in Equation (3.5). Group (1) consists of individuals whose reservation wage is below $\underline{w}$, i.e., who will accept any job offer. This purely frictional unemployment decreases in κ_0, the ratio of the job arrival rate of the unemployed over the job destruction rate. For Group (2), unemployment is partly frictional (through κ_0) and partly driven by the interplay between the reservation wage and the wage offer distribution. Unemployed individuals in this group accept some job offers but reject others, depending on the wage offer. Finally, individuals in Group (3) are permanently unemployed because their reservation wage is higher than the highest wage offer $\overline{w}$.

Effects through the wage offer distribution. For minimum wage levels below the lowest productivity level $\underline{p}$, the model predicts that a minimum wage *reduces* unemployment, as long as the minimum wage shifts up firms' optimal wage offers. The reason is that in this case unemployed individuals are now more likely to receive acceptable wage offers. With $\underline{w} = 3.00$ euros and our estimate for the wage offer function, this cutoff level is $\hat{\underline{p}} = \hat{K}^{-1}(3.00) = 3.42$ euros for the whole sample. The introduction of a minimum wage of, say, 3.10 euros limits firms' power to set wages below productivity. The lowest wage is now 3.10 euros instead of 3.00 euros and, via Equation (3.8), this increase has repercussions throughout the entire wage offer distribution. This is illustrated in Panel (a) of Figure 3.2 for the whole sample: the higher the minimum wage level, the smaller



(a) Labor force size $l(w)$

(b) Wage function $K(p)$

(c) Wage offer distribution $F(w)$

Key: No minimum wage (——); Different minimum wage levels are 4, 6, 8, 10, and 12 Euro (··········).

Source: SIAB 7510, own computations.

Figure 3.2: Equilibrium functions for different minimum wage levels (whole sample)

the workforce l that a firm attracts for a given wage offer w. Moreover, the relationship between l and w becomes less steep for higher minimum wages.

As a result of these interactions operating through $l(w)$, different minimum wage levels lead to different optimal wage offer functions $\hat{K}^{MW}$, and therefore to different wage offer distributions $\hat{F}^{MW}$. Increasing the minimum wage generally shifts $\hat{K}^{MW}$ upwards and $\hat{F}^{MW}$ to the right (cf. panels (b) and (c)). While the biggest changes occur for low wages and productivities, even high-productivity firms adjust their wage offer slightly in response to an increase in the minimum wage.

These changes in the wage offer distribution affect the steady-state unemployment rate. A minimum wage below $\underline{p}$ leads to an increase in $\underline{w}$, which in turn means that some individuals shift from Group (2) to Group (1) in Equation (3.5). As $1+\kappa_0 > 1+\kappa_0\overline{F}(b)$ for all $b \in]\underline{w},\overline{w}]$, this leads to a reduction in the unemployment rate. For individuals staying in Group (2), unemployment goes down as $\overline{F}(w)$ decreases for all w. Moreover, the highest wage offer $\overline{w}$ increases, which reduces the number of individuals who reject all job offers (Group 3).

Effects through the job arrival rates. For minimum wage levels above the lowest productivity level $\underline{p}$, the sign of the minimum wage effect on unemployment becomes ambiguous a priori. The minimum wage now affects the lowest productivity level $\underline{p}^{MW}$ that guarantees non-negative profits. As a result, the fraction of operating firms $\overline{\Gamma}(\underline{p}^{MW})$ decreases. Following Bontemps et al. (1999) and Bontemps et al. (2000), we assume that κ_0 and κ_1 are proportional to this fraction. This means that a minimum wage above $\underline{p}$ reduces the ratio of the job arrival rate over the job destruction rate. The unemployment effect of a minimum wage is now the result of two countervailing forces: the reduction in unemployment as higher wage offers lead to less frequent rejections of job offers, and the negative effect arising from the fact that job offers now arrive at a slower rate. Formally, the second effect reduces the denominators in Equation (3.5), thereby increasing the frictional component of unemployment in Groups 1 and 2.

Effects through reservation wages. So far, we have discussed the channels operating through the wage offer distribution and the job offer arrival rates. Both channels are already present in the Bontemps et al. (1999) model with homogenous λ. In our extension of the model with $\lambda_0 \neq \lambda_1$, there is an additional channel operating through A, the distribution of reservation wages ϕ. This channel is present regardless of whether the minimum wage is below or above $\underline{p}$. As shown in Equation (3.1), the reservation wage ϕ depends on κ_0, κ_1, F and $\overline{w}$, all of which are functions of the minimum wage. While an increase in $\overline{w}$ raises the reservation wage, a proportional reduction in κ_0 and κ_1 lowers it. F has a double effect on ϕ, operating both through the numerator and the denominator of the second term in Equation (3.1). Empirically, the resulting net influence on A turns out to be relatively small in our application. In fact, the different density plots of A are identical to the status-quo plot for the range of minimum wage levels considered here, and are therefore not shown. As a result, the minimum wage effects in the richer model with $\lambda_0 \neq \lambda_1$ prove to be very close to the ones in the model with homogenous λ.

Total effect on unemployment. Figure 3.3 shows the effect of different minimum wage levels on the unemployment rate and the average unemployment duration, based on the estimation results for the whole sample. The solid line in the upper panel is the effect that is actually predicted by the model. The dotted line allows for heterogeneity in b, but switches off the channel operating through the reduction in job offer arrival rates; these are held constant at their estimated status-quo levels. The dashed line shows the ratio $1/(1+\hat{\kappa}_0)$. In this case, the positive effect working through the wage offer distribution is switched off. All unemployment is purely frictional from the start, and higher minimum wages increase search frictions and thereby unemployment through the reduction in κ_0 and κ_1.

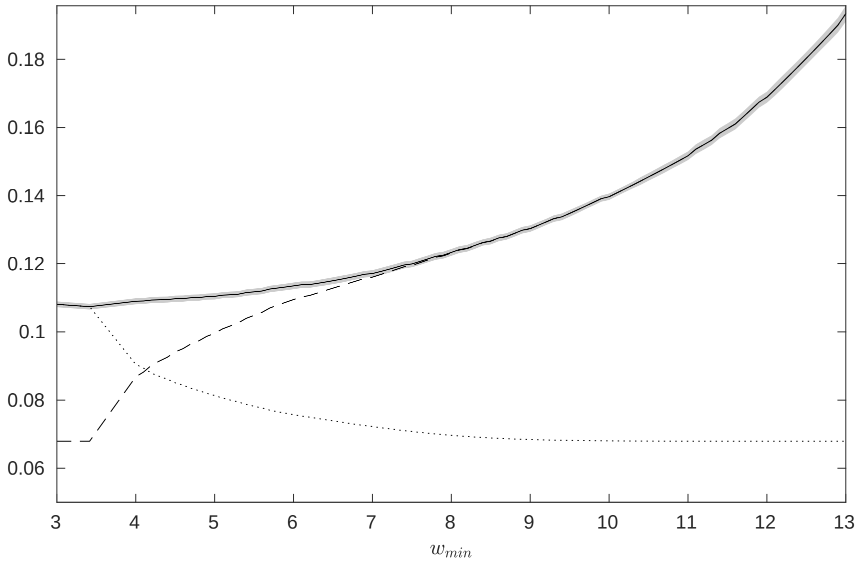
In our actual model (solid line), the relationship between u and the minimum wage level is non-monotonic; from a status-quo level (with no minimum wage) of about 11%, unemployment falls for very low levels of w^{MW} and reaches its minimum at $w^{MW} = 3.45$ euros. From there on, unemployment increases with the level of the minimum wage. The status-quo unemployment rate is again reached at 3.51 euros; this is the highest value of the minimum wage that does not lead to an increase in unemployment in relation to the status quo. For higher values, the increase in unemployment is relatively modest at first but becomes more significant above 8.00 euros. The positive effect working through higher wage offers is nearly exhausted by this point, i.e. there is little mass left in the reservation wage density. This is illustrated by the dotted line. The line shows what happens to the unemployment rate as the frictional component gains more and more in importance. Since the opportunity cost distribution H is unbounded, purely frictional unemployment (corresponding to a situation in which all unemployed individuals are in Group (1)) is reached only asymptotically. However, at a minimum wage level of 8.00 euros the dotted line is already very close to its limit as w_{min} approaches infinity (6.96% vs. $100 \times \frac{1}{1+\widehat{\kappa_0}} = 6.8\%$). For higher values of the minimum wage, the increase in unemployment resulting from the reduction in job offer arrival rates is therefore the (almost) exclusive driver of the changes in u. In Figure 3.3, this is reflected by the near convergence of the solid line and the dotted line at about 8.00 euros.

The actual minimum wage introduced in January 2015 was 8.50 euros per hour. Adjusted to 2010 prices, this corresponds to a level of around 8.00 euros, which according to our model would have increased the unemployment rate by two percentage points. In relative terms, this amounts to an increase of about 18% compared to the old steady-state value observed on 1 January 2007.

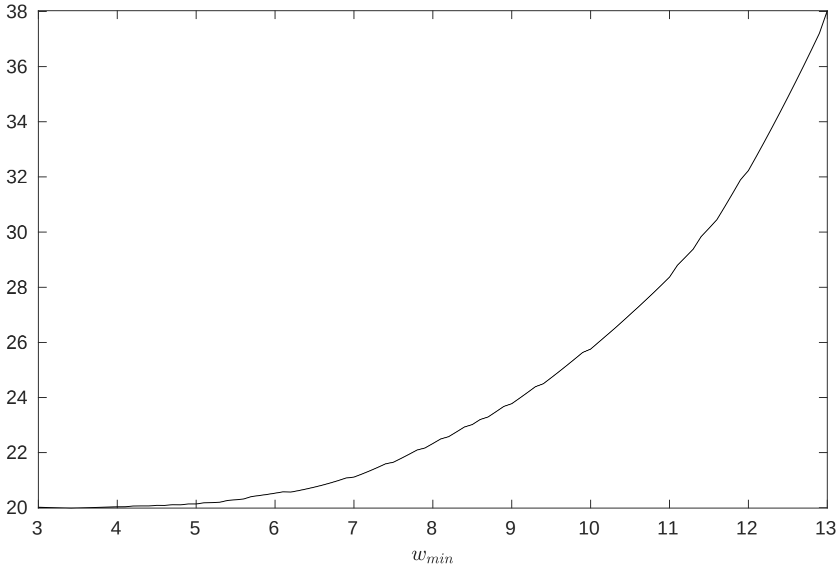
The mean unemployment duration (panel (b) of Figure 3.3) is given by

$$\int_{\underline{w}}^{\overline{w}} \frac{A_u(b)}{\lambda_0 \overline{F}(b)} \, \mathrm{d}b. \tag{3.14}$$

The three effects mentioned above in the context of the unemployment rate are again at play here. In fact, each item in the expression depends on the minimum wage level. The effect on the numerator A_u is ambiguous a priori and, given that A changes little, probably fairly small. The main change is likely to take place in the denominator, where λ_0 decreases in the minimum wage while $\overline{F}$ increases, again giving an ambiguous



(a) Unemployment rate *u*



(b) Mean unemployment duration

Key: Preferred model (——); frictional component of *u* (– – –);
model without change in job offer arrival rates (·········). *Notes:* Grey area indicates 95% confidence bands.

Source: SIAB 7510, own computations.

Figure 3.3: Change in minimum wage: unemployment (whole sample)

effect. The influence of the change in the integral limits $\underline{w}$ and $\overline{w}$ is also an empirical question. Our simulations show that the mean unemployment duration stays fairly constant at 20 weeks until a minimum wage level of about 5.00 euros, and then grows at an increasing rate.

Heterogeneity between labor markets. The simulation results discussed so far have been for the whole sample, i.e. they are based on an estimation in which observations from all twelve labor markets have been pooled (first row of Table 3.1). Figure 3.D.1 in Appendix 3.D shows the effect of different minimum wage levels on the unemployment rate for the twelve labor markets defined by region and job classification. The simulations are based on separate estimations for each labor market (cf. Table 3.1). We find that the same minimum wage level can have very different effects on unemployment depending on the labor market segment. Focusing on West Germany first, we find that even low minimum wage levels lead to an increase in unemployment in the agricultural and sales sectors, as well as for service workers and office workers, while for workers in production and craft and especially for white-collar employees the negative effects set in at much higher levels only. Importantly, there are strong differences between East and West Germany even for the same type of jobs. For agricultural workers, white-collar employees, and production/craft occupations, the minimum wage effects in East Germany tend to be similar to the West at first (albeit at a considerably higher *level* of unemployment). At minimum wage levels between 6 and 8 euros per hour, however, the two graphs clearly diverge. For sales occupations, for service workers, and for office workers, the divergence begins at even lower levels. Our simulations indicate that the critical level at which the negative unemployment effects of the minimum wage set in differ strongly by region and by job classification. If the minimum wage were to be increased above its actual level in real terms (8.50 euros in 2015, i.e. 8.00 euros in 2010 prices), the East-West difference in the unemployment effect would likely become much larger.

The simulations for the different labor markets can be aggregated in order to derive the overall unemployment rate as a function of the minimum wage level (Figure 3.D.2). The aggregated rate is very similar to the rate from Figure 3.3, where we simulated the effect on overall unemployment based on an estimation and a simulation for the sample as a whole. Figure 3.D.3 shows the share of the different labor market segments in the overall unemployment rate.

To explore to what extent the different unemployment effects across labor market segments are driven by differences in the productivity distributions and by differences in search frictions or the opportunity cost of employment, we ran two types of counterfactual simulations. In the first one, we combine the estimated productivity distribution for each segment with the parameters estimated for the sample as a whole.[9] As Figure 3.D.4 shows, the unemployment rate as a function of the minimum wage level is clearly different across labor market segments even if the estimated parameters are assumed to be identical.

In a second counterfactual experiment, we combined the productivity distribution of the whole sample with the parameters that we estimated for the different labor market segments (Figure 3.D.5). For office and service workers, and for agriculture (in West Germany), the parameters are close to those for the whole sample (Table 3.1), and the graph for the unemployment rate is therefore also similar to the simulation for the whole sample (Figure 3.3 and reproduced in each panel of Figure 3.D.5). For production/craft workers and white-collar employees, the counterfactual graphs are below the graph for the actual specification. These labor market segments stand out both for a high rate of job arrivals over job destructions (and therefore relatively low frictional unemployment) and for a high mean and variance of the reservation wage distribution (Table 3.1). Our second counterfactual experiment shows that if the entire labor market were characterized by these parameters, the unemployment rate would be lower, so the effect of the lower frictional unemployment dominates the effect of the higher reservation wages. For agriculture in East Germany and for sales we see the inverse picture in which the counterfactual graph for the unemployment rate based on the parameters for these labor market segments is above the graph based on the parameters that were actually estimated for the whole sample.

3.9 Conclusion

In this article we estimate the equilibrium job search model by Bontemps et al. (1999), extended to allow for different job offer arrival rates for the employed and the unemployed based on German administrative data. We use the model to simulate the effects of different minimum wage levels, with a focus on unemployment. Our study is the first assessment of the new German minimum wage based on a structural model allowing for search frictions. Previous ex ante studies have relied on the assumption of perfect

[9] The productivity distribution of each labor market segment is implied by the non-parametric estimation of the wage distribution in combination with the estimated model parameters (cf. Equation 3.13) for each segment. In the counterfactual combinations, we combine these segment-specific productivity distributions with the parameters that we estimated for the whole sample. In other words, while productivity is allowed to differ, search frictions and the opportunity cost of employment are constrained to be the same in each labor market segment.

competition (Ragnitz and Thum, 2008; Bauer et al., 2009 and Knabe and Schöb, 2009), i.e. on a model that by construction does not allow for positive employment effects of a minimum wage. There have also been a small number of quasi-experimental studies on the actual introduction of the minimum wage. Bossler and Gerner (2016) compare firms with employees affected by the minimum wage with a control group of firms that are not directly affected. They estimate losses of about 40,000 to 60,000 jobs because of the 2015 minimum wage introduction. In line with our simulations, they find that this employment effect is mainly driven by plants in East Germany. The other two studies use variation in the regional bite of the minimum wage. While Garloff (2016) computes the bite among full-time employees only, Caliendo et al. (2017) include part-time and marginal employment as well. Both studies find either small or no negative effects of the minimum wage on full-time employment, and a significantly negative effect on marginal employment (a decrease of 180,000 jobs in the study by Caliendo et al.). In our simulations, a minimum wage equivalent in real terms to the one actually introduced in 2015 raises the unemployment rate by two percentage points, an increase of 18% compared to the old steady-state value. Comparing the predictions of the present study with the quasi-experimental evidence is difficult for a number of reasons. First, we focus on labor markets where a wage posting model is a good approximation and therefore exclude highly skilled individuals, i.e. the group of individuals who are least likely to be affected by the minimum wage. Second, our model assumes that prices and the productivity of firms are unaffected by the minimum wage, and we do not consider non-compliance. Based on the extent to which firms can react to the minimum wage along these margins, the unemployment effects of the minimum wage will be dampened. Finally, these studies assess the short-run effects of the minimum wage (within one year), while the present comparative statics exercise does not allow us to draw any conclusions on the time horizon for the adjustment.

Our analysis has some limitations, which, nonetheless, have in them the potential for future research. First, our dataset does not contain information on working hours, and we have therefore excluded part-time workers from the analysis. These workers tend to have relatively low hourly wages, which means that they are disproportionately affected by the minimum wage. Our ability to include part-time workers, while at the same time address the amount of heterogeneity as in the analysis presented here, is currently limited by the availability of suitable data sets that contain reliable information on hours worked.

Second, our parameter estimates are based on the period 2007–2010, i.e. the years of the "Great Recession" in the wake of the financial crisis. As the equilibrium relationships are derived for a steady-state of the model, this may at first glance suggest that one important assumption underlying the relationship between the model's parameters and the endogenous variables is violated. However, the recession was actually fairly mild and short-lived in Germany compared to other countries, and our parameter estimates fall well within the range estimated for years before the recession in Germany (Holzner and Launov, 2010 and Nanos and Schluter, 2014) and for France in the 1990s (Bontemps et al., 1999). This increases our confidence that our results and conclusions are not too far off the mark.

Third, the negative unemployment effects in our model are driven by the assumption that the job offer arrival rates are proportional to the number of firms that manage to achieve positive profits at a given minimum wage level. While the assumption seems somewhat ad hoc, Bontemps et al. (1999) point out that such a relationship is consistent with a theoretical model developed by Bontemps et al. (2000). Still, future research should further corroborate this assumption, drawing for example on direct evidence concerning job offers. An alternative would be to endogenize job offer arrival rates in the model, as in Shephard (2017) and Engbom and Moser (2017).

By estimating a structural model, our study contributes to highlighting the transmission mechanisms underlying the employment effects of a uniform minimum wage. We document significant heterogeneity in search frictions and in reservation wages across labor markets differentiated by region and/or type of occupation. Given that the minimum wage is motivated by a desire to offset firms' monopsony power, this suggests that a uniform minimum wage is perhaps too blunt a tool. However, this conclusion is dependent on the extent to which the minimum rate of 8.50 euros is relevant for the different labor market segments, so differences in the search frictions alone need not be sufficient cause for deviating from a uniform rate. There may also be practical advantages of a single minimum wage, such as greater transparency and lower administrative costs. Assessing these is beyond the scope of our study. In future research, it will be interesting to study the correlates of these regional and sectoral differences in search frictions and hence firms' market power. For instance, to what extent are they driven by differences in workers' characteristics across these labor market segments, and how important is the role of firm characteristics, market structure, and union coverage?

3.Appendix

3.A Data Preparation

3.A.1 Data Cleaning and Imputation

Imputation of missing information To maximize the available information, we fill in missing values using the full dataset, i.e. prior to imposing our sample selection criteria. When imputing missing information for the variable nationality, we first use information from parallel spells for the same individual, then information from previous spells and, if there are still missing values, with information from later spells. In a similar manner, we fill in missing information on region, sector, job title, position and employment status with information of previous and following spells but only if individuals stay at the same workplace.

Educational status Missing and inconsistent data on education are corrected according to the imputation procedure IP1 described in Fitzenberger et al. (2006). This procedure relies, roughly speaking, on the assumption that individuals cannot lose their educational degrees. Information on educational status will be aggregated in three values:

- Low-skilled: High school diploma or no qualifications.
- Medium-skilled: Completed vocational training.
- High-skilled: Technical college degree or university degree.

The final sample used in the analysis consists only of low- and medium-skilled individuals.

3.A.2 Definition of Labor Market States

Employment Employment spells include continuous periods of employment (allowing gaps of up to four weeks) subject to social security contributions and (after 1998) marginal employment. For parallel spells of employment and unemployment (e.g. for those individuals who in addition to their earnings receive supplementary benefits), we treat employment as the dominant labor market state. Employment spells during which individuals receive welfare benefits on top of their wage (*Aufstocker*) are disregarded. It is possible that individuals have multiple employment spells at the same time. In this case, only the predominant employment spell is kept. The predominant spell is determined as follows: full-time spells outrank part-time spells. When choosing between two full-time or two part-time spells, the spell with the longest duration is kept. To break any remaining ties, the spell with the highest wage wins.

Unemployment Unemployment spells include periods of registered job searching as well as periods of receiving benefits. Prior to 2005, the latter include benefits such as unemployment insurance and means-tested unemployment assistance benefits. Those (employable) individuals who were not entitled to unemployment insurance or assistance benefits could claim means-tested social assistance benefits. However, prior to 2005, spells of receiving social assistance can only be observed in the data if the job seekers' history records social assistance recipients as searching for a job. After 2004, means-tested unemployment and social assistance benefits were merged into one unified benefit, known as 'unemployment benefit II' (ALG II). Unemployment spells during which individuals receive ALG II are recorded in the data from 2007 onwards, meaning that the data only provide a consistent definition of unemployment for the period 2007-2010. We therefore restrict our estimation sample to this period.

Distinction between un- and non-employment Extending the procedure proposed by Sokbae and Wilke (2009), involuntary unemployment is defined as comprising all continuous periods of registered job searching and/or receipt of benefits. Gaps between such unemployment periods or gaps between receiving benefits or job searching and a new employment spell may not exceed four weeks, otherwise these periods are considered as non-employment spells (involving voluntary unemployment or leaving the social security labor force). Similarly, gaps between periods of employment and receiving benefits or job searching are treated as involuntary unemployment as long as the gap does not exceed six weeks, otherwise the gap is treated as non-employment.

3.A.3 Weekly Hours of Work

While we observe whether an individual works full-time or part-time (defined as working less than 30 hours per week), the data lack explicit information on the number of hours worked. We only look at full-time employees and assign hours of work in the following way:

Main specification: imputation We complement the administrative data using the German Microcensus. To calculate hourly wages for full-time employment spells, we impute hours of work based on information from the German Microcensus. The imputation is done separately by region, sex, sector, job classification, and educational degree.

Alternative specification: 40 hours for everyone In a variant, we assume 40 hours of work per week for all individuals in full-time employment.

3.A.4 Assignment of Wages

In our data, continuous employment spells may consist of a sequence of different spells with time-varying information of daily wages. To address this issue, we adopt two different variants to assign wages to one continuous employment spell. We also assign part- and full-time status consistent with these rules.

Main specification: average over one year We assign the duration-weighted average wage confined to the last observed year for employment spell before and without a transition. For subsequent employment spells, the wage information used is an average daily wage in the first year after the transition. An individual is considered mainly full-time employed, if the weighted average duration of full-time spells over one year exceeds 50%.

Alternative specification: last and first observations For employment spells before a transition and employment spells without a transition, the last observed wage is assigned. For subsequent employment spells, the first observed wage is assigned. The last part-/full-time status is assigned to the previous employment spell, whereas the first part-/full-time status is assigned to any subsequent employment spell.

3.A.5 Data Preparation – SSC threshold

Gross daily wages are right-censored at the upper limit for social security contributions.

Main specification: exclusion of censored observations We do not include observations with censored wages.

Alternative specification: imputation To analyse this problem, we construct cells based on gender, year, region (East and West Germany), and educational degree. For each cell, a Tobit regression is estimated with log daily wages as the dependent variable and age, age squared, nationality, experience, experience squared, tenure in the current employment, tenure in the current employment squared, two skill dummies, occupational, sectoral as well as regional (Federal State) dummies and dummies for part-time and full-time employment as explanatory variables. As described in Gartner (2005), right-censored observations are replaced by wages randomly drawn from a truncated normal distribution whose moments are constructed by the predicted values from the Tobit regressions and whose (lower) truncation point is given by the contribution limit to the social security system. After this imputation procedure, nominal wages are deflated by the Consumer Price Index of the Federal Statistical Office Germany, normalized to 1 in 2010.

3.A.6 Definition of Sub-Samples

Region

- East Germany: Former GDR, excluding Berlin
- West Germany, including Berlin

The labor market region of an employed individual is given by the location of the workplace. For the unemployed, we use the region where an individual searches for a job. Where this information is missing, we assign the region of the previous workplace.

Job classifications

- Agriculture (*Landwirtschaftsberufe*)
- Production, craft (*Produktions-/Facharbeiter, Handwerker*)
- White-collar (*Höhere Angestellte*)
- Sales (*Vertriebs-/Verkaufstätigkeiten*)
- Office (*Bürotätigkeiten*)
- Service (*Dienstleister*)

Table 3.A.1: Employment-to-employment transitions across labor markets, percent

	After transition											
Before transition	W., Agric.	W., Prod.	W., White-c.	W., Sale	W., Office	W., Service	E., Agric.	E., Prod.	E., White-c.	E., Sale	E., Office	E., Service
W., Agric.	70.8	12.3	1.8	2.6	2.1	9.5	0.5	0.3	0.0	0.0	0.0	0.3
W., Prod.	0.3	83.9	3.6	0.9	2.6	6.3	0.0	1.9	0.1	0.0	0.1	0.2
W., White-c.	0.2	6.8	72.4	2.1	11.4	5.5	0.0	0.3	0.9	0.1	0.3	0.1
W., Sale	0.3	3.6	3.3	70.5	15.1	5.4	0.0	0.1	0.1	1.2	0.4	0.0
W., Office	0.1	1.6	4.7	3.4	85.6	2.8	0.0	0.1	0.1	0.1	1.5	0.1
W., Service	0.4	8.0	2.9	1.3	4.7	80.1	0.0	0.3	0.1	0.0	0.1	2.0
E., Agric.	7.2	1.8	0.0	0.9	0.0	0.9	67.6	10.8	1.8	0.9	0.9	7.2
E., Prod.	0.1	11.5	0.8	0.1	0.3	1.5	0.7	75.5	3.1	0.6	1.9	3.8
E., White-c.	0.0	2.7	9.8	0.2	2.2	1.3	0.2	5.8	65.4	1.5	7.5	3.5
E., Sale	0.0	0.9	0.9	15.4	3.0	0.9	0.3	4.8	2.1	58.9	7.3	5.4
E., Office	0.0	0.5	1.4	0.9	12.4	0.8	0.2	2.5	2.5	1.8	73.4	3.6
E., Service	0.1	0.8	0.4	0.1	0.9	16.0	0.5	6.7	1.6	1.3	2.5	69.1

Notes: Of a total of 43,396 employment-to-employment transitions, 95.9% remain in the same region, 83.4% remain in the same job classification, and 80.1% remain in the same region *and* job classification (labor market).
Source: SIAB 7510, own computations.

Table 3.A.2: Unemployment-to-employment transitions across labor markets, percent

	After transition											
Before transition	W., Agric.	W., Prod.	W., White-c.	W., Sale	W., Office	W., Service	E., Agric.	E., Prod.	E., White-c.	E., Sale	E., Office	E., Service
W., Agric.	78.1	11.4	0.7	1.3	2.0	5.2	1.0	0.3	0.0	0.0	0.0	0.0
W., Prod.	0.9	82.2	2.2	1.0	2.1	8.9	0.0	2.2	0.0	0.0	0.2	0.3
W., White-c.	0.7	15.2	43.5	3.2	20.8	13.8	0.0	0.7	1.1	0.0	0.4	0.7
W., Sale	0.6	17.7	3.4	43.4	15.6	15.6	0.3	0.6	0.3	0.9	0.6	0.9
W., Office	0.6	9.5	6.0	5.0	66.8	10.0	0.0	0.4	0.0	0.1	1.7	0.0
W., Service	1.1	19.7	2.7	1.7	6.7	65.1	0.0	0.8	0.1	0.1	0.2	1.9
E., Agric.	0.5	2.1	0.0	0.0	0.5	0.5	80.0	12.1	1.1	0.0	0.5	2.6
E., Prod.	0.0	7.5	0.1	0.3	0.3	1.6	2.1	76.5	1.8	0.7	1.6	7.6
E., White-c.	1.0	2.9	5.8	0.0	2.9	3.9	1.0	16.5	34.0	1.0	19.4	11.7
E., Sale	1.1	3.4	1.1	14.8	5.7	4.5	1.1	18.2	1.1	34.1	5.7	9.1
E., Office	0.0	3.6	1.8	0.6	9.6	1.8	1.2	10.8	5.4	3.6	53.9	7.8
E., Service	0.0	3.0	1.6	0.0	0.8	6.3	0.8	19.1	3.3	1.1	6.0	57.9

Notes: Of a total of 8,012 unemployment-to-employment transitions, 95.3% remain in the same region, 73.7% remain in the same job classification, and 70.7% remain in the same region *and* job classification (labor market).
Source: SIAB 7510, own computations.

3.B Descriptives

Table 3.B.1: Number of observations

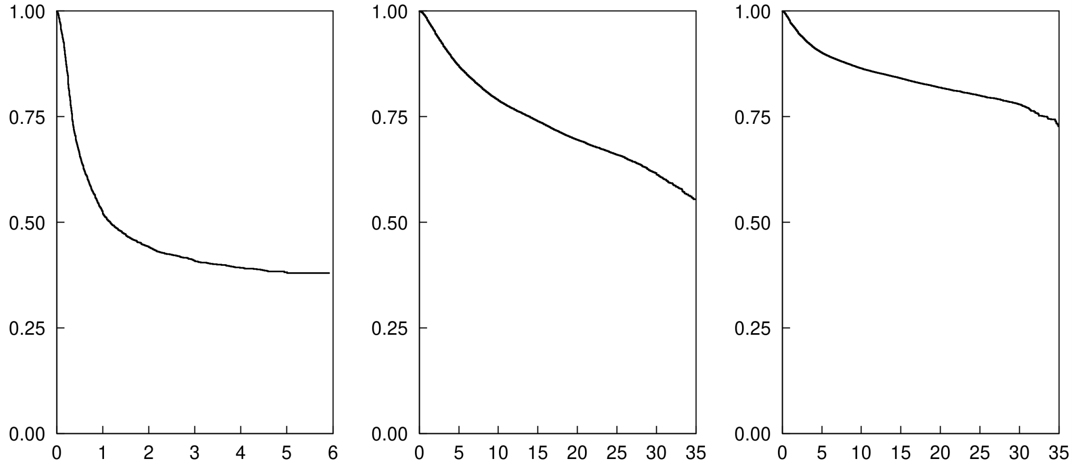
		Unemployment spells				Employment spells				
Sample	Total	Total	u → e	rc	lc	Total	e → e	e → u	rc	lc
Whole sample	235,706	17,973	8,012	9,961	3,962	217,733	43,396	27,357	146,980	2,807
West Germany										
Agriculture	2,685	456	306	150	58	2,229	390	382	1,457	9
Production, Craft	76,723	5,575	2,932	2,643	1,054	71,148	12,852	9,329	48,967	471
White-collar	16,943	728	283	445	144	16,215	3,277	1,362	11,576	109
Sales	10,080	1,086	327	759	276	8,994	2,239	1,376	5,379	42
Office	46,978	2,234	906	1,328	376	44,744	9,907	4,164	30,673	444
Service	45,250	3,837	1,448	2,389	1,126	41,413	8,577	5,193	27,643	164
East Germany										
Agriculture	1,225	311	190	121	54	914	111	237	566	76
Production, Craft	15,099	1,806	896	910	351	13,293	2,421	2,745	8,127	529
White-collar	3,081	244	103	141	54	2,837	549	330	1,958	141
Sales	1,932	273	88	185	81	1,659	331	297	1,031	29
Office	6,655	439	167	272	97	6,216	1,157	717	4,342	367
Service	9,055	984	366	618	291	8,071	1,585	1,225	5,261	426

Notes: Arrows (→) indicate that spells end in transitions to another employment spell (e) or to unemployment (u). Spells without an observed transition are right-censored (rc). Additionally, spells might be left-censored (lc).
Source: SIAB 7510, own computations.

Table 3.B.2: Percentage of spell types

		Unemployment spells				Employment spells				
Sample	Total	Total	u → e	rc	lc	Total	e → e	e → u	rc	lc
Whole sample	100.0%	7.6%	44.6%	55.4%	22.0%	92.4%	19.9%	12.6%	67.5%	1.3%
West Germany										
Agriculture	100.0%	17.0%	67.1%	32.9%	12.7%	83.0%	17.5%	17.1%	65.4%	0.4%
Production, Craft	100.0%	7.3%	52.6%	47.4%	18.9%	92.7%	18.1%	13.1%	68.8%	0.7%
White-collar	100.0%	4.3%	38.9%	61.1%	19.8%	95.7%	20.2%	8.4%	71.4%	0.7%
Sales	100.0%	10.8%	30.1%	69.9%	25.4%	89.2%	24.9%	15.3%	59.8%	0.5%
Office	100.0%	4.8%	40.6%	59.4%	16.8%	95.2%	22.1%	9.3%	68.6%	1.0%
Service	100.0%	8.5%	37.7%	62.3%	29.3%	91.5%	20.7%	12.5%	66.7%	0.4%
East Germany										
Agriculture	100.0%	25.4%	61.1%	38.9%	17.4%	74.6%	12.1%	25.9%	61.9%	8.3%
Production, Craft	100.0%	12.0%	49.6%	50.4%	19.4%	88.0%	18.2%	20.6%	61.1%	4.0%
White-collar	100.0%	7.9%	42.2%	57.8%	22.1%	92.1%	19.4%	11.6%	69.0%	5.0%
Sales	100.0%	14.1%	32.2%	67.8%	29.7%	85.9%	20.0%	17.9%	62.1%	1.7%
Office	100.0%	6.6%	38.0%	62.0%	22.1%	93.4%	18.6%	11.5%	69.9%	5.9%
Service	100.0%	10.9%	37.2%	62.8%	29.6%	89.1%	19.6%	15.2%	65.2%	5.3%

Notes: Arrows (→) indicate that spells end in transitions to another employment spell (e) or to unemployment (u). Spells without an observed transition are right-censored (rc). Additionally, spells might be left-censored (lc). Columns Total unemployment spells and Total employment spells refer to column Total as 100%. Columns u → e , rc and lc refer to Column Total unemployment spells as 100%. Columns e → e, e → u , rc and lc refer to Column Total employment spells as 100%.
Source: SIAB 7510, own computations.

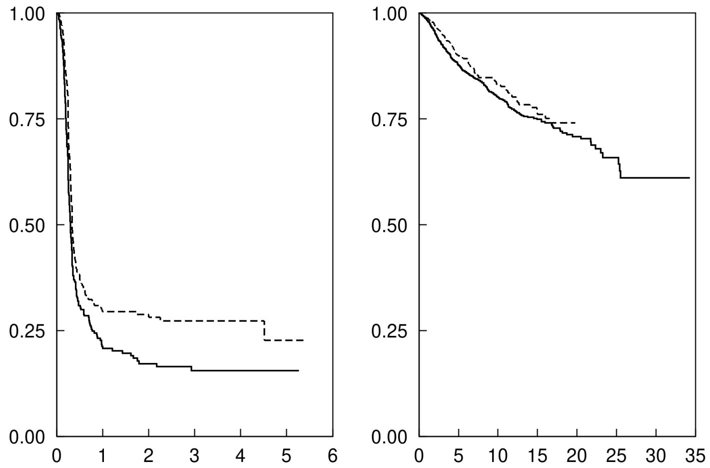


(a) u → e *(b)* e → e *(c)* e → u

Notes: Plots show Kaplan-Meier survival estimate for durations in years. Arrows (→) indicate that spells end in another employment spell (e) or unemployment (u).

Source: SIAB 7510, own computations.

Figure 3.B.1: Survival rates (whole sample)

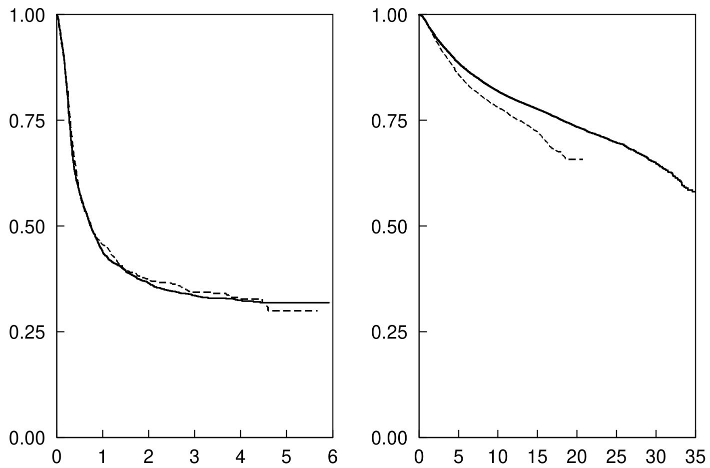


(a) u → e *(b)* e → e *(c)* e → u

Key: West (——); East (- - - -). *Notes:* Plots show Kaplan-Meier survival estimate for durations in years. Arrows (→) indicate that spells end in another employment spell (e) or unemployment (u).

Source: SIAB 7510, own computations.

Figure 3.B.2: Survival rates by region – agriculture

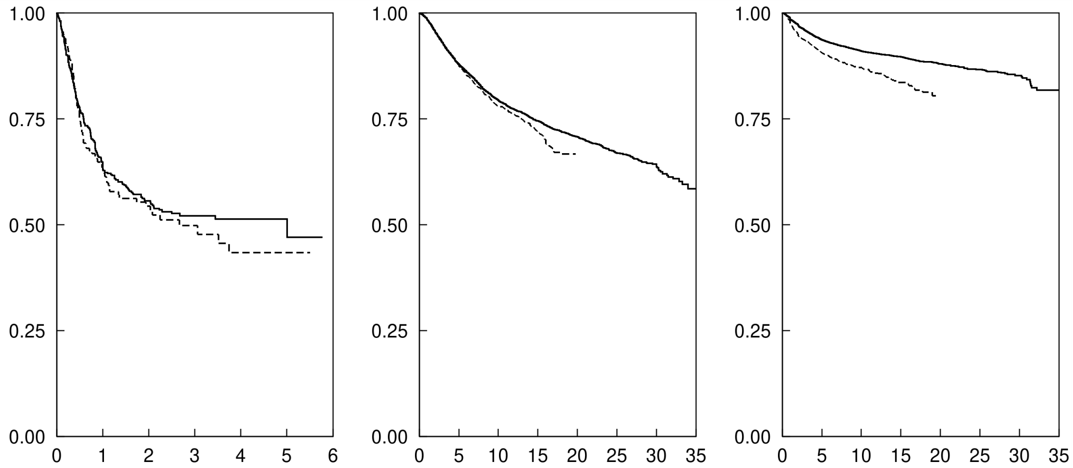


(a) u → e *(b)* e → e *(c)* e → u

Key: West (——); East (- - - -). *Notes:* Plots show Kaplan-Meier survival estimate for durations in years. Arrows (→) indicate that spells end in another employment spell (e) or unemployment (u).

Source: SIAB 7510, own computations.

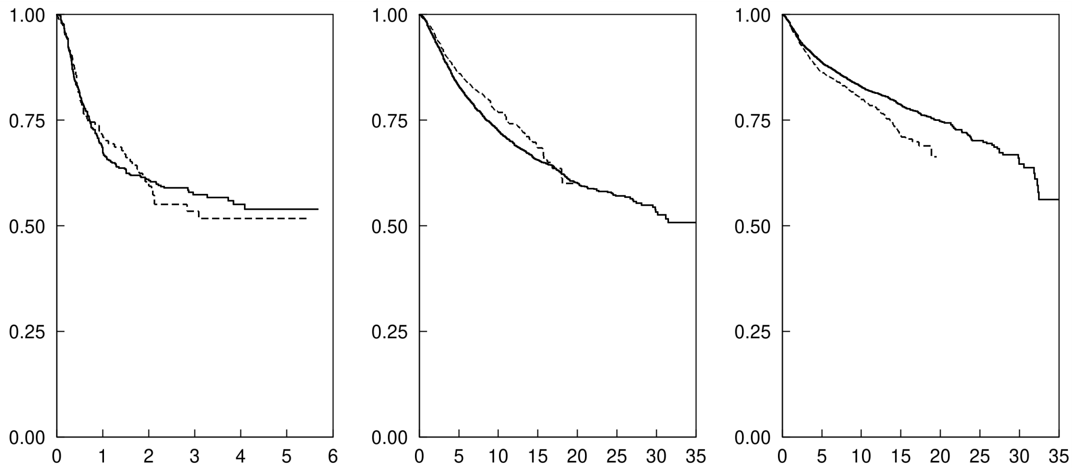
Figure 3.B.3: Survival rates by region – production, craft



(a) u → e *(b)* e → e *(c)* e → u

Key: West (——); East (- - - -). *Notes:* Plots show Kaplan-Meier survival estimate for durations in years. Arrows (→) indicate that spells end in another employment spell (e) or unemployment (u).
Source: SIAB 7510, own computations.

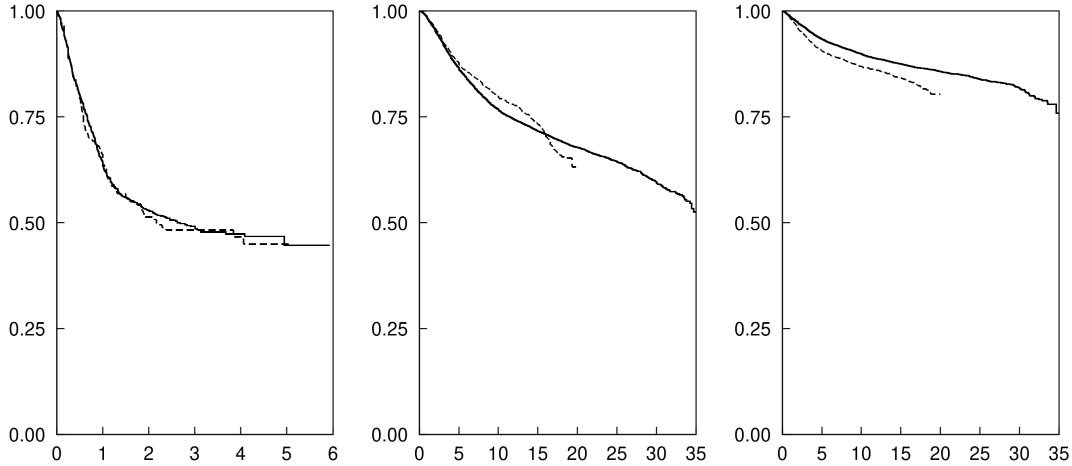
Figure 3.B.4: Survival rates by region – white-collar



(a) u → e *(b)* e → e *(c)* e → u

Key: West (——); East (- - - -). *Notes:* Plots show Kaplan-Meier survival estimate for durations in years. Arrows (→) indicate that spells end in another employment spell (e) or unemployment (u).
Source: SIAB 7510, own computations.

Figure 3.B.5: Survival rates by region – sales

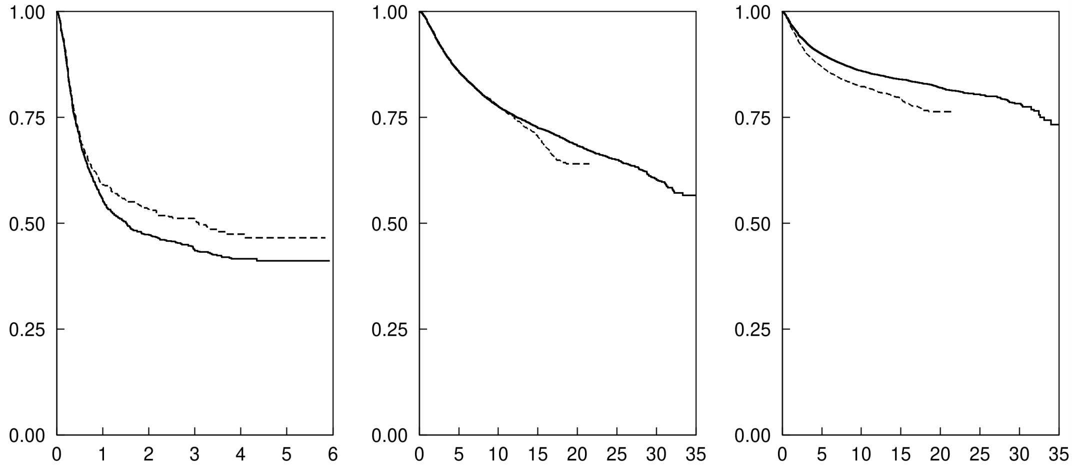


(a) u → e *(b)* e → e *(c)* e → u

Key: West (——); East (- - - -). *Notes:* Plots show Kaplan-Meier survival estimate for durations in years. Arrows (→) indicate that spells end in another employment spell (e) or unemployment (u).

Source: SIAB 7510, own computations.

Figure 3.B.6: Survival rates by region – office

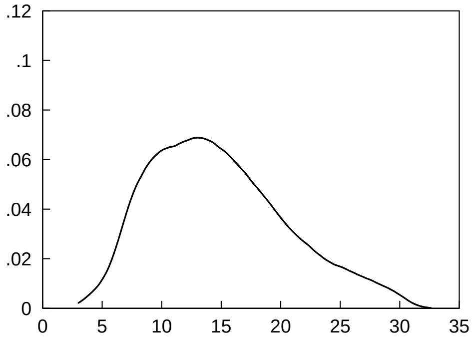


(a) u → e *(b)* e → e *(c)* e → u

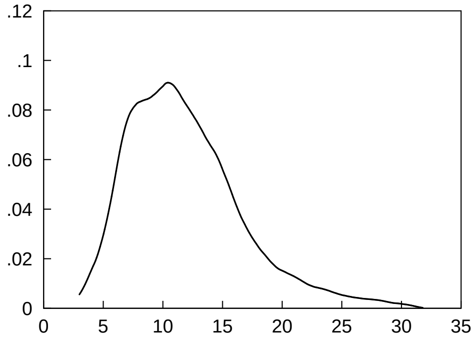
Key: West (——); East (- - - -). *Notes:* Plots show Kaplan-Meier survival estimate for durations in years. Arrows (→) indicate that spells end in another employment spell (e) or unemployment (u).

Source: SIAB 7510, own computations.

Figure 3.B.7: Survival rates by region – service

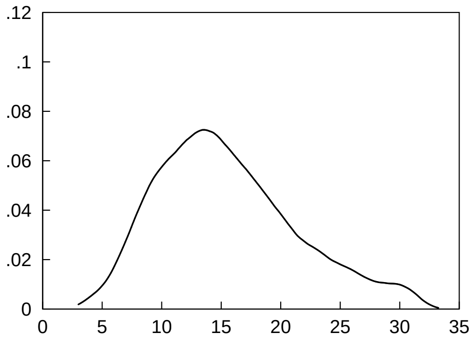


(a) e → e

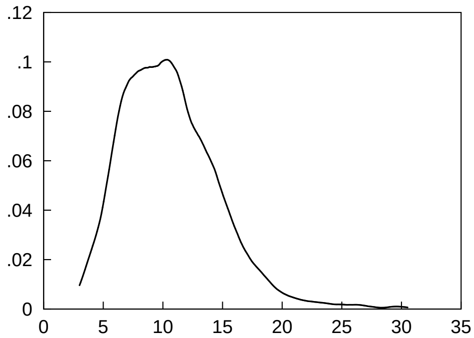


(b) e → u

Panel A – Before transition

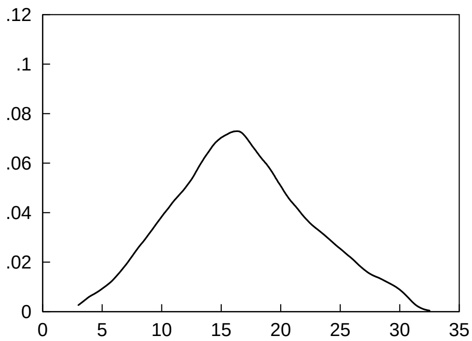


(c) e → e

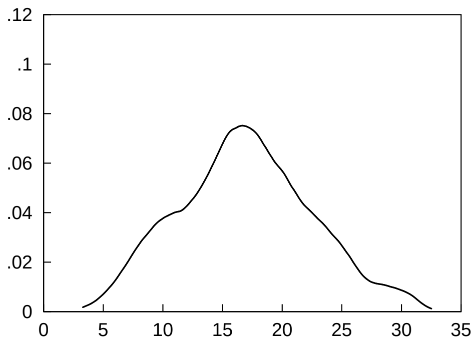


(d) u → e

Panel B – After transition



(e) right-censored

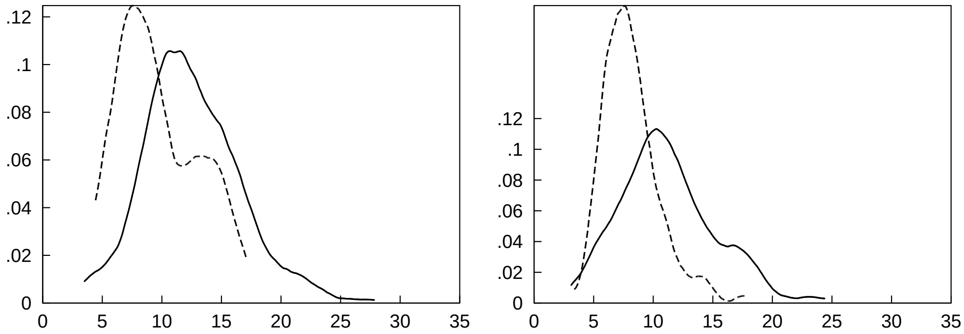


(f) left-censored

Panel C – Censored spells

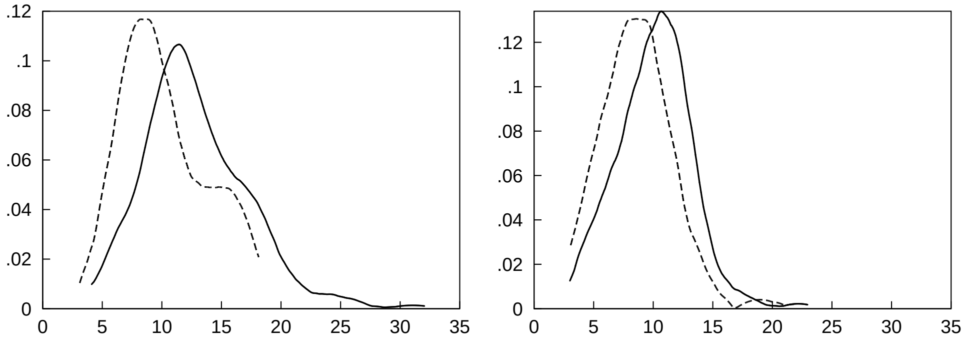
Notes: Epanechnikov kernel density estimate. Arrows (→) indicate that spells end in another employment spell (e) or unemployment (u). Spells without an observed transition are right-censored. Additionally, spells might be left-censored.
Source: SIAB 7510, own computations.

Figure 3.B.8: Density of hourly wages (whole sample)



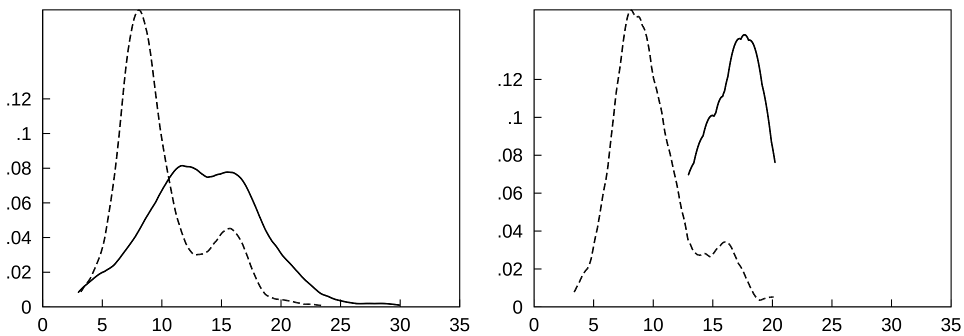
(a) e → e *(b)* e → u

Panel A – Before transition



(c) e → e *(d)* u → e

Panel B – After transition



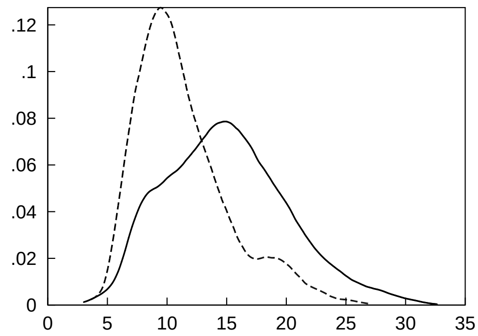
(e) right-censored *(f)* left-censored

Panel C – Censored spells

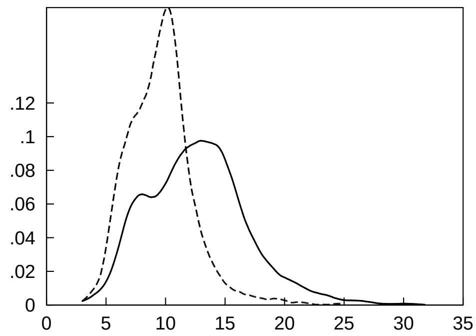
Key: West (——); East (- - - -). *Notes:* Epanechnikov kernel density estimate. Arrows (→) indicate that spells end in another employment spell (e) or unemployment (u). Spells without an observed transition are right-censored. Additionally, spells might be left-censored.

Source: SIAB 7510, own computations.

Figure 3.B.9: Density of hourly wages by region – agriculture

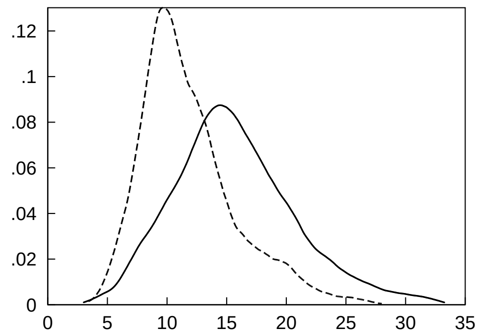


(a) e $\rightarrow$ e

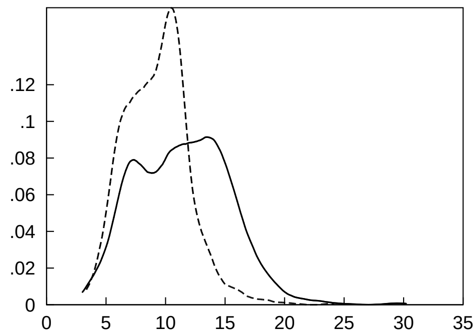


(b) e $\rightarrow$ u

Panel A – Before transition

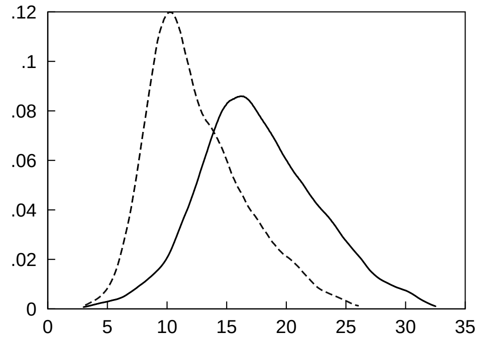


(c) e $\rightarrow$ e

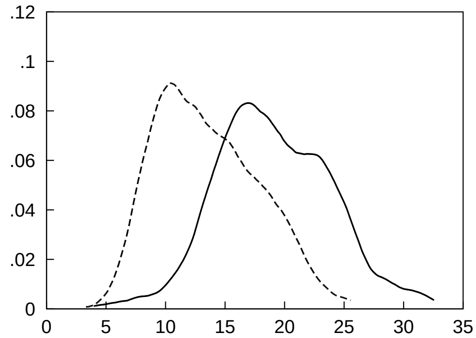


(d) u $\rightarrow$ e

Panel B – After transition



(e) right-censored



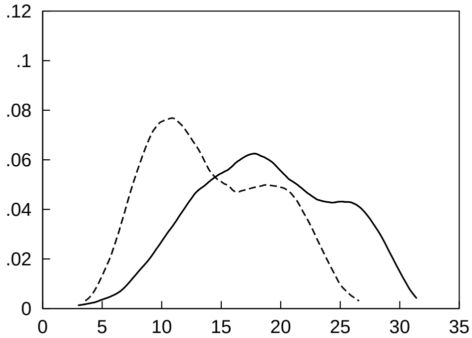
(f) left-censored

Panel C – Censored spells

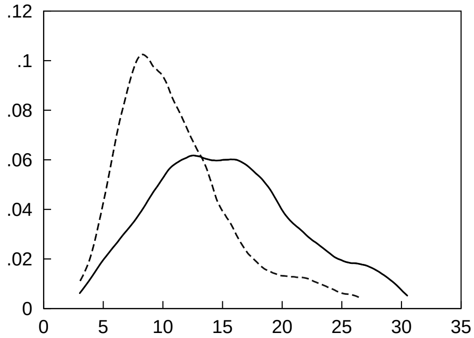
Key: West (——); East (- - - -). *Notes:* Epanechnikov kernel density estimate. Arrows ($\rightarrow$) indicate that spells end in another employment spell (e) or unemployment (u). Spells without an observed transition are right-censored. Additionally, spells might be left-censored.

Source: SIAB 7510, own computations.

Figure 3.B.10: Density of hourly wages by region – production, craft

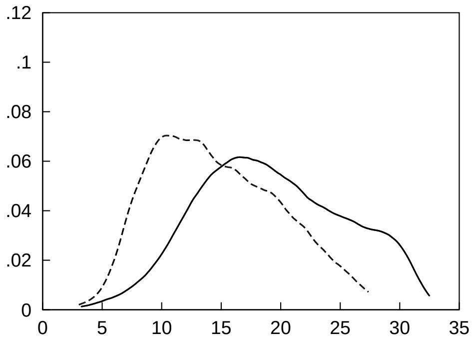


(a) e → e

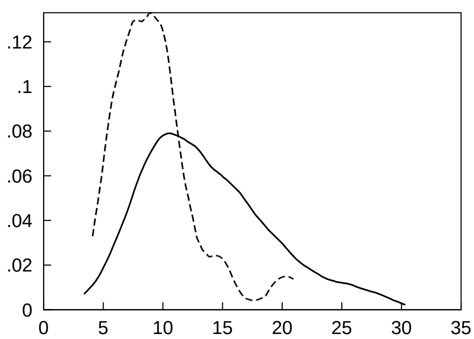


(b) e → u

Panel A – Before transition

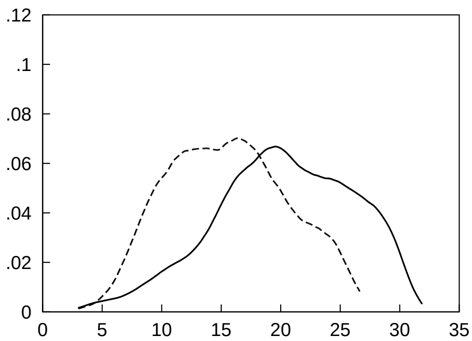


(c) e → e

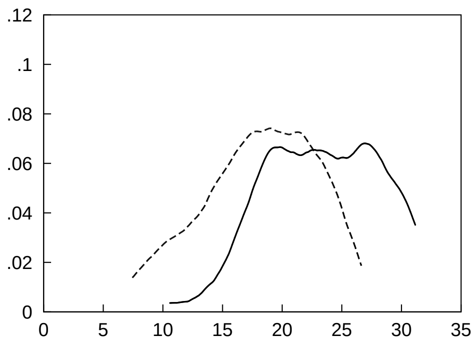


(d) u → e

Panel B – After transition



(e) right-censored



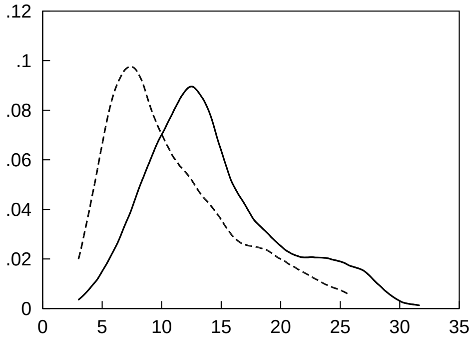
(f) left-censored

Panel C – Censored spells

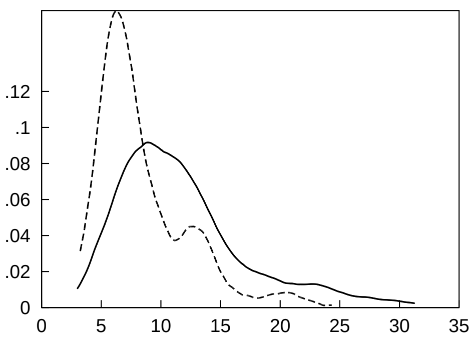
Key: West (——); East (- - - -). *Notes:* Epanechnikov kernel density estimate. Arrows (→) indicate that spells end in another employment spell (e) or unemployment (u). Spells without an observed transition are right-censored. Additionally, spells might be left-censored.

Source: SIAB 7510, own computations.

Figure 3.B.11: Density of hourly wages by region – white-collar

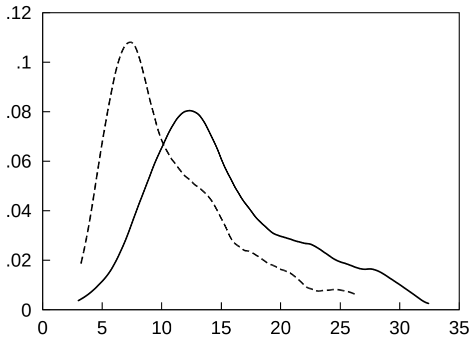


(a) e → e

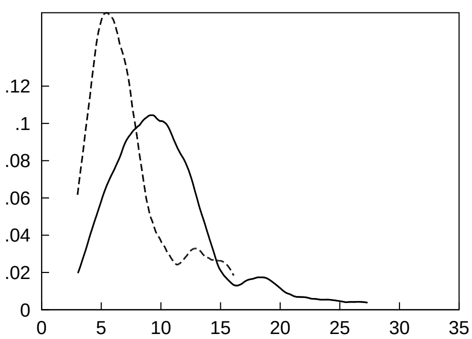


(b) e → u

Panel A – Before transition

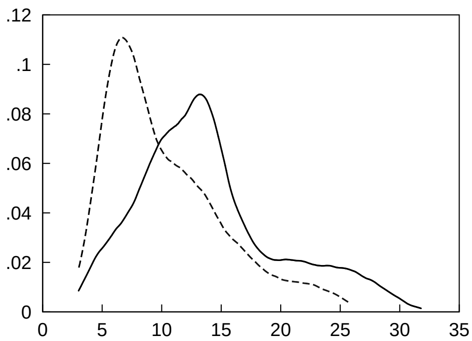


(c) e → e

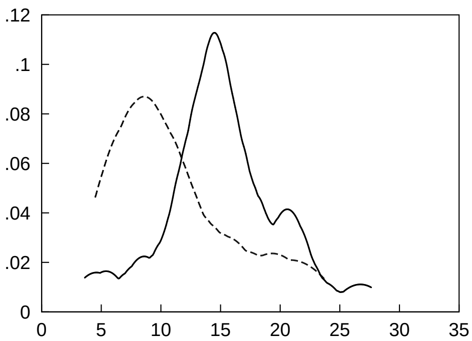


(d) u → e

Panel B – After transition



(e) right-censored



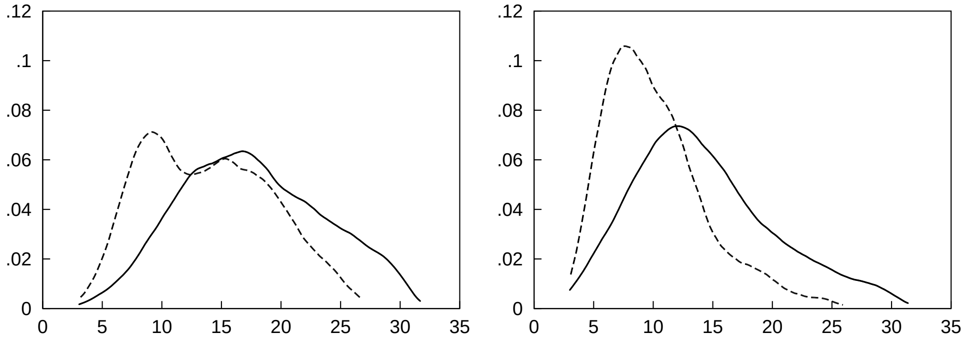
(f) left-censored

Panel C – Censored spells

Key: West (——); East (- - - -). *Notes:* Epanechnikov kernel density estimate. Arrows (→) indicate that spells end in another employment spell (e) or unemployment (u). Spells without an observed transition are right-censored. Additionally, spells might be left-censored.

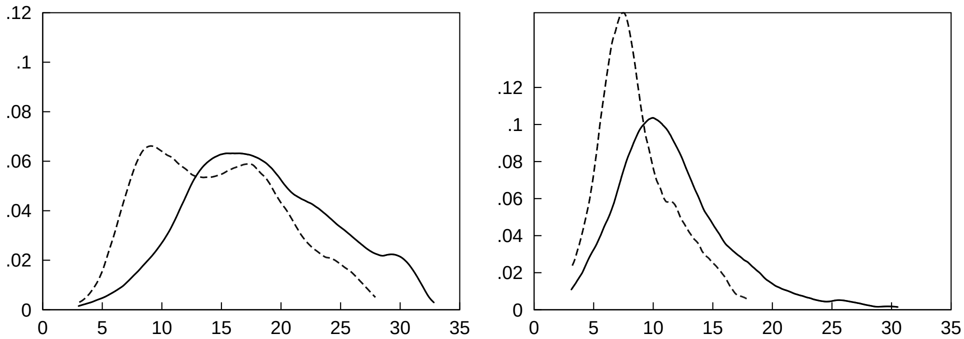
Source: SIAB 7510, own computations.

Figure 3.B.12: Density of hourly wages by region – sales



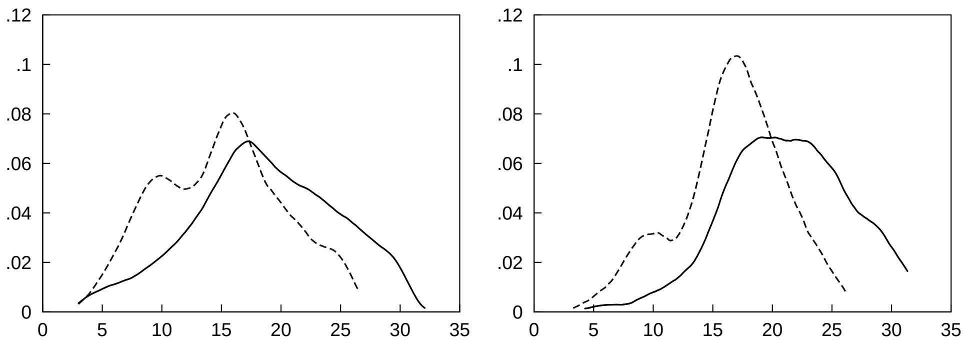
(a) e → e *(b)* e → u

Panel A – Before transition



(c) e → e *(d)* u → e

Panel B – After transition



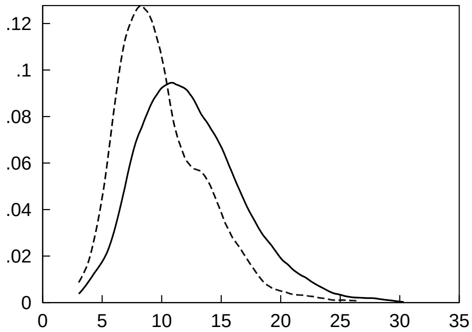
(e) right-censored *(f)* left-censored

Panel C – Censored spells

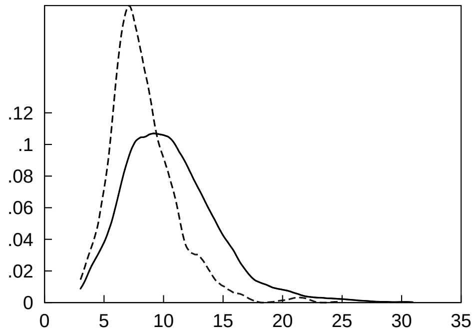
Key: West (——); East (----). *Notes:* Epanechnikov kernel density estimate. Arrows (→) indicate that spells end in another employment spell (e) or unemployment (u). Spells without an observed transition are right-censored. Additionally, spells might be left-censored.

Source: SIAB 7510, own computations.

Figure 3.B.13: Density of hourly wages by region – office

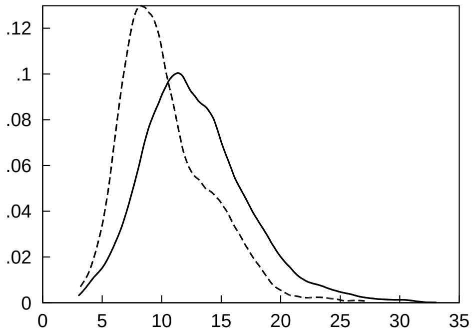


(a) e → e

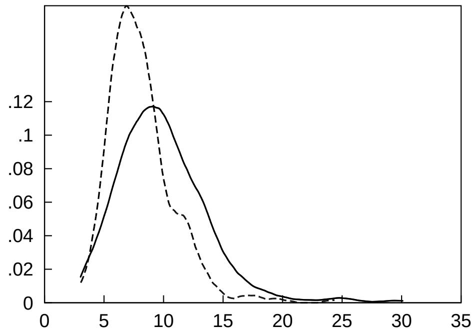


(b) e → u

Panel A – Before transition

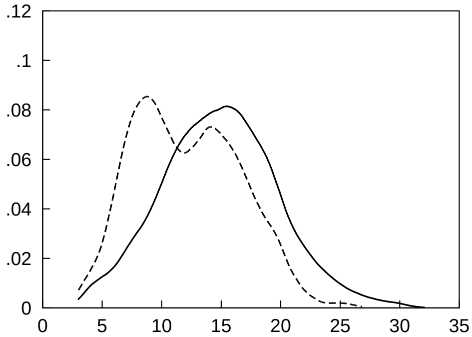


(c) e → e

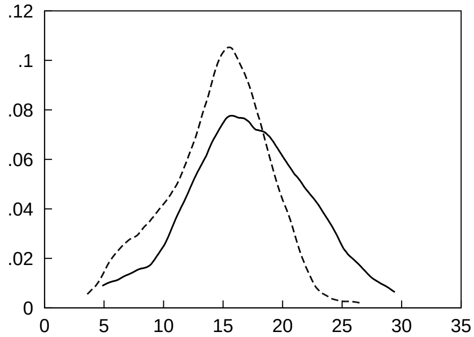


(d) u → e

Panel B – After transition



(e) right-censored



(f) left-censored

Panel C – Censored spells

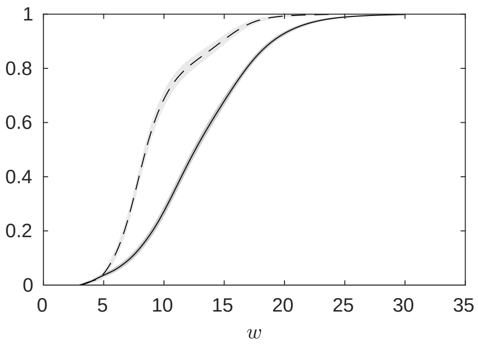
Key: West (——); East (- - - -). *Notes:* Epanechnikov kernel density estimate. Arrows (→) indicate that spells end in another employment spell (e) or unemployment (u). Spells without an observed transition are right-censored. Additionally, spells might be left-censored.

Source: SIAB 7510, own computations.

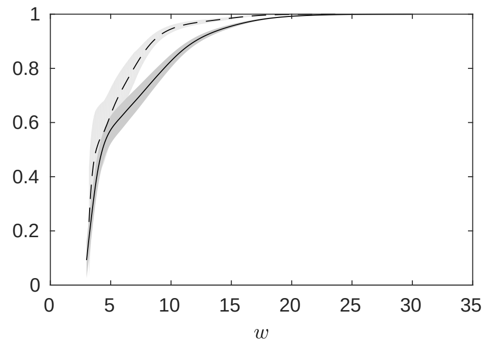
Figure 3.B.14: Density of hourly wages by region – service

3.C Estimation Results

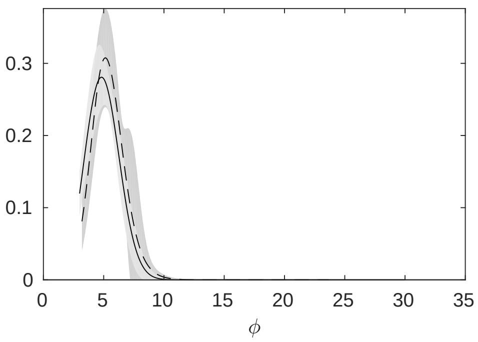
3.C.1 Equilibrium Outcomes By Job Classification and Region



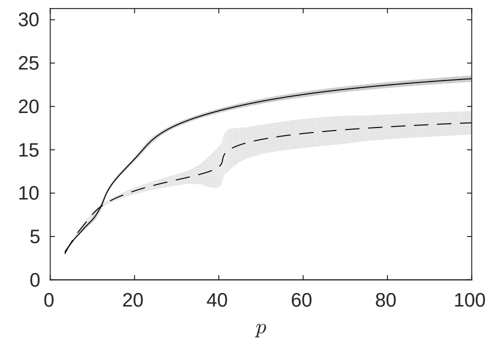
(a) Wage distribution $G(w)$



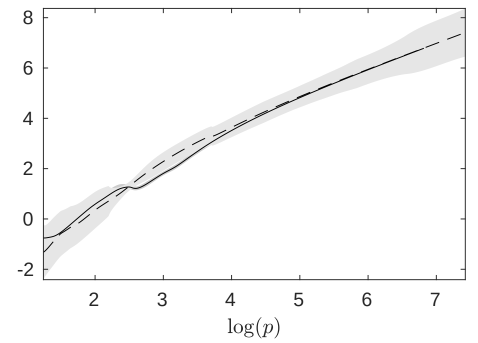
(b) Wage offer distribution $F(w)$



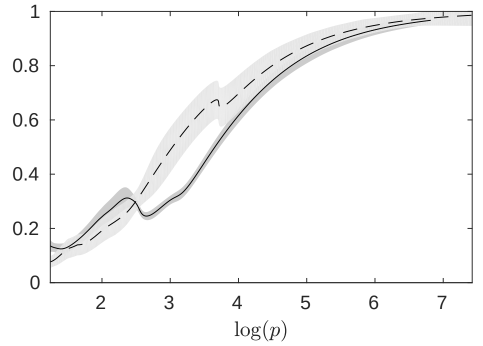
(c) Reservation wage density $A(w)$



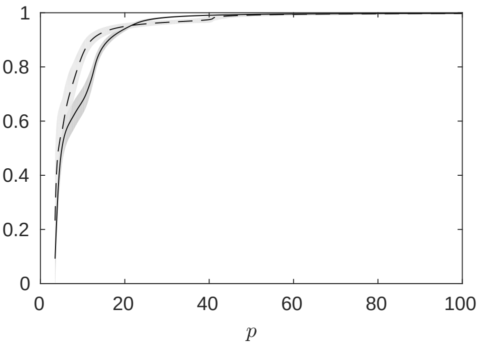
(d) Wage function $K(p)$



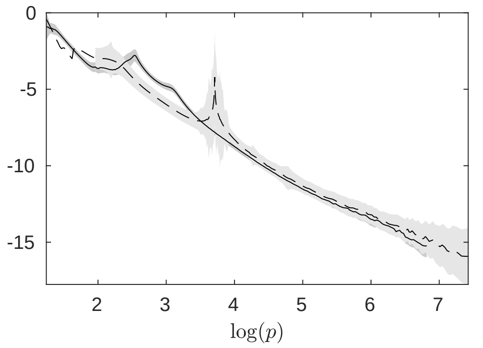
(e) Log absolute markup



(f) Monopsony power $[p-K(p)]/p$



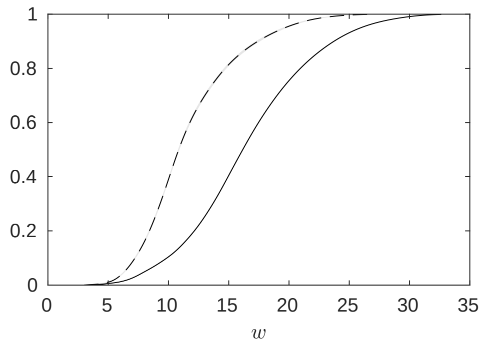
(g) Productivity distribution $\Gamma(p)$



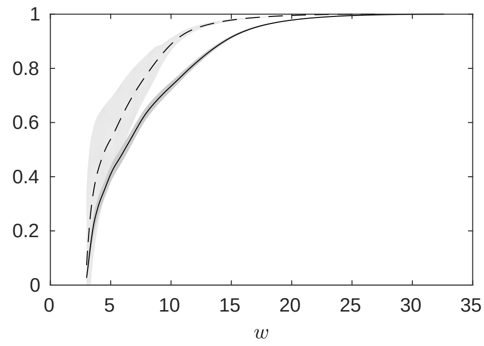
(h) Productivity log density $\log(\gamma(p))$

Key: West (——); East (- - - -). *Notes:* Grey areas indicate 95% confidence bands. *Source:* SIAB 7510, own computations.

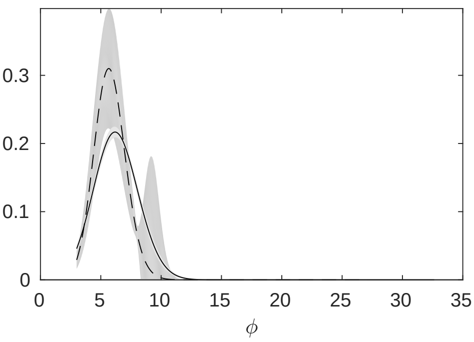
Figure 3.C.1: Main equilibrium functions by region – agriculture



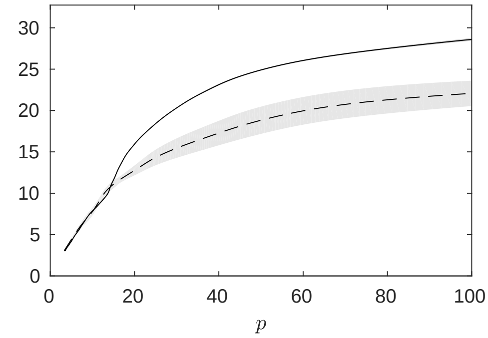
(a) Wage distribution $G(w)$



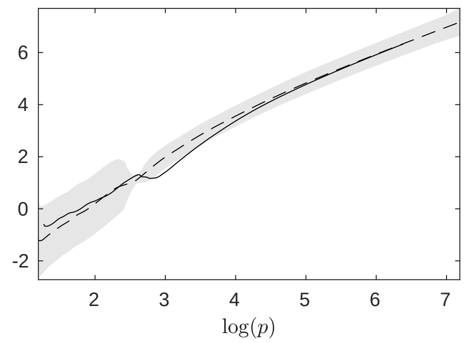
(b) Wage offer distribution $F(w)$



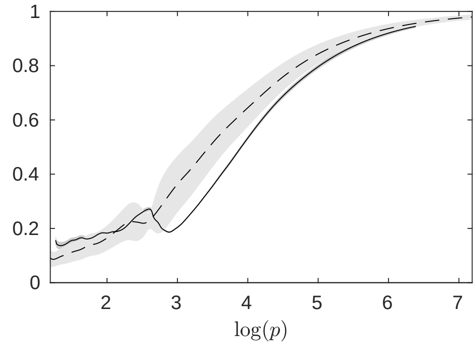
(c) Reservation wage density $A(w)$



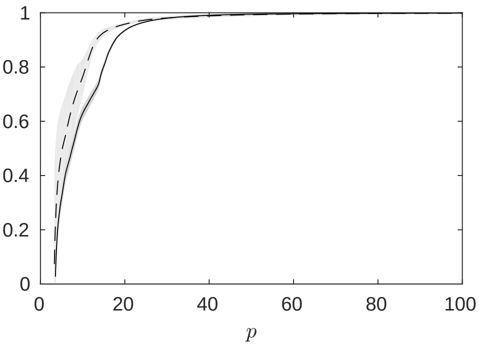
(d) Wage function $K(p)$



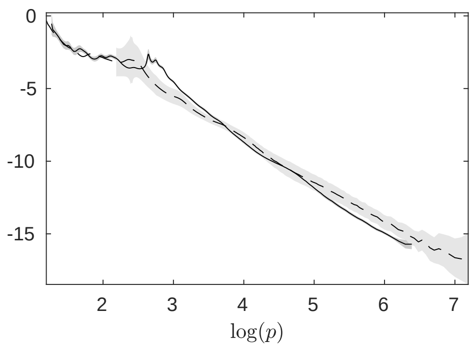
(e) Log absolute markup



(f) Monopsony power $[p-K(p)]/p$



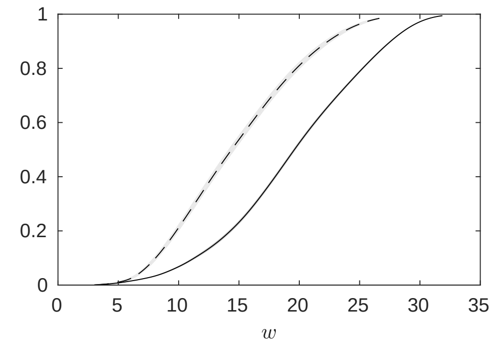
(g) Productivity distribution $\Gamma(p)$



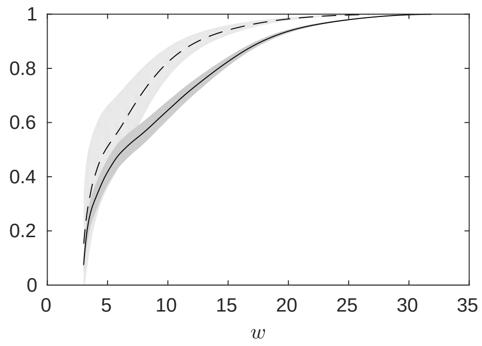
(h) Productivity log density $\log(\gamma(p))$

Key: West (——); East (- - - -). *Notes:* Grey areas indicate 95% confidence bands. *Source:* SIAB 7510, own computations.

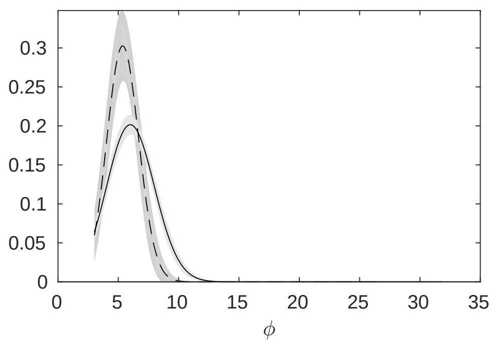
Figure 3.C.2: Main equilibrium functions by region – production, craft



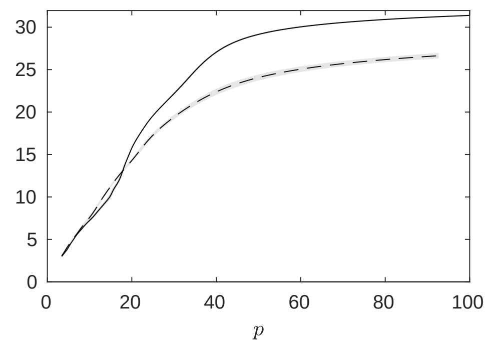
(a) Wage distribution $G(w)$



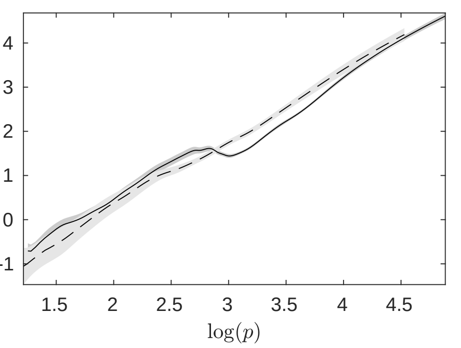
(b) Wage offer distribution $F(w)$



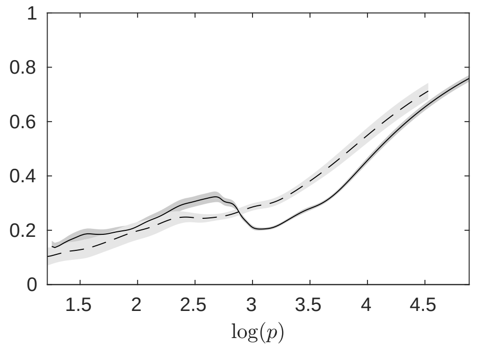
(c) Reservation wage density $A(w)$



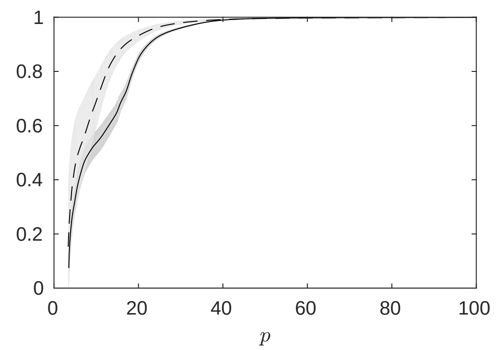
(d) Wage function $K(p)$



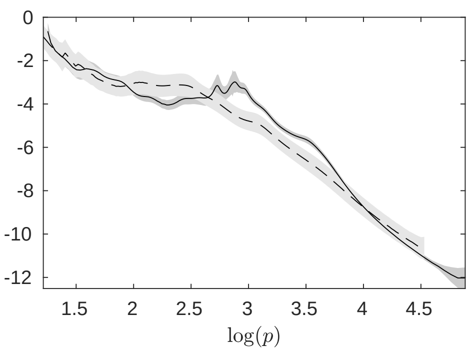
(e) Log absolute markup



(f) Monopsony power $[p - K(p)]/p$



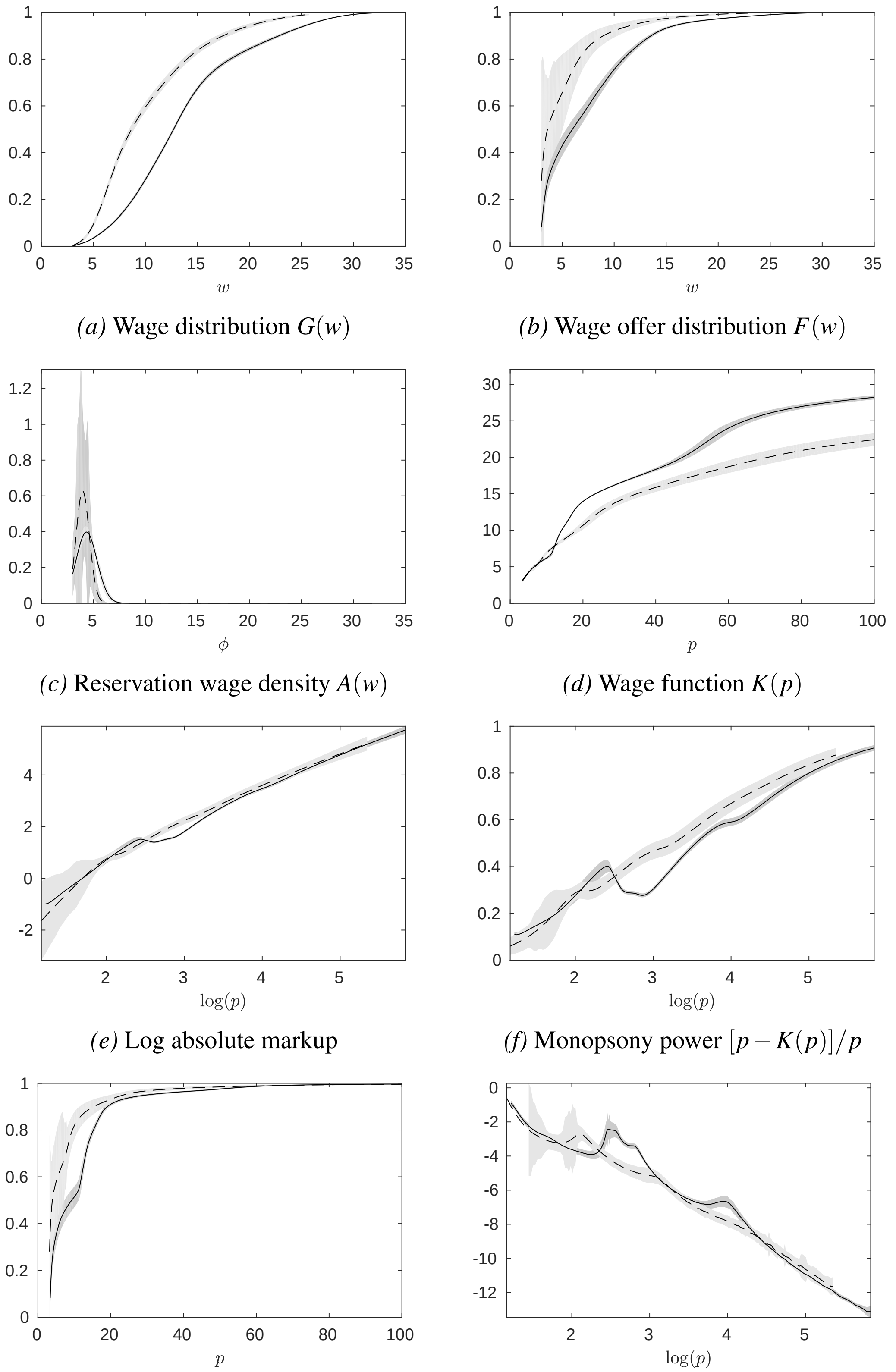
(g) Productivity distribution $\Gamma(p)$



(h) Productivity log density $\log(\gamma(p))$

Key: West (——); East (– – – –). *Notes:* Grey areas indicate 95% confidence bands. *Source:* SIAB 7510, own computations.

Figure 3.C.3: Main equilibrium functions by region – white-collar



(a) Wage distribution $G(w)$

(b) Wage offer distribution $F(w)$

(c) Reservation wage density $A(w)$

(d) Wage function $K(p)$

(e) Log absolute markup

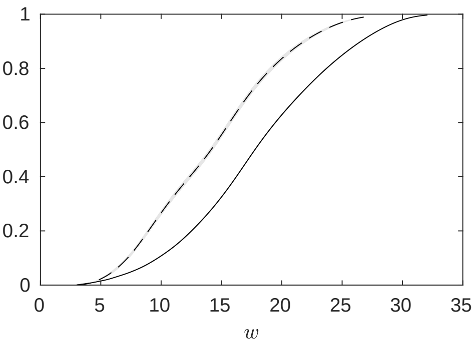
(f) Monopsony power $[p-K(p)]/p$

(g) Productivity distribution $\Gamma(p)$

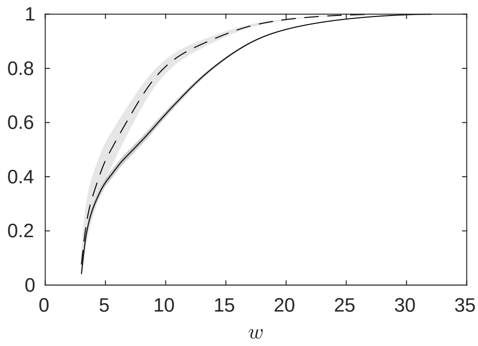
(h) Productivity log density $\log(\gamma(p))$

Key: West (——); East (– – – –). *Notes:* Grey areas indicate 95% confidence bands. *Source:* SIAB 7510, own computations.

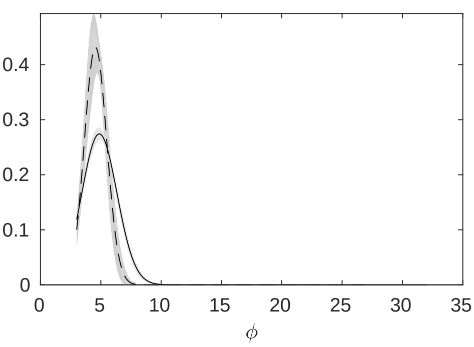
Figure 3.C.4: Main equilibrium functions by region – sales



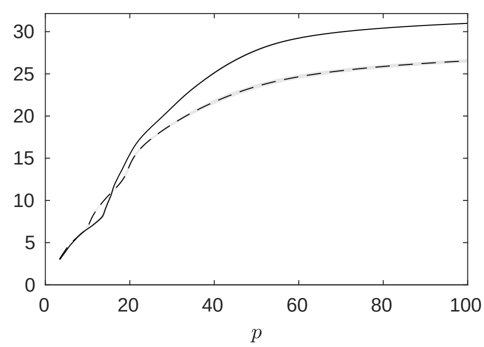
(a) Wage distribution $G(w)$



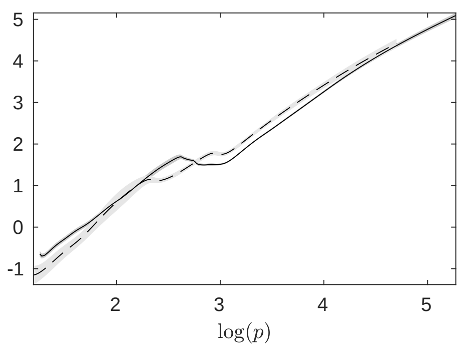
(b) Wage offer distribution $F(w)$



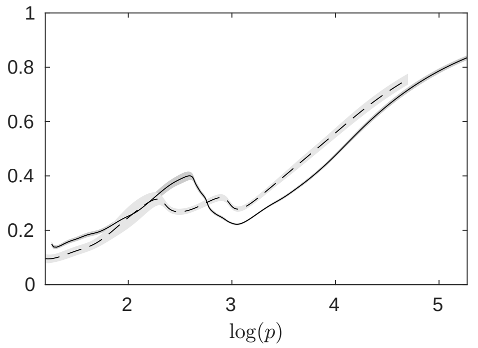
(c) Reservation wage density $A(w)$



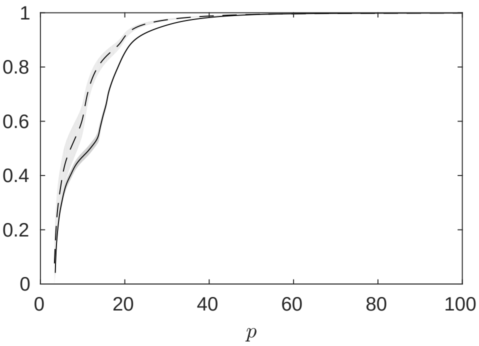
(d) Wage function $K(p)$



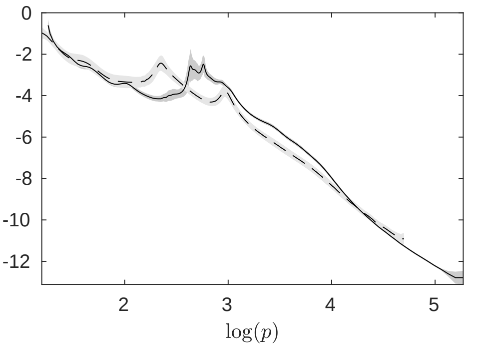
(e) Log absolute markup



(f) Monopsony power $[p - K(p)]/p$



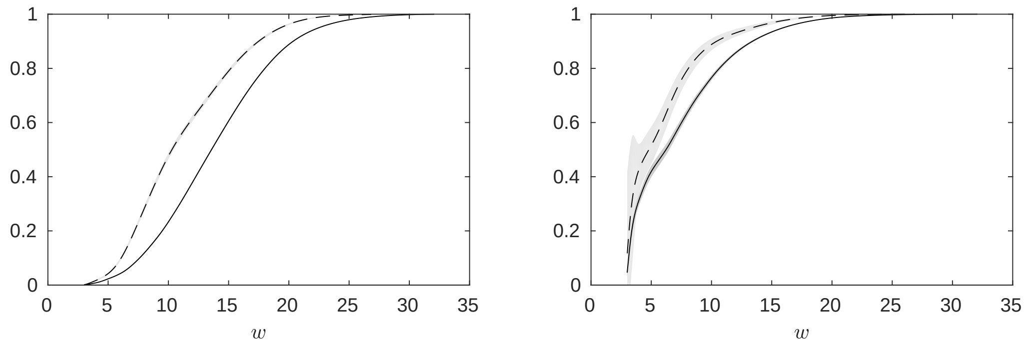
(g) Productivity distribution $\Gamma(p)$



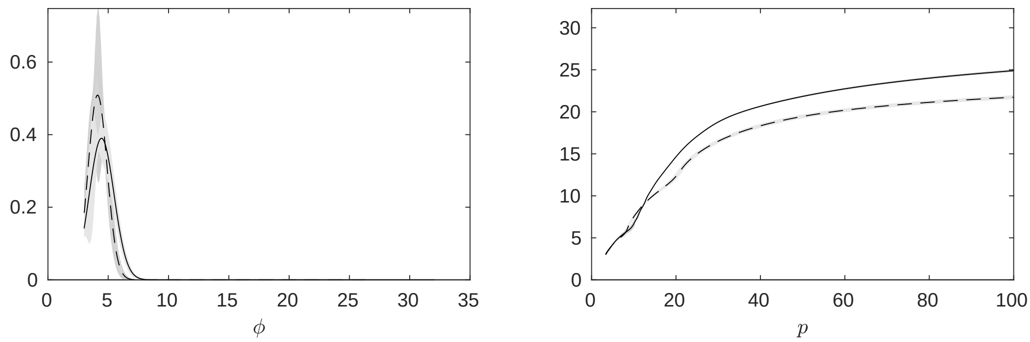
(h) Productivity log density $\log(\gamma(p))$

Key: West (——); East (- - - -). *Notes:* Grey areas indicate 95% confidence bands. *Source:* SIAB 7510, own computations.

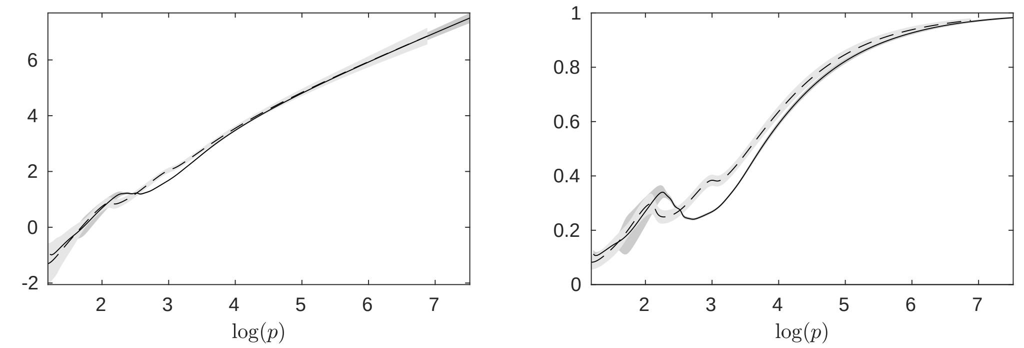
Figure 3.C.5: Main equilibrium functions by region – office



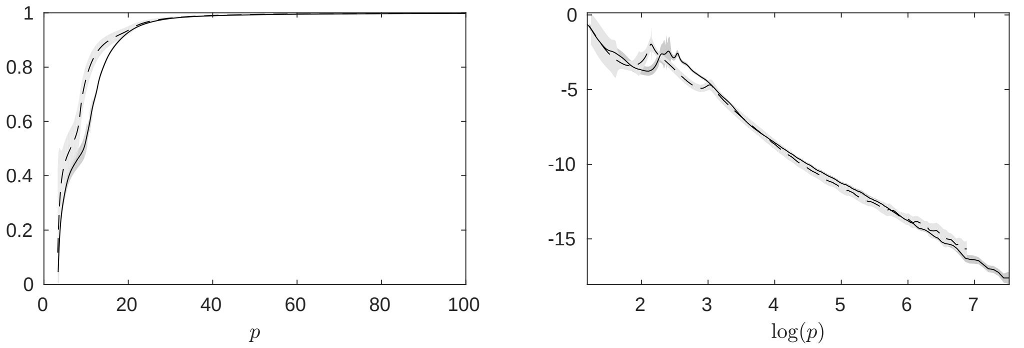
(a) Wage distribution $G(w)$ *(b)* Wage offer distribution $F(w)$



(c) Reservation wage density $A(w)$ *(d)* Wage function $K(p)$



(e) Log absolute markup *(f)* Monopsony power $[p - K(p)]/p$



(g) Productivity distribution $\Gamma(p)$ *(h)* Productivity log density $\log(\gamma(p))$

Key: West (——); East (– – – –). *Notes:* Grey areas indicate 95% confidence bands. *Source:* SIAB 7510, own computations.

Figure 3.C.6: Main equilibrium functions by region – service

3.C.2 Robustness Checks

Table 3.C.1: Robustness checks (whole sample)

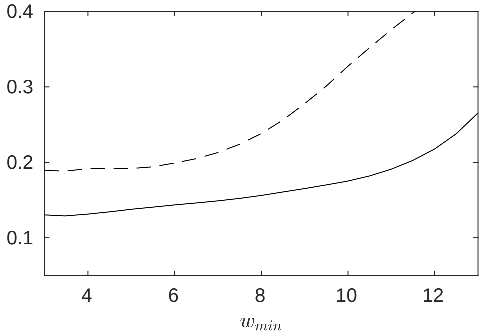
SSC threshold	Hours	Wage measure	Truncation	ρ	N	$\underline{w}$	$\overline{w}$	δ	κ_1	κ_0	μ_ϕ	μ_b	σ_b	β	u
Censored	Imputed	Avg. one year	3 Euro	0.004	235706	3.00	32.63	0.0063	8.18	13.72	4.74	0.00	1.85	0.63	0.1081
								(0.0000)	(0.09)	(0.17)	(0.03)	(0.00)	(0.02)	(0.00)	(0.0004)
Censored	40	Avg. one year	3 Euro	0.004	235756	3.00	31.46	0.0063	8.20	13.81	4.78	0.00	1.86	0.63	0.1083
								(0.0000)	(0.09)	(0.17)	(0.02)	(0.00)	(0.02)	(0.00)	(0.0004)
Imputed	Imputed	Avg. one year	3 Euro, 95%	0.004	235282	3.00	31.34	0.0063	8.19	13.74	4.74	0.00	1.85	0.64	0.1080
								(0.0000)	(0.09)	(0.17)	(0.02)	(0.00)	(0.01)	(0.00)	(0.0004)
Imputed	Imputed	Avg. one year	3 Euro, 99%	0.004	245577	3.00	47.55	0.0061	8.44	13.69	4.71	0.00	1.78	0.65	0.1067
								(0.0000)	(0.08)	(0.15)	(0.02)	(0.00)	(0.02)	(0.00)	(0.0004)
Censored	Imputed	Last and first obs.	3 Euro	0.004	234165	3.00	32.51	0.0063	8.05	13.43	4.68	0.00	1.86	0.64	0.1097
								(0.0000)	(0.10)	(0.18)	(0.03)	(0.00)	(0.02)	(0.00)	(0.0004)
Censored	Imputed	Avg. one year	2 Euro	0.004	237118	2.00	32.63	0.0063	9.37	14.77	3.97	0.00	2.22	0.64	0.1116
								(0.0000)	(0.13)	(0.23)	(0.03)	(0.00)	(0.02)	(0.00)	(0.0004)
Censored	Imputed	Avg. one year	4 Euro	0.004	234191	4.00	32.63	0.0063	7.43	13.18	5.20	0.00	1.47	0.63	0.1046
								(0.0000)	(0.07)	(0.14)	(0.02)	(0.00)	(0.02)	(0.00)	(0.0004)
Censored	Imputed	Avg. one year	3 Euro	0.002	235706	3.00	32.63	0.0063	8.26	13.14	4.64	0.00	1.73	0.32	0.1094
								(0.0000)	(0.09)	(0.16)	(0.03)	(0.00)	(0.02)	(0.00)	(0.0004)
Censored	Imputed	Avg. one year	3 Euro	0.006	235706	3.00	32.63	0.0063	8.08	14.26	4.82	0.00	1.96	0.95	0.1068
								(0.0000)	(0.08)	(0.17)	(0.03)	(0.00)	(0.02)	(0.00)	(0.0004)

Notes: First row: preferred data preparation as reported in Table 3.1. For alternative data handling at the SSC threshold, see Appendix 3.A.5. For alternative definitions of weekly hours, see Appendix 3.A.3. For wage variants, see Appendix 3.A.4. Bootstrapped standard errors in parentheses (100 runs).
Source: SIAB 7510, own computations.

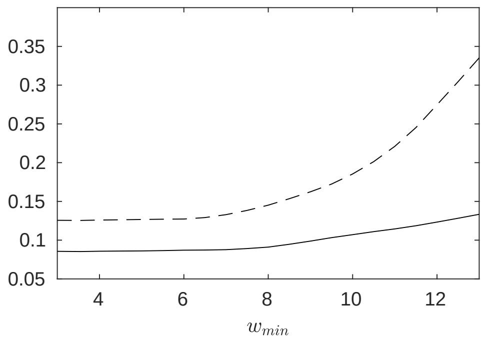
3.C.3 Bootstrapping

We report bootstrapped standard errors. In very rare cases we exclude bootstrap runs with extreme outliers according to the following criteria: a) If the likelihood does not converge: occurs in 1 of 101 bootstrap runs in whole sample, in 1 of 101 bootstrap runs in W. Agric., in 26 of 126 bootstrap runs in E. Agric., in 5 of 105 bootstrap runs in E. Sale, in 1 of 101 bootstrap runs in E. Office, in 1 of 101 bootstrap runs in the robustness check with truncation of wages at the 99th percentile, in 1 of 101 bootstrap runs in the robustness check with $\rho = 0.002$, in 3 of 103 bootstrap runs in the robustness check with $\rho = 0.006$. b) If the estimated job offer arrival rate λ_1 is 100 times higher than the job destruction rate δ: occurs in 1 of 101 bootstrap runs in E. Serv.

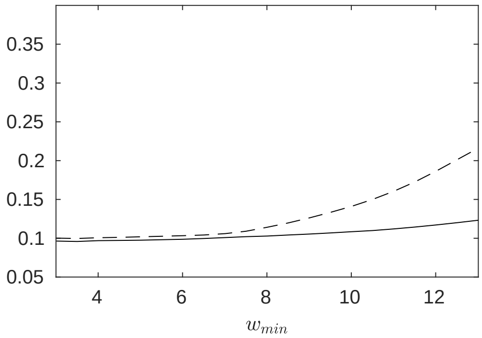
3.D Minimum Wage Simulations: Heterogeneity Between Labor Markets



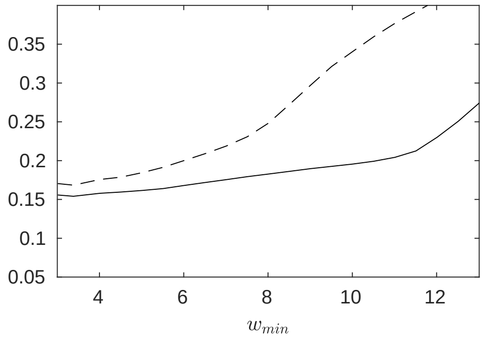
(a) Agriculture



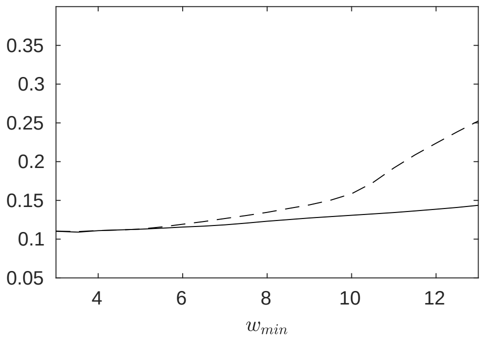
(b) Production, craft



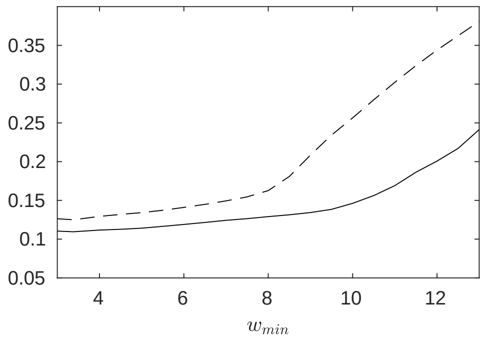
(c) White-collar



(d) Sales



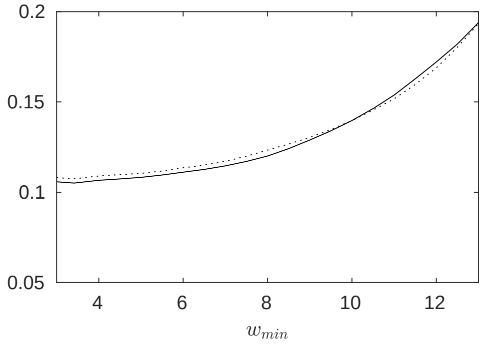
(e) Office



(f) Service

Key: West (——); East (- - - -). *Source:* SIAB 7510, own computations.

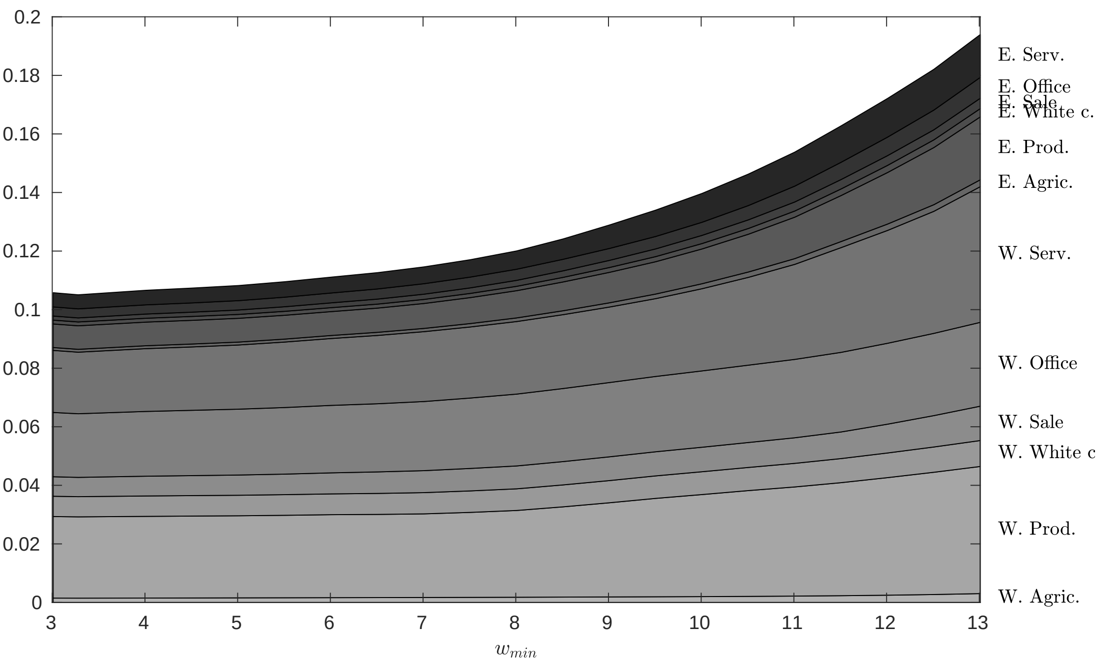
Figure 3.D.1: Unemployment rate u by job classification and region



Key: Separate estimation for each labor market (——); whole sample (·········).

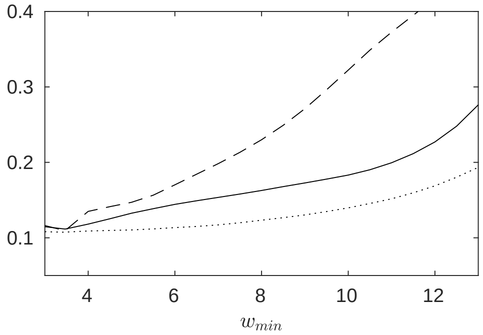
Source: SIAB 7510, own computations.

Figure 3.D.2: Unemployment rate u for different minimum wages

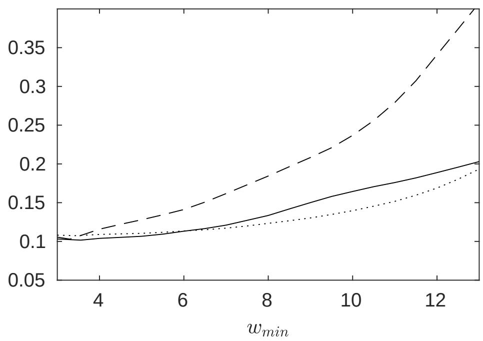


Source: SIAB 7510, own computations.

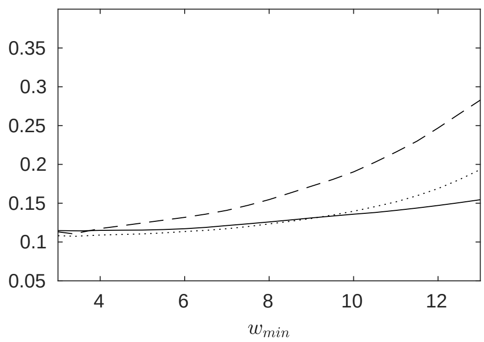
Figure 3.D.3: Composition of the unemployment rate u for different minimum wages



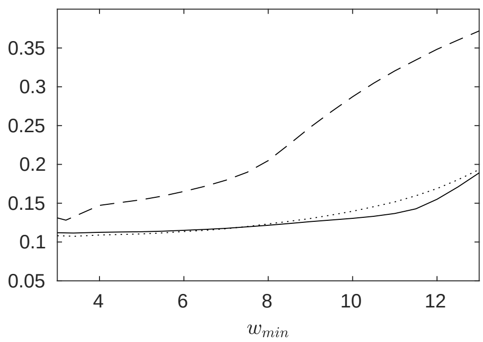
(a) Agriculture



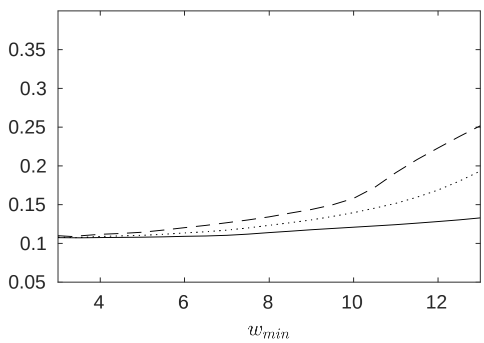
(b) Production, craft



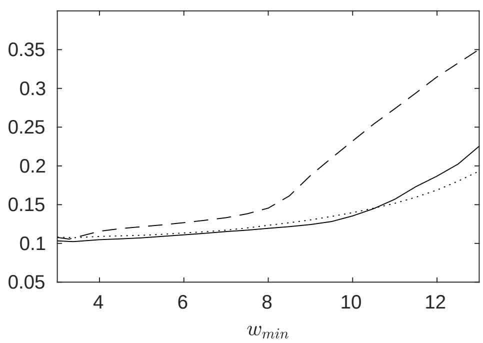
(c) White-collar



(d) Sales



(e) Office

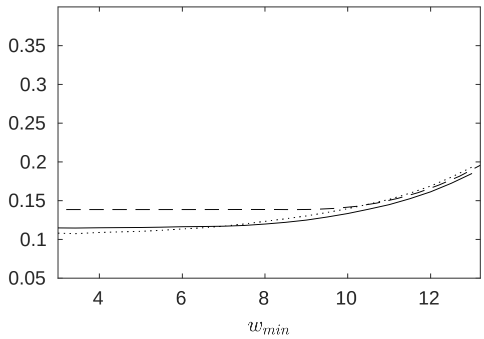


(f) Service

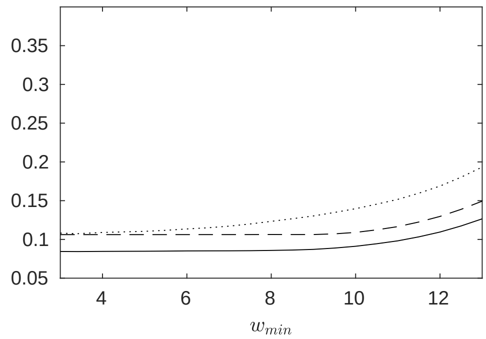
Key: West (——); East (- - - -); whole sample (⋯⋯). *Notes:* Productivity distributions are taken from the different labor markets and combined with estimated parameters for the whole sample.

Source: SIAB 7510, own computations.

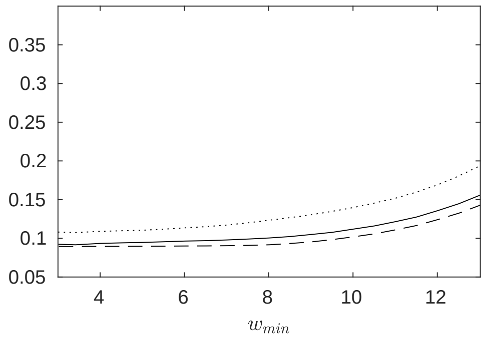
Figure 3.D.4: Unemployment rate u for different productivity distributions by job classification and region



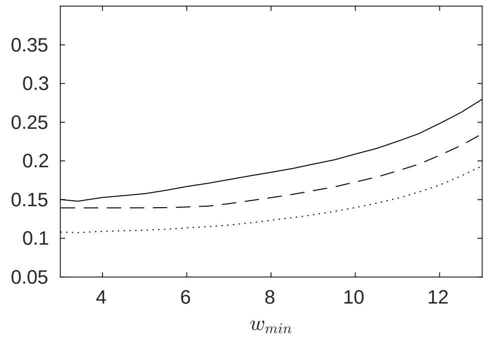
(a) Agriculture



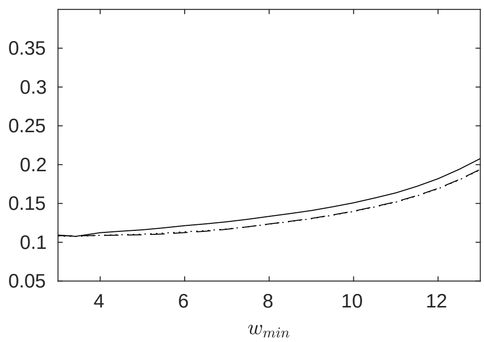
(b) Production, craft



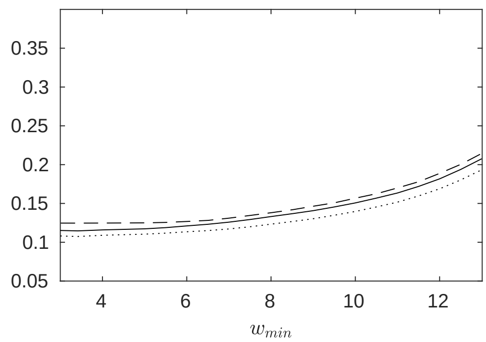
(c) White-collar



(d) Sales



(e) Office



(f) Service

Key: West (——); East (- - - -); whole sample (··········). *Notes:* Productivity distributions are taken from the whole sample and combined with estimated parameters for the different labor markets.

Source: SIAB 7510, own computations.

Figure 3.D.5: Unemployment rate u by job classification and region given the productivity distribution of the whole sample

Chapter 4

Does the Internet Help Unemployed Job Seekers Find a Job? Evidence from the Broadband Internet Expansion in Germany*

4.1 Introduction

The emergence of the internet as a mass medium has led to a dramatic decline in the cost of acquiring and disseminating information. During the last two decades, this has brought about a significant reduction in all kinds of information frictions, such as in the areas of elections as well as insurance, goods, housing and labor markets. Against this background, there has been a surge of empirical studies dealing with the internet's impact on outcomes such as product market performance (Brynjolfsson and Smith, 2000, Brown and Goolsbee, 2002), voting behavior (Falck et al., 2014) and crime (Bhuller et al., 2013) amongst others. In the context of labor markets, one of the major features that are likely to be affected by the internet is the way how workers and employers search for each other and eventually form a match (Autor, 2001).

The goal of this study is to identify the effect of the emergence of the internet on job search outcomes in the German labor market. Germany provides an interesting case,

*This chapter is joint work with Nicole Gürtzgen (IAB and University of Regensburg), André Nolte (ZEW and IAB) and Gerard van den Berg (University of Bristol, IFAU, IZA, ZEW and CEPR). An earlier version of this chapter is published in Nolte (2017). We are grateful to Andreas Moczall for providing us with the figures from the IAB Job Vacancy Survey. This chapter has benefited from comments and suggestions by Antonio Ciccone, Andreas Peichl, Stephan Thomsen, Carsten Trenkler, Johannes Voget and Andrea Weber.

as - even though access to the internet has been improving considerably over the recent decade - there is still substantial regional variation in households' access to high-speed internet. Closing the last remaining gaps in internet coverage especially in Germany's rural areas is therefore currently considered a major policy goal. Against this background, our study shall help to improve our understanding of whether and to what extent the spread of the internet may have facilitated job search among unemployed job seekers. To investigate the impact of the emergence of high-speed internet on job search outcomes, we explore the effect of the introduction of the digital subscriber line (DSL) technology on reemployment probabilities of unemployed job seekers. To do so, we will exploit variation in DSL availability at the regional level in Germany in order to quantify the net effect of an increase in regional internet availability on the fraction of unemployed individuals who experience a transition into employment.

In exploring the impact of the internet expansion on search outcomes, our study contributes to the (still small) literature that concentrates on different job search channels - especially searching via the internet - and their impact on labor market outcomes. Kuhn and Skuterud (2004) were the first to exploit individual variation in internet *usage* and to evaluate the impact of online job search on unemployment durations for the years 1998-2000 based on the Current Population Survey (CPS). The results from their duration analysis suggest that after controlling for observables, unemployed workers searching online do not become reemployed more quickly than their non-online job-seeking counterparts. This leads the authors to conclude that either internet job search does not reduce unemployment durations or that workers who look for jobs online are negatively selected on unobservables. Based on the same data set, Fountain (2005) performs logistic regressions with a job finding indicator as the dependent variable. Her results provide evidence of a small internet advantage compared to non-online job search in 1998. Moreover, she finds that internet searching advantages had disappeared by 2000. Kuhn and Mansour (2014) replicate the analysis by Kuhn and Skuterud (2004) combining information from the CPS with the National Longitudinal Survey of Youth (NLSY). Comparing the relationship between internet usage and unemployment durations in 1998/2000 and 2008/2009, the authors find that while internet usage was ineffective one decade ago, it was associated with a reduction in the duration of unemployment by about 25% in 2008/2009. Using the German Socio-Economic Panel (GSOEP), Thomsen and Wittich (2010) explore the effectiveness of various job search channels for the job finding probability among unemployed job seekers in Germany. The authors find that internet usage does not significantly raise the reemployment probabilities for this group of job seekers.

By presenting new evidence on the internet's impact on search outcomes for Germany, our study makes several important contributions to this literature. First, other than the studies cited above, our empirical approach explicitly accounts for the endogeneity of job search channels. Finding exogenous variation in the availability and use of the internet is a key challenge, as individuals - as well as employers - are likely to self-select into different search channels. Moreover, when looking at regional variation in internet availability, regions with high-speed internet access are likely to differ from those with low-speed internet access along many dimensions. While much of the literature is not able to deal with these issues, our analysis exploits exogenous variation in the availability of high-speed internet access at the German municipality level. The source of this variation, as put forward by Falck et al. (2014), stems from technological restrictions in the roll-out of the first generation of DSL in the early 2000s in Germany. We concentrate on DSL availability as this is the dominant broadband technology in Germany. More specifically, the variation was caused by technological peculiarities of the traditional public switched telephone network (PSTN), through which the early generations of DSL had been implemented. As described by Falck et al. (2014), almost one-third of West German municipalities could not readily employ the new technology as early DSL availability relied on the copper wires between the household and the main distribution frame (MDF) of the regional PSTN. The crucial issue causing exogenous variation in DSL availability is that, while the length of the copper wires connecting households and MDFs - whose distribution was determined in the 1960s - did not matter for telephone services, it strongly affected the DSL connection. In particular, there exists a critical value of 4,200 meters, with municipalities further than this threshold from the MDF having no access to DSL. The only way to provide internet access was to replace copper wires with fiber wires, which took time and was costly. This exogenous variation in internet availability during the early DSL years allows us to use each municipality's distance to the next MDF as an instrument for DSL availability. This enables us to identify an intention to treatment effect (ITT) of an expansion in internet availability on the reemployment prospects of unemployed individuals for less agglomerated municipalities in West Germany.

A second feature that distinguishes our study from previous work is that our analysis relies on administrative data sources. In particular, we use German register data, the universe of the Integrated Employment Biographies (IEB) of the Federal Employment Agency. The data provide an ideal basis for estimating the internet's impact on individual unemployment durations for several reasons: First, the data permit us to precisely measure the duration of different labor market states and transitions between them, most notably transitions between unemployment and employment. Second, due to their administrative nature, the IEB are less prone to panel attrition than comparable information from survey data. This is especially relevant as panel attrition has been recognized to give rise to biased estimates of the rates at which unemployed individuals become employed (van den Berg et al., 1994). An additional advantage over survey data is the considerably larger number of observations. The latter allows us to construct an inflow sample into unemployment, thereby avoiding the typical length bias that may arise in stock samples of unemployment durations.

Based on this empirical strategy, we document the following key results. Overall, we find that the OLS estimates of the DSL expansion on the reemployment prospects of unemployed individuals in Western German municipalities are downward biased. After accounting for potential endogeneity, our estimates point to modest positive effects for the pooled sample. Breaking down the analysis by socio-economic characteristics suggests that the internet's positive effect is particularly pronounced for males after about a quarter in unemployment. In terms of magnitude, moving from an "unlucky" municipality (i.e., one that could not readily be supplied with high-speed internet) to a "lucky" counterpart increases the reemployment probability for males by about 2-3% points.

Given that the above strategy identifies an ITT, we seek to provide more direct evidence on the relationship between an expansion in internet availability and job seekers' search behavior. To do so, we investigate job search strategies at the individual level, using survey data from the Panel Study on Labour Markets and Social Security (PASS). In particular, we address first-stage effects by looking at whether the availability of internet at home has a causal impact on the incidence of online job search, i.e. the *use* of the internet as a job search channel. To gain further insights into potential crowding out effects, we also look at whether the availability of internet at home affects the use of alternative job search channels. The results show that home internet access increases online job search activities and that especially male and skilled job seekers with a previous white-collar occupation are more likely to search online for a job. At the same time, we find some evidence for a reduction in the use of non-online search channels for skilled and white-collar workers. These findings suggest that the expansion in internet availability led to better reemployment prospects especially for male job seekers by increasing their overall search intensity, whereas the results for skilled white-collar workers suggest modest crowding out effects.

Finally, our study is also related to the literature on the effects of the broadband internet expansion on regional labor market performance. Looking at city-level unemployment rates, Kroft and Pope (2014) exploit geographic and temporal variation in the availability of online search induced by the expansion of the U.S. website Craigslist. The authors fail to detect any effects on local city-level unemployment rates. In a similar vein, the results obtained by Czernich (2014) point to no effect of internet availability on regional unemployment rates in Germany. The author exploits regional variation in broadband internet availability and addresses the endogeneity of internet availability using the same identification approach as in our study.[1] Finally, a large body of empirical research has set out to analyze the link between broadband internet and employment as well as economic growth. Much of this literature relies on regional variation in the broadband internet infrastructure and documents a positive relationship between broadband availability and economic as well as employment growth. Examples include the study by Crandall et al. (2007), who exploit regional variation at the U.S.-state level and find a positive association between broadband deployment and private-sector non-farm employment. This evidence is confirmed by Whitacre et al. (2014) and Kolko (2012) for the U.S., who also document a positive association between the expansion of broadband infrastructure and employment growth.[2] In a similar vein, using cross-country variation in OECD countries, Czernich et al. (2011) also establish a positive association between broadband penetration and economic growth.[3]

The remainder of the chapter is structured as follows. The next section provides descriptive evidence for the diffusion of broadband internet at the individual and employer level and its importance for job search and recruiting behavior. Section 4.3 presents some theoretical considerations of how online job search may be expected to affect reemployment probabilities. While Section 4.4 deals with the sources of empirical identification, Section 4.5 lays out the overall empirical strategy. The data sources and the sample selection are described in Section 4.6. Section 4.7 shows descriptive statistics. Section 4.8 presents the empirical results, while Section 4.9 provides further empirical

[1] The study is confined to unemployment stocks in the years 2002 and 2006 and does not take into account inflows and outflows into unemployment. Note that a zero aggregate unemployment effect is not necessarily informative on the internet's effect on frictions as it may mask individual level effects because of search externalities, or because of potential effects of the internet on job destruction rates.

[2] Using municipality data from Germany, Fabritz (2013) finds a moderate positive association between broadband availability and employment. The results are based on fixed-effects regressions without accounting for endogeneity in internet availability.

[3] There is evidence at the firm level that information and communication technologies have a positive impact on firm performance (see for example a survey by Kretschmer, 2012). Using Dutch data, Polder et al. (2010) find that broadband internet is positively correlated with product and process innovation. Using data for Germany during the early phase of the DSL introduction between 2001 and 2003, Bertschek et al. (2013) show that there exists a causal link between broadband internet and innovative activity. Exploiting exogenous variation in internet expansion for Italy, Canzian et al. (2015) establishes a causal effect of the internet on annual sales turnover and value added, whereas no effect is found on the number of employees in corporate enterprises.

evidence on potential mechanisms underlying individuals' job search behavior. The final Section 4.10 concludes.

4.2 Broadband Internet, Online Job Search and Recruiting

Broadband internet diffusion. The diffusion of high-speed internet in Germany started during the years 2000/01 and was based entirely on digital subscriber line technologies (DSL). The fraction of non-DSL broadband technologies such as hybrid fiber coax (HFC) cable or satellite was relatively low at 8% (Bundesnetzagentur, 2012). Prior to the introduction of broadband internet, internet access was only feasible via low-speed technologies such as modems or integrated services digital network (ISDN). DSL provides an access speed that is at least 6-times faster than the old technologies and therefore leads to a considerable reduction in waiting times for loading webpages and downloading files. At the individual level, the fraction using the internet increased within five years from about 37% at the beginning of the new century to 55% in 2005.

Online job search and recruiting tools. Turning to the role of the internet for online job search and recruiting, the most important tools include (1) online job boards, which provide websites including searchable databases for job advertisements; (2) job postings on the companies' websites which may (but do not necessarily) solicit online applications as well as (3) networks such as LinkedIn or Xing permitting online search on behalf of employers or headhunters targeting suitable candidates via their online CVs. Online job boards in Germany are typically divided into private job boards such as Monster and StepStone and public job boards, such as that from the Federal Employment Agency. As of 2005, there existed more than 1,000 online job boards in Germany (Crosswaters, 2005). In terms of market shares, the Federal Employment Agency's job board was the most important one, with about 325,000 jobs posted in February 2005, followed by JobScout24 and Monster with about 20,000 jobs. Regarding page views, it was also most frequently used by job seekers, with about 201 million views per month in 2005 compared to 41 million clicks at Monster and 9.2 million clicks at JobScout24 (Grund, 2006).

Other than market shares, the efficiency of the (job board) technology is rather difficult to measure. In December 2003, the Federal Employment Agency implemented a new online job board with the main purpose of aggregating 25 different single systems (*BA-Einzel-Börsen*) into one single portal, the "*Jobbörse*" (Bieber et al., 2005). By incorporating profile matching, this new system was explicitly designed to increase the efficiency of the match between job seekers and employers.[4]

Still, there exists evidence that the new technology was characterized by a couple of inefficiencies at the start of the DSL period. There is some evidence that customers used to stick to the traditional Federal Employment Agency's search engine and did not quickly adapt to the newly established *Jobbörse*, which may reflect initial limitations of its user-friendliness.[5] As described by Bieber et al. (2005), this may have been due to fact that the new job board was too complex for a broad customer segment. This was likely to be particularly relevant for simple jobs and tasks, such as cleaning staff or other low-wage occupations. Overall, these considerations point to a quite limited usability of the *Jobbörse* at the start of the DSL period.

Online search among employers. While the use of online recruiting tools among employers was already widespread in the mid 2000s in Germany, its importance has continued to increase during the last decade.[6] Based upon representative data, recent evidence from the IAB Job Vacancy Survey (Brenzel et al., 2016) supports the importance of online recruiting tools for German employers. In 2015, over 50% of all completed hires were preceded by job postings on the companies' websites and 41% by advertisements on online job boards. Looking at the success rates, however, reveals that among completed hires only 22% (30%) of the vacancies posted on companies' websites (job boards) were successfully filled through these specific recruitment channels. The remaining fraction was eventually filled through other mechanisms such as social networks, newspaper advertisements and private and public employment agencies.

The study by Brenzel et al. (2016) also suggests that online recruiting channels and their success rates appear to play a larger role for high-skilled than medium- and

[4]Related to that, Belot et al. (2016) provide experimental evidence on the effects of online advice to job seekers by suggesting relevant occupations. Their results point to a larger number of job interviews, which may provide some evidence in favor of an improvement in the technology to match job seekers and employers.

[5]For example, the first year was characterized by frequent system crashes, long waiting times and confusing search results. There is also evidence that already entered search criteria got deleted after pushing the "back" button.

[6]At the employer level, evidence based on firm-level survey data indicates that about 94% of all firms already had access to the internet in 2002. In 2007 the fraction increased to 98%, of whom 93% had high-speed internet access, with 86% having access via DSL or dedicated lines (ZEW ICT-Survey, 2007). Overall, the diffusion of high-speed internet in Germany in the early years of the 2000s suggests that any restriction in internet access was likely to be more binding for individual job seekers than for employers. According to a survey among 1,000 large German employers, the fraction of vacancies that were advertised on the surveyed companies' websites (via job boards) rose from 85% (52%) in 2005 to 90% (70%) in 2014, respectively. Moreover, among the surveyed companies the fraction of hires that resulted from online recruiting has increased from 50% in 2005 to over 70% in 2014 (Keim et al., 2005, Weitzel et al., 2015).

low-skilled jobs. These figures provide some first evidence on an important selection issue, namely the type of jobs being posted online. This is of particular relevance, as the jobs individuals search for online might systematically differ from those job seekers search for via alternative search channels. This, in turn, might be correlated with the length of the unemployment period. The question which jobs are posted online is not only relevant for selection issues, but also important when assessing the internet's effectiveness in helping unemployed job seekers find a job. Clearly, the intensity with which employers use the internet for recruiting purposes is an important prerequisite for the internet's ability in improving job finding prospects. Unfortunately, empirical evidence on the incidence of online recruiting for different types of occupations during the early 2000s is lacking. For this reason, we complement the evidence with further descriptions from the IAB Job Vacancy Survey.[7] Panel (A) of Figure 4.A.1 in Appendix 4.A shows the overall fraction of jobs being posted online among all successful hirings. Panel (B) and (C) show the respective shares broken down by selected occupational categories. The graphs are shown for the years 2005 to 2008, which in most studies are considered to be the DSL period in Germany. Three noteworthy facts emerge from these graphs: First, the fraction of jobs posted online increased by about 15% points from 2005 to 2008 (Figure 4.A.1 Panel (A)). Second, in terms of levels, the fraction of jobs being posted online is larger for more skilled white-collar occupations (Figure 4.A.1 Panel (B)) than less skilled or blue-collar occupations (Figure 4.A.1 Panel (C)).[8] Third, the graphs also illustrate that the first group of occupations experienced an increasing trend in online recruiting during this time period, whereas the relevance of online recruiting for the latter group rather remained constant.

Online search among job seekers. There is also some evidence on the incidence of online job search at the individual level in Germany. According to a survey among individual job seekers, the share of individuals preferring online over print applications rose from

[7] The IAB Job Vacancy Survey is based on a repeated annual cross-section of German establishments, whose sampling frame encompasses all German establishments that employ at least one employee paying social security contributions. The data are available from 1989 onwards, with the most recent waves covering about 15,000 establishments. Apart from information on various establishment attributes, such as size, industry and regional affiliation, the surveyed establishments are asked to report information on their most recent (randomly determined) hiring process. This information includes individual characteristics of the hired employee and characteristics of the specific position to be filled. The data also contain information on employers' adopted search channels relating to the most recent hiring, such as social networks, newspaper ads, private and public employment agencies and most notably the use of companies' websites and online job boards.

[8] Skilled white-collar occupations include managers, technicians, professionals and clerical support workers, whereas less skilled or blue-collar occupations include service and craft workers, plant and machine operators as wells as agricultural jobs.

48 to 88% between 2003 and 2014 (Weitzel et al., 2015). Using information from the German Socio-Economic Panel (GSOEP), Grund (2006) focuses on unemployed job seekers who were searching online in 2003. Consistent with the international evidence (e.g. Kuhn and Skuterud, 2004), his results suggest that the incidence was higher among younger and better qualified (unemployed) individuals. This pattern is confirmed by Thomsen and Wittich (2010) based on the same data set, who document an increase in the share of unemployed job seekers searching online from 37% in 2003 to 53% in 2007. Exploiting also the GSOEP, Mang (2012) focuses on job changers. His results suggest that the fraction of job changers who found a new job via the internet was in the year 2007 six times as high as in 2000. To date there is few evidence as to what extent an expansion in internet availability has translated into an increase in online job search and has given rise to potential crowding out effects of other job search channels. Against this background, we will complement the empirical evidence by own empirical analyses based on the PASS survey data in Section 4.9.

4.3 Theoretical Considerations

One of the major explanations for the increasing importance of the internet is its facilitating impact on search. First, job boards make it much easier to search for keywords and provide more information on more jobs than comparable newspaper print advertisements. Second, because job offers can be published on the internet without major time delays, they are also more up-to-date than comparable print offers. A third advantage for employers is that job boards involve a wider dissemination at a considerably lower cost than print advertisements (Autor, 2001). A similar argument holds for individual job seekers, who are also likely to get more information and to incur lower application costs when applying on the internet, albeit probably at a somewhat lower cost advantage than employers. Despite the importance of the internet in making the transmission of search relevant information much cheaper, there have been barely any attempts yet to quantify the average decline in search costs for both employers and job seekers.

The above reasoning suggests that the internet may facilitate search by lowering search costs and by increasing the rate at which information about job offers arrives. In standard job search models, an isolated decline in search costs unambiguously raises individuals' opportunity costs of employment and their reservation wages. This, in turn, makes job seekers more selective in terms of accepted wage offers and gives rise to longer unemployment durations. A necessary prerequisite for the internet leading to lower unemployment durations is, therefore, an additional effect on the probability of receiving a job offer. In job search models, the latter is typically parametrized within a Poisson process by the job offer arrival rate, λ, which may be either assumed to be exogenous or may be a direct function of search effort.[9] Models with endogenous search effort generally predict a decline in marginal search costs to increase search effort (Mortensen, 1986) and often assume the job offer arrival rate to be proportional to search intensity (e.g., Mortensen and Pissarides, 1999, Christensen et al., 2005). An increase in the job offer arrival rate may also be rationalized in a matching framework. Provided that the internet raises the number of matches between job seekers and employers, this will raise the job offer arrival rate as the ratio between the number of matches and job seekers.[10] Against this background, internet job search may generally be expected to produce higher overall job offer arrival rates, either by raising the intensity of search or by directly increasing the rate at which job offers arrive (van den Berg, 2006).

In addition to single search channel models, a decline in frictional unemployment may also be rationalized in a framework dealing with the relative effectiveness of different search channels. While much of the related literature typically deals with formal versus informal job search, the results are likely to carry over to online versus traditional search methods. For example, Holzer (1988) sets up a model with endogenous search effort where individuals may choose between different search channels. The model predicts that a decline in the channel-specific search costs will induce an increased use of this channel if the methods are either substitutes or independent in the production of job offers. van den Berg and van der Klaauw (2006) build up a model with two search channels, in which each channel is associated with its own structural parameters and search intensity. Assuming equal wage offer distributions across channels, the authors derive relatively mild conditions under which an increase in the arrival rate of one specific channel raises the exit rate out of unemployment.

The above considerations thus far have ignored that the internet not only reduces search costs for the unemployed, but also for those who are engaged in on-the-job search. This creates an additional source of ambiguity with respect to the overall effect on unemployed job seekers' job finding probabilities. To the extent that the internet also raises the job finding prospects of employed job seekers, the resulting search externalities may mitigate or counteract the internet's effect on unemployed job seekers' job finding

[9] Strictly speaking, a higher job offer arrival rate has been recognized to have an ambiguous impact on unemployment durations. The reason is that, in addition to increasing job offers, a higher arrival rate makes job seekers more selective and leads to an increase in their reservation wages. van den Berg (1994) derives regularity conditions under which an increase in the job offer arrival rate will reduce unemployment durations.

[10] In particular, if $M(u,v)$ denotes the number of matches as a function of the number of vacancies, v, and the unemployed, u, the job offer arrival rate, λ, is given by $\lambda = \frac{M(u,v)}{u}$.

rates. This is particularly relevant given that employed job seekers are likely to differ in productivity from unemployed job seekers and potentially may make more effective use of the internet than their unemployed counterparts.[11]

4.4 Identification

Identifying the effects of internet availability on labor market outcomes suffers from several endogeneity issues. Regions (in our case: municipalities) with high-speed internet access are different compared to regions with lower speed. By simply comparing e.g. unemployed job seekers' reemployment propensities across municipalities with two different high-speed internet levels, one would not be able to estimate the true causal effect. As a result, a simple regression of DSL availability on labor market outcomes at the municipality level would potentially be biased. The same is true when controlling for (municipality) observables, since the expansion of broadband internet might still be correlated with time-variant unobservables (see below).

To overcome potential endogeneity biases, we will make use of regional peculiarities of the West German traditional public switched telephone network (PSTN), which determined the capacity to provide DSL in certain municipalities. As described in Falck et al. (2014) and Steinmetz and Elias (1979), early DSL availability required copper wires between households and the main distribution frames (MDFs). The distribution of MDFs was originally determined in the 1960s with the overall purpose to provide telephone services in West Germany. While municipalities with a high population density have at least one MDF, less agglomerated areas typically share one MDF. The reason is that hosting a MDF required the acquisition of lots and buildings. As the distance to the next MDF did not affect the quality of telephone services, the choice of MDF locations in less agglomerated areas was determined by the availability of such facilities. The crucial issue causing exogenous variation in DSL availability is that, while the length of the copper wires connecting households and the MDFs did not matter for telephone

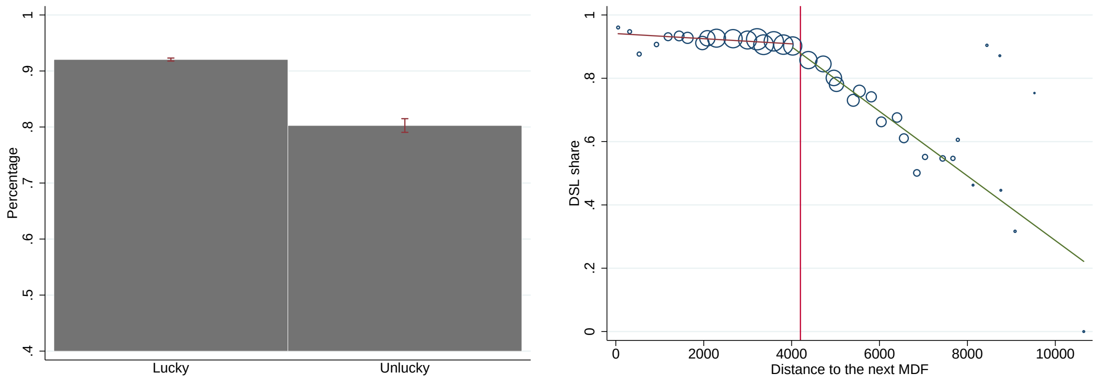
[11] In the literature, such externalities are referred to as "congestion" externalities. These capture the fact that competing job seekers who more rapidly find a job do not internalize that they match with other job seekers' potential employers. At the same time, an increase in search intensity implies that it will be easier for firms to find a match, which gives rise to a second type of externality, the "thick market" externality. Shimer and Smith (2001) argue that for more productive agents the "thick market" externality typically dominates the "congestion" externality, i.e. when stopping search they fail to internalize the inability of other firms to match with them. Thus, ex-ante heterogeneity renders the market solution of search and matching inefficient and implies that productive agents do not search enough, whereas the less productive ones search too much. To the extent that employed job seekers are more productive and potentially make more effective use of the internet than their unemployed counterparts, the internet may create a kind of search subsidy for the more productive job seekers, thereby leading to a gain in efficiency. In Section 4.9.3, we directly address this issue and do not find any evidence in favor of such an effect.

services, it strongly affected the DSL connection. In particular, there exists a critical value of 4,200 meters, with municipalities situated beyond this distance from the MDF had no access to DSL. The only way to provide internet availability was to replace copper wires by fiber wires, which took time and was costly. These technical peculiarities provide a quasi-experimental setting for less agglomerated municipalities without an own MDF, for whom the distance to each municipality's regional centroid to the MDF can be used as an instrument for DSL availability. We exploit this quasi-experimental set-up for West German municipalities that are connected to a MDF located in another municipality and where no closer MDF is available.[12] Because of the quasi-experimental setting spelled out above, we label municipalities with a distance below the threshold of 4,200 meters as *lucky* ones and municipalities with a distance above the threshold as *unlucky* ones.[13] To illustrate DSL availability rates at the household level for both groups, Figure 4.1 Panel (A) plots the mean fraction of households having access to DSL from 2007 to 2008. Municipalities with relatively short distances to the next MDF (below 4,200 meters) exhibit a fraction of about 92% of households for whom DSL is available. The low confidence intervals at the top of the bars indicate only little variation across municipalities. Once the distance surpasses 4,200 meters, the share drops considerably to about 82% with a higher variation across municipalities as reflected by the higher confidence intervals.

Panel (B) plots the DSL shares against the distances to the next MDF for 250 meter bins. The size of the circles corresponds to the number of municipalities. Lucky municipalities below the threshold exhibit a constant DSL share, whereas the DSL share decreases monotonically with higher distances among the unlucky municipalities. There are, however, some municipalities that exhibit a large distance to the next MDF, while simultaneously having relatively high DSL shares. Note that this might violate the exogeneity assumption. To address potential endogeneity concerns for these municipalities, we will later perform robustness checks by excluding these outliers. Moreover, we will also narrow the bandwidth around the threshold, which creates a set of municipalities that are likely to be more comparable in terms of their observables.

[12] Our analysis concentrates on West German municipalities because East Germany modernized the distribution frames after German unification. The average size of western German municipalities corresponds to a radius of about 3.1 kilometers.

[13] Roughly one third of the municipalities used in our analysis are unlucky municipalities.



(A) Broadband by treatment

(B) Broadband by distance

Notes: The figures plot the fraction of households with broadband internet (DSL) availability for lucky and unlucky West German municipalities between 2007 and 2008. The left Panel (A) reports averages by treatment status (lucky and unlucky municipalities). 95% confidence intervals are reported at the top of each bar in Panel (A). Panel (B) plots the DSL shares against the distance to the next main distribution frame. The size of the circles in Panel (B) corresponds to the number of municipalities within 250 meter bins. The figures are based on the German municipalities used in the empirical analysis.
Source: IEB, *Establishment History Panel* , MUP, *Breitbandatlas Deutschland* and Falck et al. (2014), own computations.

Figure 4.1: Share of households with DSL availability

4.5 Empirical Model

In our empirical analysis, we first compare changes in outcomes across municipalities i with different changes in DSL availabilities. Δ_t measures changes from a defined pre-DSL period to the DSL period, indexed by t. Thus, we regress the change in the outcome variable on the change of the share of households who technically have home internet access in municipality i and time period t, ΔDSL_{it}, and a vector of differences in covariates ΔX_{it}:

$$\Delta y_{itm} = \beta_{0m} + \beta_{1m} \cdot \Delta DSL_{it} + \Delta X_{it}' \cdot \beta_{2m} + (MDF_i \times \delta_t) + \varepsilon_{itm} \tag{4.1}$$

Given that DSL availability is zero in the pre-DSL period, equation (4.1) regresses the change in the outcome variable on the actual level of households with DSL availability, DSL_{it}. ΔX_{itm} is a vector of characteristics at the municipality level (see Table 4.1) and ε_{itm} is an idiosyncratic error term. Moreover, we introduce MDF-fixed effects (MDF_i), thus comparing two municipalities that are connected to the same MDF but differ in their distance to the MDF.[14] In terms of the outcome variable, we concentrate on monthly reemployment probabilities by calculating the share of unemployed individuals experiencing a transition into employment in municipality i in month m.[15] As we estimate

[14]We interact the MDF-fixed effects with time-fixed effects δ_t, thus, allowing for heterogeneous trends within smaller (MDF) regional units.

[15]See Section 4.6 for a precise definition of this variable.

this equation separately by month m after the inflow into unemployment, the coefficients and the changes in the outcome variable are indexed by m as well.

The empirical model in equation (4.1) might be subject to endogeneity issues. Individuals in municipality i might acquire broadband internet in order to search for a job. Moreover, individuals' unobserved productivity attributes, such as the level of motivation and propensity to work, might be correlated with the willingness to pay for broadband internet, such that compositional changes at the regional level might also be correlated with the expansion in high-speed internet. To account for time-varying unobserved effects that are correlated with both, labor market performance and DSL availability at the municipality level, we follow an instrumental variable (IV) approach. As spelled out above, we use as an instrument the distance from each municipality's center (population-weighted) to the next MDF. The first-stage can thus be written as:

$$\Delta DSL_{it} = \gamma_0 + \gamma_1 \cdot PSTN_i + \Delta X_{it}' \cdot \gamma_2 + (MDF_i \times \delta_t) + \psi_{it} \tag{4.2}$$

In the first stage, $PSTN_i$ is a dummy variable that takes on the value of 1 for unlucky (treated) municipalities.[16] This IV strategy identifies a local average treatment effect for the compliant municipalities. The first stage does not contain a subscript for month m because the DSL variable only varies with t for each municipality.

4.6 Data and Sample Selection

Data. The data used in this study stem from different data sources. We measure high-speed internet availability by the share of households at the municipality level for whom digital subscriber line technologies (DSL) are potentially available. The original data stem from the broadband atlas (*Breitbandatlas Deutschland*) published by the Federal Ministry of Economics and Technology (2009). The telecommunication operators self-report covered households with a minimum data transfer rate of 384 kb/s. Hence, for these covered households a high-speed internet connection is technically available. The self-reported data is available for the universe of German municipalities from 2005 onwards. In this study, we use the territorial boundaries of the municipalities from the year 2008. In the literature, the DSL period is typically defined as covering the years from 2005 to 2008, whereas the pre-DSL period refers to the years 1996 to 1999 (Falck et al., 2014).

Even though we measure broadband availability at the household level, it might be

[16] In a robustness check, we also use the distance as a continuous measure instead of a dummy variable as an instrument.

conceivable that DSL effects capture some potential demand-side dynamics. Higher broadband internet availability might, e.g., alter the dynamics of firm entries and exits. If labor demand is affected by an increase in high-speed internet availability, unemployed individuals might experience different unemployment durations without necessarily searching online for a job. In our empirical analysis we therefore include demand-side controls in order to isolate the effect of online job search from potential demand-side effects. Using data provided by the *Mannheim Enterprise Panel* (MUP), we retrieve information on the number of firm exits and entries at the municipality level.[17] We further include variables provided by the *Establishment History Panel* of the Federal Employment Agency. These include the total number of establishments and establishment size.

The main outcome variable in this study is a measure of unemployment duration. To measure unemployment durations and reemployment probabilities, we will use German register data, the Integrated Employment Biographies (IEB) of the Federal Employment Agency provided by the IAB (for detailed information of a sub-sample of this data set, see e.g. Oberschachtsiek et al., 2009 and Table 4.B.2 in Appendix 4.B for a description of all labor market states). This administrative data set covers the universe of all individuals who have at least one entry in their social security records from 1975 on in West Germany and starting from 1992 in East Germany. The data cover approximately 80% of the German workforce and provide longitudinal information on individual employment biographies. Self-employed workers, civil servants, and individuals doing their military service are not included in the data set. For our empirical analysis, we use the universe of unemployed individuals who experienced at least one unemployment spell in the above defined subset of municipalities during our time period of consideration (1996-2008).[18]

The data provide daily information on employment records subject to social security contributions, unemployment records with transfer receipt as well as periods of job search. This permits us to precisely measure the duration of different labor market states and transitions between them, most notably transitions between unemployment and employment. The data do not allow for a distinction between voluntary and involuntary unemployment, though. We therefore follow Lee and Wilke (2009) and define involuntary unemployment as periods of registered job search and/or transfer receipt without a parallel employment relationship. Further information on the definition

[17] The data set covers the universe of firms in Germany including a municipality identifier. The earliest available representative year is 2000. Thus, we use the year 2000 as the pre-DSL year.

[18] When constructing the outcome variables as well as some control variables, we exploit the universe of individuals who experienced at least one unemployment spell in the above defined subset of municipalities during our time period of consideration as well as a random 50%-sample of employed individuals living in the above defined subset of municipalities.

of un- and non-employment can be found in Appendix 4.B. As the IEB are based on employers' notifications to the social security authorities, they are less prone to measurement error than comparable information from survey data, like e.g. the German Socio-Economic Panel (GSOEP). Additional advantages over survey data include the much lower extent of panel attrition and most notably the possibility to construct an inflow sample, which captures also shorter unemployment spells. To construct a measure of municipality-specific reemployment propensities, we link the universe of individuals with an employment to unemployment transition in every single year during the pre-DSL and DSL period (referred to as the *unemployment inflow sample*) with a municipality identifier at either the individual or establishment level. This allows us to merge the administrative data with information from other data sources (see Table 4.B.1 in Appendix 4.B).[19] In our analysis, we concentrate on individuals who were at least three months employed before they became unemployed. Doing so, we exclude individuals with short employment spells who are less likely to be engaged in true search activities during unemployment.

Sample selection and main outcome variable. In our empirical analysis, the pre-DSL period covers the years 1998 and 1999, whereas the DSL period covers 2007 and 2008. We focus on these later DSL years for several reasons. First, as set out earlier, we will complement our analysis with individual-level survey data that are available from 2007 onwards. This restricts us in documenting first stage effects starting from 2007 only. Second, there is evidence that the early DSL years may be considered as transition years towards a new technology equilibrium. This appears to be particulary true for the less agglomerated municipalities, which typically have no own MDF and hence form the basis for our empirical analysis. To support this notion, Figure 4.C.1 in Appendix 4.C plots the distribution of DSL availability against time. Panel (A) of Figure 4.C.1 displays the development for agglomerated municipalities, whereas Panel (B) shows the distributions for less agglomerated municipalities. The graphs illustrate that the transition phase among less agglomerated municipalities took apparently longer as compared to urban regions. Third, online search and recruiting technologies appear to have become more elaborated over the course of time. Some evidence for this consideration was documented in Section 4.2, pointing to some inefficiencies of the Federal Employment Agency's job board technology during the early DSL period. Some further evidence for

[19] More specifically, the municipality identifier in the administrative data is based on individuals' place of residence. If the place of residence is missing, we use the municipality identifier of individual spells from the previous or subsequent five years or - in a final step - information on individuals' workplace (establishment) location.

improvements of the underlying technologies is given by the increasing importance of online recruiting among employers. According to figures from the IAB Vacancy Survey, between 2005 and 2008 the fraction of hirings that were preceded by online recruiting increased from about 45% to over 60% (see Figure 4.A.1 in Appendix 4.A).

As to our main outcome variable of interest, we compute reemployment propensities as the municipality-specific share of individuals reentering employment within m months after the inflow into unemployment, relative to the number of individuals at risk, i.e. those who are still unemployed. Cumulative reemployment probabilities are defined as the complement of the survival function, which is estimated by the non-parametric Kaplan-Meier estimator.[20] Figure 4.C.2 (4.C.3) in Appendix 4.C plots the distribution of the number of observed individuals in the data set by municipality and period (year). In the median municipality, 93 individuals were entering unemployment during the whole DSL period. The median over all pre-DSL years equals 87. To calculate meaningful averages at the municipality level, we further condition the sample on observing at least five individuals per year and municipality in our final unemployment inflow sample. Due to this condition, the final sample of municipalities (2,988) covers 90% of all available less agglomerated municipalities (3,339) that fulfill the requirements described above.

4.7 Descriptive Statistics

Given that our empirical strategy focuses on less agglomerated municipalities without an own main distribution frame (MDF), we provide descriptive statistics for the above defined subset of 2,988 municipalities.

Municipality-level variables. Table 4.1 shows that in West Germany during the years 2007 and 2008 DSL was, on average, available for a fraction of 88% of households at the municipality level. In addition to broadband internet information, the table provides information on further regional characteristics at the municipality level.[21] Panel B of Table 4.1 shows the main control variables used in the empirical analysis. The first set of variables indicates that the population was aging, the average real daily wage

[20] Formally, the estimator is given by: $\hat{S}(m) = \prod_{i:m_i \leq m} (1 - \frac{d_i}{n_i})$, where d_i is the number of spells that transit into employment in month, m_i, and n_i is the total number of individuals at risk during the time interval $[m_i, m_{i+1}]$.

[21] The descriptive statistics of the municipality characteristics shown in Panel B of Table 4.1 are based on re-weighted averages. As our sample consists of the universe of the unemployed and a 50% sample of employed individuals, we re-weight the averages to match the official unemployment rates. Some further regional characteristics for the pre-DSL and DSL years are also available from Falck et al. (2014) (see Table 4.B.1 in Appendix 4.B).

Table 4.1: Descriptive statistics

	pre-DSL years 1998/99 (1)	DSL years 2007/08 (2)
Panel A: Broadband availability		
DSL	0.000	0.878
	(0.000)	(0.190)
Panel B: Municipality characteristics		
Inflow unemployed	30.983	32.022
	(32.195)	(33.774)
Population	1375.209	1384.957
	(1416.369)	(1436.977)
Female population share	0.500	0.502
	(0.018)	(0.037)
Population share aged 18-65	0.659	0.616
	(0.030)	(0.055)
Population share > 65	0.161	0.186
	(0.034)	(0.036)
Net migration rate	0.005	-0.001
	(0.021)	(0.018)
Unemployment rate	0.040	0.040
	(0.015)	(0.020)
Average real daily wage	97.526	98.631
	(12.002)	(17.088)
Low-skilled	0.169	0.151
	(0.045)	(0.037)
Medium-skilled	0.774	0.776
	(0.048)	(0.046)
High-skilled	0.056	0.073
	(0.034)	(0.038)
Foreign nationals	0.025	0.024
	(0.027)	(0.025)
Regional occupational structure		
Agriculture	0.025	0.025
	(0.023)	(0.022)
Production	0.361	0.298
	(0.088)	(0.076)
Salary	0.109	0.116
	(0.041)	(0.038)
Sale	0.066	0.071
	(0.023)	(0.022)
Clerical	0.205	0.212
	(0.057)	(0.055)
Service	0.226	0.270
	(0.063)	(0.073)
Panel C: Inflow characteristics		
Age	35.376	35.832
	(3.415)	(3.398)
Female share	0.370	0.416
	(0.140)	(0.133)
Low-skilled	0.201	0.214
	(0.112)	(0.109)
Medium-skilled	0.756	0.732
	(0.119)	(0.118)
High-skilled	0.043	0.053
	(0.057)	(0.060)
Foreign nationals	0.037	0.035
	(0.058)	(0.054)
Number of individuals in inflow sample	175,426	181,306
Number of municipalities	2,988	2,988

Notes: The table reports municipality-level descriptive statistics for West Germany. The pre-DSL period covers the years 1998 and 1999. The DSL period covers the years 2007 and 2008. The numbers are averaged within the pre-DSL and the DSL years, respectively. Panel A reports the DSL availability rate. Panel B reports municipality characteristics. Panel C reports age, female, education and nationality structure for the unemployment inflow sample. Further control variables are reported in Table 4.C.1 in Appendix 4.C.
Source: IEB, *Establishment History Panel* , MUP, *Breitbandatlas Deutschland* and Falck et al. (2014), own computations.

increased over time and that the population became more skilled. The second set of variables refers to the occupational structure at the municipality level. The figures reveal that for less agglomerated Western German municipalities the occupational structure became more service oriented and less production-intensive. Panel C of Table 4.1 displays the main characteristics of the unemployment inflow sample. The average age exhibits a slight increase from 35.4 to 35.8 years. The same pattern is observed for the share of females among those entering unemployment. Moreover, as expected, low-skilled individuals and foreigners tend to be disproportionately represented in the inflow sample as compared to the overall average skill level and the share of foreigners at the municipality level (see Panel C of Table 4.C.1 for further inflow characteristics).

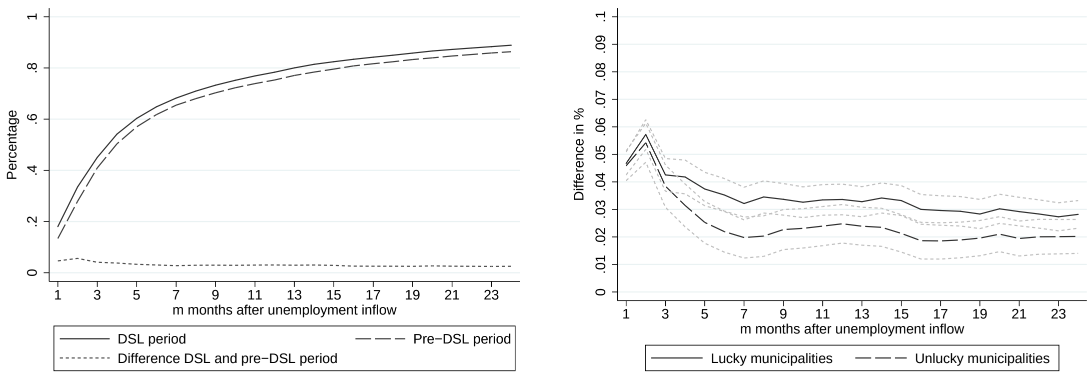
Demand-side variables. Table 4.C.1 in Appendix 4.C displays firm and establishment information at the municipality level. The figures indicate that the average number of establishments increased in West Germany, whereas average establishment size decreased slightly and amounted to above six. As to firm entries and exits, the table documents that less firms entered and more firms exited the market, while total sales increased.[22]

Cumulative reemployment probabilities. Based on the inflow sample at the municipality level, Panel (A) of Figure 4.2 shows cumulative reemployment probabilities at the municipality level for month, *m*, after the entry into unemployment, separately for the DSL (2007/08) and the pre-DSL years (1998/99). For example, the cumulative probability of having experienced a transition into employment by month 12 after entering unemployment was about 78% during the defined DSL years, whereas during the pre-DSL years the respective probability was about 75%. At the end of the second year, we observe that the cumulative reemployment probability increased further by 10% points. This indicates that much of the dynamics already occurs during the first 12 months of unemployment. For this reason, we concentrate in our empirical analysis on the first year of unemployment.[23] The bottom line in Figure 4.2 (A) plots the difference between the two upper graphs against time. Overall, this line illustrates that during the DSL years the cumulative probability of experiencing a transition into employment is

[22] In Table 4.C.2 in Appendix 4.C, we document that there appears to be no *causal* effect of an increase in DSL availability at the municipality level on the number of firm entries and exits as well as net firm creation. Note, however, that our broadband internet measure refers to the household level and that a large fraction of firms already had access to broadband internet, for example, via dedicated lines.

[23] A further reason is that after one year of unemployment, individuals are counted as long-term unemployed and experience different state-governed treatments, such as lower unemployment benefits and increased job search assistance.

larger than in the pre-DSL period. Over the first 12 months, cumulative reemployment probabilities increased, on average, by 3.5% points. Panel (B) of Figure 4.2 further distinguishes between lucky and unlucky municipalities. The graphs show that after the third month lucky municipalities show higher cumulative reemployment probabilities than their unlucky counterparts. This indicates, on a descriptive basis, that municipalities with higher DSL availability experienced a larger increase in reemployment probabilities and, as a result, a larger decline in unemployment durations over the two defined periods.



(A) Overall *(B) Difference by treatment*

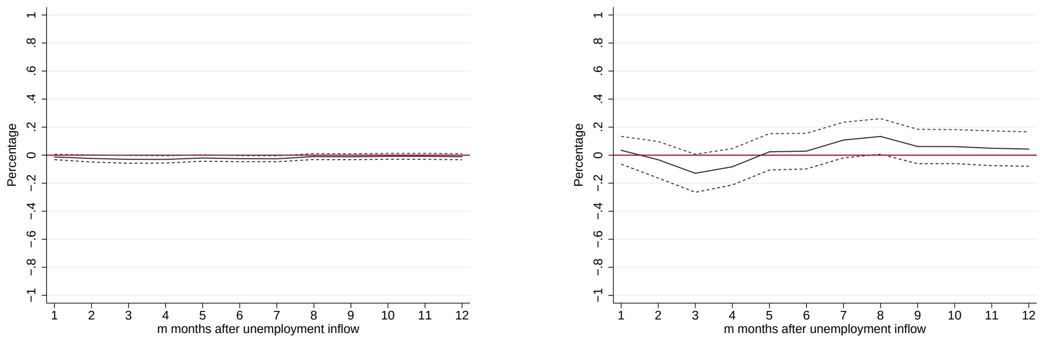
Notes: Panel (A) plots the cumulative probability of becoming reemployed within *m* months after an inflow into unemployment averaged at the municipality level, distinguishing between the DSL (2007/08) and the pre-DSL (1998/99) period. The bottom line plots the difference between the two upper lines against time. Panel (B) plots the same difference separately for lucky and unlucky municipalities. Grey dotted lines represent 95% confidence intervals.
Source: IEB, *Establishment History Panel* , MUP, *Breitbandatlas Deutschland* and Falck et al. (2014), own computations.

Figure 4.2: Reemployment probability and difference between lucky and unlucky municipalities

4.8 Empirical Results

4.8.1 Transitions from Unemployment to Employment

Baseline effects. We now turn to regression models in order to calculate standard errors and conduct hypothesis tests. We start our regression analysis by looking at differences in outcomes between the pre-DSL years (1998/99) and the DSL years (2007/08) over a constant time span. More specifically, we keep the differences between the periods constant at nine years, by connecting 2007 with 1998 and 2008 with 1999. We cluster standard errors at the municipality level as the identifying variation is measured at this level. Figure 4.3 displays the estimated effects of a 1% point increase in the municipality-specific share of households with DSL availability on the cumulative probability of reentering employment within *m* months after their inflow into unemployment. The left figure shows the ordinary least squares (OLS) estimates of the first difference model controlling for observable characteristics and MDF-by-year-fixed effects. The OLS coefficients are negative and partly significant at the 10% level during the first months after the inflow into unemployment. According to these estimates, a 1% point increase in DSL reduces the cumulative reemployment probability by about 0.03% points. The right figure shows the IV estimates. The Kleibergen-Paap F-Statistics is 84.0 and the first stage treatment coefficient equals 0.054, indicating that unlucky municipalities have on average 5% points lower DSL rates. Therefore, weak identification issues do not apply here. In the IV model the point estimates become positive and partly significant after seven months in unemployment. In terms of magnitude, the coefficient amounts to 0.13 in month eight, which corresponds to up to 1.3% points higher cumulative reemployment probabilities after moving from an unlucky to a lucky municipality, where the unconditional difference in DSL rates (shown in Figure 4.1) is roughly 10% points.



(A) OLS *(B) IV*

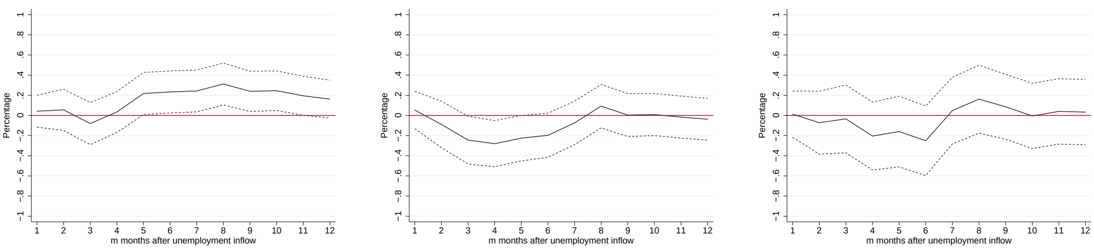
Notes: The figure shows the effects of a 1% point increase in the share of households with DSL availability on the cumulative transition probability from unemployment to employment within *m* months for an inflow sample of individuals who entered unemployment between 1998/1999 and 2007/2008. The regressions are population-weighted and performed separately for each month. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table 4.B.1 in Appendix 4.B). Dotted lines present the 90% confidence intervals. Standard errors are heteroskedasticity robust and clustered at the municipality level. Panel (A) plots the effects using OLS. Panel (B) corresponds to the IV model, where the distance is measured from the geographic centroid to the MDF and weighted by the location of the population. Regressions are based on 2,988 municipalities and 850 MDFs. The Kleibergen-Paap F-Statistic for the first stage in Panel (B) is 83.98.
Source: IEB, *Establishment History Panel* , MUP, *Breitbandatlas Deutschland* and Falck et al. (2014), own computations.

Figure 4.3: IV regression results of DSL on unemployment-to-employment transitions

Heterogeneous effects by socio-economic characteristics. The results from the pooled sample might mask heterogeneous effects across different subgroups. In particular, it might be conceivable that more skilled individuals or younger workers have greater exposure to the internet and thereby make more efficient use of online job search tools. We test this hypothesis by estimating the regressions for different subgroups of the unemployment inflow sample. We first break down the sample by gender as well as age, by distinguishing young (< 35 years) and old workers (≥ 35 years). We further test the hypothesis that the intensity with which employers use the internet for recruitment purposes may matter for its effectiveness in raising reemployment prospects for job seekers. Given that the descriptives from the IAB Job Vacancy Survey (see Section 4.2) suggested that vacancies for more skilled and white-collar occupations were more likely to be advertised online, we restrict our sample to these occupations. We do so by looking at skilled individuals (who have completed a vocational training or hold a university degree/technical school degree) entering unemployment from a *white-collar* job, with the latter comprising higher clerks, service, clerical or sales occupations. Figure 4.4 plots the estimated coefficients along with their confidence intervals. Compared with the estimates from the pooled sample, Panel (A) of Figure 4.4 point to a clearer picture for unemployed males, for whom the positive effect of higher DSL availability is particularly pronounced after month four. In terms of magnitude, moving from an unlucky to a lucky municipality increases the cumulative reemployment probability by 2.3% points on average after four months in unemployment. For skilled individuals who entered unemployment from white-collar jobs and young job seekers, we observe slightly negative effects during the first six months in unemployment with significant point estimates for young individuals. This negative effect may point to an inefficient use by the group of individuals below 35 years of age. During the second half of the first year in unemployment, the cumulative reemployment probability stays relatively close at zero. Overall, the comparison of the IV and OLS estimates points to different selection mechanisms. Males seem to be negatively selected, whereas the results for young individuals indicate a slightly positive selection.

Figure 4.D.1 in Appendix 4.D further plots the coefficients measuring the effects on monthly hazard rates rather than on cumulative probabilities. For males, the effects on monthly hazard rates exhibit a similar pattern as the effects on cumulative reemployment probabilities, as there are positive effects between 2%-4% points after four months in unemployment. For skilled white-collar workers and to some extent for young individuals, we document positive effects between 5% and 6% points in month seven and eight. These significant higher monthly reemployment probabilities do not translate into higher cumulative reemployment probabilities, though (see Figure 4.4). Still, the estimates indicate that - conditional on being at risk - especially skilled white-collar workers experience positive internet effects on their hazard rates later in their unemployment spells.

The results so far suggest that the increase in DSL availability appears to raise the cumulative reemployment probabilities especially for males. Moreover, a further finding is that the positive effect on reemployment probabilities shows up or becomes significant only with a certain time delay after entering unemployment. In Section 4.9, we will turn to the underlying mechanisms and address the question to what extent this finding may be explained by heterogeneous changes in job search related outcomes across subgroups, such as job seekers' adopted search channels and their application behavior.



(A) Male *(B) Young* *(C) Skilled white-collar*

Notes: The figure shows the effects of a 1% point increase in the share of households with DSL availability on the cumulative transition probability from unemployment to employment within *m* months for an inflow sample of individuals who entered unemployment between 1998/1999 and 2007/2008 separately for males, young individuals (below 35 years) and skilled white-collar individuals. The regressions are population-weighted and performed separately for each month. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table 4.B.1 in Appendix 4.B). Dotted lines present the 90% confidence interval. Standard errors are heteroskedasticity robust and clustered at the municipality level. The distance is measured from the geographic centroid to the MDF and weighted by the location of the population. Regressions are based on 2,551 municipalities and 803 MDFs for males, 2,359 municipalities and 765 MDFs for young individuals and 2,066 municipalities and 713 MDFs for skilled white-collar individuals. The Kleibergen-Paap *F*-Statistic for the first stage is 60.0, 53.4 and 57.6 for the three groups, respectively.
Source: IEB, *Establishment History Panel* , MUP, *Breitbandatlas Deutschland* and Falck et al. (2014), own computations.

Figure 4.4: IV regression results of DSL on unemployment-to-employment transitions by socio-economic characteristics

4.8.2 Robustness Checks

Sample specification and weighting. In this subsection, we conduct several robustness checks. We start by providing regressions results for different sample specifications. First, we include all individuals in the inflow sample irrespectively of the length of their previous employment spell. Second, to address the issue that the results might be driven by small municipalities with few inflows into unemployment, we re-estimated our specifications by conditioning on municipalities with at least 500 inhabitants (in addition to conditioning on at least five individuals entering unemployment). As a third check, we allow for a non-employment gap of six months between two unemployment spells as well as between unemployment and reemployment and count this period as unemployment. Finally, we show the results without weighting the municipality-level variables by the number of inhabitants. The estimates shown in Figure 4.E.1 in Appendix 4.E suggest that the overall pattern of results remains unaltered. However, without conditioning on the length of the previous employment spell (Panel *2-A*), the negative effect for young job seekers becomes close to zero.

Recalls. A further concern could be that our estimates are affected by potential recalls, e.g. individuals who return to their pre-unemployment establishment.[24] In particular, it might be conceivable that unemployed individuals who are reemployed by the same employer do not actively search for a new job. There is evidence that individuals with recalls experience shorter unemployment durations and lower search intensities as compared to unemployed job seekers entering a new job (Nekoei and Weber, 2015, Fujita and Moscarini, 2013). This could be a potential explanation for the non-positive DSL effect at the beginning of the unemployment spell. Due to the endogeneity of recalls, we refrain from conditioning on this outcome, but rather re-estimate our model after excluding industries with a priori high recall rates. These industries include agriculture, construction, hotels and restaurant, passenger transport and delivery services. Figure 4.E.2 in Appendix 4.E presents the results. For males and young workers, the point estimates are higher than in the baseline specifications, with the estimates for males indicating a DSL effect of up to 5% points.

Empirical specification. We further conduct several robustness checks with respect to the empirical specification. In particular, we start by narrowing the distance around the threshold and excluding outlier municipalities in terms of their distance to the threshold and their broadband availability shares. In our baseline model, we have relied on 9-year differences in outcomes, by connecting e.g. 1998 and 2007 and 1999 and 2008. Given this procedure, a concern might be that our results are driven by (differences in) outcomes in specific years. To address this issue, we perform two robustness checks with respect to the definition of differences. We first average all variables within the pre-DSL and the DSL years, respectively, and then compute the difference between the averaged pre-DSL and DSL variables per municipality. This procedure is also likely to mitigate potential outlier values in specific years of our variables of interest. Second, to construct differences, we rely on 1998 as the only pre-DSL year, by taking the differences between 2007 and 1998 as well as 2008 and 1998. This robustness check gives rise to different lengths of the measured distances and provides a test of whether the distances and/or specific years matter for the estimated DSL effects. Figure 4.E.3 in Appendix 4.E gives

[24] In our sample, 25% of all individuals who become unemployed in a given year return to their previous employer.

the results for the three socio-economic groups. The figures corroborate the pattern of results that has been found earlier.

Treatment intensity - continuous instrument. The analysis so far has used a dichotomous treatment variable dividing municipalities into lucky and unlucky ones. Panel (B) of Figure 4.1 shows that the treatment intensity increases with higher distances. As a further robustness check, we therefore specify the first stage equation using the distance as a continuous measure of treatment intensity:

$$\Delta DSL_{it} = \gamma_0 + \gamma_1 \cdot PSTN_i \cdot distance_i + \Delta X'_{it} \cdot \gamma_2 + (MDF_i \times \delta_t) + \psi_{it}, \qquad (4.3)$$

where *PSTN* takes on the value of *1* if a municipality is located more than 4,200 meters away from the MDF (unlucky) and zero otherwise. To measure different treatment intensities among the unlucky municipalities, the treatment dummy is interacted with the actual distance to the next MDF centered at the threshold value of 4,200 meters.[25] Figure 4.E.4 in Appendix 4.E presents the results. The positive effect for males stays at around 0.2. The results for young individuals and skilled white-collar workers are similar to the baseline results. Overall, the main pattern of results remains unaltered across these different specifications, suggesting that higher internet availability has helped male unemployed job seekers finding a job.

4.8.3 Effects During the Early DSL Years

Appendix 4.F presents the results for the early DSL years (2005/06), which have been shown to characterize a transition period towards a new technology equilibrium especially for the less agglomerated municipalities. Figure 4.F.1 presents the baseline results. The overall pattern that emerges from the baseline estimates is that higher DSL availability does not affect the cumulative reemployment probabilities for all defined subgroups. For males, the estimates even point to *lower* cumulative reemployment probabilities during the first 3 months in unemployment.[26] Overall, the results point to the absence of causal internet effects on cumulative reemployment probabilities during the first 12 months in unemployment. A potential explanation for these findings may be that employers and job seekers were still adapting to the new technology and that job search technologies, such as that from the Federal Employment Agency, were still characterized by inefficiencies

[25] It should be noted that any change of the IV specification that tries to capture the observed distribution would be entirely data driven. However, it may still be informative to assess the validity of the instrument by changing the empirical specification as shown above.

[26] This effect is relatively robust across the different specifications presented for the years 2007/08.

during the early DSL period. Taken together, the comparison of the early and late DSL years leads us to conclude that the effectiveness of the internet appears to have considerably improved across these periods. Note that this is in line with the findings of Kuhn and Mansour (2014), who showed that the relationship between internet job search and unemployment durations became more efficient over time.

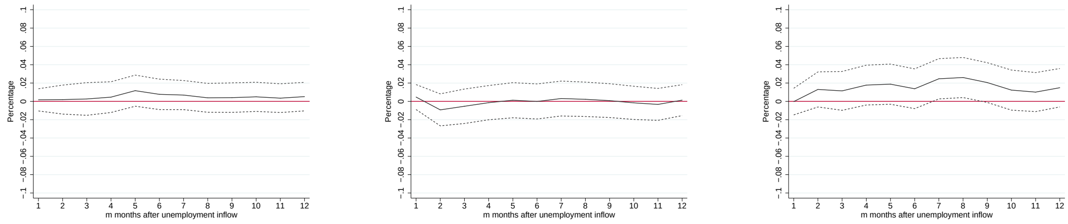
4.8.4 Placebo Test

To test for the similarity or divergence in time trends across lucky and unlucky municipalities during the pre-DSL period, we further conduct a placebo test. In particular, we compute the differences in outcomes and covariates between 1999 and 1995 and regress the treatment dummy (and further controls including MDF fixed effects) on the change in the fraction of unemployed entering employment during the first 12 months after entering unemployment. The results in Figure 4.5 show that the treatment dummy is insignificant and close to zero for each month after the inflow into unemployment for males and young workers. For skilled white-collar workers, results point to significant positive effects after six months indicating that during the pre-DSL period this group exhibits larger cumulative reemployment probabilities in unlucky municipalities as compared to their lucky counterparts. This trend during the pre-DSL years might lead to a downward bias in the estimated DSL coefficients. The placebo estimates for males and young workers point to a similar pre-treatment trend across lucky and unlucky municipalities and suggest that both groups performed similarly during the pre-DSL years. Overall, this suggests a causal interpretation of the DSL effect on cumulative reemployment probabilities.

4.9 Mechanisms

4.9.1 Individual-Level Job Search Strategies Based on Survey Data

Given that our strategy thus far identified an ITT, the question of to what extent the established effects arise from changes in individuals' job search behavior remains unanswered at this stage. To provide evidence on the underlying mechanisms, we complement our analysis by exploiting survey data on job search channels among job seekers from the survey *Panel Study on Labour Markets and Social Security* (PASS). A detailed description of the variables used in this study can be found in Appendix 4.G (Table 4.G.1). The survey started in 2007 as a panel, with the main purpose of surveying low-income



(A) Change 1999-1995 *(B) Change 1999-1995* *(C) Change 1999-1995*

Male *Young* *Skilled white-collar jobs*

Notes: The figure shows the effects of the treatment dummy on the cumulative transition probability from unemployment to employment within *m* months for an inflow sample of individuals who entered unemployment in 1995 and 1999 separately for males, young individuals (below 35 years) and skilled white-collar individuals. The endogenous variable is the change between 1999 and 1995. The regressions are population-weighted and performed separately for each month. The list of control variables includes the employment structure, occupational shares and industry shares (see Table 4.B.1 in Appendix 4.B). Due to data availability constraints we cannot control for firm dynamics, total population and age structure. Dotted lines present the 95% confidence interval. Robust standard errors in parentheses.
Number of municipalities: Male: (A): 2,529; Young: (B): 2,339; Skilled white-collar: (C): 2,049.
Source: IEB, *Establishment History Panel* , MUP, *Breitbandatlas Deutschland* and Falck et al. (2014), own computations.

Figure 4.5: Placebo results

households. We use the first three waves of the data set which correspond to the years 2007 to 2009 (see Trappmann et al., 2010 for a detailed description of the data).[27] If respondents are currently looking for a job, they are asked to report their specific adopted job search channels. Possible categories include online job search, search via newspapers, friends/relatives, private brokers, the local employment agencies or further (non-specified) search channels. Moreover, the survey also asks whether a job seeker's household possesses a computer with an internet connection.[28] Table 4.G.2 in Appendix 4.G shows on a descriptive basis that home internet access is positively correlated with the incidence of online job search. Overall, the fraction of job seekers searching online is more than 25% points higher among job seekers with home internet access as compared to those with no home internet access.[29]

In what follows, we explore whether home internet access has a causal effect on the incidence of online job search and on other job search channels. Similar to our empirical

[27]The first wave is conducted mainly in 2007. 73% of all individuals used in our sample are interviewed in 2007. 23% are interviewed in 2008/09. The remaining 4% correspond to the year 2006. This restricts the explanation of the mechanism behind the identified ITT to the later DSL years 2007/08.

[28]The survey does not specifically ask about broadband internet connection. This can induce misclassification of our explanatory variable. Depending on the extent of misclassification, IV estimates would therefore represent an upper bound.

[29]To estimate the causal impact of home internet access on online job search, we exploit information on both, unemployed and employed, job seekers. However, most individuals were unemployed at the time of the interview date (82%, see Table 4.G.3 in Appendix 4.G). Moreover, about 16% of the employed individuals entered unemployment between two interview dates. Thus, we capture some individuals who search in anticipation of future unemployment. This provides greater comparability with the administrative data sample which includes individuals with very short unemployment spells.

strategy at the municipality level, we again make use of regional identifiers provided by the Federal Employment Agency. Apart from the municipality identifier, we are also able to take advantage of the postal codes provided by PASS. This is a particularly attractive feature of the data, as the combination of the municipality identifier and the postal code provides greater scope for variation in the treatment indicator that is needed for the IV regression (see Figure 4.G.1 in Appendix 4.G for a graphical illustration).

Survey evidence on search channels. Table 4.2 reports the estimates of the effect of home internet access on the probability of searching online for a job. The F-Statistic in the full sample is close to the benchmark value of 10. This value decreases when analyzing subsamples. While weak instruments in just-identified models are of no major concern as long as the first stage coefficient differs from zero, they are associated with higher standard errors (Angrist and Pischke, 2008, Angrist and Pischke, 2009). Overall, the IV estimates suggest that the OLS estimates are downward biased. This downward bias has also been documented in the analysis using the administrative data. Home internet access causes a strong and significant increase in the probability of online job search. Moreover, the results suggest that this effect is most pronounced among males, whereas the point estimate for young individuals is insignificant and lower as compared to that for the pooled sample. Note that the insignificant effect for young job seekers is broadly consistent with the municipality-level results suggesting no positive internet effects on the job finding prospects for this group. A potential explanation for the absent effect of home internet access on online job-search among the young might relate to time-consuming online activities other than job search.[30] Turning to our final subgroup, we are not able to condition on skilled individuals with white-collar occupations (if unemployed, in their previous job) due to sample size restrictions. For this reason, we provide separate estimations for skilled individuals and individuals whose (previous) occupation was a white-collar job. The results show that the point estimate for skilled individuals is of the same order of magnitude as in the pooled sample and significant at the 10% level, whereas individuals whose last job was a white-collar job feature the highest point estimates. Consistent with our considerations in Section 4.2, this result lends support to the notion that the frequency with which employers' use the internet for recruiting purposes may matter for the intensity with which job seekers make use of online job search channels.

[30] Kolko (2010) shows that broadband internet leads to more music downloads and online shopping, which is likely to be particulary relevant for younger individuals. There is also evidence that primarily young males spend time on computer games (e.g. first person shooter games) and fulfill the need for social interaction through playing in an online network (Jansz and Tanis, 2007, Frostling-Henningsson, 2009).

Table 4.2: Estimation results for home internet on online job search

	Full sample OLS (1)	Full sample IV (2)	Male IV (3)	Young IV (4)	Skilled IV (5)	White-collar jobs IV (6)
Home internet access	0.273***	0.674**	0.685**	0.517	0.699*	0.774*
	(0.018)	(0.317)	(0.346)	(0.530)	(0.391)	(0.426)
Threshold (first stage)		-0.118***	-0.160***	-0.123*	-0.112***	-0.117**
		(0.037)	(0.053)	(0.067)	(0.043)	(0.048)
F-statistic		10.00	9.06	3.41	6.67	5.99
Observations	2,914	2,914	1,478	1,133	1,884	1,624

Notes: The table reports regression results of home internet access on online job search for individuals in West Germany. The results are based on linear probability models. Home internet access is instrumented by a threshold dummy indicating whether the distance of the centroid of a person's home municipality to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleibergen-Paap F-Statistic. Standard errors are heteroskedasticity robust and clustered at the household level. The number of observations (2,914) refers to the first observation of individuals during the first three waves. Thus, if we observe an individual multiple times during the first three waves, we use the first information only. The list of control variables includes individual characteristics, household information, father's education and information on the labor market history (see Table 4.G.1 in Appendix 4.G). Tables 4.G.2 and 4.G.3 provide descriptive statistics. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.
Source: PASS-ADIAB 7515, *Breitbandatlas Deutschland*, Falck et al. (2014) and *Geodaten Deutschland*, own computations.

While the results from Table 4.2 thus far suggest that home internet access leads to more online job search, it might be conceivable that online job search crowds out non-online job search channels. To address this issue, we further analyze the effect of home internet access on job seekers' use of the remaining reported job search channels provided by the PASS data. Panel A of Table 4.G.2 in Appendix 4.G further reports the share of individuals adopting different search methods broken down by home internet access. The figures point to a slight negative correlation between home internet access and the incidence of non-online job search channels. On average, individuals without home internet access make use of 2.2 non-online search channels, whereas individuals with home internet access use 2.0 non-online channels, with the difference being significant. Note, however, that the internet's effect on job finding probabilities via possible substitution effects is, in general, ambiguous as the overall effect is likely to depend on the relative efficiency of the different channels. To explore which channels are potentially affected by crowding out effects, Table 4.3 reports the results of home internet access on search via newspapers, referrals of friends or relatives, the local employment agency and the jobseeker's own initiative. The last column reports the effect on the sum of all non-online job search channels, which also includes private brokers and others.

The figures provide some weak evidence for a negative effect of home internet access on referrals by friends or relatives (column(2)). The estimated coefficient in the pooled sample is of the same order of magnitude as the corresponding effect on online job

Table 4.3: Estimation results for home internet on other job search channels

	Newspapers (1)	Referral (2)	Empl. Agency (3)	Own-initiative (4)	Sum non-online (5)
Panel A: Full sample					
Home internet access	0.228	-0.646*	-0.495	0.056	-1.006
	(0.261)	(0.355)	(0.345)	(0.060)	(0.773)
Panel B: Male					
Home internet access	0.157	-0.380	-0.068	0.063	-0.117
	(0.300)	(0.347)	(0.363)	(0.094)	(0.765)
Panel C: Young					
Home internet access	0.791	-0.567	-0.190	0.165	0.231
	(0.666)	(0.574)	(0.534)	(0.102)	(1.049)
Panel D: Skilled					
Home internet access	0.116	-0.398	-0.196	0.019	-0.504
	(0.296)	(0.394)	(0.379)	(0.077)	(0.899)
Panel E: White-collar jobs					
Home internet access	0.494	-0.584	-0.266	0.076	-0.498
	(0.385)	(0.423)	(0.402)	(0.050)	(0.962)

Notes: The table reports regression results of home internet access on various non-online job search channels for individuals in West Germany. The results are based on linear probability models. Home internet access is instrumented by a threshold dummy indicating whether the distance of the centroid of a person's home municipality to the next MDF is above 4,200 meters. The F-tests of excluded instruments refer to the Kleibergen-Paap F-Statistic and are equal to those reported in Table 4.2. Standard errors are heteroskedasticity robust and clustered at the household level. The number of observations is equal to that reported in Table 4.2. The number of observations (2,914) refers to the first observation of individuals during the first three waves. Thus, if we observe an individual multiple times during the first three waves, we use the first information only. The list of control variables includes individual characteristics, household information, father's education and information on the labor market history (see Table 4.G.1 in Appendix 4.G). Tables 4.G.2 and 4.G.3 provide descriptive statistics. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.
Source: PASS-ADIAB 7515, *Breitbandatlas Deutschland*, Falck et al. (2014) and *Geodaten Deutschland*, own computations.

search from the previous table. Moreover, we find a negative but insignificant effect on job search via the federal employment agency (column (3)). For the subgroups, the effects on referrals and the employment agency are also negative but insignificant. On the other hand, the estimates in column (4) indicate that own-initiative search and job search via newspapers are positively affected by home internet access (accompanied by large standard errors). This suggests that home internet access induces individuals to search more proactively. Turning to the sum of all non-online job search channels in column (5), the figures reveal insignificant but negative effects (except for young individuals) of home internet access on non-online search. Overall, these findings suggest an (insignificant but sizeable) reduction in non-online job search especially for skilled and white collar jobs, whereas for men crowding out effects seem to play a minor role.

Survey evidence on application intensity. Apart from job search channels, the data set allows us to analyze the number of job applications as a measure of search intensity as well as the number of (realized) job interviews. While the number of applications may be considered as a further measure of search intensity, the number of job interviews is likely to be an important prerequisite of job offers and may therefore be viewed as a (weak) proxy for the arrival of job offers. Table 4.4 reports the estimation results of the effects of home internet access on these outcomes.[31] For the pooled sample, none of the coefficients from the IV regressions turn out to be significant in Panel A. Comparing the point estimates from the IV specification with the OLS results reveals that the OLS coefficients are downward biased. Turning to the subsamples shows that especially males exhibit a positive home internet access effect on the number of applications. In particular, the local average treatment effect shows that home internet raises the number of applications by more than 12. This substantial increase in search intensity does not translate into a larger number of realized job interviews, though. For the pooled sample as well as for the subgroups, the figures from the last two columns indicate that all estimated coefficients are either negative or very small and insignificant at conventional levels.

Table 4.4: Estimation results for home internet on application intensity

	Full sample OLS (1)	Full sample IV (2)	Male IV (3)	Young IV (4)	Skilled IV (5)	White-collar jobs IV (6)
Panel A: # Own-initiative applications						
Home internet access	-0.031	5.212	12.033**	0.688	5.832	3.147
	(0.229)	(3.781)	(4.805)	(7.279)	(4.805)	(4.376)
Panel B: # Job interviews						
Home internet access	0.007	0.040	-0.636	0.626	-0.126	-0.415
	(0.066)	(0.897)	(1.170)	(1.364)	(1.156)	(1.147)
Observations	2,914	2,914	1,478	1,133	1,884	1,624

Notes: The table reports regression results of home internet access on the number of applications and realized job interviews for individuals in West Germany. The results for indicator outcome variables are based on linear probability models. Home internet access is instrumented by a threshold dummy whether the distance of the centroid of a person's home municipality to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleibergen-Paap F-Statistic and is the same as in Table 4.2. Standard errors are heteroskedasticity robust and clustered at the household level. The number of observations (2,914) refers to the first observation of individuals during the first three waves. Thus, if we observe an individual multiple times during the first three waves, we use the first information only. The list of control variables includes individual characteristics, household information, father's education and information on the labor market history (see Table 4.G.1 in Appendix 4.G). Tables 4.G.2 and 4.G.3 provide descriptive statistics. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.
Source: PASS-ADIAB 7515, *Breitbandatlas Deutschland*, Falck et al. (2014) and *Geodaten Deutschland*, own computations.

[31] More specifically, the survey asks respondents to report the number of *own-initiative* applications as well as the number of realized job interviews during *the last 4 weeks*.

4.9.2 Dynamics Within Individual Unemployment Spells

The overall pattern of results from our municipality analysis is that the positive effect on reemployment probabilities shows up or becomes significant only with a certain time delay after entering unemployment. What might explain this time pattern? Our considerations from Section 4.3 suggested that an absent internet effect in the beginning of an unemployment spell may be rationalized within a search theoretic framework, where the internet's negative effect on search costs initially dominates its positive effect on job offer arrival rates. Such a possible delay in the increase in job offers may be explained, for example, by the fact that the initial decline in search costs implies that job seekers can potentially apply to considerably more job advertisements as compared to non-online job search channels. Thus, when applying to online job advertisements, job seekers are confronted with considerably more potential jobs and employers that need to be evaluated against each other. This takes time and may therefore provide an explanation for the delay in the (internet-induced) increase in the arrival of job offers.

To test this notion, we analyze the dynamics of job interviews over the duration of an unemployment spell again using the PASS survey data. In particular, we look at how the number of job interviews evolve over the elapsed length of an unemployment spell. As explained earlier, the number and incidence of job interviews is the only measure that is available to operationalize job offers in our data sources. A pattern that would support the above considerations would involve a delayed increase in the incidence of job interviews. Due to data restrictions, we provide the analysis on a purely descriptive basis, by comparing the outcomes of interest between individuals with different unemployment durations. Restricting the analysis to individuals who were unemployed for a maximum of one year reduces the sample size considerably and renders a causal analysis unfeasible.[32]

To rationalize the established delay of the internet's effect on reemployment probabilities, we need to document different time patterns of job interviews over the spell's duration across those with and without home internet access. In this regard, Table 4.4 has pointed to insignificant (and often negative) effects of home internet access on the number and incidence of job interviews. In what follows, we explore whether the established insignificant effects might be due to time-varying effects over the duration of

[32]We also show in Appendix 4.G dynamics of online job search over the unemployment spell and document that the incidence of online job search increases during the first year of unemployment among individuals with home internet access. The relative increase is more pronounced among males after four months in unemployment. Among skilled white-collar workers, this increase starts after six months in unemployment (see Figures 4.G.2 in Appendix 4.G). This time pattern of online job search effort matches the results for the monthly hazard rates, indicating that reemployment hazards are 5-6% points higher in month 7 and 8.

an unemployment spell. To address this issue, Figure 4.G.3 plots the difference in the fraction of unemployed with job interviews by home internet access against different unemployment durations. Overall, the graphs illustrate that among those with home internet access the probability of job interviews is greater during the second to fourth quarter in unemployment as compared to their counterparts without home internet access. However, we wish to note that due to the small sample size these differences are estimated quite imprecisely. For males, home internet access raises the incidence of job talks even more pronounced during the second to fourth quarter in unemployment - but again imprecisely estimated. The time gap is found to match that from the municipality level estimations. This may potentially account for the delay of the established positive effects of the internet on unemployed job seekers' reemployment probabilities. Overall, these patterns are consistent with the internet expansion raising job offer arrival rates with a certain time delay of at least one quarter in unemployment.

Figure 4.G.2 in Appendix 4.G also shows the corresponding graphs for the other three subgroups. For young individuals, job interviews seem to be lower during the first quarter. Along with the insignificant overall online job search incidence this result may provide a rationale for the negative DSL coefficient documented in Section 4.8. The increase in the incidence of job interviews during the second to fourth first quarter in unemployment is also visible for young and skilled white-collar workers, but less pronounced than for males.

4.9.3 Search Externalities

In this section, we address potential search externalities. A first source of spill-overs relates to interdependencies across lucky and unlucky municipalities. If the job finding prospects of unemployed job seekers located in lucky municipalities improve due to better online job search opportunities, this might, in turn, reduce the respective prospects of those located in unlucky municipalities. The underlying notion is that job seekers in lucky and unlucky municipalities are likely to compete for jobs in the same local labor market, such that those benefitting from the internet expansion may impose a congestion externality on their "unlucky" counterparts. The quantitative relevance of such spill-overs is likely to depend on individuals' and employers' search radius and the extent to which this radius has been altered by the internet expansion. As long as interdependencies arise from employers' behavioral changes, this should limit the scope for spill-overs. The reason is that employers' search radius was likely to comprise unlucky as well as lucky municipalities already in the pre-DSL period. At the same time, however, there is evidence that the pre-DSL restrictions in internet access were more likely to be binding for workers than for firms (see Section 4.2). Thus, we would expect that potential spill-over effects primarily arise from the behavior of individual job seekers, whose search radius was likely to be affected by the internet. Note that in the presence of such externalities, our estimated coefficients would have to be interpreted as effects inclusive of potential general equilibrium spill-overs.

While we are not able to directly deal with such kinds of externalities, we attempt to address externalities caused by a different group of job seekers, who are not included in our treatment and control group. As set out in Section 4.3, the internet expansion not only reduces search costs for the unemployed, but also for those searching on-the-job. To the extent that the internet also raises the job finding prospects of the employed, the resulting search externalities may mitigate or counteract the internet's effect on unemployed job seekers' job finding rates. To test this notion, we explore whether the expansion in broadband availability has led to an increase in job-to-job transitions among employed individuals. To rule out potential match quality effects, we confine our analysis to employment relationships that had already started prior to the DSL-period. To do so, we construct a stock sample of individuals who were employed at the cut-off date of 30th of June 2000 and who were still employed at the same employer at the start of 2007. For this sample, we then calculate the fraction of job-to-job transitions at the municipality level during the late DSL years 2007/08. To compare this outcome with the pre-DSL period, we construct an analogous sample and outcome variable for the pre-internet period, based on individuals who were employed at the cut-off date of 30th of June in 1991 and who were still employed at the same employer at the start of 1998 (see Table 4.H.1 in Appendix 4.H for basic descriptive statistics for both samples). This implies that we exclude individuals from our sample who experienced a transition from employment to unemployment or non-employment during the pre-DSL and DSL period, respectively. While this procedure allows us to rule out match quality effects, which - depending on the direction of the internet's effect on match quality - are also likely to affect the extent of job-to-job transitions, it comes at the cost of restricting the analysis to very stable employment relationships.

Columns (1) and (2) of Table 4.5 show the OLS and IV results for the full sample. The coefficients are negative and not significantly different from zero. An increase in DSL availability does not affect the probability of a direct job-to-job transition at the municipality level. If anything, the results point to slightly negative effects. A similar result holds if the regressions are performed separately by subgroups. Overall, these findings argue against the view that increased competition from employed job seekers should have played a significant role for the internet's effect on the job finding prospects of their unemployed counterparts. To the extent that employed individuals may have made use of their workplace internet access for job search, these results are consistent with the fact that the restrictions in internet access were less likely to be binding for employers than for private households during the DSL period.

Table 4.5: Spill-over estimation results, job-to-job transitions

	Full sample		Male	Young	Skilled white-collar
	OLS	IV	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)
ΔDSL	-0.016	-0.049	-0.101	-0.025	-0.026
	(0.012)	(0.063)	(0.082)	(0.108)	(0.089)
Threshold (first stage)		-0.054***	-0.053***	-0.051***	-0.050***
		(0.006)	(0.006)	(0.006)	(0.006)
F-Statistic		71.07	67.95	63.84	61.76
Municipalities	2,523	2,523	2,497	2,376	2,424

Notes: The table shows the effects of a 1% point increase in the share of households with DSL availability on the probability of job-to-job transitions for a stock sample of employed individuals (see Table 4.H.1 in Appendix 4.H). The estimates in columns (1) and (2) are based on a sample of individuals whose employment relationship started prior to the DSL/pre-DSL period. Columns (3)-(5) show the results separately for males, young individuals (below 35 years) and skilled white-collar individuals. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and the firm structure (see Table 4.B.1 in Appendix 4.B). Standard errors are heteroskedasticity robust and clustered at the municipality level. The distance is measured from the geographic centroid to the MDF and weighted by the location of the population. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.
Source: IEB, *Establishment History Panel* , MUP, *Breitbandatlas Deutschland* and Falck et al. (2014), own computations.

4.10 Discussion and Conclusions

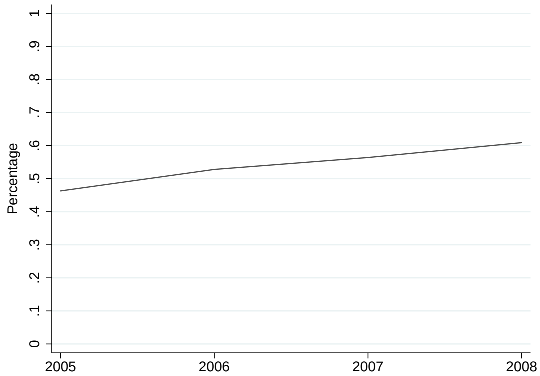
In this chapter, we study the effects of the expansion in broadband internet (DSL) on reemployment probabilities among unemployed job seekers by exploiting regional peculiarities of the traditional public switched telephone network in West Germany. Overall, our results suggest that effects of the internet on the reemployment prospects of unemployed individuals based on OLS estimates are downward biased. After accounting for the endogeneity in internet availability, our estimates for the pooled sample provide slight positive internet advantages for unemployed job seekers with a certain time delay. Breaking down the analysis by socio-economic characteristics suggests that the internet's positive effect is particularly pronounced for male job seekers after spending four months in unemployment.

Given that the above strategy identifies an ITT, we also address first-stage effects by retrieving information on the adoption of job search channels from the PASS survey data. Using these data, we explore whether the availability of internet at home has a causal impact on job seekers' *use* of the internet as a search channel. To gain further insights into potential crowding out effects, we also look at whether home internet access causally affects the use of alternative job search channels. The results, which are based on the same IV strategy as in the municipality-level analysis, indicate that home internet access causes an increase in online job search activities. Consistent with our municipality-level results, especially male and skilled white-collar job seekers are found to increase online job search if they have home internet access. The results provide also some evidence for crowding out effects on non-online job search, which are most pronounced (albeit insignificant) for white-collar and skilled workers and which appear to play less of a role for male job seekers. These findings lead us to conclude that the expansion of internet availability led to better reemployment prospects especially for males via raising the intensity with which this group has made use of the internet to search for jobs, without at the same time reducing their overall search effort.

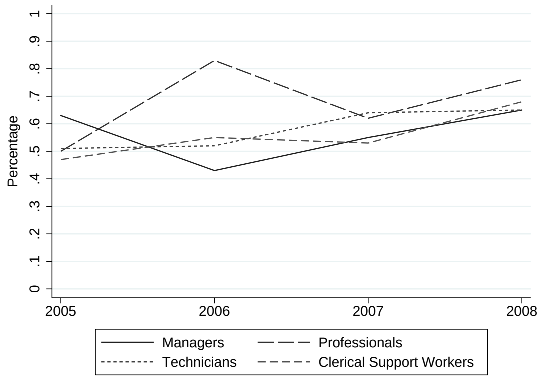
The survey data also reveal that home internet access raises the number of own-initiative applications, especially for males. A further finding was that the positive effect on reemployment probabilities shows up or becomes significant only with a certain time delay after entering unemployment. This time pattern may be rationalized within a search theoretic framework, where the internet's negative effect on search costs initially dominates its positive effect on job offer arrival rates. To provide empirical support for a delayed positive effect on job offers, we further explore whether the incidence of job interviews across those with and without home internet access varies over the duration of an unemployment spell. Our findings provide some tentative evidence that internet access appears to give rise to an increase in the incidence of job interviews with a certain time delay, which appears to match the delay found in the municipality level analysis. Even though these findings are derived on a merely descriptive basis, they are consistent with the view that online job search puts job seekers in a situation where they need to compare more *potential* jobs and employers, which takes time and may delay the arrival of job offers. These results also offer potential directions for future research. Given that the internet raises the number of potential jobs that need to be evaluated against each other, future research should examine in more detail the internet's effect on job quality, e.g. whether the internet helps job seekers find a better job.

4.Appendix

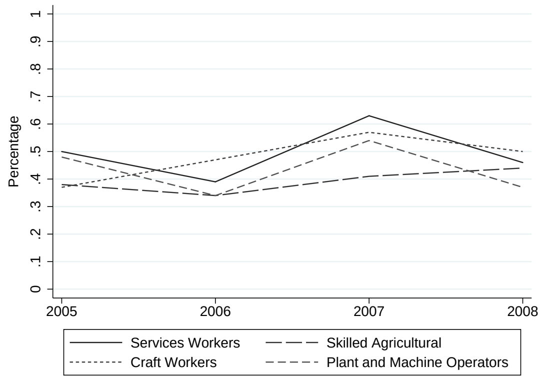
4.A Evolution of Online Recruiting



(A) Overall online recruiting



(B) Online recruiting by occupation - I



(C) Online recruiting by occupation - II

Notes: The plots show the fraction of vacancies being posted online among all successful hirings. Panel (A) shows the overall time trend. Panel (B) and Panel (C) show the trend by different occupational categories.
Source: IAB Job Vacancy survey, own computations.

Figure 4.A.1: Evolution of online recruiting

4.B Administrative Data Addendum

Table 4.B.1: Definition of variables

Labor market variables	Description
Reemployment probability	Reemployment probabilities are based on a yearly inflow sample of individuals into unemployment. Reemployment probabilities are estimated at the municipality level as the share of individuals with a transition into employment.
	Source: IEB, Federal Employment Agency
Internet variables	
Broadband internet	Fraction of households in municipality i at year t with a subscription to DSL defined by an access speed of 384 kb/s or above. Documented numbers start in 2005.
	Source: Breitbandatlas Deutschland
Treatment	Equals 1 for municipalities in West Germany with a distance of more than 4,200 meters to the next main distribution frame (MDF). The distance is calculated using the geographic centroid weighted by the location of the population.
	Source: Falck et al. (2014)
Control variables	
Population	Number of inhabitants in municipality i at year t.
	Source: Falck et al. (2014)
Inflow unemployed	Number of individuals who became unemployed in municipality i at year t.
	Source: IEB, Federal Employment Agency
Female population share	Fraction of females in municipality i at year t. The female share is also measured for the inflow-specific sample.
	Source: Falck et al. (2014) and IEB, Federal Employment Agency
Population aged 18-65	Fraction of the population aged between 18 and 65 years in municipality i at year t. The pre-DSL fraction refers to the year 2001.
	Source: Falck et al. (2014)
Population aged > 65	Fraction of the population aged above 65 years in municipality i at year t. The pre-DSL fraction refers to the year 2001.
	Source: Falck et al. (2014)
Net migration	Net migration rate in municipality i at year t. The pre-DSL fraction refers to the year 2001.
	Source: Falck et al. (2014)
Unemployment rate	Unemployment rate in municipality i at year t. The pre-DSL fraction refers to the year 2001.
	Source: Falck et al. (2014)
Foreign nationals	Fraction of foreigners in municipality i at year t. The nationality is also measured for the inflow-specific sample.
	Source: IEB, Federal Employment Agency

Table 4.B.1: Definition of variables *(continuation)*

Control variables	Description
Occupation	Occupational shares in municipality *i* at year *t* calculated for the categories agriculture, production, salary, sale, clerical and service (ref. service sector). The occupation is also measured for the inflow-specific sample. Source: IEB, Federal Employment Agency
Industry	Industry shares in municipality *i* at year *t* calculated for the categories agriculture/energy/mining, production, steel/metal/machinery, vehicle construction/apparatus engineering, consumer goods, food, construction, finishing trade, wholesale trade, retail trade, transport and communication, business services, household services, education/helth, organizations, public sector, else. Source: IEB, Federal Employment Agency
Skill level	Skill level in municipality *i* at year *t*. *Low skilled*: No degree/ highschool degree *Medium skilled*: Vocational training *High skilled*: Technical college degree or university degree. Skill level is also measured for the inflow-specific sample. Missing and inconsistent data on education are corrected according to the imputation procedure described in Fitzenberger et al. (2006). This procedure relies on the assumption that individuals cannot lose their educational degrees. Source: IEB, Federal Employment Agency
Real daily wage	Average real daily wage in municipality *i* at year *t* calculated among full-time employees. Gross daily wages are right-censored due to the upper social security contribution limit. To address this problem, we construct cells based on gender and year. For each cell, a Tobit regression is estimated with log daily wages as the dependent variable and age, tenure, age squared, tenure squared, full-time dummy, two skill dummies, occupational, sectoral as well as regional (Federal State) dummies as explanatory variables. As described in Gartner (2005), right-censored observations are replaced by wages randomly drawn from a truncated normal distribution whose moments are constructed by the predicted values from the Tobit regressions and whose (lower) truncation point is given by the contribution limit to the social security system. After this imputation procedure, nominal wages are deflated by the CPI of the Federal Statistical Office Germany normalised to 1 in 2010. Source: IEB, Federal Employment Agency
Number of establishments	Number of establishments in municipality *i* at year *t*. Source: Establishment History Panel, Federal Employment Agency
Size of establishments	Number of employees per establishment in municipality *i* at year *t*. Source: Establishment History Panel, Federal Employment Agency
Number of firm entries	Number of firms entering the market in municipality *i* at year *t*. The pre-DSL fraction refers to the year 2000. Source: Mannheim Enterprise Panel
Number of firm exits	Number of firms exiting the market in municipality *i* at year *t*. The pre-DSL fraction refers to the year 2000. Source: Mannheim Enterprise Panel
Total sales	Total sales based on firm information in municipality *i* at year *t*. The pre-DSL fraction refers to the year 2001. Source: Mannheim Enterprise Panel

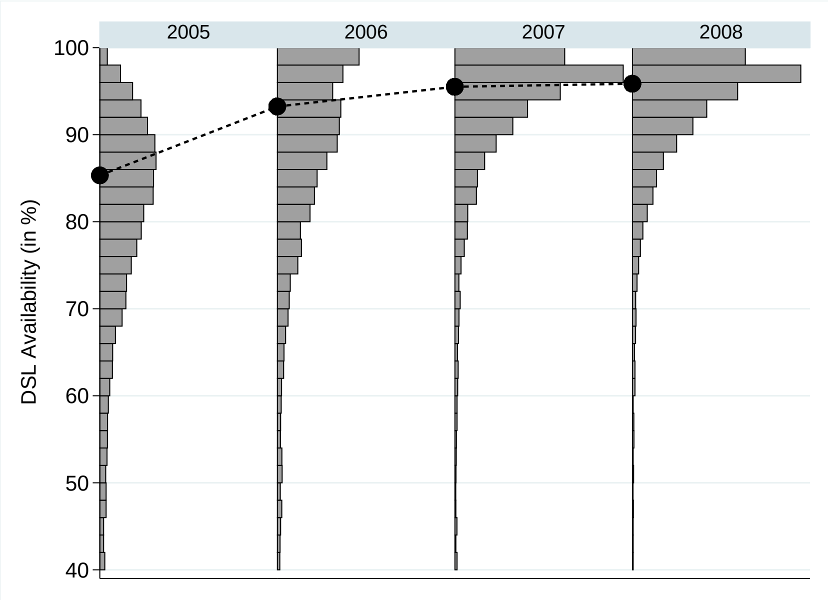
Table 4.B.2: Definition of labor market states

Employment: Employment spells include continuous periods of employment (allowing gaps of up to one month) subject to social security contributions and (after 1998) marginal employment. For parallel spells of employment and unemployment (e.g. for those individuals who in addition to their earnings receive supplementary benefits), we treat employment as the dominant labor market state.

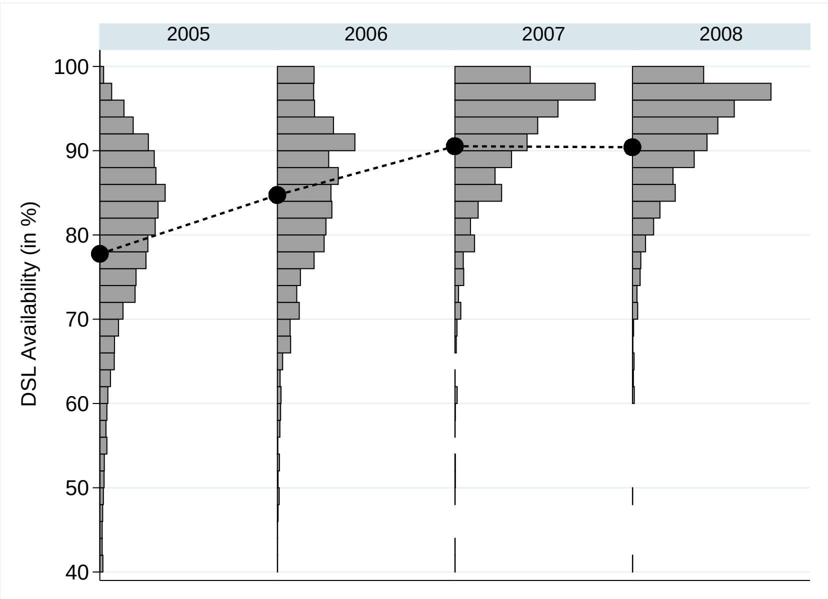
Unemployment Unemployment spells include periods of job search as well as periods with transfer receipt. Prior to 2005, the latter include benefits such as unemployment insurance and means-tested unemployment assistance benefits. Those (employable) individuals who were not entitled to unemployment insurance or assistance benefits could claim means-tested social assistance benefits. However, prior to 2005, spells with social assistance receipt may be observed in the data only if the job seekers' history records social assistance recipients as searching for a job. After 2004, means-tested unemployment and social assistance benefits were merged into one unified benefit, also known as 'unemployment benefit II' (ALG II). Unemployment spells with receipt of ALG II are recorded in the data from 2007 onwards, such that the data provide a consistent definition of unemployment only for the period 2007-2010.

Distinction between un- and non-employment Extending the procedure proposed by Lee and Wilke (2009), involuntary unemployment is defined as comprising all continuous periods of registered job search and/or transfer receipt. Gaps between such unemployment periods or gaps between transfer receipt or job search and a new employment spell may not exceed three months, otherwise these periods are considered as non-employment spells (involving voluntary unemployment or an exit out of the social security labor force). Similarly, gaps between periods of employment and transfer receipt or job search are treated as involuntary unemployment as long as the gap does not exceed six weeks, otherwise the gap is treated as non-employment.

4.C Descriptive Statistics



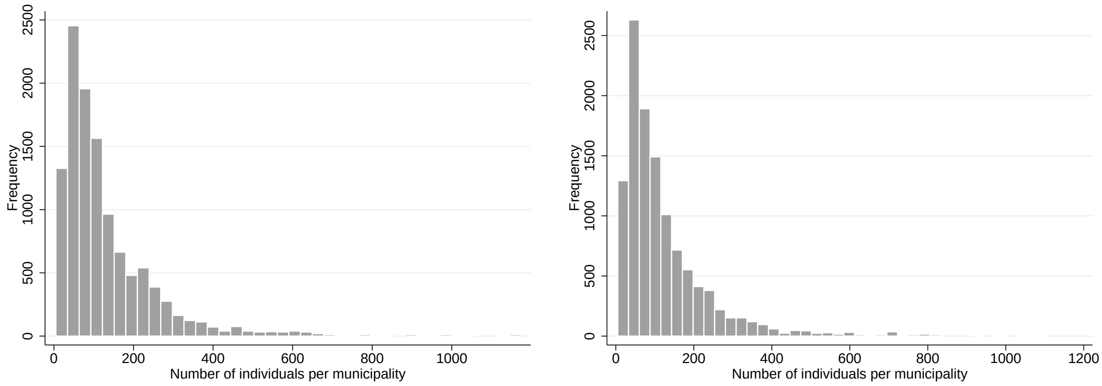
(A) Agglomerated municipalities



(B) Less agglomerated municipalities

Notes: The figures show histograms of DSL availability (measured as a percentage of households for which DSL is technically available) in German municipalities for the defined DSL years 2005 to 2008. Panel (A) shows the development for agglomerated municipalities. Panel (B) shows the results for less agglomerated municipalities (used in the IV approach) without an own MDF and no closer MDF available. The graphs are truncated at 40%. The dotted line connects the population-weighted mean availabilities for all years.
Source: IEB, *Establishment History Panel* , MUP, *Breitbandatlas Deutschland* and Falck et al. (2014), own computations.

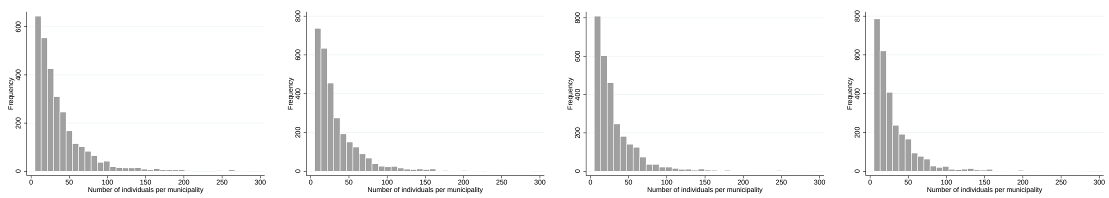
Figure 4.C.1: Empirical distribution of DSL availability by sample



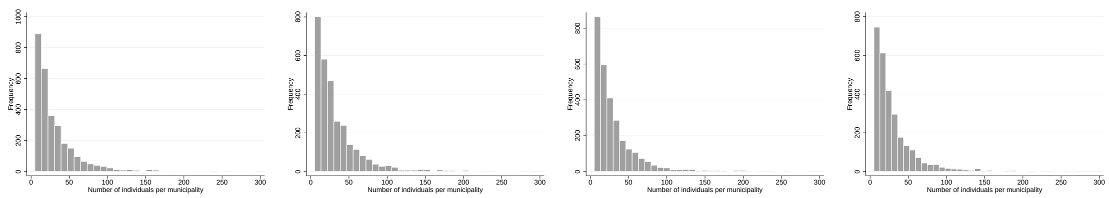
(A) DSL period *(B) Pre-DSL period*

Notes: The figures plot the distribution of the number of individuals in the unemployment inflow sample per municipality for the DSL (2005-2008) and the pre-DSL period (1996-1999). The median over all DSL years equals 93. The median over all pre-DSL years equals 87.
Source: IEB, *Establishment History Panel* , MUP, *Breitbandatlas Deutschland* and Falck et al. (2014), own computations.

Figure 4.C.2: Observed individuals per municipality by period



(1-A) 2005 *(1-B) 2006* *(1-C) 2007* *(1-D) 2008*



(1-A) 1996 *(2-B) 1997* *(3-C) 1998* *(4-D) 1999*

Notes: The figures plot the distribution of the number of individuals in the unemployment inflow sample per municipality for each pre-DSL and DSL year.

Figure 4.C.3: Observed individuals per municipality for all DSL and pre-DSL years

Table 4.C.1: Further descriptive statistics

	Pre-DSL years 1998/99 (1)	DSL years 2007/08 (2)
Panel A: Demand-side variables		
Number of establishments	30.167	40.939
	(39.655)	(52.480)
Establishment size	6.244	6.121
	(5.213)	(4.572)
Number of firm entries	2.742	2.378
	(3.773)	(3.284)
Number of firm exits	1.867	3.290
	(3.010)	(4.478)
Sales	30.089	64.663
	(413.700)	(568.570)
Panel B: Sector composition		
Agriculture/Energy/Mining	0.034	0.033
	(0.027)	(0.026)
Production	0.065	0.049
	(0.052)	(0.040)
Steel/Metal/Machinery	0.091	0.085
	(0.062)	(0.060)
Vehicle construction/Apparatus engineering	0.041	0.038
	(0.044)	(0.039)
Consumer goods	0.055	0.042
	(0.039)	(0.028)
Food	0.035	0.033
	(0.024)	(0.022)
Construction	0.068	0.042
	(0.040)	(0.027)
Finishing trade	0.049	0.037
	(0.023)	(0.018)
Wholesale trade	0.052	0.049
	(0.027)	(0.024)
Retail trade	0.093	0.098
	(0.033)	(0.030)
Transport and communication	0.047	0.054
	(0.026)	(0.023)
Business services	0.084	0.105
	(0.034)	(0.037)
Household services	0.066	0.081
	(0.039)	(0.036)
Education/Health	0.120	0.136
	(0.045)	(0.045)
Organizations	0.018	0.021
	(0.013)	(0.013)
Public sector	0.057	0.056
	(0.026)	(0.023)
Panel C: Inflow characteristics		
Occupation		
Agriculture	0.056	0.043
	(0.075)	(0.060)
Production	0.452	0.391
	(0.159)	(0.144)
Salary	0.074	0.073
	(0.072)	(0.068)
Sale	0.061	0.073
	(0.062)	(0.065)
Clerical	0.142	0.146
	(0.100)	(0.096)
Service	0.215	0.274
	(0.112)	(0.122)

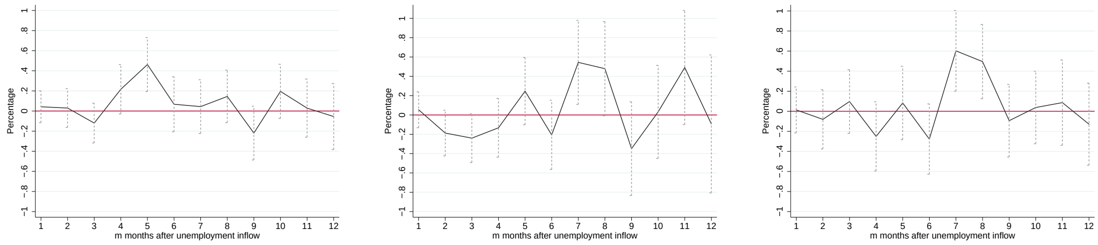
Notes: The table reports municipality-level descriptive statistics for West Germany. The numbers are averaged within the pre-DSL and the DSL years, respectively. Panel A reports demand-side variable. Panel B report the sector structure. Panel C reports occupational for the unemployment inflow sample.
Source: IEB, *Establishment History Panel* , MUP, *Breitbandatlas Deutschland* and Falck et al. (2014), own computations.

Table 4.C.2: Estimation results analyzing demand-side effects

	Net firm creation (1)	Firm entry (2)	Firm exit (3)	Sales (4)
Δ DSL	0.270	-0.136	-0.495	86.62
	(0.716)	(0.510)	(0.504)	(97.32)
F-Statistic (first stage)	211.7	213.7	211.7	211.7
Observations	6,568	6,678	6,568	6,566
Number of Municipalities	3,284	3,339	3,284	3,283

Notes: The figure shows the effect of a 1% point increase in the share of households with DSL availability on selected demand-side variables. Sales are measured in million euro. The pre-DSL year refers to the year 2000. The DSL period covers the years between 2007 and 2008. The list of control variables includes population structure, employment structure, occupational shares and industry shares. The *F*-test of excluded instruments refers to the Kleibergen-Paap *F*-Statistic. Standard errors are heteroskedasticity robust and clustered at the municipality level. The distance is measured from the geographic centroid to the MDF and weighted by the location of the population. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.
Source: IEB, *Establishment History Panel* , MUP, *Breitbandatlas Deutschland* and Falck et al. (2014), own computations.

4.D Empirical Hazard Rates

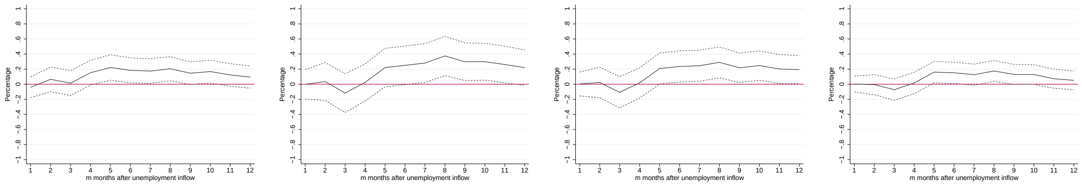


(A) Male *(B) Young* *(C) Skilled white-collar*

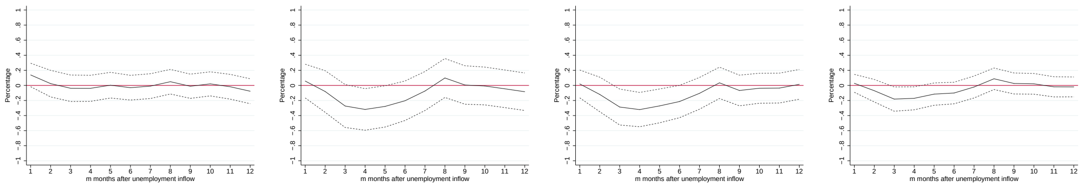
Notes: The figure shows the effects of a 1% point increase in the share of households with DSL availability on the transition probability from unemployment to employment in month *m* for an inflow sample of individuals who entered unemployment between 1998/1999 and 2007/2008 separately for males, young individuals (below 35 years) and skilled white-collar individuals. The regressions are population-weighted and performed separately for each month. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table 4.B.1 in Appendix 4.B). Dotted lines present the 90% confidence interval. Standard errors are heteroskedasticity robust and clustered at the municipality level. The distance is measured from the geographic centroid to the MDF and weighted by the location of the population. Regressions are based on 2,551 municipalities and 803 MDFs for males, 2,359 municipalities and 765 MDFs for young individuals and 2,066 municipalities and 713 MDFs for skilled white-collar individuals. The Kleibergen-Paap F-Statistic for the first stage is 60.0, 53.4 and 57.6 for the three groups, respectively.
Source: IEB, *Establishment History Panel* , MUP, *Breitbandatlas Deutschland* and Falck et al. (2014), own computations.

Figure 4.D.1: IV regression results of DSL on unemployment-to-employment transitions by socio-economic characteristics

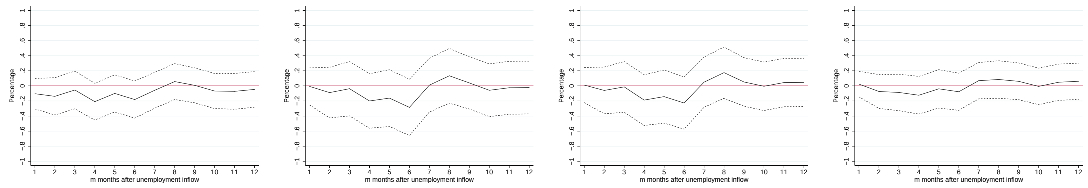
4.E Sensitivity and Robustness Results



(1-A) All tenure, Male *(1-B) Population 500+, Male* *(1-C) 6 month gap, Male* *(1-D) Without weighting, Male*



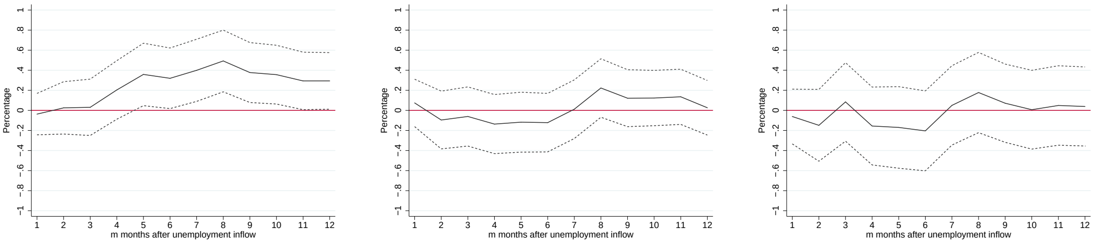
(2-A) All tenure, Young *(2-B) Population 500+, Young* *(2-C) 6 month gap, Young* *(2-D) Without weighting, Young*



(3-A) All tenure, Skilled white-collar *(3-B) Population 500+, Skilled white-collar* *(3-C) 6 month gap, Skilled white-collar* *(3-D) Without weighting, Skilled white-collar*

Notes: The figure shows the effects of a 1% point increase in the share of households with DSL availability on the cumulative transition probability from unemployment to employment within *m* months for an inflow sample of individuals who entered unemployment between 1998/1999 and 2007/2008 separately for males, young individuals (below 35 years) and skilled white-collar individuals. Panel (A) performs the analysis for an inflow sample of individuals without excluding persons who have been less than three months employed before entering unemployment. Panel (B) performs the analysis conditional on the local municipality size of at least 500 inhabitants. Panel (C) performs the analysis for an inflow sample by allowing for gaps in the administrative records between unemployment and another labor market state of at most six months. Panel (D) performs the analysis without population-weighting. The regressions in Panel (A) - (C) are population-weighted and performed separately for each month. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table 4.B.1 in Appendix 4.B). Dotted lines present the 90% confidence interval. Standard errors are heteroskedasticity robust and clustered at the municipality level.
Number of municipalities: Male: (1-A): 2,812, (1-B): 2,046, (1-C): 2,553, (1-D): 2,551; Young: (2-A): 2,688, (2-B): 2,009, (2-C): 2,363, (2-D): 2,359; Skilled white-collar: (3-A): 2,405, (3-B): 1,842, (3-C): 2,072, (3-D): 2,066.
Source: IEB, *Establishment History Panel* , MUP, *Breitbandatlas Deutschland* and Falck et al. (2014), own computations.

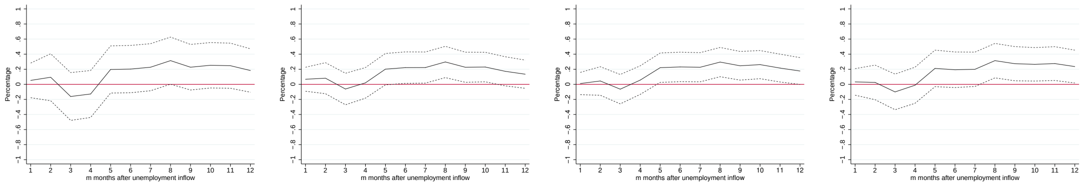
Figure 4.E.1: IV regression results of DSL on unemployment-to-employment transitions, sample specification



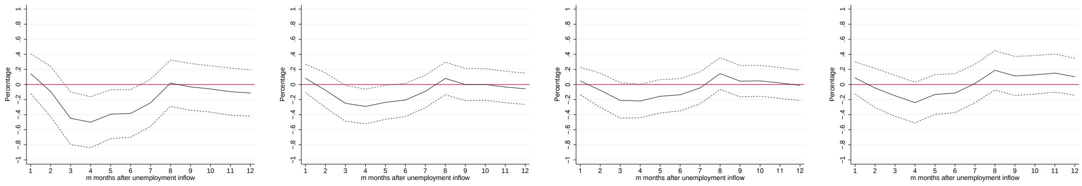
(A) Male *(B) Young* *(C) Skilled white-collar*

The figure shows the effects of a 1% point increase in the share of households with DSL availability on the cumulative transition probability from unemployment to employment within *m* months for an inflow sample of individuals who entered unemployment between 1998/1999 and 2007/2008 separately for males, young individuals (below 35 years) and skilled white-collar individuals. The regressions exclude individuals entering unemployment from sectors with a priori high recall rates (e.g. agriculture, construction, hotel and restaurant, passenger transport and delivery service). The regressions are population-weighted and performed separately for each month. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table 4.B.1 in Appendix 4.B). Dotted lines present the 90% confidence interval. Standard errors are heteroskedasticity robust and clustered at the municipality level.
Number of municipalities: Male: (A): 2,529; Young: (B): 2,350; Skilled white-collar: (C): 2,064.
Source: IEB, *Establishment History Panel* , MUP, *Breitbandatlas Deutschland* and Falck et al. (2014), own computations.

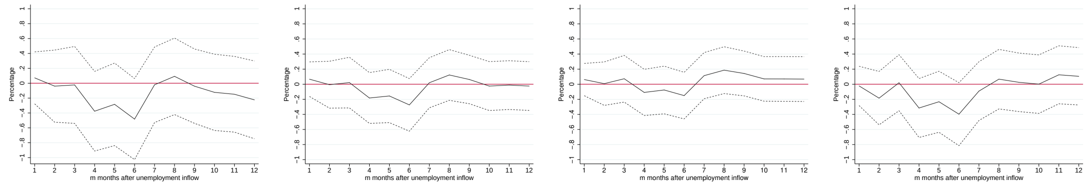
Figure 4.E.2: IV regression results of DSL on unemployment-to-employment transitions, excluding recall industries



(1-A) 2000m, Male | *(1-B) Outlier, Male* | *(1-C) Mean, Male* | *(1-D) Pre-DSL year 1998, Male*



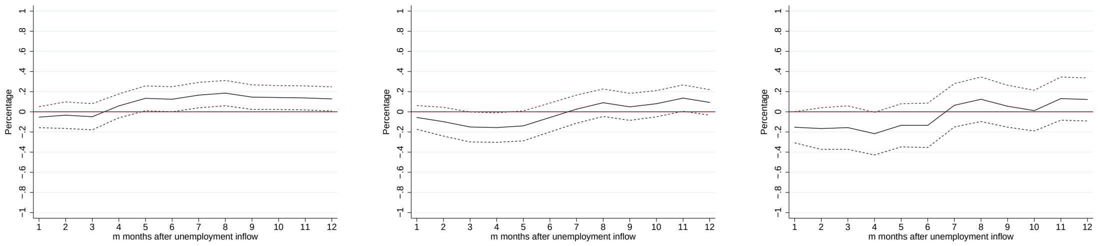
(2-A) 2000m, Young | *(2-B) Outlier, Young* | *(2-C) Mean, Young* | *(2-D) Pre-DSL year 1998, Young*



(3-A) 2000m, Skilled white-collar | *(3-B) Outlier, Skilled white-collar* | *(3-C) Mean, Skilled white-collar* | *(3-D) Pre-DSL year 1998, Skilled white-collar*

Notes: The figure shows the effects of a 1% point increase in the share of households with DSL availability on the cumulative transition probability from unemployment to employment within *m* months for an inflow sample of individuals who entered unemployment between 1998/1999 and 2007/2008 separately for males, young individuals (below 35 years) and skilled white-collar individuals. Panel (A) performs the analysis on municipalities whose distance to the next MDF is less than 2,000 meters from the threshold. Panel (B) performs the analysis by excluding outlier municipalities (see above). Panel (C) performs the analysis by averaging over the single years within the DSL and pre-DSL period. Panel (D) performs the analysis by assigning the year 1998 to every DSL year and then calculate the differences. The regressions are population-weighted and performed separately for each month. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table 4.B.1 in Appendix 4.B). Dotted lines present the 90% confidence interval. Standard errors are heteroskedasticity robust and clustered at the municipality level.
Number of municipalities: Male: (1-A): 1,928, (1-B): 2,537, (1-C): 2,812, (1-D): 2,455; Young: (2-A): 1,785, (2-B): 2,347, (2-C): 2,359, (2-D): 2,254; Skilled white-collar: (3-A): 1,545, (3-B): 2,054, (3-C): 2,066, (3-D): 1,902.
Source: IEB, *Establishment History Panel* , MUP, *Breitbandatlas Deutschland* and Falck et al. (2014), own computations.

Figure 4.E.3: IV regression results of DSL on unemployment-to-employment transitions, empirical specification

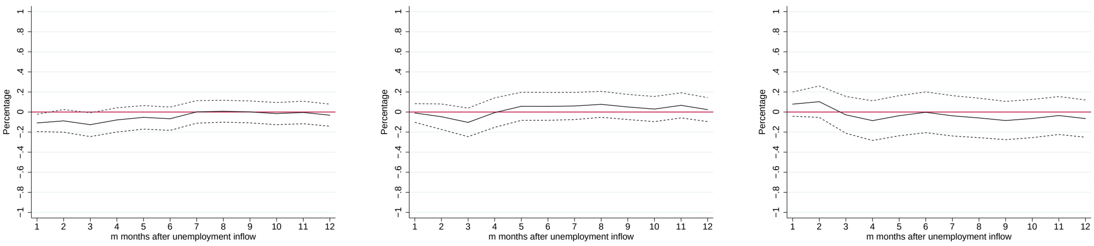


(A) Male *(B) Young* *(C) Skilled white-collar*

Notes: The figure shows the effects of a 1% point increase in the share of households with DSL availability on the cumulative transition probability from unemployment to employment within *m* months for an inflow sample of individuals who entered unemployment between 1998/1999 and 2007/2008 separately for males, young individuals (below 35 years) and skilled white-collar individuals. All regressions include a continuous instrument by interacting the treatment dummy with the actual distance. The regressions are population-weighted and performed separately for each month. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table 4.B.1 in Appendix 4.B). Dotted lines present the 90% confidence interval. Standard errors are heteroskedasticity robust and clustered at the municipality level.
Number of municipalities: Male: (A): 2,551; Young: (B): 2,359; Skilled white-collar: (C): 2,066.
Source: IEB, *Establishment History Panel* , MUP, *Breitbandatlas Deutschland* and Falck et al. (2014), own computations.

Figure 4.E.4: IV regression results of DSL on unemployment-to-employment transitions, continuous instrument specification

4.F Estimation Results for the Years 2005/06



(A) Male *(B) Young* *(C) Skilled white-collar*

Notes: The figure shows the effects of a 1% point increase in the share of households with DSL availability on the cumulative transition probability from unemployment to employment within *m* months for an inflow sample of individuals who entered unemployment between 1996/1997 and 2005/2006 separately for males, young individuals (below 35 years) and skilled white-collar individuals. The regressions are population-weighted and performed separately for each month. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table 4.B.1 in Appendix 4.B). Dotted lines present the 90% confidence interval. Standard errors are heteroskedasticity robust and clustered at the municipality level. The distance is measured from the geographic centroid to the MDF and weighted by the location of the population. Regressions are based on 2,724 municipalities and 820 MDFs for males, 2,541 municipalities and 790 MDFs for young individuals and 2,127 municipalities and 724 MDFs for skilled white-collar individuals. The Kleibergen-Paap *F*-Statistic for the first stage is 110.6, 101.6 and 76.6 for the three groups, respectively.
Source: IEB, *Establishment History Panel* , MUP, *Breitbandatlas Deutschland* and Falck et al. (2014), own computations.

Figure 4.F.1: IV regression results of DSL on unemployment-to-employment transitions by socio-economic characteristics 2005/06

4.G PASS Data Addendum

Table 4.G.1: Definition of variables

Outcomes	Description
Job search	Dummies for job search channels used by individuals who are looking for a job at the interview date: online job search, search via newspapers, friends/relatives, private broker, the local employment agency, own-initiative or non-specified search channels
Number of applications	Number of own-initiative applications during last four weeks
Number of job interviews	Number of job interviews during last four weeks
Individual characteristics	
Main employment status	Dummies for main employment status at interview date: employed, program participant, reference category: unemployed
Age	Dummies for age groups: age 26 - 35 years, age 36 - 45 years, age 46 - 55 years, age 56 - 65 years, reference category: age $\leq$ 25 years
Immigrant	Dummy for being an immigrant
Female	Dummy for being female
Professional qualification	Dummies for highest professional qualification level: certificate of secondary education (*Hauptschulabschluss*, *Realschulabschluss*) without vocational training, high school diploma (*Fachhochschulreife*, *Hochschulreife*) without vocational training, certificate of secondary education with vocational training, high school diploma with vocational training, Foreman (*Meister, Techniker*) or diploma of Berufsakademie (BA), technical college (TC) or university degree, reference category: no degree
Married	Dummy for being married
Attitudes to work	Dummies for work attitude based on four item-scale ranging from 1 (disagree) to 4 (totally agree) to valuate four statements ("Work is only a means to earn money", "Having a job is the most important thing in life", "Work is important, because it gives you the feeling of being part of society", "I would like to work even if I didn't need the money"): high ($\geq$ 4), medium (> 0 and < 4), missing, reference category: low ($\leq$ 0)
Household information	
HH income	Dummies for household income per month in €: 1,000 - 1,499, 1,500 - 1,999, 2,000 - 2,999, 3,000 - 3,999, 4,000 - 4,999, $\geq$ 5,000, reference category: $\leq$ 1,000
Means-tested HH	Dummy for household receiving unemployment benefits II
HH size	Dummies for household size: two persons, three persons, more than three persons, reference category: single household
Housing situation	Dummy for being home owner, for living in a shared flat, reference category: rent
Father's education	
Professional qualification	Dummies for highest professional qualification level: certificate of secondary education (*Hauptschulabschluss*, *Realschulabschluss*) or high school diploma (*Fachhochschulreife*, *Hochschulreife*) without vocational training, certificate of secondary education or high school diploma with vocational training, Foreman (*Meister, Techniker*) or diploma of Berufsakademie, technical college or university degree, father's education is missing, reference category: no degree
Labor market history	
Unemployment duration	Dummies for cumulative unemployment duration in months: categories are spitted according to percentiles: 0 - 25, 25 - 50, 50 - 75, > 75, reference category: 0
Tenure	Dummies for length of last employment spell (with social security contributions) in months: categories are spitted according to percentiles: 0 - 25, 25 - 50, 50 - 75, > 75, reference category: 0
Daily wage	Daily wage of last employment spell (with social security contributions) in 2010 €
History missing	Dummy for information on labor market history based on administrative data is missing

Source: PASS-ADIAB 7515, own computations.

Table 4.G.2: Home internet access, job search methods and application intensity

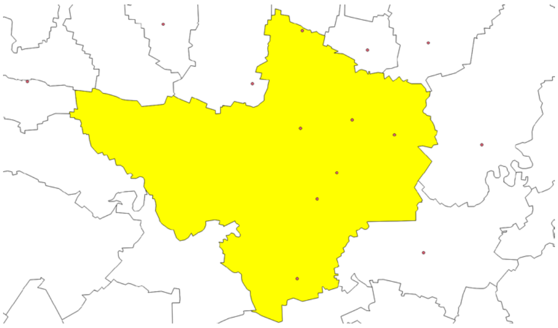
	N (1)	No home internet (2)	Home internet (3)	*p*-value (4)
Panel A: Job search				
Job search: online	2,914	0.578	0.848	0.000
Job search: newspaper	2,914	0.864	0.813	0.000
Job search: referral	2,914	0.669	0.601	0.000
Job search: empl. agency	2,914	0.423	0.342	0.000
Job search: private broker	2,914	0.151	0.139	0.349
Job search: own-initiative	2,914	0.018	0.013	0.225
Job search: else	2,914	0.129	0.100	0.013
Sum non-online search	2,914	2.255	2.008	0.000
Panel B: Application				
No. of applications (own-initiative)	2,914	2.525	2.465	0.781
No. of job interviews	2,914	0.597	0.600	0.967

Notes: The number of observations refers to individuals observed during the first three waves (2006/07, 2008 and 2009).
Source: PASS-ADIAB 7515, *Breitbandatlas Deutschland*, Falck et al. (2014) and *Geodaten Deutschland*, own computations.

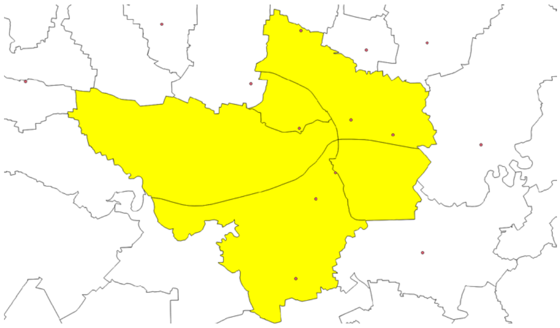
Table 4.G.3: Descriptive statistics of individual characteristics

	N (1)	Mean (2)	No home internet (3)	Home internet (4)	*p*-value (5)
Employed	2,914	0.177	0.105	0.232	0.000
Program participant	2,914	0.122	0.128	0.117	0.399
Age $\leq$ 25	2,914	0.140	0.142	0.138	0.757
Age 26-35	2,914	0.249	0.232	0.263	0.055
Age 36-45	2,914	0.314	0.290	0.333	0.014
Age 46-55	2,914	0.230	0.253	0.212	0.010
Age 56-65	2,914	0.067	0.083	0.054	0.002
Immigrant	2,914	0.125	0.149	0.107	0.001
Female	2,914	0.493	0.473	0.508	0.068
No degree	2,914	0.061	0.098	0.033	0.000
Sec./Interm. no training	2,914	0.257	0.289	0.232	0.001
TC/Abitur no training	2,914	0.035	0.024	0.044	0.003
Sec./Interm. with training	2,914	0.407	0.438	0.384	0.003
TC/Abitur with training	2,914	0.058	0.040	0.072	0.000
Foremen/BA	2,914	0.076	0.055	0.092	0.000
TC, University	2,914	0.105	0.056	0.143	0.000
Married	2,914	0.315	0.251	0.364	0.000
Female and married	2,914	0.122	0.084	0.151	0.000
Work attitude: missing	2,914	0.136	0.124	0.145	0.103
Work attitude: low	2,914	0.235	0.215	0.250	0.027
Work attitude: medium	2,914	0.394	0.423	0.371	0.004
Work attitude: high	2,914	0.236	0.238	0.235	0.841
Household information					
HH income less 1000	2,914	0.414	0.556	0.306	0.000
HH income 1000 - 1500	2,914	0.286	0.289	0.284	0.798
HH income 1500 - 2000	2,914	0.144	0.102	0.175	0.000
HH income 2000 - 3000	2,914	0.102	0.048	0.142	0.000
HH income 3000 - 4000	2,914	0.038	0.006	0.063	0.000
HH income 4000 - 5000	2,914	0.009	0.000	0.015	0.000
HH income more 5000	2,914	0.010	0.001	0.016	0.000
Means-tested HH	2,914	0.721	0.814	0.650	0.000
HH = 1	2,914	0.284	0.392	0.202	0.000
HH = 2	2,914	0.269	0.282	0.260	0.185
HH = 3	2,914	0.221	0.174	0.257	0.000
HH = 4-11	2,914	0.225	0.152	0.281	0.000
Home owner	2,914	0.133	0.069	0.181	0.000
Flat-sharing	2,914	0.071	0.079	0.066	0.192
Father's education					
Degree missing	2,914	0.263	0.300	0.234	0.000
No degree	2,914	0.060	0.086	0.041	0.000
School degree no training	2,914	0.080	0.092	0.070	0.031
School degree with training	2,914	0.432	0.411	0.448	0.043
Foremen/BA	2,914	0.083	0.062	0.099	0.000
TC, University	2,914	0.083	0.049	0.108	0.000
Labor market history					
Unemployment duration	2,315	67.206	76.124	60.352	0.000
Tenure	2,315	15.856	14.906	16.587	0.232
Daily wage	2,022	48.529	44.389	51.700	0.000
History missing	2,914	0.206	0.202	0.208	0.697

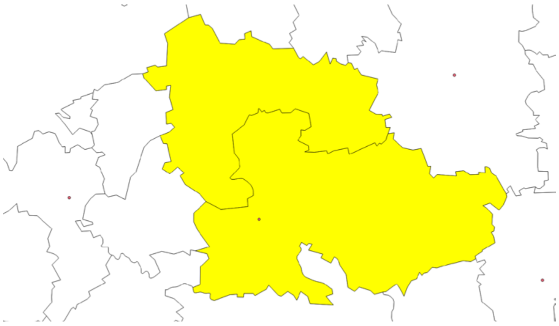
Notes: The number of observations refers to individuals observed during the first three waves (2006/07, 2008 and 2009).
Source: PASS-ADIAB 7515, *Breitbandatlas Deutschland*, Falck et al. (2014) and *Geodaten Deutschland*, own computations.



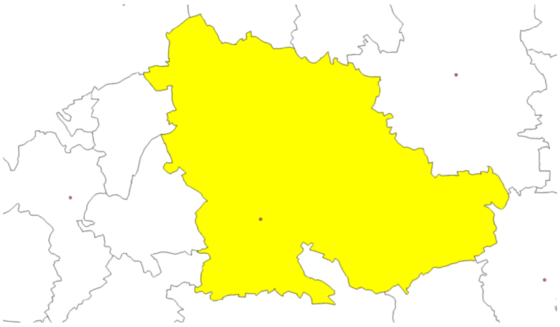
(1-A) Ingolstadt - municipality region



(1-B) Ingolstadt - postal codes



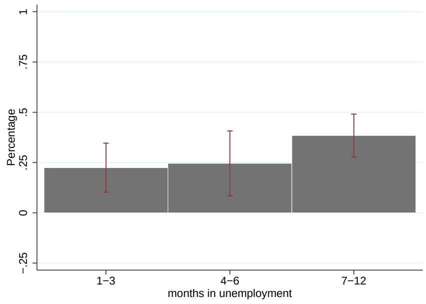
(2-A) Ingelfingen/Kränzelsau - municipality region



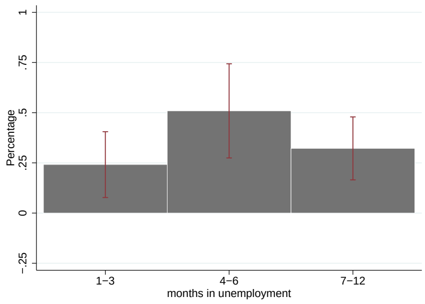
(2-B) Ingelfingen/Kränzelsau - postal codes

Notes: The figures present examples, where the smallest regional unit is either the postal code or the municipality. The combination of the municipality identifier and the postal code provides greater scope for variation in the treatment indicator that is needed for the IV regression. To illustrate this, the figure provides two examples where the municipality identifier is preferred over the postal code and vice versa. Panels (1-A) and (1-B) show the borders from Ingolstadt. Panel (1-A) depicts the municipality and (1-B) the postal code borders. The dots represent the main distributions frames. For the example of Ingolstadt, using the postal code would provide an advantage over using the municipality as the geographic centroid of the western postal code region is more than 4,200 meters away from the next MDF. The lower figures draw the borders of a less agglomerated region, where two municipalities share the same postal code. In this setting, the municipality ID would be preferred over the postal code.
Source: Geodaten Deutschland, own computations.

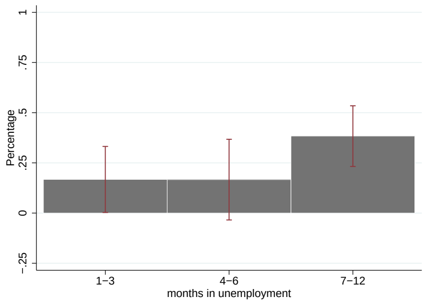
Figure 4.G.1: Exploiting municipality and postal code information for the instrument



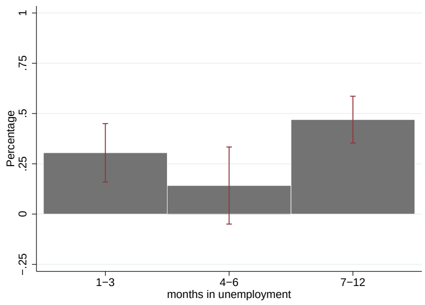
(1) Full sample



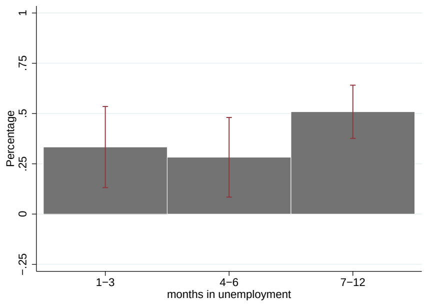
(2-A) Male



(2-B) Young



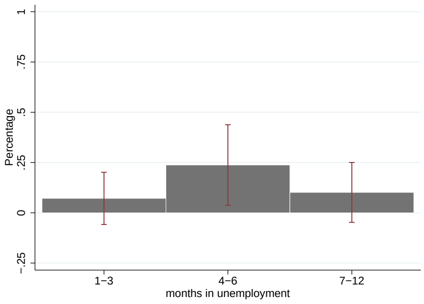
(2-C) Skilled



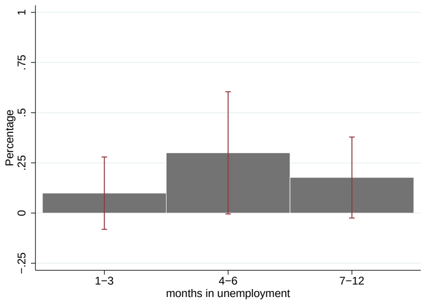
(2-D) White-collar jobs

Notes: The figures plot the difference in the share of individuals searching online for a job by home internet access dependent on the elapsed unemployment duration. Panel (1) shows the results for the full sample. Panel (2) shows the results by socio-economic characteristics. 90% confidence interval at the top of each bar.
Number of individuals: 1^{st} category (1-3 months): Full sample: 155, Male: 83, Young: 86, Skilled: 99, White-collar jobs: 58; 2^{nd} category (4-6 months): Full sample: 68, Male: 32, Young: 36, Skilled: 49, White-collar jobs: 49; 3^{rd} category (7-12 months): Full sample: 133, Male: 68, Young: 67, Skilled: 92, White-collar jobs: 82.
Source: PASS-ADIAB 7515, *Breitbandatlas Deutschland*, Falck et al. (2014) and *Geodaten Deutschland*, own computations.

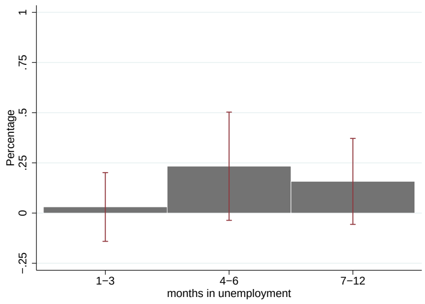
Figure 4.G.2: Difference in online job search by home internet access, three unemployment intervals



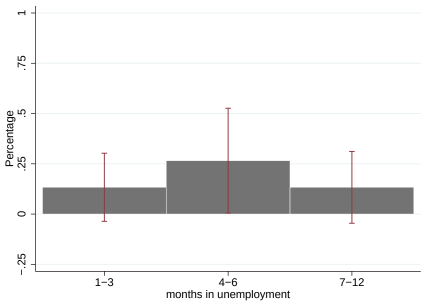
(1) Full sample



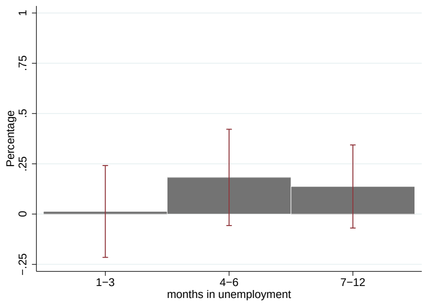
(2-A) Male



(2-B) Young



(2-C) Skilled



(2-D) White-collar jobs

Notes: The figures plot the difference in the share of individuals with job interviews by home internet access dependent on the elapsed unemployment duration. Panel (1) shows the results for the full sample. Panel (2) shows the results by socio-economic characteristics. 90% confidence interval at the top of each bar.
Number of individuals: 1^{st} category (1-3 months): Full sample: 155, Male: 83, Young: 86, Skilled: 99, White-collar jobs: 58; 2^{nd} category (4-6 months): Full sample: 68, Male: 32, Young: 36, Skilled: 49, White-collar jobs: 49; 3^{rd} category (7-12 months): Full sample: 133, Male: 68, Young: 67, Skilled: 92, White-collar jobs: 82.
Source: PASS-ADIAB 7515, *Breitbandatlas Deutschland*, Falck et al. (2014) and *Geodaten Deutschland*, own computations.

Figure 4.G.3: Difference in interview probability by home internet access, three unemployment intervals

4.H Search Externalities

Table 4.H.1: Descriptive statistics from the sample of employed individuals

	Pre-DSL years 1998/99 (1)	DSL years 2007/08 (2)
Panel A: Sample construction		
Sampling date	June 30, 1991	June 30, 2000
Number of individuals per municipality	72.757	89.676
	(80.129)	(93.422)
Panel B: Outcome variable		
Job-to-job transitions	0.325	0.222
	(0.098)	(0.077)
Panel C: Baseline characteristics		
Age	35.629	39.121
	(2.027)	(1.652)
Female share	0.329	0.419
	(0.092)	(0.080)
Low-skilled	0.131	0.104
	(0.072)	(0.054)
Medium-skilled	0.815	0.824
	(0.079)	(0.067)
High-skilled	0.055	0.073
	(0.049)	(0.054)
Foreign	0.033	0.031
	(0.051)	(0.045)
Occupation		
Agriculture	0.014	0.015
	(0.027)	(0.026)
Production	0.406	0.329
	(0.127)	(0.107)
Salary	0.121	0.136
	(0.068)	(0.063)
Sale	0.046	0.056
	(0.039)	(0.038)
Clerical	0.241	0.246
	(0.091)	(0.082)
Service	0.171	0.217
	(0.077)	(0.077)

Notes: The table reports basic municipality-level descriptive statistics for West Germany on the sample used to estimate the effects of broadband internet availability on job-to-job transitions. Job-to-job transitions are defined as employer changes allowing gaps of up to one month. The pre-DSL period covers the years 1998 and 1999. The DSL period covers the years 2007 and 2008. The numbers are averaged within the pre-DSL and the DSL years, respectively. The sample for the DSL period is based on employed individuals at the reference date 30th June 2000 who are still employed at the same employer at the start of the year 2007. The sample for the pre-DSL period is based on employed individuals at the reference date 30th June 1991 who are still employed at the same employer at the start of the year 1998. Individuals who experience a transition from employment to unemployment or non-employment in the pre-DSL and DSL period, respectively are excluded from the analysis.
Source: IEB, *Establishment History Panel* , MUP, *Breitbandatlas Deutschland* and Falck et al. (2014), own computations.

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ZEW ICT-Survey (2007). *Unternehmensbefragung zur Nutzung von Informations- und Kommunikationstechnologien*. Mannheim: Centre for European Economic Research (ZEW).

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EDUCATION

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REFEREED PUBLICATIONS

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