The economic and non-economic consequences of job loss, unemployment, and inadequate re-employment in Germany and Europe

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Overview article

The economic and non-economic consequences of job loss, unemployment, and inadequate re-employment in Germany and Europe

1. Motivation

Job loss and unemployment are major social problems in modern market economies (Ehlert, 2016).[1] Understanding their consequences is of key concern to the social sciences, because they imply significant economic and non-economic costs for the affected individuals, their families, and societies as a whole (Brand, 2015; von Wachter, 2010). As high rates of job loss and rising levels of unemployment are not only due to periodic economic recessions, but also arise from continuous economic adjustments, they will remain important issues in the future (Gangl, 2003). In fact, many scholars state that European labor markets have been confronted with several interrelated structural changes that have taken place over the last four decades. Globalization, international trade, and technological change are believed to have increased the risk of workers losing their jobs and reduced their chances for standard and adequate re-employment with significant negative effects on their careers and beyond (Mills et al., 2006).[2]

The experience of unemployment deprives workers of their main source of income which has a negative impact on their own financial well-being and that of the household. Large welfare transfers are necessary to protect families' standard of living in the short run (Ehlert, 2016). In addition to these direct economic consequences, job loss also affects workers' subsequent careers (von Wachter, 2010). For example, it has negative effects on long-term employment chances, earnings and wages, and many other facets of non-monetary job quality (e.g., Brand, 2006). Some studies even show that these effects are persistent and that workers never fully recover.

Job loss is also detrimental to health and well-being, as it not only implies material deprivation, but also the loss of a major social role and identity (Jahoda, 1982; Nordenmark and Strandh, 1999). Other non-economic consequences are persons' lower social participation, trust, and political engagement (e.g., Brand and Burgard, 2008). Moreover, these negative effects are not limited to isolated individuals, but also spill over on economically dependent and emotionally close family members (Brand, 2015; Ström, 2003). For example, some stud-

[1] Throughout this thesis I refer to "job loss and unemployment" to highlight my interest in job loss or in unemployment that follows after job separation. The use of "job loss and unemployment" does not imply that I am only interested in their joint occurrence. While in some articles I examine the consequences of job loss and distinguish different reasons (e.g., plant closures, layoffs, dismissals), in others I investigate the effects of unemployment that follows after job separation. Subsection 4.1.2 discusses the definitions of job separation, job loss, job displacement, and unemployment and explains their implementation in the articles of this thesis.

[2] Mills et al. (2006) define globalization as four interrelated structural changes: the internalization of markets, the intensification of competition, the spread of global networks and knowledge via new ICTs, and the rising importance of markets and their dependence on random shocks. Other authors have separately considered the roles of technological change and international trade (e.g., Acemoglu and Autor, 2011; Oesch, 2013).

ies show that the negative health and well-being consequences are shared by partners and children (e.g., Baranowska-Rataj and Strandh, 2017; Ström, 2003) and others find increased risks of divorce (e.g., Charles and Stephens, 2004). The role of families is further exemplified by research showing that spouses change their daily routines in the market and domestic spheres in reaction to partners' unemployment (e.g., Gough and Killewald, 2011). Therefore, comprehensive assessments of the costs of job loss and unemployment not only require investigating the economic and non-economic consequences for extended periods of time, but also taking account of affected workers' social environment.

The described individual- and family-level effects likely add up to negative impacts on society (Ehlert, 2016). The fact that persons who are able, ready, and willing to work are excluded from the system of economic production and exchange can be regarded as inefficient. The loss of produced goods and services leads to lower levels of income which negatively affect consumption and economic demand (Gangl, 2003). Job loss is also seen critically, because it devalues investments in firm-, occupation-, or industry-specific skills, implying that countries forfeit economic capabilities, which are important drivers of growth. In addition to the macroeconomic consequences, findings that unemployment reduces families' health and well-being imply significant public health costs. Moreover, the results on family disruption and reduced social participation, trust, and political engagement suggest that job loss can contribute to the undermining of social cohesion with potentially severe consequences for democratic governance (Gangl and Giustozzi, 2018).

High levels of unemployment also raise issues of inequity, because the risk and consequences of job loss are socially stratified (Gangl, 2003). Specifically, if job loss and unemployment have persistent negative effects and persons of lower socio-economic status are more often exposed or more vulnerable to these events, an accumulation of advantages and disadvantages across different life domains and over time results (DiPrete and Eirich, 2006).

Given that the consequences of job loss are unequally distributed, welfare states assume a central role (DiPrete, 2002; Ehlert, 2016; Gangl, 2006). They not only buffer the direct economic effects through unemployment insurance (UI) and benefits (UBs), but also moderate the long-term effects through a range of passive (PLMPs) and active labor market policies (ALMPs) as well as employment protection legislation (EPL). Depending on the design of such labor market policies, welfare states can partly determine the inequality-enhancing potential of unemployment.

Historically, research on the consequences of job loss has been motivated by major economic crises. The seminal study by Jahoda et al. (2002 [1933]) was the first sociological inquiry showing how massive job destruction in the realm of the 1930s "Great Depression" affected the lives of a whole community. Similarly, the worldwide financial crisis in 2007/2008 and the subsequent "Great Recession" resulted in sharp increases in unemployment that stipulated new research on its effects for a wide range of outcomes (e.g., Baumann, 2016; Gangl and Giustozzi, 2018).

However, scholars have also stated that job loss and unemployment are to some extent "normal" (Ehlert, 2016). Indeed, it is often argued that the creation and destruction of jobs in reaction to structural changes represents a key source of productivity growth (Caballero, 2010). At the same time the reallocation of a large share of employment implies that many workers lose their jobs. If their skills are relatively fixed and not in line with the requirements of the expanding sectors, they are likely confronted with difficult transitions, including extended job searches, inadequate re-employment, and increased risks to lose their jobs again.

De-industrialization is one example of such a profound transformation of the occupational structure which shifted employment out of the manufacturing and into the service sector (Oesch, 2013). Its key driver was (skill-biased) technological change and, especially, advancements in the ability to automate certain tasks. While these changes not necessarily led to higher unemployment, they contributed to a polarization of employment and workers who lost jobs in production faced difficulties in finding adequate re-employment.[3] Ongoing economic adjustments are also reflected in recent debates about the digitalization of the economy (e.g., OECD, 2017). However, the increasing use and diffusion of artificially intelligent robots has aroused fears that more jobs will be destroyed than created leading to technological unemployment (e.g., Frey and Osborne, 2017). Arntz et al. (2016) suggests that these fears may be overstated, because studies reaching such conclusions assume that whole occupations rather than specific tasks can be replaced.

In addition to technological advancement, globalization and international trade are also thought to have contributed to an increased risk of job loss, for example, through the out-

[3] Oesch (2013) summarizes two common scenarios. The skill-biased technological change (SBTC) hypothesis suggests that technology complements high-skilled and substitutes low-skilled labor. A refinement of this is the routinization hypothesis (Autor et al., 2003; Goos and Manning, 2007) stating that it is the easy-to-codify routine tasks of middle-range occupations (e.g., production jobs, administrative or clerical jobs) that are most likely replaced, resulting in a polarization of low-skilled interpersonal service jobs and high-skilled occupations.

sourcing and offshoring of production to countries with lower labor costs. Overall, many scholars have argued that these interrelated structural changes have raised job insecurity for workers in modern market economies (Mills et al., 2006).

Another source of higher employment instability is the related growth of non-standard employment (Hipp et al., 2015).[4] Whereas in the post-war period, European economies were characterized by stable growth and low unemployment and standard employment was the norm, in the beginning of the 1970s several supply side shocks and the described structural changes put European labor markets under severe pressure (Gebel, 2010). While the same challenges affected the United States (US), economists often argue that Europe's rigid labor market institutions prevented adjustments via wage flexibility which led to rising levels of unemployment (Nickell, 1997).[5] Given these trends and the growing flexibility demands of employers, many governments have reformed key labor market and welfare state institutions hoping to ease the (re-)integration of labor market outsiders and, especially, the unemployed (DiPrete et al., 2006; Hipp et al., 2015).

One pan-European reaction has been the promotion of non-standard work and, in particular, the deregulation of employment protection, which led to a strong growth of temporary employment (Gebel, 2010). However, European countries differed in their implementation as some reduced employment protection for all workers, implying greater overall risks of job loss, while others focused on labor market outsiders and primarily increased employers' opportunities to use temporary employment. This resulted in a partial and targeted deregulation (Esping-Andersen and Regini, 2000) or a flexibilization at the margins (Barbieri, 2009). Temporary jobs often go along with greater risks of job loss and unemployment and, in general, it is assumed that non-standard employment overall provides a lower job quality than standard employment (OECD, 2002, 2010; OECD/European Union, 2017).

In addition to promoting alternative working arrangements, governments have also reformed the welfare state. They have reduced out-of-work benefits, leading to a re-commodification of

[4] Kalleberg et al. (2000: 258) define standard employment as "the exchange of a worker's labor for monetary compensation from an employer … with work done on a fixed schedule – usually full time – at the employer's place of business, under the employer's control, and with the mutual expectation of continued employment." Therefore, non-standard employment includes temporary, part-time, and self-employment (Hipp et al., 2015).

[5] More detailed historic accounts are provided by Gangl (2003) and Gebel (2010). The institutional rigidity refers to high out-of-work benefits, high labor costs, strong unions and high collective bargaining, and low employment flexibility. DiPrete et al. (2006) question the idea that high unemployment was the only way for European countries to cope with economic pressure. Specifically, they point to increasing inequalities in job security.

workers, and placed a stronger emphasis on active labor market policies.[6] While the latter provide services that aim at increasing the employability of the unemployed such as education and training programs or create private and public job opportunities, they also involve enforcement to work, the conditionality of rights, and increasing obligations (Dingeldey et al., 2007; Kluve et al., 2007). Overall, these changes are thought to have reduced unemployed persons' opportunities to search for jobs that match their skills and qualifications increasing the risk of inadequate re-entries (Pollmann-Schult and Büchel, 2005). However, similar to the deregulation of employment protection, European countries differed in the type and extent of such reforms.

Together the described developments imply increased risks of job loss and unemployment due to ongoing structural changes. At the same time workers who lost their jobs face greater pressures to accept non-standard or inadequate re-employment, because employment protection has been deregulated, out-of-work benefits have been reduced, and activation polices have been extended. The question remains whether quick re-employment is sustainable or puts workers at long-term disadvantages. As European economies differed in their reactions to the economic pressures and also have varying institutional set-ups today, the effects of job loss and unemployment and workers' ability to locate adequate re-employment are likely to vary across countries and over time.

Overall, the highlighted trends motivate the three *general research questions* of this thesis: First, what are the short- and long-term consequences of job loss and unemployment for individuals' careers and their own and families' lives? Second, how do individuals and families react to the events? While the reaction is part of the consequences, I here distinguish it as a separate research question, as some of the articles in this thesis put human's agency at the center of investigation. Third, how do the effects of the events of interest vary across different contexts, including countries and time, which differ in their economic situation and labor market policies?

To address these general research questions this *cumulative thesis* raises and answers different and more *specific research questions* in five articles. These specific research question are not only motivated from the trends of rising job insecurity, but further take account of some of the insights of previous studies. First, job loss and unemployment have economic- and non-

[6] Re-commodification is the opposite of Esping-Andersen's (1990) de-commodification which describes a principle by which welfare states make individuals' life chances less dependent on market forces.

economic consequences. Second, the effects may be lasting. Third, they may also spill over to other family members. Fourth, the events may contribute to growing inequalities between individuals over time which, however, in part depends on countries' institutional set-ups.

To further organize these insights section 1.1 introduces the key principles of the life course perspective. The latter is a guiding framework for studying the effects of life events. Based on this background, section 1.2 will present the specific research questions for Articles 1 to 3 of this thesis which all focus on the economic consequences of job loss, unemployment, and inadequate re-employment. Subsequently, in section 1.3 I will describe the specific research questions for Articles 4 and 5 which both focus on the non-economic consequences of the events of interest.

1.1 A life course perspective on research on job loss and unemployment

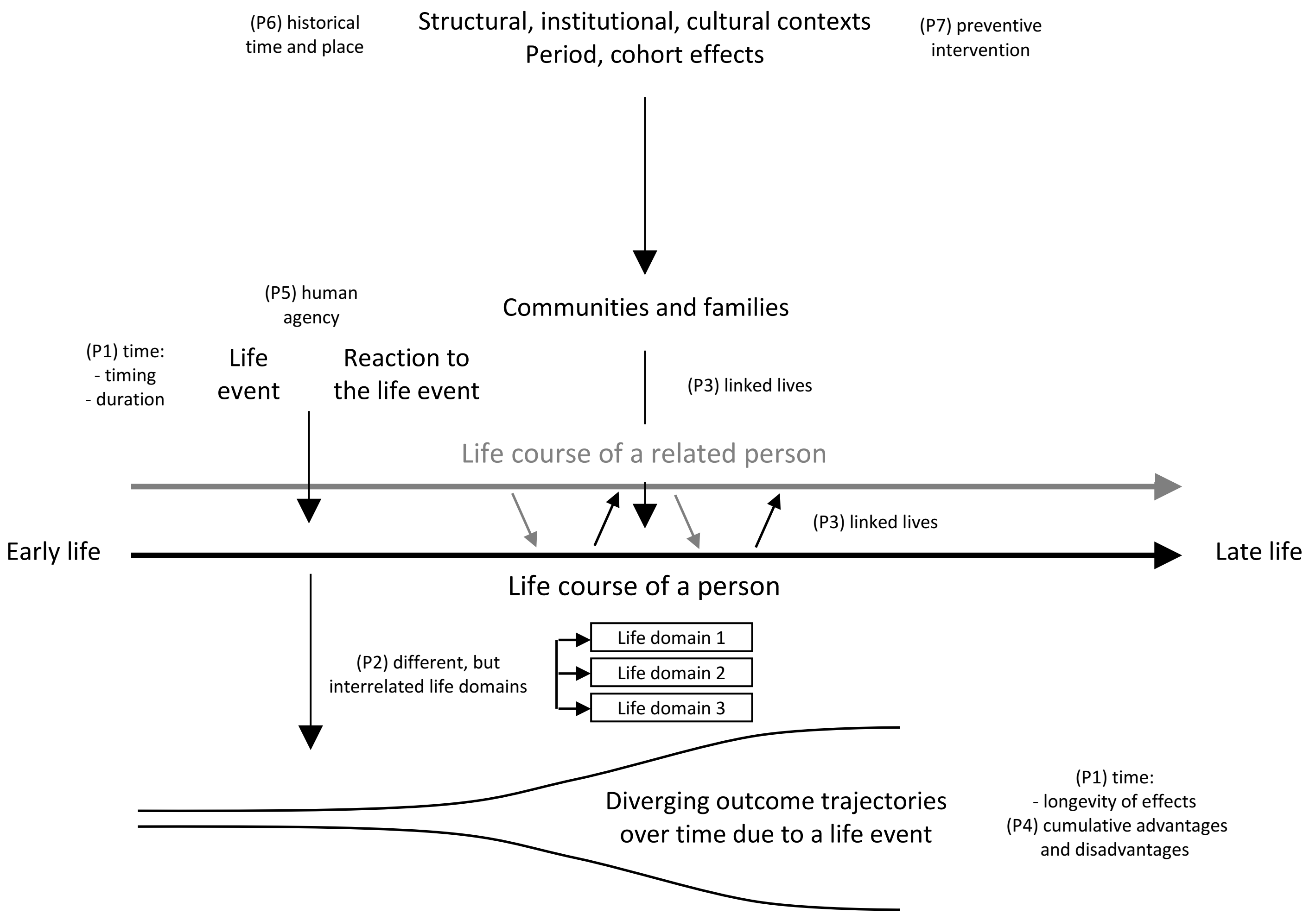
This thesis has scientific roots in different disciplines in the social sciences. Labor economists were the first and remain the most frequent researchers of the economic effects of job loss and unemployment. Sociologists and, especially, stratification researchers have conducted comparative studies emphasizing the importance of the structural and institutional context in which unemployment occurs. They also have stressed that the risk and consequences of job loss are unequally distributed. Social epidemiologists, psychologists, and sociologists provided the first enquiries of the health and well-being effects of unemployment, broadening the previously narrow focus on employment and income. Demographers and family researchers were the first to notice that job loss has also an impact on the lives of other family members and that the latter participate in unemployed persons' reactions to job loss. While over the last decades disciplinary boundaries have started to blur, research on the economic and non-economic consequences of job loss and unemployment remains fragmented. Therefore, a central goal of this thesis is to provide a comprehensive and interdisciplinary analysis that uses the life course perspective as a guiding framework.

The life course perspective represents a consensus across many social sciences on how to study the lives of individuals using longitudinal data and methods. For example, previous stratification research has used some of its insights for cross-national comparative analyses. Mills et al. (2006) examined how individuals are affected by structural changes such a globalization. These changes are, however, channeled through institutional filters such as welfare regimes, education systems, or employment relation systems. In a similar way, DiPrete (2002) incorporated the life course perspective in his trigger event framework, which has recently been refined by Ehlert (2016). This framework is concerned with explaining cross-national differences in the income mobility of households as triggered by adverse life events (DiPrete, 2002; DiPrete & McManus, 2000). However, as previous studies only partly made use of the life course perspective or only focused on households' financial well-being (Ehlert, 2016), in this thesis, I rely on reviews of leading life course researchers to derive seven key principles that can be applied to the study of life events (Elder and Giele, 2009; Mayer, 2009). The relation of these key principles to the trigger event framework are nevertheless highlighted below.

Figure 1 illustrates the seven key principles of the life course perspective for research on the effects of life events. These have been derived by integrating the four principles highlighted by Elder and Giele (2009) and the six-point summary by Mayer (2009). I also blended in some additional aspects coming from foundational research on the non-economic consequences of life events (e.g., Pearlin et al., 1981). In the following, I present the key principles in an order that reflects the focus of this thesis: the effects of life events on different, but interrelated life domains at the individual-level. The key principles are also numbered from P1 to P7 as they are referred to throughout this overview article and Figure 1 can always be used as a reference.

Principle 1 (P1) highlights the importance of *time*. In addition to the externally assigned or personally chosen *timing* of life events, the *duration* of specific states and the *longevity of effects* should be examined. The latter also involves investigating the dynamics in outcomes over time. Specifically, researchers have to consider whether the effects of life events are temporary blemishes or persistent scars (Ruhm, 1991). As shown in Figure 1, life courses are further comprised of *different, but interrelated life domains*. Specifically, *principle 2 (P2)* suggests that research on life events should be interdisciplinary and study how different life domains affect each other. Moreover, multiple outcomes within each life domain should be considered to take account of their multidimensionality and highlight potential complementarities and trade-offs. *Principle 3 (P3)* refers to *linked lives*. This means that related life courses affect each other and that persons are placed in collectives such as communities and families. It also implies that the effects of life events are not limited to isolated individuals, but spill over to economically and emotionally dependent persons. It further suggests that the latter take part in the reaction to life events.

Figure 1 Key principles of the life course perspective for research on the effects of life events



Sources: Own illustration based on Elder and Giele (2009) and Mayer (2009).

Principle 4 (P4) about *cumulative advantages and disadvantages* emphasizes that initial (dis-)advantages lead to further relative (dis-)advantages. As Figure 1 shows life events are one mechanism through which outcome trajectories of advantaged and disadvantaged groups diverge over time. Specifically, if a life event has persistent negative effects *and* disadvantaged groups are more likely exposed (differential exposure) or more vulnerable to it (differential vulnerability), outcome trajectories diverge. In the trigger event framework one explanation of persistent effects are *mobility-reinforcing events* meaning that an adverse life event in one life domain results in adverse life events in the same or other life domains. To examine the inequality-enhancing potential of life events, research should, thus, consider the longevity of effects (P1: time), the selection into life events, and effect heterogeneity.

Principle 5 (P5) points to the importance of *human agency* as individuals construct their life courses through choices and actions within opportunities and constraints. This suggests that persons have different degrees of control over and show different reactions to life events. In the trigger event framework this principle is reflected in the concept of *counter-mobility events* which allow persons to overcome the negative consequences. Human agency (P5) further refers to the idea that individuals attach different interpretations to different life events affecting their actions and choices. Figure 1 further illustrates that the opportunities and constraints are determined by contexts of communities and families (P3: linked lives) and *historical time and place* (*principle 6 (P6)*). The latter concerns period and cohort effects as well as structural, institutional, and cultural contexts that are defined by region (e.g., countries) or time (e.g., years). The trigger event framework highlights that in studies on life events contexts take a key role, because countries' institutional set-ups, which in their entirety form *mobility-regimes*, affect the *rate* and the *consequences* of life events. The consequences of life events are in part directly affected by institutions, for example, insurances buffer the negative effects of life course risks, but also indirectly by affecting the rates of mobility-reinforcing and counter-mobility events. Therefore, the principle of historical time and place (P6) is closely related to *principle 7 (P7)* stating that life course research evaluates and informs social policies aiming at *preventive intervention*. In applied research this is mostly achieved by analyses about how policies that vary across regions and over time moderate the effects of life events on different, but interrelated life domains (P2) and multiple outcomes within these.

How do the seven key principles motivate the research presented in the five articles? Sections 1.2 and 1.3 explain how they figure into the specific research questions about the economic and non-economic consequences of job loss, unemployment, and inadequate re-employment.

1.2 The economic consequences from a life course perspective

Articles 1 to 3 examine the economic consequences of job loss and unemployment and consider how persons' reactions to unemployment affect their further careers. The articles share some commonalities in using the key principles of the life course perspective. Specifically, they all consider multiple outcomes within the career domain taking account of its multidimensionality and pointing to complementarities and trade-offs (P2: different, but interrelated domains). They, thereby, also factor in research showing that careers and, especially, job quality cannot be measured by employment and income alone. This has implications for policy-makers as they have to consider complementarities and trade-offs between multiple outcomes (e.g., employment vs. job quality) in thinking about the design of preventive policies (P7). Moreover, all articles take a medium- to long-term perspective, because the inequality-enhancing potentials of life events (P4: cumulative advantages and disadvantages) are only present if their effects are persistent (P1: time).[7] Next to these similarities, each article considers additional (aspects of the) key principles.

In Article 1 *"Losing standard employment in Germany: The consequences of displacement and dismissal for workers' subsequent careers"* I ask: *What are the short- and long-term consequences of job loss for workers' subsequent careers in Germany?* Specifically, I examine workers' labor market statuses, labor incomes, and risks for non-standard employment for up to five years after job loss. I also focus on job loss instead of all job separations and distinguish between workers who have been displaced due to plant closure and those who have lost their job due to dismissal. This is motivated by the fact that the consequences of life events vary by "their desirability, by the degree of control people have over their occurrence, or by whether or not they are scheduled" (Pearlin et al., 1981: 339; P5: human agency). Whereas job loss is involuntary and, thus, likely has severe negative effects, voluntary job separation often reflects upward mobility or workers leaving unsatisfying situations (Wheaton, 1990). Moreover, the effects of displacements due to plant closures and dismissals may also differ as economic theory suggests that future employers attached different interpretations to the different reasons for job loss when considering to hire job applicants (P5: human agency).

[7] The definition of long-term differs from one research area to another. For the economic consequences a long-term perspective is often assumed for follow-ups of five years. For studies on the health and well-being consequences a long-term perspective only refers to research spanning different life stages (e.g., early to late life).

Article 2 *"The effects of unemployment on non-monetary job quality in Europe: The moderating role of economic situation and labor market policies"* takes up the idea that life events can have different medium-term consequences, depending on the structural and institutional contexts in which they occur (P6: historical time and place). Specifically, I address two research questions: *First, what are the effects of unemployment on four different facets of non-monetary job quality? Second, to what extent do countries' economic situation and labor market policies moderate these effects?* By taking a comparative perspective and examining economic situation and different labor market policies, this article also assesses the potential for preventive intervention (P7).

Article 3 *"Better overeducated than unemployed? The short- and long-term effects of an overeducated labour market re-entry"* changes perspectives and focuses on unemployed persons' decisions on when to reject or accept different kinds of job offers. This highlights the importance of human agency (P5) and the use of counter-mobility events to overcome negative effects. Specifically my co-author and I raise the following research question: *What are the effects of an overeducated labor market re-entry as compared to remaining unemployed and continuing the job search for adequate employment on short- and long-term employment chances and job quality?*

While Articles 1 to 3 already take account of five of the seven key principles of the life course perspective, two additional aspects have not yet been considered. First, even if careers are measured in more detail than usually, they only represent a single life domain (P2: different, but interrelated life domains). Second, the life course perspective clearly states that effects spill over on family members and that families react to life events collectively (P3: linked lives). This will be addressed in the next section presenting the specific research questions of Articles 4 and 5.

1.3 The non-economic consequences from a life course perspective

The life course perspective entertains the idea that early life events in one life domain affect later outcomes in others (P2: different, but interrelated life domains). Therefore, in Article 4 of this thesis my co-authors and I ask: *What are the long-term effects of an early-career job loss on persons' late life health in Europe? To what extent do subsequent unemployment risks and employment instability mediate the potential negative effects?* Next to focusing on job loss and distinguishing between displacements due to plant closures and layoffs (P5: human agency), we also take account of the timing of events, as it is often argued that life events in sensitive periods, such as the early-career, have particularly negative effects (P1: time). Moreover, this focus allows taking a very long-time span examining persons' health more than 30 years after job loss assessing the longevity of effects (P1: time). We are further interested in processes of cumulative advantages and disadvantages (P4) and examine to what extent the potential negative health effects can be attributed to mobility-reinforcing events as reflected in higher unemployment risks and employment instability throughout workers' subsequent careers.

Article 5 emphasizes the interrelation between the career and family domains by examining how job loss and unemployment alter families' daily routines and especially their time spent on domestic tasks (P2: different, but interrelated life domains). Specifically, my co-author and I ask: *What are the effects of unemployment on couples' reallocation of housework and total household production and how do they vary by the gender of the unemployed spouse? How do the effects vary by the specific tasks considered? How do the effects change with the duration of unemployment or non-employment?* Therefore, this article not only considers an outcome that has been ignored in most previous studies, but also illustrates the idea of linked lives (P3). Moreover, it pays attention to the question of how families' reactions change the longer the unemployed spouse remains unemployed or non-employed (P1: time).

While sections 1.1 to 1.3 highlight how this thesis draws on the key principles of the life course perspective to motivate specific research questions, the articles are also located within different areas of research that already have offered relevant empirical evidence. Therefore, the next chapter provides a detailed literature review pointing out what is known and what not to further explain the contribution of each article.

2. The state of research and the contributions of this thesis

In this chapter I review the state of research separately for the economic and non-economic consequences of job loss, unemployment, and inadequate re-employment. Section 2.1 reviews research on the effects of job loss and unemployment on workers' subsequent careers and section 2.2 summarizes studies that examine how workers' decisions to take up non-standard or inadequate re-employment compared to remaining unemployed and continuing the job search affects their careers. In section 2.3 I review research that examines the health and well-being effects and in section 2.4 studies investigating how job loss and unemployment affect couples' division of housework and total household production are summarized.

The aim of this literature review is to provide a detailed background about the areas of research each article is located in and to highlight the limitations of previous studies and the contributions of the five articles of this thesis. For this purpose, each section and subsection follows one of two types of reviews. If the literature is well developed, I start by highlighting the *(1) central findings* which apply more or less universally across studies. I then discuss explanations for heterogeneity across studies. These may include *(2) treatment heterogeneity*, that is, differences due to the definitions of the independent variables, *(3) effect heterogeneity*, referring to *(3a) worker*- or *(3b) context-level factors* that moderate the effect of the independent variable, and in some cases *(4) methodological differences* that are unique to the respective area of research. If the literature is less developed, I only summarize the key findings or separately report the empirical evidence from the studies available. Irrespective of the type of review, I end each section or subsection by highlighting the *limitations* of previous studies and stating the *contributions* of the articles. Section 2.5 explains the structure of this thesis and also describes its overall contributions.

2.1 The effects of job loss and unemployment on the subsequent career

A large literature has examined how job loss and unemployment affect workers' subsequent careers. Research on the economic consequences took off in the 1980s (see Hammermesh, 1989 for a review) with an increasing number of studies having been conducted ever since. From more recent reviews (Brand, 2015; Fallick, 1996; OECD, 2013; von Wachter, 2010), a number of important points stand out that affect the structure and scope of the literature review for section 2.1.

In line with a growing literature highlighting the multidimensionality of careers (Gallie, 2007; Green, 2006; Kalleberg, 2007; Muñoz de Bustillo et al., 2011), the previous reviews emphasize that the economic consequences of job loss and unemployment cannot be assessed by only focusing on employment and income. Therefore, my review is separated into three subsections each focusing on one of the following outcome groups: labor market status (subsection 2.1.2), labor income (subsection 2.1.3), and other job characteristics (subsection 2.1.4). The last group comprises research on the type of re-employment (e.g., non-standard or inadequate re-employment) and studies examining different facets of non-monetary job quality.

The previous reviews further highlight the need to distinguish job loss from unemployment and to consider different reasons for job loss. With respect to the economic consequences, I focus on studies that investigate displacements due to plant closures or layoffs and job losses due to dismissals, because these events have been the priority of this area of research and are also closely related to the articles of this thesis. However, if specific aspects of the literature are less developed (e.g., comparative studies), I also incorporate studies that have investigated transitions from employment to unemployment or focused on unemployment in general.

Another point emphasized in previous reviews is the large variation in findings across studies. This can be either attributed to methodological differences or is explained theoretically. While the review in subsections 2.1.2 to 2.1.4 focuses on theoretical reasons, methodological differences should be kept in mind when considering the reasons for variation. Because there are some *common methodological differences* across the studies of the three outcome groups, these are reviewed next (subsection 2.1.1) such that they do not have to be repeated throughout. This also provides insights about the methodologies used in research on the economic consequences and highlights that, in spite of some apparent issues, most studies have relatively sophisticated research designs.

2.1.1 Common methodological differences

There are some common methodological differences across studies on the economic consequences which may explain the large variation in findings. These include: the types of data, samples (and their restrictions), and methods used and the use (or not) of a control group and the definitions of job loss and the control group (Kuhn, 2002; von Wachter, 2010). I here reflect on how these may affect the findings, because the remaining review only comments on methodological differences that are unique to each outcome group. Arguments for the methodological choices of the five articles of this thesis are discussed in chapter 4.

While most studies are based on longitudinal data, differences may arise from using administrative or survey data and for the latter whether studies rely on prospectively (panel) or retrospectively collected (life history) data. In administrative data, coming from tax or social security records and holding information on both employers and employees, displacements due to plant closures or mass layoffs are the focus and the following definitions are independent of any subsequent unemployment. Plant closures are identified from vanishing identifiers, necessitating many ad-hoc adjustments to separate establishment or firm "deaths" from other processes. Mass layoffs are arbitrarily defined as a specific share of employees (usually 30 to 80 percent) leaving establishments or firms of predefined sizes (usually at least 50 employees) in a certain time period (usually one year). These definitions have the (dis-)advantage of focusing on all workers who leave in a specific time period meaning that they include some "normal turnover", but also "early-leavers". The latter are not identified in survey data and some studies find that they are positively selected such that their exclusion leads to an overestimation of the costs of job loss (Schwerdt, 2011). However, adding "normal turnover" also leads to bias, as including workers who leave the labor force voluntarily likely overestimates the negative effects on employment and underestimates the negative effects on labor incomes due to capturing upward mobility. As these indirect measurements of displacements often require focusing on large establishments, especially for mass-layoffs, they miss the large number of job losses in small firms. Moreover, administrative samples are often large and besides examining effect heterogeneity, many studies have used this to focus on homogenous subgroups of workers (e.g., prime-age males with stable pre-job loss employment and high tenure) which may be affected more negatively (von Wachter, 2010).[8]

In survey data displacements due to plant closures and layoffs and dismissals are self-reported such that all kinds of job losses are represented. However, here it is often difficult to distinguish whether dismissals are due to layoffs or workers are fired for individual reasons. Studies using survey data also vary in whether to include ambiguous reasons for job separation (e.g., mutual agreements, completion of temporary employment) and many do not differentiate or examine different reasons for job separation at all. The terminology (e.g., dismissed, fired, laid off, made redundant) and definitions of different reasons further vary across countries making cross-national comparisons more complicated.

For retrospectively (life history) in contrast to prospectively collected (panel) data additional restrictions with respect to methodological decisions exist. Specifically, these data often do not include information on the outcomes before the event of interest or even on the exact timing of the event, making it impossible to apply longitudinal methods to control for unobserved characteristics of workers and more difficult to use and clearly define a control group. Not controlling adequately for worker heterogeneity likely results in an overestimation of negative effects. Not using a control group probably overestimates the negative employment effects, but underestimates income losses, as positive income trends can be expected in the absence of job loss. Definitions of the control group vary, too. If one compares affected and unaffected workers in a specific time period this allows that the latter may experience changes in em-

[8] Studies using administrative data are often only representative for a specific region (e.g., a state in a country). The attentive reader notices that they, in general, trade off external validity for internal validity. While homogenous samples of workers, clear definitions of job loss, and the ability to observe workers for many years before job loss improve causal inferences, these inferences are often restricted in scope and not easily extrapolated.

ployment and income in the future. However, many studies have required the control group to be stably employed throughout the whole observation window overestimating the stability during the absence of job loss (Krolikowski, 2017). Such a static definition of the control group also precludes research on employment as an outcome and has ambiguous effects for income analyses as greater stability excludes downward but also some forms of upward mobility. In other areas of research this problem has been referred to as conditioning on future outcomes which should be avoided in studies aiming at evaluation (e.g., Sianesi, 2004).

With respect to the methods and, especially, the question of causal inference, research on the economic consequences is nevertheless relatively sophisticated, in particular, when compared to studies on the non-economic consequences (see sections 2.3 and 2.4). For example, most studies focus on concrete events that, in part, can be considered exogenous (e.g., plant closure, (mass-)layoffs). They also strongly rely on longitudinal data and apply methods that control for time-constant unobserved heterogeneity. Moreover, many researchers are aware of the fact that one should only control for variables that affect the risk of job loss and the outcomes and, thus, only condition on pre-treatment variables. Here administrative data offer an advantage as they allow focusing on very homogenous subgroups (which is one way of conditioning). In contrast to survey data, which provides detailed information on workers' pre-treatment situation, they are, however, often restricted to demographic information and pre-treatment outcomes in their sets of control variables. More details on these issues are discussed in chapter 4 which explains the approaches used in the five articles of this thesis.

Administrative data also differ from survey data in the outcomes available. They often only have information on (specific forms of) dependent employment and registered unemployment and the respective incomes. However, they hold no information on working hours and workers' situation if they do not fall into the labor market statuses recorded. This likely overestimates the negative employment effects as specific forms of re-employment are missed. For labor incomes this also means that administrative data do not take account of all income sources and that they cannot be used to calculate hourly wages.

While it is often possible to formulate clear expectations about the impact of these methodological differences and the literature is mostly aware of these issues (e.g., Kuhn, 2002; von Wachter, 2010), relatively little systematic knowledge has been gained, for example, by varying these decisions in single studies (see Krolikowsi, 2017 for a recent exception) or by conducting meta-analyses on the impacts of each of these aspects.

2.1.2 The effects on labor market status

In this and the next two subsections, I follow the first type of review described at the beginning of chapter 2. The *(1) central findings* are that displacements and dismissals decrease workers' subsequent employment chances and increase their future unemployment risks (Brand, 2015; Fallick, 1996; von Wachter, 2010). The effects are largest at the time of job loss, but become smaller over time. However, no consensus has been reached on whether any effects remain or how long it takes before they fade out. Some studies find that displaced or dismissed workers are able to catch up by around five years after the event (e.g., Ruhm, 1991; Upward and Wright, 2017), but others suggest that much longer time periods are required (e.g., Schmieder et al., 2010). However, the literature has agreed that the durations of joblessness vary greatly across workers with some being able to avoid unemployment entirely and others staying out for very long (Brand, 2015; Fallick, 1996). The latter have sometimes been labeled as "structurally unemployed" (Brand, 2006: 277). The variation across workers also translates into differences between studies, especially, for the question how lasting the negative effects are. These can be explained by several factors including common methodological differences (see subsection 2.1.1), but in the following I focus on *(2) treatment heterogeneity* and *effect heterogeneity* at the *(3a) worker-* and *(3b) context-level*. Moreover, I consider *(4) methodologically differences* that are unique to the measurement of non-employment or unemployment. While the central findings reported up to here are similar for the US and Europe, it must be noted that the large majority of studies are still based on US data. In the remainder of this and the following two subsections, country differences are explicitly discussed in reviewing effect heterogeneity at the *(3b) context-level*.[9]

With respect to *(2) treatment heterogeneity*, Gibbons and Katz (1991) show that layoffs are associated with longer unemployment durations than displacements due to plant closure. They interpret this finding using an adverse signaling model stating that markets cannot infer a negative signal from plant closures as their causes are external to the employees. However, layoffs signal workers' lower ability or productivity to future employers, because there is some discretion in decisions on who is let go. This finding has been questioned by studies on labor incomes (see subsection 2.1.3), but research looking at employment is rather scarce.

[9] The results about treatment heterogeneity and effect heterogeneity at the worker-level are themselves likely to vary across countries and over time. A review of this additional variation is beyond the scope of chapter 2.

Another reason for variation is *effect heterogeneity*. Some general findings at the *(3a)* w*orker-level* have been documented (Brand, 2015; Fallick, 1996; Kletzer, 1998; Kuhn, 2002; OECD, 2013), in particular, in a series of papers by Farber and colleagues (e.g., Farber, 2005, 2017; Farber et al., 1993, 1997) based on several rounds of the Displaced Worker Survey (DWS) for the US.[10] Therefore, the following results are relatively uncontroversial: non-employment lasts longer for women and non-whites. For women this is often explained by the availability of alternative roles, but also by their lower geographical mobility due to the fact that they often depend on their partners' career choices. With respect to age re-employment is lowest among the oldest age group (about 55 years and older), with no clear patterns for differences between young and prime-age workers. The former finding is likely due to a combination of older workers facing greater difficulties to find re-employment and greater possibilities to exit the labor force. The most unambiguous finding concerns the skill divide in workers' subsequent employment patterns. Higher educated and more skilled workers are more likely to find re-employment and to stay employed.

Results for other pre-job loss employment characteristics are more mixed. Some studies find that workers with very low and high tenure are less likely to be re-employed with the former suspected to have less stable overall careers and the latter having to bridge larger quality gaps between the lost jobs and the available job offers (Fallick, 1996; Farber and colleagues). However, Kuhn (2002) points out that the latter finding may be specific to countries with low employment protection, while in those with high job security high-tenured workers have fewer problems in finding a job. Other findings are that workers with higher earnings, unionized workers, and those who were employed in part-time have longer durations of joblessness.

Effect heterogeneity may also stem from variation at the *(3b) context-level*. A standard finding highlights the importance of the economic situation (Brand, 2015; Fallick, 1996; Farber and colleagues; Kletzer, 1998; OECD, 2013). Higher local or state unemployment rates tend to increase durations of non-employment, although this also depends on the macro-economic environment and workers' willingness to be mobile. Poor economic conditions in workers' former industries, whether locally or nationally, also increase joblessness and returning back into the labor market takes longer in recessions than expansions. The greatest problems have been documented during the worldwide economic recession following the 2007/08 financial crisis (e.g., OECD, 2013).

[10] This series of papers is referred to as Farber and colleagues from here on.

Context-level effect heterogeneity does not only concern structural factors such as the economic situation but also differences in countries' institutional set-ups. However, while some studies provide results for several countries (e.g., OECD, 2013) and it is generally assumed that employment chances are a more relevant outcome in Europe than the US (von Wachter, 2010), there are very few studies with an explicit cross-national perspective, examining the moderating role of UI, PLMPs, ALMPs, EPL, and general wage setting policies. The few studies that address such questions usually do not distinguish different reasons for job loss or only examine transitions from employment into unemployment (Brandt and Hank, 2014; DiPrete, 2002; Ehlert, 2016; Gangl, 2004, 2008; Kuhn, 2002; Layte et al., 2000).

For example, Layte et al. (2000) examined four countries representing different welfare state or employment regimes. They found smaller effects of past on current unemployment in Sweden and the Netherlands as compared with Italy and the United Kingdom (UK). They attributed this to the stronger ALMPs in the former, which were assumed to foster re-employment. Brandt and Hank (2014) also used retrospective life history data and examined European workers aged 50 years and older. Grouping countries into welfare state regimes, they showed that unemployment in the early- and mid-career increased the risks of late-career unemployment with stronger effects in socio-democratic than conservative or southern welfare states. The authors explained the counterintuitive finding for Scandinavian countries by highlighting the particularly negative signal of individual joblessness in contexts of low aggregate unemployment.[11] Kuhn (2002) reports on a comparative project including ten countries that explicitly examined displacements and dismissals. In his summary of the findings, he notes that joblessness lasted longer in more generous welfare states such as Germany or France as compared with the UK or US. Another explanation for these differences may be the former countries' stricter EPL, which despite decreasing the risk of unemployment after job loss likely increases re-employment barriers for workers who are not able to find a new job immediately.

Similar cross-country patterns were found in a number of studies comparing Germany and the US. Mostly based on previous research, DiPrete (2002) provides a stylized picture suggesting that German workers experience longer unemployment, but that US workers face larger earnings losses (see subsection 2.1.3). These results have been updated in empirical analyses of Gangl (2004) and Ehlert (2016). Focusing on workers who transition from employment to

[11] However, they report odds ratios which are known for their difficult interpretation as well as for their problems in comparisons across models (Mood, 2010).

unemployment, Gangl (2004) found longer unemployment durations in West Germany than the US and based on a simulation he attributed this to the more generous German UI. For both countries he also showed that workers with UI remained out of work longer and that a good economic environment increased re-employment rates. Ehlert (2016) defined job loss as transitions from employment to unemployment following displacements due to plant closures, dismissals, or the completion of temporary jobs. He found lower chances for quick re-employment in Germany than the US, in particular, during the 1990s. The latter finding was not only due to the economic difficulties after reunification as it also held for West Germany.

A commonality of the comparative literature is its focus on qualitative cross-country comparisons, where the effects of job loss and unemployment are examined for a small number of countries and differences are interpreted against the blueprints of welfare state or employment regime typologies. While these studies highlight how effects vary with institutional set-ups, they are not able to quantify the moderating role of specific policies. Such an analysis was provided by Gangl (2008) who estimated the effects of a transition from employment to unemployment using panel data for 13 countries. In line with previous studies, the descriptive findings showed higher rates of non-employment in Germany than in the US or UK, in particular, in the long run. The quantitative comparative analyses also revealed that GDP growth reduced the negative effects on employment. Furthermore, strict EPL and generous UI mitigated the negative effects of job loss, but only if they were not combined. Thus, generous UI helped in flexible instead of strongly regulated labor markets and strict EPL was positive in countries with low instead of high UBs.[12]

Besides treatment and effect heterogeneity, *(4) methodological differences* in the measurement of non-employment or unemployment may explain different findings across studies. Researchers have measured these outcomes by examining the occurrence of a positive spell, the duration conditional on a positive spell, the unconditional duration, and non-employment or unemployment at a particular survey date (Kuhn, 2002). The above reported results mainly concern the latter two outcome measurements, because they not only capture workers' difficulties in finding a job but also in keeping it, which makes sense given that studies show that displacements and dismissals often occur repeatedly (e.g., Stevens, 1997). A related point that has only been highlighted recently is the sole focus on the level of non-employment ignoring

[12] Gangl (2008) notes that his findings were associated with a high uncertainty, as only 13 countries were examined and the models included several macro-level variables and sometimes even interaction terms.

what workers do if they are without a job. The fact that only unemployment is distinguished is likely due to administrative data lacking detailed information. Among the survey research that has examined other labor market statuses a few specialist studies have considered the effects of job loss on (early-)retirement.

Two studies by Chan and Stevens (1999, 2001) showed for the US that displacement led to a significant re-evaluation of the trade-off between work and retirement. Although job loss had large and lasting negative effects on older workers' employment chances, which were due to their difficulties in finding and keeping work, those who did return and remained employed often delayed their retirement as a long-term reaction to job loss. Tatsiramos (2010) examined older workers in Germany, Italy, Spain, and the UK to shed light on the role of different rules concerning UBs and (early-)retirement, pointing out that older workers in some countries also have fewer incentives to search for re-employment. Examining transitions out of non-employment and into employment or retirement, he found that workers in Germany and Spain were less likely re-employed if they were displaced at age 55 years and older, and they were more likely to retire after age 60 years. In Italy and the UK older workers were, however, less likely to exit to retirement. Germany and Spain offered the possibility to retire as early as 60 years creating disincentives to search for a job. They also provided opportunities for early retirement for workers above 60 years.[13]

Apart from these specialist studies on older workers' (early-)retirement, only two studies have examined different forms of non-employment. Oesch and Baumann (2015) found for Switzerland that 1.5 to 2.5 years after plant closure, almost half of the non-employment effect was due to workers out of labor force with retirement being the main source of this. A recent UK study shows that other statuses such as education or family care are as important as unemployment in explaining the long-term non-employment effects (Upward and Wright, 2017).

Overall, the literature on the effects of displacements and dismissals on labor market status has several *limitations* that are addressed in this thesis: First, it strongly focuses on the level of non-employment ignoring its composition. But some forms of non-employment (e.g., inactivity) raise greater concerns among policy-makers than others (e.g., education or training)

[13] Tatsiramos (2010) used data for the time period 1994-2001. However, over the last decades, early-retirement paradigms have changed. For example, in Germany it was long supported, because it relieved economic pressures, but today workers are expected to stay employed longer with few opportunities to leave the labor force through a combination of extended unemployment benefits and early-retirement schemes (see Buchholz, 2013 for a detailed review).

(OECD, 2013). For example, workers updating their skills (i.e., in education or training) or actively looking for a job (i.e., unemployed) likely have a stronger labor market attachment than inactive persons. Re-employment of the registered unemployed can also be supported by ALMPs. In contrast, governments have little influence over discouraged workers who left the labor force and are unlikely to return (i.e., inactive). Retirement may or may not be considered problematic depending on whether governments use early retirement to relieve the labor market from economic pressure. Second, of the few studies that investigated other types of non-employment most have focused on retirement or are subject to methodological restrictions. For example, Oesch and Baumann's (2015) study was based on a small number of specific plants which mostly had an older workforce. Moreover, they only looked at the short- and medium-term effects and did not use a control group. Third, while these limitations were overcome by Upward and Wright (2017) who examined the general UK population, they did not distinguish different reasons for job loss.

As knowledge on the size of the non-employment effects and their composition has important policy implications, Article 1 *"Losing standard employment in Germany: The consequences of displacement and dismissal for workers' subsequent careers" contributes* to the literature by providing first empirical evidence for Germany using a representative sample and differentiating non-employment into unemployment, education or training, retirement, and inactivity. Moreover, it distinguishes displacements due to plant closures and dismissals and also examines the varying importance of different forms of non-employment over time, for example, because decisions to leave the labor force only manifest after unsuccessful job searches. Additional contributions of Article 1 will be explained in subsections 2.1.3 and 2.1.4.

2.1.3 The effects on labor income

The *(1) central findings* are that displacement and dismissal cause large earnings and wage losses, which are often shown to be more persistent than the negative employment effects (Brand, 2015; Fallick, 1996; von Wachter, 2010). For example, Davis and von Wachter (2011) found relevant earnings losses for more than 20 years after workers had been laid off. Although most studies show that the negative effects on earnings decrease over time, the patterns are less clear for wages. As for employment, the findings vary greatly across studies. Estimates of the immediate effects range between 5 to 60 percent and for the persistent effects (usually about five years after job loss) losses between 0 to 30 percent are reported (e.g., Couch and Placzek, 2010; Ehlert, 2013; Gangl, 2006; Jacobsen et al., 1993; OECD, 2013;

Ruhm, 1991). Reasons for this variation may be common methodological differences (see subsection 2.1.1), but also theoretical reasons pointing to the importance of *(2) treatment heterogeneity* as well as *effect heterogeneity* at the *(3a) worker*- and *(3b) context-level*. Similar to research on employment, studies show some *(4) methodological differences* in their definitions of the outcome, but in case of labor income this plays and even greater role. Specifically, some studies focus on households' total welfare losses while others examine the effects on individuals' earnings and wages. While I focus on the latter, the main results of research on household income and poverty are also briefly summarized.

Concerning *(2) treatment heterogeneity*, Gibbons and Katz (1991) found support for their adverse signaling argument. They report smaller earnings losses for workers who were displaced due to plant closure than for those who were laid off. Subsequent studies have focused on earnings or wages and provided mixed empirical evidence for differences by the reason for job loss. While many of the studies summarized in Kuhn (2002) report less negative effects for plant closures compared to other reasons, Grund (1999) found almost no differences between German workers displaced due to plant closure and those who were dismissed, casting some doubt on the idea of stigma effects. Other studies have confirmed the original findings, but have challenged the adverse signaling interpretation. Stevens (1997) argues that the finding is largely accounted for by larger pre-displacement wage cuts for displaced than laid-off workers. This leads to smaller negative effects for the former if one compares wages in the year before job loss to those reported after the event. Krashinsky (2002) also re-interprets the findings by pointing out that laid-off workers are employed in larger firms and, therefore, have higher pre-job loss wages than displaced workers. This may be understood from the fact that larger firms are more likely to weather the storm by layoffs whereas smaller firms face increased risks of closure in case of economic shocks. While the empirical evidence is ambiguous and the correct interpretation contested, the reason for job loss is shown to matter both theoretically and empirically.

Although no group of workers is exempt from earnings and wage losses, there exist some regularities concerning *effect heterogeneity* at the *(3a) worker-level* (Brand, 2015; Fallick, 1996; Farber and colleagues; Kletzer, 1998; Kuhn, 2002; OECD, 2013; von Wachter, 2010). The negative effects are larger for workers with higher tenure and incomes as well as for the older and less educated. This is often interpreted as losses in rewards to specific human capital and differences in the transferability of skills. Some other findings concern heterogeneity by potential effects of job loss. The importance of specific skills is illustrated by studies showing that income losses are larger for workers who have to switch industries, occupations, or firms to find a job. It is also known that losses are larger among workers who experience multiple subsequent job losses, have longer non-employment or unemployed durations, and take up part-time instead of full-time jobs. The finding for joblessness may be interpreted as human capital depreciation, stigma effects, or as being due to compositional differences.[14] For demographic variables surprisingly little is known. In terms of race no clear differences between whites and non-whites have been established and for gender findings differ widely across studies. Some authors report more negative effects for women (Gangl, 2006; Kuhn, 2002; Strauß and Hillmert, 2011), while others show greater losses for men (OECD, 2013) or find no large differences (Farber and colleagues).

Effect heterogeneity may also arise from differences at the *(3b) context-level*. Earnings and wage losses are cyclical meaning that they are larger in times of recessions than expansions. Positive economic conditions at the local level have been shown to reduce the negative effects. In line with this, workers who are displaced or dismissed from industries with shrinking employment levels are known to experience more severe earnings and wage losses.

Next to these structural factors, institutional differences have been examined in a small number of studies (DiPrete, 2002; Ehlert, 2012, 2013; Gangl, 2004, 2006; Kuhn, 2002). However, with the exception of Kuhn (2002) they do not take account of the reason for job loss. DiPrete (2002) summarizes previous results and suggests that job loss leads to longer unemployment durations in Germany as compared to the US, but also to smaller earnings losses and lower poverty risks. This trade-off was also highlighted by Gangl (2004) who showed that having UI and the more generous UI in West Germany compared to the US allowed workers to search longer for re-employment increasing durations of joblessness but decreasing the risks of significant earnings losses. In another article comparing these countries, DiPrete and McManus (2000) found larger negative effects of unemployment in terms of earnings in Germany, while the effects on household income after taxes and transfers were considerably smaller in Germany, pointing to the importance of the welfare state. These findings have been updated and extended by Ehlert (2012, 2013) who reports greater earnings losses for Germany. This may be explained by the fact that these studies consider foregone income in periods of non-employment and the latter usually lasts longer in Germany.

[14] The first two explanations suggest a causal effect of longer non-employment or unemployment. This is called true duration dependence and distinguished from the compositional effect labeled spurious duration dependence. In the literature on job loss the relative importance of these explanations is not well understood (Brand, 2015).

Kuhn (2002) summarizes a collection of studies for ten different countries. In general these found that wage losses vary considerably but they were especially pronounced for workers with high tenure in the US, Canada, and the UK. In other countries such as Germany or France wage losses were overall lower. Kuhn (2002) interprets the larger wage losses in the former countries against the background of their de-centralized wage-setting institutions (i.e., weaker unions and less collective wage bargaining, no or lower minimum wages) which bring along higher wage inequality increasing workers' risks for downward mobility. However, for Germany and France, Bender et al. (2002) found larger negative effects for workers who remained unemployed for over a year, suggesting that these do not benefit as much from compressed wage structures.

Gangl (2006) provides the only quantitative comparative study. He compared the effects of unemployment across 13 countries and generally found the smallest negative effects in Scandinavia and to some extent southern Europe, intermediate effects in continental Europe, and the most negative consequences in liberal market economies such as the US and UK. Moreover, in multi-level analyses positive moderating effects of EPL and UI were found with the latter unfolding its buffering effect more strongly if combined with a flexible and permeable labor market. The theoretical explanation for this is that workers in these countries are more de-commodified and at the same time a dynamic labor market provides a greater number of re-employment opportunities. Contrary to his predictions Gangl (2006) also found lower earnings losses in strict EPL countries. While he expected that regulated labor markets create difficulties in securing quick re-employment, he explained this by a greater wage compression in these countries such that moves from one job to another were associated with lower risks of large earnings losses.

(4) Methodological differences in the measurement of income likely contribute to the variation in findings. A key distinction is between studies focusing on individuals' earnings and wages, capturing losses in productive capacities and wage rents (von Wachter, 2010), and research on the total welfare losses of households. As the latter has some implications for measurement differences in studies on earnings and wages, I here summarize the main results.

Research on the transition from employment to unemployment and its impact on households' welfare has a strong tradition in sociology (e.g., DiPrete, 2002; DiPrete and McManus, 2000; Gallie, 2004; Gallie and Paugam, 2000; see Ehlert, 2016 for a detailed review). Within this literature studies that focus on relative poverty can be distinguished from research examining changes in household income. A central finding of the former is that job loss triggers transitions into poverty and that re-employment facilitates transition out of poverty. As its focus is on the household level, this literature also highlights the role of other family members' employment in ending poverty (e.g., Bane and Ellwood, 1986; McGinnity, 2004). Another key result is that poverty risks after unemployment are smaller in more generous welfare states regimes such as the Scandinavian countries, whereas less protection is offered by residual welfare states such as the UK (e.g., Layte and Whelan, 2003).[15]

While studies on the poverty consequences have stressed the importance of the family and the welfare state, analyses of the mitigating effects of private help and the tax and transfer system have mostly been provided by research on household income (e.g., DiPrete and McManus, 2000; Ehlert, 2012, 2013, 2016). Comparisons of earnings losses with losses in household incomes before taxes and transfers highlight the role of other family members' incomes, whereas comparison of pre- and post-government household incomes shed light on the impact of the tax and transfer system. The mentioned studies all compared Germany and the US showing that for couples the economic consequences are similar, but that for men in the US family resources are the more important buffering factor while in Germany the welfare state assumes a central role. Private help is the main source of buffering in case of women's unemployment. In single households, the income consequences are much more pronounced in the US, because no family support is available and the welfare state provides less protection.

Studies focusing on poverty and household incomes have to be distinguished from research on individuals' earnings and wages which is the main focus of this review. However, even in this literature measurement differences are key (von Wachter, 2010) and in part related to the focus on total welfare losses. For example, some studies include zero income during periods of non-employment to acknowledge that foregone income contributes to the full costs of job loss (e.g., Ehlert, 2013; OECD, 2013). Other research has restricted their samples to workers with some positive hours or earnings, to focus on losses due to lower working hours and hourly wages (e.g., Couch and Placzek, 2010; Jacobsen et al. 1993). However, estimated earnings losses increase with the length of the reference period that is used for labor income reporting (OECD, 2013). This is because studies that examine annual or quarterly earnings are affected to a greater extent by periods of non-employment even if they focus on workers who are sta-

[15] However, see Kohler et al. (2012) for contradicting findings. They showed increased poverty risks after unemployment in both Germany and the US, but German workers overall faced larger and more lasting poverty risks.

bly re-employed by setting some kind of positive hours and earnings restrictions (Kuhn, 2002). A further difference lies in research that examines wages conditional on re-employment and studies that already take into account the negative effects of job loss on working hours by only investigating hourly wages.

Despite these differences in the earnings and wage measurements, few studies have decomposed the total labor income losses into its effects via non-employment, lower working hours, and lower hourly wages. A recent exception is the study by Upward and Wright (2017) for the UK showing that the short-term total labor income losses are mostly due to higher non-employment, but that in the long run negative income effects are explained to a larger extent by lower hourly wages than reduced hours of work. Lachowska et al. (2018) found for the US that at the time of mass-layoff, non-employment and fewer working hours accounted for 80 percent of the overall income effect, while five years later lower wage rates were the main reasons.

Overall, the literature on the effects of displacements and dismissals on labor income has a number of *limitations*: First, while the measurement differences are mostly acknowledged, only two state-of-the-art studies for the UK (Upward and Wright, 2017) and the US (Lachwoska et al., 2018) have decomposed the total labor income losses into different sources. Second, these studies have not distinguished different reasons for job loss leaving unexplored whether the decompositions differ for displacements due to plant closures or dismissals. Third, these studies have been conducted for flexible labor markets such as the UK and US with no evidence being available for Germany which represents a conservative welfare state. However, examining Germany may help integrating seemingly disparate findings about larger total income losses in conservative welfare states (e.g., Ehlert, 2013) but greater earnings and wage scars in liberal regimes (Gangl, 2006). Specifically, the negative effects in Germany may be more apparent for earnings due to the longer durations of unemployment and non-employment, while conditional on re-employment negative effects are stronger in labor markets that require workers to accept jobs with insufficient working hours and lower wage rates.

Given these limitations, Article 1 *"Losing standard employment in Germany: The consequences of displacement and dismissal for workers' subsequent careers"*, next to other *contributions* (the subsections 2.1.2 and 2.1.4), connects research on the total labor income losses with studies on earnings and wage scars, by decomposing the former into its effects via non-employment, lower working hours, and lower hourly wages. In contrast to previous research I focus on Germany, which provides an interesting contrasting case. I also estimate the effects on different labor incomes separately for displacements due to plant closures and dismissals showing how the relative importance of each of these sources differs by the reason for job loss and over time.

2.1.4 The effects on other job characteristics

The literature on the economic consequences of displacements and dismissals has primarily focused on employment and income. However, some scholars argue that careers and, especially, job quality cannot be measured by money alone (Muñoz de Bustillo et al., 2011). While research on job quality has a long tradition (e.g., Jencks et al., 1988; Treiman, 1977), these themes have only been revived over the last two decades (Gallie, 2007; Green, 2006; Kalleberg, 2007). In studies on displacements and dismissals, this has manifested itself in two different, but interrelated research strategies. The first involves examining job quality indirectly by looking at workers' non-standard re-employment. The second focuses on more direct measures of non-monetary job quality as given by occupational status, job authority, job autonomy, and employer-offered benefits. They are interrelated, because a lot of research casts doubt on the job quality of non-standard employment. For example, many studies measure job insecurity, which workers rate as more important than income (Muñoz de Bustillo et al., 2011: 16), by fixed-term contracts (Dieckhoff, 2011; Oesch and Baumann, 2015). While for temporary employment many authors agree that it indicates greater job insecurity and lower job quality in general (e.g., OECD, 2002), this is less clear for part-time and self-employment which are more heterogeneous in their reasons for use and job quality (e.g., OECD, 2010; OECD/European Union, 2017). On the other hand, it has been argued that part-time and self-employment after job loss are more likely to be among the alternative working arrangements that provide lower job quality than standard employment.

In the following, I distinguish studies on non-standard re-employment and research that focuses on more direct measures, because the former not necessarily implies lower job quality and is often understood as a means of labor market re-integration. In fact, many governments have promoted its growth by supporting marginal part-time jobs next to unemployment, employers' use of temporary employment, and entrepreneurship as a route out of joblessness (Gebel, 2010, 2013; Hipp et al., 2015; Lietzmann et al., 2017). As much less is known for these groups of outcomes, I here separately report the empirical evidence from the studies available.

The only encompassing assessment on non-standard re-employment was offered by Farber (1999). He merged data from the DWS to identify displaced workers with information on non-standard jobs from the Contingent and Alternative Employment Arrangements Supplements (CAEAS) to the CPS. He distinguished independent contractors from other self-employed as well as identified temporary workers as those with temporary jobs, those in temporary agency work, on-call workers, day laborers, and contract workers. All others were considered regular workers with further distinctions made between full-time and part-time employment. Among the latter Farber also identified involuntary part-time jobs. Disadvantages of his data were the complex merging procedures necessary and that reported changes in the risk of non-standard employment over time did not concern the same workers. The retrospective data also made it difficult to define a suitable control group. Nevertheless, his study still provides the most comprehensive picture. He found that displaced workers were not more or less likely to become independent contractors and were less likely to be other self-employed. This may cast doubt on the idea of self-employment as low quality work. He also showed that displaced workers faced significantly higher risks of temporary and involuntary part-time employment. These risks declined over time (though no control group was used for temporary jobs) suggesting that non-standard jobs may provide a stepping stone to standard work.

A few years ago the OECD (2013) updated Farber's (1999) study, showing that workers' risks of part-time, temporary, self-, and informal employment increased from before to after displacement.[16] However, the analyses were restricted to displaced workers who found re-employment within one year, such that no control group or over time analysis was provided, making it impossible to judge whether non-standard re-employment is transitory or persistent.

While these are the only studies examining different forms in parallel, others have focused on specific types only. With respect to part-time jobs, Farber and colleagues provide interesting results, although these studies do not use control groups. Their results suggest that displaced workers have increased risks of part-time employment compared to their situation before. This is also the case for workers who are full-time employed before job loss such that the findings are not easily explained by voluntary labor supply decisions. These studies also show that part-time rates decline with time since job loss highlighting that it may serve as a bridge into full-time jobs. For workers displaced from full-time jobs, the risk of part-time work is

[16] The forms of non-standard and, especially, temporary work examined differed by country. They included casual jobs, fixed-term contracts, temporary agency work, daily hires, as well as seasonal and interim contracts.

greater in recessions than expansions. Farber and colleagues highlight that these differences in the part-time risks are largely explained by involuntary part-time employment.

Temporary employment and, especially, fixed-term contracts have also been examined as indicators of job quality (Dieckhoff, 2011; Oesch and Baumann, 2015). Investigating five plant closures in Switzerland, Oesch and Baumann (2015) found that about two years after job loss 14 percent of the re-employed workers held fixed-term contracts. They consider this proportion low, despite the Swiss labor market's weak EPL. However, without a control group and an over time analysis, their findings are difficult to interpret. Dieckhoff (2011) examined the effects of a transition from employment into unemployment on fixed-term employment and other more direct measures of job quality (see below). She considered Austria, Denmark, UK, and Spain to analyze effect heterogeneity arising from countries' institutional set-ups. Being the only study that used a suitable control group and offered an over time analysis, she found the highest risks of fixed-term contracts for workers from Spain both in the short and long run. Relevant effects were also revealed for Austria with somewhat smaller risks for Denmark and the UK. In all countries the effects declined with time since job loss but remained substantial. These results are consistent with institutional differences as Spain and Austria have the strongest EPL for regular jobs, such that job seekers more often have to use temporary jobs to re-enter the labor market. The somewhat smaller effects in Austria may also be explained by workers' higher bargaining power due to more generous UBs, albeit the differences between Austria, Denmark, and the UK were small, in particular, in the long run.

Von Greiff (2009) analyzed whether displacement increases the risk of self-employment in the year after the event. Using Swedish administrative data she included all workers who were displaced due to plant closures in 1987 and 1988 and used a random sample of employed individuals to form a control group. The results showed that displacement almost doubles the risk of self-employment with an effect of about 1.2 percentage points, but no over time analyses were conducted. She also found that the groups most prone to enter self-employment in reaction to displacement were workers with less favorable positions. This suggests that many workers are pushed into self-employment, as they cannot find regular jobs in the first place.

While research on non-standard re-employment assumes that these jobs offer a lower job quality, a few studies have also used more direct measures. Gangl (2004) compared Germany and the US and examined the job quality at re-employment after workers experienced a transition from employment to unemployment. Next to earnings, he looked at downward occupational status mobility and the likelihood of entering jobs that lasted less than 12 and 6 months. In general, workers who became unemployed faced downward status mobility and higher job instability with larger effects in the US than Germany. The latter finding is consistent with Stevens (1997) who reports increased risks for multiple job losses after displacements and dismissals. Gangl (2004) further provided empirical evidence for positive effects of UI on post-unemployment job quality with clearer results for job stability compared to downward status mobility. The cross-country differences were to a substantial part explained by differences in UI, suggesting that de-commodification helps workers in locating adequate re-employment.

Brand (2006) was the first study for the US that systematically examined a wide range of other job characteristics. Using the Wisconsin Longitudinal Study (WLS) workers were classified as displaced if their jobs were terminated due to downsizing, restructuring, business closure, or relocation. Extending earlier studies, Brand analyzed six outcomes including occupational status (i.e., occupational income and education), job autonomy (i.e., worker is not supervised) and authority (i.e., worker supervises) as well as employer-offered pension and health insurance. She found negative effects on occupational status, job authority, pension benefits and health insurance benefits with job autonomy being the only characteristics not consistently affected. She also examined effect heterogeneity with the results depending on the measure of job quality. For example, men and low educated workers experienced larger losses in occupational status and employer-offered benefits, while women and high-educated workers had larger losses in job authority. Upper white-collar and non-manufacturing workers experienced greater negative effects in terms of occupational status, job autonomy, and job authority, while blue-collar and manufacturing workers had greater losses in employer-offered benefits.

Lippman and Rosenthal (2008) investigated the effects of job loss on occupational prestige by using data from the DWS and the Employee Tenure Supplement of the CPS. Occupational prestige reflects evaluations of the social standing of occupations by the general population and corresponds well with workers' appreciation of jobs that are interesting, helpful to others, and useful to society (Muñoz de Bustillo et al., 2011: 16). The study showed that, on average, displaced workers experienced downward occupational prestige mobility, but that the effects varied for different subgroups. Specifically, those with high levels of education did better upon re-employment.

Dieckhoff (2011) not only studied fixed-term re-employment (see above) but also three more direct measures. Specifically, she considered job authority (i.e., supervision and coordination of personnel), satisfaction with job security, and satisfaction with the type of job. The results for job security mirrored the findings for fixed-term contracts as Spanish workers experienced the most negative effects followed by Austrian and Danish employees and no long-term effects in the UK. While these findings are in line with institutional differences, the negative effects for job authority were very similar across countries, questioning the importance of differences in labor market policies. Dieckhoff (2011) also found no consistent effects on satisfaction with the type of job suspecting that this may be due to workers changing their job values in reaction to unemployment. Overall, she found mixed support for the role of institutions in moderating the effect of unemployment on subsequent non-monetary job quality.

Nedelkoska et al. (2015) used German administrative data to assess skill (mis-)matches of displaced and dismissed workers. Specifically, they examined occupational changes and characterized these on a horizontal (distance) and a vertical dimension (direction). They found that job loss has large positive effects on changing occupations and that conditional on this the new occupations are less demanding and provide fewer opportunities to learn new skills. This shows that workers not only face higher risks of non-standard, but also of inadequate re-employment. Similarly, Pollmann-Schult and Büchel (2005) examined transitions from unemployment into adequate employment and overeducation using West-German life history data. They showed that workers who receive unemployment benefits have longer job searches, but also lower risks to take up a job for which they are overeducated, suggesting that the welfare state can mitigate the negative effects of unemployment on job quality to some extent.

Overall, a growing number of studies have examined other job characteristics with some focusing on non-standard or inadequate re-employment and others on more direct indicators of job quality. However, the majority of these studies share several *limitations*. First, they examined only one outcome making it difficult to highlight potential complementarities or trade-offs. Second, a lot of research has not considered the reason for job loss. Third, no control groups were used making it difficult to judge how job quality would have changed in the absence of job loss. Fourth, many studies only took a short-term perspective, but what this area of research is most concerned with is the persistency of the effects, as some of the alternative work arrangements are suggested to only be transitory. Fifth, there are almost no comparative studies and not a single study has taken a quantitative comparative approach to estimate the moderating effects of policies.

Article 1 *"Losing standard employment in Germany: The consequences of displacement and dismissal for workers' subsequent careers"* next to other *contributions* (the subsections 2.1.2 and 2.1.3), provides first evidence for Germany on workers' use of non-standard re-employment. In contrast to most previous studies, I examine different forms of non-standard work in parallel and in some analyses even distinguish varying types of temporary, part-time, and self-employment. Moreover, I consider the reason for job loss and distinguish displacements due to plant closures from dismissals, make use of suitable control groups, and follow workers for five years after job loss to investigate whether the take up of non-standard work is transitory or persistent. Article 2 *"The effects of unemployment on non-monetary job quality in Europe: The moderating role of economic situation and labor market policies" contributes* to the literature by examining the effects of unemployment on four different facets of non-monetary job quality: occupational status, autonomy, authority, and job security. Specifically, I extend the few available cross-country comparisons by estimating the moderating effects of specific policies using a quantitative comparative analysis instead of interpreting differences among countries in view of their institutional set-ups.

2.2 The effects of non-standard or inadequate re-employment on the subsequent career

The literature review in the last subsection suggests that workers who lose their job and become unemployed have increased risks for non-standard or inadequate re-employment. Workers may be willing to accept these jobs, because they expect quick re-employment to still offer better career outcomes than remaining unemployed and continuing the job search.

Gebel (2010, 2013, 2015) offers a valuable framework for thinking about the effects of non-standard or inadequate (re-)employment. He surveys research on labor market (re-)entry in temporary or inadequate jobs, but his observations apply to non-optimal jobs in general. Specifically, he states that most studies concerned with non-standard or inadequate employment have performed "upward" comparisons to standard or adequate jobs. Not surprisingly these studies find negative effects on future employment chances and job quality. However, in line with other scholars (e.g., Korpi and Levin, 2001), Gebel argues that to evaluate non-optimal jobs' integrative function, "downward" comparisons are necessary, too, contrasting non-standard or inadequate (re-)entries with the alternative of remaining unemployed and continuing the job search. Therefore, the following subsections review empirical studies that compare non-standard (subsection 2.2.1) and inadequate re-employment (subsection 2.2.2) to unemployment. The review for non-standard work is related to Article 1 as results on the downward comparison are helpful in assessing how problematic increased risks of non-standard employment are. It also is the foundation for the literature on inadequate re-employment, which is the focus of Article 3.

2.2.1 The effects of non-standard re-employment

Most research on the downward comparison of non-standard re-employment to unemployment has focused on temporary employment in general (see Gebel, 2010, 2013 for reviews) or on temporary agency work in particular (see Houseman, 2014 for a review).[17] Based on these reviews and some additional studies (Barbieri and Sestito, 2008; De Graaf-Zijl et al., 2011; Hagen, 2004; Jahn and Rosholm, 2014; Korpi and Levin, 2001, Kvasnicka, 2009; Lehmer, 2012, Picchio, 2008) the following *(1) central findings* stand out.[18] Workers often only take up temporary work if they cannot find permanent jobs over an extended job search. Moreover, starting a temporary job as compared to staying unemployed reduces subsequent unemployment and inactivity risks and also improves the chances for permanent employment. It further increases subsequent job quality as measured by wages and other indicators.

However, the results differ considerably with respect to the type of temporary employment indicating *(2) treatment heterogeneity*. While substantial positive effects are apparent for fixed-term contracts, for temporary agency work only small positive effects on subsequent employment are found and most studies show no evidence for a stepping-stone effect into regular or other high-quality employment, in particular, in the long run. In addition, some studies even found negative effects for temporary jobs that are part of job creation schemes. Therefore, comparisons across studies need to consider the different types of temporary jobs examined. Although many studies have examined *effect heterogeneity* at the *(3a) worker-level* no clear patterns have been established. The general contention is that the effects should be more positive for disadvantaged groups, but some studies find stronger stepping-stone effects for more educated workers. Very little research has compared the integrative function of temporary employment across different countries or over time to study *heterogeneity at the (3b) context-level*. The few available studies find that the stepping-stone effects of temporary em-

[17] Temporary employment is often defined as work that is limited in time (Gebel, 2013). This usually includes fixed-term contracts, temporary agency work, job creation schemes or subsidized temporary jobs, and training contracts. Some studies also include on-call work, day labor, contract work, casual work, or seasonal and interim jobs.

[18] I only focus on studies that contrast non-standard employment and unemployment (downward comparison). See Gebel (2010) and Houseman (2014) for more encompassing reviews that also compare temporary to permanent employment and report results on studies examining the transition out of temporary employment.

ployment and, in particular, temporary agency work are stronger in good economic conditions and in countries with higher EPL for regular employment, because in these countries temporary jobs are used a screening devices.

For part-time employment the empirical evidence on the downward comparison is much scarcer. Therefore, I only report the key findings without systematically distinguishing forms of heterogeneity. Most studies for part-time work come from Germany (Caliendo et al., 2016; Freier and Steiner, 2008; Lehmer, 2012; Lietzmann et al., 2017) and focus on marginal employment which are jobs with low working hours that can be combined with unemployment to top up benefits and where earnings are not or only partially susceptible to social security contributions. Over the last years, additional studies have been conducted for other countries (Austria: Böheim and Weber, 2011; Finland: Kyyrä, 2010; Denmark: Kyyrä et al., 2013). Across all studies, the results show that taking up marginal employment as compared to unemployment leads to small reductions in future unemployment and inactivity, but that it has no or only small stepping-stone effects into regular employment. With respect to other measures of job quality the findings are mixed. Overall, the effects are very heterogeneous with the exception that the positive effects on employment chances and regular employment increase the longer persons have been unemployed before taking up marginal jobs.

For self-employment compared to unemployment the evidence is even more limited. While there are no studies that are comparable to those for temporary or part-time jobs, some information is offered by research on start-up programs, as these studies often compare workers who receive subsidies or training to start self-employment with those that remain unemployed and continue the job search. A recent review by Dvouletý and Lukeš (2016) shows that self-employment out of these programs has overall positive effects on employment and earnings, although the evidence is mostly limited to Germany (e.g., Caliendo and Künn, 2011, 2015) and it has to be considered that many workers remain solo self-employed.

The reported findings have some implications for the evaluation of the results of Article 1 which examines the effects of displacements due to plant closures and dismissals on non-standard employment risks. For temporary employment, quick re-employment may have positive career consequences, but mostly only for fixed-term contracts. For part-time jobs almost all research points to the specific role of marginal employment, which, on average, neither helps nor hinders workers' re-employment, with little evidence for regular part-time. For self-employment the only evidence comes from research on ALMPs that foster entrepreneurship such that it is unclear whether the results can be transferred to workers starting self-employment on their own terms, but the distinction of solo- from other self-employment is shown to be important. Next to taking these heterogeneities within non-standard employment into account in Article 1, this review also is the foundation for the following summary of studies comparing inadequate work to unemployment.

2.2.2 The effects of inadequate re-employment

While most of the literature on inadequate employment has performed an upward comparison to adequate jobs (see McGuinness, 2006 for a review) or examined transitions out of inadequate employment (e.g., Pollmann-Schult and Büchel, 2004a; Scherer, 2004), almost no studies have investigated the career effects of taking up inadequate work compared to remaining unemployed and continuing the job search. Therefore, I review these studies separately.

Pollmann-Schult and Büchel (2004b) used life history data for West Germany to examine transitions from unemployment into adequate employment. They found higher transition rates for workers who directly changed from unemployment to adequate employment as compared to those who had an intermediate spell of inadequate work. This suggests that overeducation is not a bridge into adequate employment. However, Pollmann-Schult and Büchel (2004b) note that inadequate re-employment may increase overall employment chances.

Two methodologically advanced studies are Baert et al. (2013) and Baert and Verhaest (2014). Baert et al. (2013) investigated whether unemployed graduates in Flanders who accept a job below their level of education sped up or slowed down their transition to adequate employment using the timing of events approach which accounts for selection into overeducation due to time-constant unobserved characteristics. They found that overeducation trapped young workers, because those who took up inadequate jobs had much lower transition rates. However, this effect depended on the elapsed unemployment duration and for the long-term unemployed the results showed that an overeducated re-entry may be preferable. Baert and Verhaest (2014) focused on estimating the stigma effects of unemployment and overeducation using a field experiment. They sent out applications from fictitious candidates who only differed in their labor market activity. They distinguished those who graduated a few months before, those who graduated a year earlier and had been unemployed, and those who graduated at the same time, but were overeducated. Their experimental study showed that applications with unemployment received the fewest positive reactions suggesting a negative signaling effect even in comparison to graduates who were overeducated.

The research on inadequate re-employment has several important *limitations*: First, only very few studies have compared inadequate re-employment and unemployment at all reflecting that most research ignored overeducation's integrative potential. Second, almost no studies have examined the trade-off between higher employment chances and at the same time lower chances for long-term adequate employment. Third, most studies have taken a short-term perspective by focusing on transitions from overeducation into the next job instead of following the careers of workers over an extended period of time. Fourth, the current evidence is strongly focused on graduates and early-career workers, ignoring workers who lost their job and are confronted with the decision to either reject or accept an overeducated re-entry.

Therefore, in Article 3 *"Better overeducated than unemployed? The short- and long-term effects of an overeducated labour market re-entry"* my co-author and I *contribute* to the literature by examining both the short- and long-term effects of an overeducated re-entry compared to unemployment. This complements the large number of studies that have documented overeducation's inferiority to adequate work. It also extends the few previous studies on the downward comparison by offering first evidence on the German labor market for all levels of education and labor market experience including some effect heterogeneity analyses. Moreover, my co-author and I apply sophisticated methods of causal analysis that allow for an appropriate comparison of an overeducated re-entry to unemployment. Furthermore, we investigate both employment chances and chances of adequate employment for up to five years after re-employment, allowing for a comprehensive assessment of the short-and long-term effects as well as highlighting potential complementarities or trade-offs.

2.3 The effects of job loss and unemployment on health and well-being

Next to their economic consequences, job loss and unemployment are also thought to have negative effects on individuals' health and well-being. In subsection 2.3.1 I review what is known about the direct or short-term effects, which have been the focus of a large and interdisciplinary literature. Based on this, subsection 2.3.2 summarizes the state of research concerning the potential long-term consequences of job loss and unemployment as this is the area of research in which Article 4 is located and seeks to make several contributions.

2.3.1 The direct or short-term effects

The health and well-being effects of job loss and unemployment have been studied across the social sciences and several meta-analyses and literature reviews exist (Bartley et al., 2006; Dooley et al., 1996; Ezzy, 1993; McKee-Ryan et al., 2005; Murphy and Athanasou, 1999;

Paul and Moser, 2009; Roelfs et al., 2011; Rogge, 2013; Voßemer and Eunicke, 2015; Wanberg 2012). Using the reviews, I here summarize the *(1) central findings* and results about *(2) treatment heterogeneity* and *effect heterogeneity* at the *(3a) worker*- and *(3b) context-level*.

The *(1) central finding* is a negative association of unemployment and health, which is stronger for mental health and well-being than for physical health and health behaviors. This result has been established in different contexts, time periods, and research designs (Voßemer and Eunicke, 2015). Concerning the question whether the found associations are due to causal effects, health selection (reverse causality), or confounding, the literature has highlighted the importance of all three explanations. Although it is methodologically less sophisticated than the research on the economic consequences (see section 2.1), support for the hypothesis of a causal effect of unemployment on health is provided by studies based on panel data and applying methods to control for previous health as well as time-constant unobserved confounding. Another strategy that has been used is to approximate causal effects by focusing on job losses due to exogenous reasons (e.g., plant closures) as well as studies that provide bounding analyses based on the potential outcomes framework.[19] While the question about how much of the association is due to causation or selection (or confounding) is still highly contested, Fryer (1997) argues that it will never be settled by a single study and researchers should rather focus on the interplay of different explanations.[20] Theoretically, the negative effects are assumed to be explained by either of two channels (see Nordenmark and Strandh, 1999 for a theoretical synthesis; see Voßemer and Eunicke, 2015 for a review of theories). The direct income losses following from job loss and unemployment and the loss of an important social role that provides identity. Empirically the relative importance of these explanations is contested, but the majority of studies find the negative psychosocial effects to be more relevant (e.g., Knabe and Rätzel, 2011a; Winkelmann and Winkelmann, 1998; Young, 2012). While these central findings highlight some striking similarities, research has also paid a lot of attention to different sources of heterogeneity.

[19] Some studies finding effects of job loss and unemployment are: Burgard et al. (2007), Strully (2009), Browning and Heinesen (2012), Cygan-Rehm et al. (2017), and Krug and Eberl (2018). There are also sophisticated studies that do not find such evidence (e.g., Böckerman and Ilmakunnas, 2009; Browning et al., 2006). A close reading of the literature suggests that the evidence for effects of job loss and unemployment is much stronger for mental health and well-being than for physical health (Gebel and Voßemer, 2014; Krug and Eberl, 2018).

[20] Although many studies entertain the idea that either of these explanations is true and that more sophisticated studies will proof one or the other wrong, it is more likely that the causative and selective parts producing the association vary across contexts and time periods such that it is plausible that well-done studies show evidence for both.

Some researchers have focused on *(2) treatment heterogeneity* or differences in the experiencing of unemployment (e.g., Voßemer and Eunicke, 2015). The most researched factors are repeated unemployment and unemployment duration. Theoretically, adaptation or habituation after the first unemployment experience or with increased duration is distinguished from the idea that repeated and cumulated exposure result in more negative effects. With respect to repeated unemployment no meta-analytical evidence exists and the available studies provide mixed findings (e.g., Booker and Sacker, 2012; Strandh et al. 2014). More negative effects are found for longer durations (McKee-Ryan et al., 2005) and some support is provided for a peak effect (Paul and Moser, 2009) with an incomplete adaptation occurring afterwards. Other aspects of treatment heterogeneity that have received limited attention concern whether unemployment follows after employment and the role of different reasons for job separations (Voßemer and Eunicke, 2015). Many studies do not differentiate whether unemployment follows employment or inactivity (see Young, 2012 for a discussion) and relatedly most longitudinal studies mix the effects of unemployment and re-employment (see Gebel and Voßemer, 2014 for a differentiation). Similarly, the research on health and well-being has been more concerned with unemployment than the economic literature which has focused on displacements and dismissals. However, an increasing number of studies that concern the short-term effects of unemployment have looked at specific reasons (e.g., Burgard et al., 2007; Strully, 2009). While few systematic comparisons are available, it is generally assumed that studies that make fewer of the mentioned differentiations underestimate the negative effects of job loss, as job searches after inactivity are not problematic and transitions from employment to unemployment include voluntary job separations (e.g., Wheaton, 1990; Young, 2012).

Concerning *effect heterogeneity* at the *(3a) worker-level*, the three moderating factors that have received most attention are gender, age, and socio-economic status. Most studies show that women suffer less than men and this is usually attributed to their lower work norms and the availability of alternative roles as wives and mothers. While this has been found repeatedly and also in a meta-analysis (Paul and Moser, 2009), there are also several studies, including a meta-study (McKee-Ryan et al., 2005), that show no differences or even suggest that women are more negatively affected. As Voßemer and Eunicke (2015) argue these disparate findings may be reconciled if more attention is paid to study context. For example, studies that find no differences often took place in more gender-equal contexts or are based on younger workers where gender norms are already more similar. With respect to age early studies assumed and found a non-linear moderation with prime-age workers being more negatively affected than younger and older workers. However, the meta-analytic evidence is ambiguous (Murphy and Athanasou, 1999) or even finds the opposite (Paul and Moser, 2009) which is partly supported in another summary of the literature that suggests that it is the youngest workers who are off worst (McKee-Ryan et al., 2005). The latter finding is often interpreted in view of the life course perspective, where it is argued that labor market entrants are in a sensitive period such that they are particularly susceptible to negative life events that hinder their establishment in the world of work and as adults in general. For socio-economic status, which is usually measured by education, occupational status, or class (McKee-Ryan et al., 2005; Paul and Moser, 2009), theories assume that individuals with higher education and occupational status or class have greater resources to buffer the negative effects of unemployment. A contrary argument states that these persons have more to lose as they experience larger income losses and are often more invested in their jobs. The empirical evidence is very mixed, especially with respect to the moderating effects of education (McKee-Ryan et al., 2005; Voßemer and Eunicke, 2015). Clearer results are available for occupational status or class where the majority of studies and meta-analyses show more negative effects for blue-collar as compared to white-collar workers (Norström et al., 2014; Paul and Moser, 2009).

Effect heterogeneity has also been investigated at the *(3b) context-level*. Most research has used variation across regions or over time and examined factors out of three broad groups: structural contexts, institutional set-ups, and culture. Voßemer and Eunicke (2015) provide a detailed review but some general findings are summarized here. The evidence is mixed for structural factors such as the unemployment rate. Some research shows that unemployment hurts less if there is more of it around, which is potentially explained by lower work norms in these contexts (e.g., Clark, 2003). Other research highlights that the prospects for re-employment are poor if the economic outlook is bad, suggesting more negative effects. Sophisticated studies such as Oesch and Lipps (2013) have found little evidence for the moderating role of regional unemployment and this is supported by meta-analyses which either find weak evidence for more negative effects (Paul and Moser, 2009) or no moderating effects (McKee-Ryan et al., 2005). Clearer results are available with respect to institutional set-ups and specifically PLMPs. A review and meta-analytic evidence (McKee-Ryan et al., 2005; O'Campo et al., 2015; Paul and Moser, 2009) provide strong support for the idea that unemployment has less negative effects on health and well-being if welfare states mitigate the negative financial effects through sustained income replacement. This is also found in recent studies (Voßemer et al. 2018; Wulfgramm, 2014) and the few contrary results (Eichhorn,

2014) may be explained by different measurements of benefit generosity. For other labor market policies such as ALMPs and EPL, the findings are more ambiguous and differ across the studies available (Voßemer et al. 2018; Wulfgramm, 2014). There is almost no evidence for the role of culture and studies that examine the moderating role of countries' work ethic report differing results (Eichhorn, 2013; Gallie and Russell, 1998).

While the direct- or short-term effects of job loss and unemployment for health and well-being have been thoroughly investigated, much less is known about the long-term effects, which will be the focus of Article 4 and are reviewed next.

2.3.2 The long-term effects

I here separately review the few studies that have focused on estimating the long-term effects of job loss and unemployment (see Voßemer and Eunicke, 2015; Baranowska-Rataj et al., 2016). In contrast to studies on the economic consequences, which were concerned with the scarring nature of job loss from the beginning, research on the long-term health and well-being consequences has only taken off in recent years (Brydsten et al., 2015; Daly and Delaney, 2013; Mossakowski, 2009; Schröder, 2013; Strandh et al., 2014, 2015).

Korpi (1997) focused on the well-being of Swedish youth. Based on cross-sectional data he found a negative effect of the number of month in unemployment since the end of compulsory education. However, when using longitudinal data only current unemployment showed an effect implying that the well-being consequences of unemployment may be transitory. A similar study for the general population is Clark et al. (2001). Using German panel data they showed that past unemployment was negatively associated with current life satisfaction for men. However, they rather studied medium-term effects as their measure of past unemployment only involved the last three years. Using more waves of the same data, Knabe and Rätzel (2011b) replicated these results and also found that the effect of past unemployment was explained by workers greater fears to lose their job again. While they did not cast their results in terms of a mediation analysis, their study offers first evidence that the total effect of past unemployment on well-being may operate through the mechanism of lower job security.

Research that looks beyond medium-term effects usually relies on one of two longitudinal cohort studies. The National Child Development Survey (NCDS) which follows persons born in 1958 in Great Britain or the Northern Swedish Cohort (NSC) following all pupils in their last year of compulsory school in a medium-sized industrial town in Sweden. These data have the advantage that they cover long time periods and also include measures of childhood socio-economic status and health. Using the NCDS, Wadsworth et al. (1999) showed that the cumulated month of unemployment between the ages 16 and 33 years were negatively associated with an index of health capital (i.e., capturing body-mass index, exercising, diet, and smoking) at age 33 years, after controlling for childhood socio-economic status, intelligence, and health. Bell and Blanchflower (2011) used the same data and estimated the effects of month spent in unemployment between the ages of 16 to 23 years on various indicators of well-being (i.e., life satisfaction, subjective health status, feeling miserable or depressed) when respondents were aged 50 years finding negative effects of early unemployment for all outcomes. Also using the NCDS, Daly and Delaney (2013) showed that the cumulated years of unemployment between ages 16 and 50 years were positively associated with psychological distress at age 50 years controlling for childhood psychological factors at age 11 years and psychological distress at age 23 years.

Three studies have used the NSC to investigate the long-term health effects of youth unemployment (Brydsten et al., 2015; Hammarström and Janlert, 2002; Strandh et al., 2014).[21] Hammarström and Janlert (2002) showed that cumulated unemployment of 6 months or longer between the ages 16 and 21 years increased daily smoking and psychological symptoms (i.e., nervous and depressive symptoms, sleeping problems) but not excess alcohol consumption at age 30 years. For men only, a statistically significant effect on somatic symptoms was found. Strandh et al. (2014) estimated the effects of unemployment (i.e., 6 months or more) between the ages 18-21, 21-30, and 30-42 years on changes in mental health (i.e., nervous and depressive symptoms, sleeping problems) at ages 21, 30, and 42 years. Unemployment at ages 18-21 years resulted in decreases in mental health between the ages 16 to 30 and 16 to 42 years. Brydsten et al. (2015) used the same data to show that the number of month in unemployment between the ages 16 to 21 years increased somatic symptoms at age 42 years, but only for men. Their study is special in that they explicitly examined whether the effects of youth unemployment were explained by later unemployment which was not the case.[22]

[21] Strandh et al. (2015) is a related study which used the NSC to, in addition, compare the experience of open youth unemployment to the participation in ALMPs between the ages 18 to 21 years. They find negative effects of unemployment on mental health at the age of 21 and 42, but not for those who participated in the ALMPs.

[22] While all studies based on the NCDS and the NCS have the advantages that they can control for early-life health and other important confounders, with few exceptions they controlled for potential mediators (such as employment status or health after the initial unemployment) and, therefore, likely underestimate the total effects on late life health. While such adjustments after having estimated the total effect offers insights on their role as mediators, controlling for it right away is usually not informative and may result in wrong conclusions.

Two further studies have used other data to examine long-term effects. Mossakowski (2009) used a youth survey for the US and found that cumulative unemployment between the ages 14 to 22 years was positively associated with depressive symptoms at the ages 27 to 39 years. Schröder (2013) used data from the Survey of Health, Ageing, and Retirement in Europe (SHARE) and compared persons who lost their job due to plant closures or layoffs with persons who never lost a job. Controlling for childhood socio-economic status and health, he showed that job loss has negative effects on various health measures for 25 years and more after the event. His study is sophisticated in that it is the only one on long-term effects that differentiates reasons for job loss. However, his definitions of only experiencing plant closure or layoff and the choice of the control group as persons who never experienced either event is problematic, as it implicitly conditions on some of the consequences of job loss.

The review in section 2.3 highlights some important *limitations* of the literature on the health and well-being consequences. First, the majority of studies have focused on short-term effects. Second, the few studies that have investigated the long-term effects only considered specific cohorts from the UK and Sweden and strongly focused on measures of cumulated unemployment which bring along all the issues reviewed on treatment heterogeneity. Third, almost no studies have empirically investigated the mechanisms for the long-term effects. Fourth, while the majority of research on the long-term effects has focused on youth no study has examined the effects of job loss in the early-career and differentiated reasons for job loss.

My co-authors and I *contribute* to the research on the long-term effects in several ways. Article 4 *"The effect of an early-career involuntary job loss on later life health in Europe"* makes use of so far underutilized data. These allow estimating the effects of an early-career job loss on late life health for more than 30 years after the event and concern 14 European countries making it possible to examine if the results of previous studies extrapolate beyond specific birth cohorts. Further, we distinguish displacements due to plant closures from layoffs and are among the first studies that empirically investigate to what extent subsequent unemployment risks and employment instability mediate the total effect of job loss on health. Thereby, we also connect the research on the economic and non-economic consequences of job loss.

2.4 The effects of job loss and unemployment on partners' division of housework

The impact of job loss and unemployment on families is another topic concerning the non-economic consequences. Specifically, the negative effects are assumed to spill over to other family members. For example, research has shown that job loss affects couples' fertility and marital stability and also partners' well-being and children's socio-economic outcomes (Brand, 2015; Ström, 2003). The role of the family has mostly been examined in studies on economic well-being arguing that households should be the center of analysis (see subsection 2.1.2), because other family members take part in the reaction to unemployment (DiPrete, 2002; DiPrete and McManus, 2000; Ehlert, 2012, 2013, 2016). Many researchers have examined the added-worker effect which refers to partners' adjustment of labor supply in case of unemployment by taking up work or increasing their hours in order to mitigate the negative income consequences (e.g., Bredtmann et al., 2017; Stephens, 2002). While changes in households' market activity have been extensively researched little is known about how couples alter their division of housework and total household production and how this depends on the gender of the unemployed person. As only few studies have been conducted, I here separately summarize their results.

Shamir (1986) conducted the first study on the effect of unemployment on households' division of housework. He used a small sample of unemployed Israeli academics that were aged 27 to 47 years and married and he excluded individuals with unemployed partners. Shamir (1986) collected two waves of data that were 6 month apart and respondents indicated for 11 tasks of housework, including neutral, female-typed, and male-typed activities and childcare, to which extent these were performed by themselves or their partners. The cross-sectional analyses showed that the unemployed compared to the employed performed more of the tasks mostly by themselves and no moderating effect of gender was found. Therefore, no such interactions were investigated in the longitudinal analyses which compared re-employed and continuously unemployed persons. The latter showed that re-employment reduced the extent of tasks that were mainly performed by the respondent and no important role of the duration of unemployment was found.

Ström (2002) used samples of unemployed and employed persons from different Swedish data with the former being re-interviewed after about a year. She focused on heterosexual cohabiting or married partners that lived together and her data included information on different tasks which, however, all reflected routine chores. Cross-sectional analyses showed that the unemployed had a higher domestic activity with stronger effects for women than men. Comparisons within the unemployed sample revealed that unemployment had a positive effect on respondents' housework share, but only if the spouse was employed with similar effects for males and females. Moreover, unemployment of the spouse only was associated with lower housework shares for women. In longitudinal analyses, Ström (2002) showed that finding a job decreased the domestic activities of the unemployed. Overall, the question whether and how these effects differ by gender received mixed answers depending on the analyses used.

Gough and Killewald (2011) used the Panel Study of Income Dynamics (PSID) for the US and focused on cohabiting and married couples that lived together for at least a year and were younger than 60 years. Responses were provided by one household member only and they estimated match-specific fixed-effects regression models. However, their models used all variation in employment status and did not distinguish transitions into and out of unemployment. The dependent variables were the weekly housework hours of the husband and wife and the question mostly addressed routine housework though “other work” was included and some respondents may have reported childcare responsibilities. They found that for both men and women unemployment increased own housework hours, with the effect being, however, twice as large for women compared to men. If partners were unemployed, own housework decreased with similar effects for men and women. Another relevant finding was that the total amount of housework increased. In additional analyses the authors showed that the effect of husbands’ unemployment was strongest for wives who worked full- or part-time compared to those being homemakers suggesting that changes in the household labor division depend on spouses’ relative labor market commitments.

Van der Lippe et al. (2018) provide the only comparative study. Based on data from the European Social Survey (ESS) in 2004 and 2010 and information on 28 countries they analyzed the effects of persons’ and partners’ employment status on persons’ time spent on housework, which was not further defined. Information on all variables was reported by one respondent. Using separate multilevel models for men and women they found that unemployed men and women spent more time on housework than their employed counterparts, but that the effect was larger for women. Unemployment of the partner had a smaller effect on own housework with men reducing their housework somewhat more than women if their partner was unemployed. The authors conclude from models that control for more variables that women spent more time in housework when their husband is unemployed and that men do not react to their wives’ unemployment. However, in these models they controlled for many of the mechanisms of unemployment including working hours such that these interpretations are rather problematic.

Although in recent years more studies have considered the effects of unemployment on housework and the research has become more elaborate, various *limitations* remain. First, the evidence is still scarce and most of the available studies rely on cross-sectional data or only use short-run panel data. Second, while the study by Gough and Killewald (2011) addressed this by using large-scale panel data and applying sophisticated longitudinal methods, it is restricted in several ways. Although it uses panel data it does not distinguish transition into unemployment, which may be regarded as a rough indication of job loss, from transitions out of unemployment. Moreover, while they examined the effects of unemployment of one partner on both spouses' housework and also considered changes in total household production, they did not differentiate domestic tasks. Third, although the literature often raises the question about how the effects of unemployment develop with the unemployment or non-employment duration expecting either a lagged adaption or an increasing withdrawal of men such predictions have not been tested with panel data so far.

In Article 5 *"Unemployment and housework in couples: Task-specific differences and dynamics over time"* my co-author and I *contribute* to this literature by studying the effects of unemployment on both partners' housework hours and their total housework production. In contrast to previous research, we use panel data and fixed-effects models that only focus on transitions into unemployment. By providing separate analyses for men and women we also offer evidence on competing theories about the gendered division of labor. Moreover, as we distinguish between female-, male-, and neutral housework tasks and examine how the effects change with the duration of unemployment or non-employment to test specific theoretical expectations, we arguably provide stronger evidence on the opposing theoretical perspectives than previous research. Specifically, we can distinguish in which areas partners adapt their housework and whether men follow the predictions of a lagged adaptation or an increasing withdrawal from housework.

2.5 Structure and contributions of this thesis

As highlighted in chapter 1 this is a cumulative thesis consisting of this overview article and five additional articles. The latter present theory-guided empirical studies which my co-author(s) and I have either published in international peer-reviewed journals, listed in the Social Science Citation Index (SSCI), or which have been submitted to journals. An overview is provided in Table 1. Because in the social sciences most journals have strict word limits details have often been left out of the main text. Therefore, each article has a supplementary material which is reproduced in this thesis and in the case of the published articles is also available on the journals' websites.

Table 1 Overview of the articles

#	Author(s)/Share	Year	Title	Journal/Status
1	Voßemer, J. / 100%	2018	Losing standard employment in Germany: The consequences of displacement and dismissal for workers' subsequent careers	Submitted to *Research in Social Stratification and Mobility*
2	Voßemer, J. / 100%	2018	The effects of unemployment on non-monetary job quality in Europe: The moderating role of economic situation and labor market policies	2nd revise and resubmit at *Social Indicators Research*
3	Voßemer, J. / 60% Schuck, B. / 40%	2016	Better overeducated than unemployed? The short- and long-term effects of an overeducated labor market re-entry	Published in *European Sociological Review*, 32(2), 251–265.
4	Voßemer, J. / 55% Gebel, M. / 15% Nizalova, O. / 15% Nikolaieva, O. / 15%	2018	The effect of an early-career involuntary job loss on later life health in Europe	Published in *Advances in Life Course Research*, 35, 69–76.
5	Voßemer, J. / 60% Heyne, S. / 40%	2018	Unemployment and housework in couples: Task-specific differences and dynamics over time	1st revise and resubmit at *Journal of Marriage and Family*

Source: Own illustration.

As highlighted throughout chapter 2 the five articles of this thesis take up limitations of previous studies to make several contributions to the areas of research they are located in. Next to these specific contributions, chapter 1 emphasized that this thesis' value lies in a comprehensive and interdisciplinary analysis of the economic and non-economic consequences of job loss and unemployment based on the life course perspective. Three overall contributions can be distinguished.

First, drawing on the life course perspective I develop a *general theoretical model* of the economic and non-economic consequences of job loss, unemployment, and inadequate re-employment (chapter 3). Although the life course perspective is not a theory in itself, because it does not explain why and how job loss, unemployment, and inadequate re-employment affect individuals' and families' careers and lives, its key principles allow putting the central concepts of this thesis into a logical order, make assumptions about how they affect each other, and highlight the interrelations between the theoretical mechanisms. For example, it relates the economic and sociological labor market theories focusing on the career domain with interdisciplinary theories in the health and well-being and family domains. In addition to presenting a general theoretical model, chapter 3 also offers an overview and summary of the theoretical mechanisms and hypotheses of the five articles.

Second, I examine the economic and non-economic consequences of job loss, unemployment, and inadequate re-employment using an *encompassing evaluation design* motivated by the life course perspective (chapter 4). Specifically, I investigate different, but interrelated life domains and consider multiple outcomes within each of these. This not only allows understanding how different life domains affect each other, but also makes it possible to take account of the multidimensionality of each life domain as well as to consider complementarities and trade-offs. All articles either examine the effects of job loss, unemployment, and inadequate re-employment on more than one outcome in the career domain or investigate how changes in the career domain go along with changes in the health and well-being and the family domains. Furthermore, other key principles of the life course perspective point to the importance of treatment and effect heterogeneity. To take account of the former I provide clear definitions of treatments and control groups for all articles. In the majority of the articles my co-authors and I also empirically examine treatment heterogeneity by considering the reason for job loss or the duration of unemployment or non-employment. With respect to effect heterogeneity three of the five articles explicitly test hypotheses about the moderating effects of worker- or context-level characteristics. Most importantly, research based on the life course perspective must take into account that the effects of life events may be transitory or lasting, because the longevity of effects plays an important role in the accumulation of advantages and disadvantages. Therefore, all articles take a medium- to long-term perspective and in the majority of these my co-authors and I study the dynamics in the outcomes over time. In one article, we explicitly examine whether the effects of job loss are persistent through triggering further mobility-reinforcing events.

Third, *state-of-the-art methods* of causal and multi-level analysis are applied (chapter 4). These are chosen against the background of the research questions and the theoretically derived hypotheses. In line with the life course perspective, in the majority of the articles prospectively collected panel data are used. In one article my co-authors and I use retrospectively collected life history data and in another article I rely on repeated cross-sectional data with some retrospective questions. Therefore, all data include at least basic information on the timing of the independent and dependent variables allowing for a clear temporal order and assessing the longevity of effects. In the majority of the articles, the data further make it possible to examine the duration of specific states and the dynamics in outcomes over time. The methods of causal analysis include (dynamic) (propensity score) matching, fixed-effects panel regressions, and combinations of these approaches. In chapter 4 these methods are discussed against the background of the potential outcomes framework, which has become the backbone of causal analysis in the social sciences. Two articles also use data with multi-level structures. In chapter 4 I explain different motivations of and approaches to multi-level analysis taking account of recent methodological debates. Specifically, in Article 2, which examines the moderating role of structural and institutional contexts, I apply the very flexible two-stage approach to multi-level analysis allowing for different relationships between the independent and dependent variables in each context. Moreover, this article draws on repeated cross-sectional data to apply the logic of a longitudinal within-unit analysis at the macro-level by examining changes in countries over time.

3. Theory and hypotheses

In section 3.1 I present a general theoretical model of the economic and non-economic consequences of job loss, unemployment, and inadequate re-employment. Using the life course perspective, I put the central concepts of this thesis into a logical order, make assumptions about how they affect each other, and highlight the interrelations between the theoretical mechanisms. In sections 3.2 to 3.5, I review the theoretical mechanisms underlying each article and provide an overview and a summary of the theoretical mechanisms and hypotheses.

3.1 A general theoretical model

Figure 2 shows a general theoretical model of the economic and non-economic consequences of job loss, unemployment, and inadequate re-employment. It specifies Figure 1, which highlighted the key principles of the life course perspective, by focusing on the central concepts of this thesis.

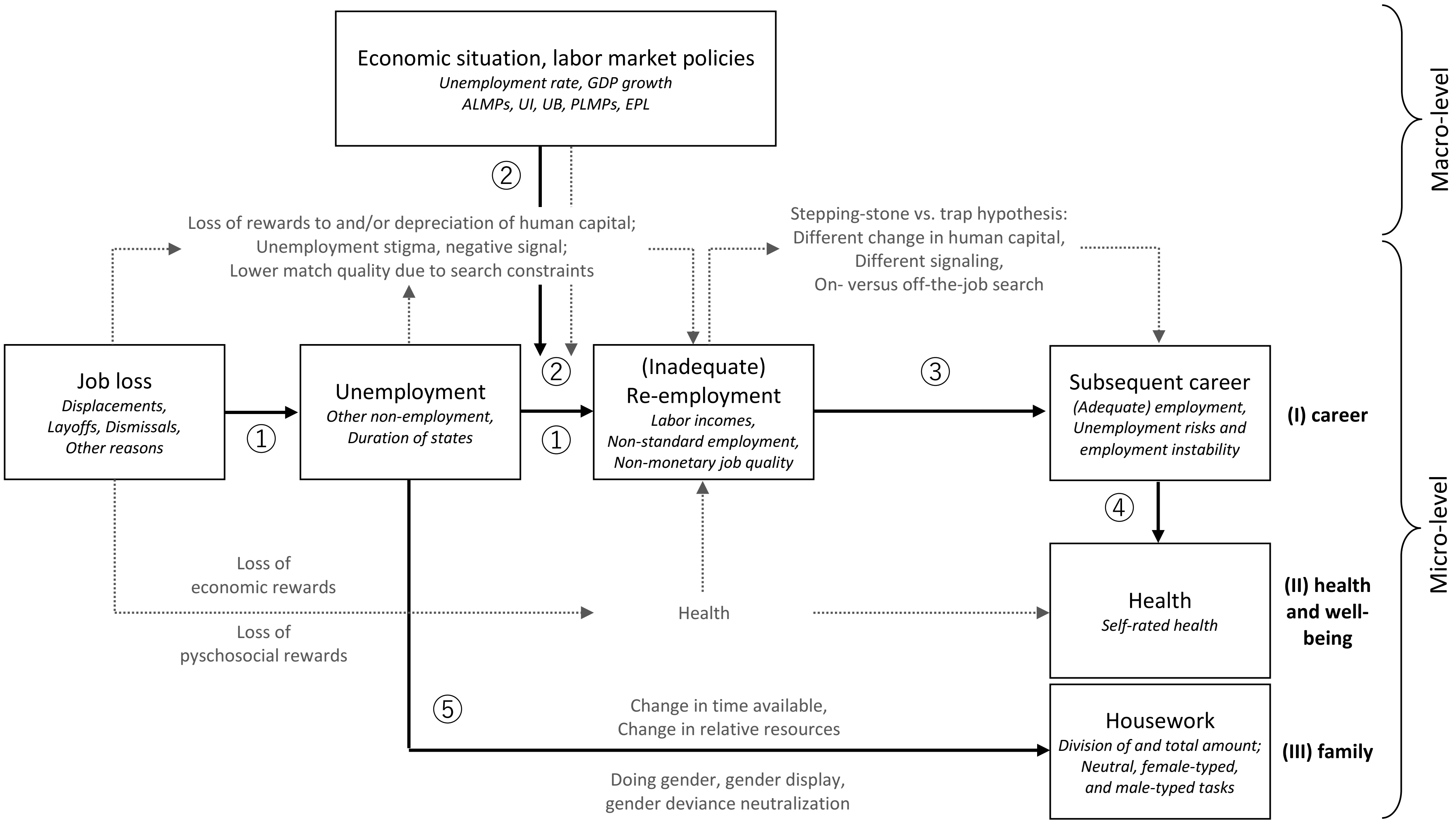
Based on the key principles of time (P1), cumulative advantages and disadvantages (P4), and human agency (P5), the central concepts (boxes) of “Job loss”, “Unemployment”, “(Inadequate) Re-employment”, and “Subsequent career” are put into a logical order and assumptions about how they affect each other are shown as solid black arrows. The italic texts in the boxes specify the empirical content of the central concepts. Figure 2 shows that job loss can trigger unemployment (mobility-reinforcing event), but that individuals may react to this in terms of (inadequate) re-employment (counter-mobility event). The decision for or against (inadequate) re-employment compared to remaining unemployed and continuing the job search is assumed to affect workers’ subsequent careers. Drawing on the key principle of historical time and place (P6), Figure 2 also shows that the economic situation and labor market policies are expected to moderate the assumed effects at the career domain.

Moreover, the ideas of different, but interrelated life domains (P2) and linked lives (P3) are exemplified by three different levels for the career (I), health and well-being (II), and family (III) domains and also by the assumed effects between these. As stated in section 2.5, the life course perspective itself is not a theory. Therefore, Figure 2 adds the theoretical mechanisms (grey dotted arrows and grey text) that are thought to bring about the assumed effects.[23] They are based on economic and sociological labor market theories for the career domain and interdisciplinary theories for the health and well-being and family domains. Their details are discussed in sections 3.2 to 3.5. However, the life course perspective suggests that the theoretical mechanisms put forward in one life domain are also important for understanding the consequences in other life domains. Therefore, in the following I locate each article within the general theoretical model to further highlight the interrelations between the theoretical mechanisms. The focus of each article is illustrated by the circled numbers in Figure 2.

Article 1 examines the effects of displacements due to plant closures and dismissals on workers' subsequent careers measured by their labor market statuses, labor incomes, and non-standard employment risks. In Figure 2 this corresponds to the path "Job loss"–"Unemployment"– "(Inadequate) Re-employment". The theoretical micro-level mechanisms rest on three different, but interrelated labor market theories (see subsection 3.2.1) including human capital, signaling, and job search theories. In short these theories state that the loss of rewards to and the depreciation of human capital, unemployment stigma or negative signaling, and a lower match quality due to search constraints explain the negative effects on multiple outcomes of the career domain. Article 2 uses the same theoretical micro-level mechanisms, because it investigates the effects of unemployment on non-monetary job quality. However, as it takes a comparative perspective to examine how the effects vary across countries and time, I also theoretically argue that depending on the economic situation and different labor market policies these micro-level mechanisms are strengthened and weakened (see subsection 3.2.2). This is shown in Figure 2 by the grey dotted arrow that starts at "Economic situation, labor market policies" and passes through the micro-level mechanisms (grey dotted text) that explain the effect of "Unemployment" on "(Inadequate) Re-employment". The same

[23] They are shown as grey dotted arrows, because they are not tested empirically. In some instances, different theoretical arguments or mechanisms predict different effects, allowing assessing their relevance indirectly. An example of this is presented in Article 5, which explains why no grey dotted arrow is used here.

Figure 2 A general theoretical model



Notes: Boxes are the central concepts and solid black arrows show the assumed effects. Italic texts in boxes specify the empirical content of the central concepts. Grey dotted arrows and grey text show and explain the theoretical mechanisms. The circled numbers highlight the focus of each article of this thesis.
Sources: Own illustration.

theories re-appear in Article 3 that investigates the career consequences of an overeducated re-entry in comparison to remaining unemployed and continuing the job search. In contrast to Articles 1 and 2, the interpretation of the theories is, however, more ambiguous with the stepping-stone and trap hypotheses making opposing predictions about each states opportunities for the development of human capital, for signaling one's productivity to future employers, and for successful job searches (see section 3.3).

Article 4 focuses on the long-term effects of job loss on health. One explanation for these is that the direct effect, once it occurred, persists over time. Figure 2 shows that the direct effect is explained by the loss of economic and psychosocial rewards (grey dotted line). Another mechanism states that the long-term effects are due to changes in the subsequent career such as increased unemployment risks or employment instability. In Figure 2 this is highlighted by the indirect path "Job loss"–"Unemployment"–"(Inadequate) Re-employment"–"Subsequent career"–"Health". This emphasizes the interplay between the economic and non-economic consequences of job loss and also shows the interrelations between their theoretical mechanisms (see section 3.4). Article 5 concerns the effects of unemployment on couples' division of housework and total household production. Figure 2 omits some apparent theoretical links reflecting, for example, the idea that re-employment depends on and results in changes in the domestic sphere. These are ignored, because the empirical analyses focus only on workers who become unemployed and remain non-employed to test specific hypotheses that follow from theoretical debates in the research on the division of labor (see section 3.5).

3.2 The economic consequences of job loss and unemployment

In subsection 3.2.1 I review the micro-level mechanisms explaining why job loss and unemployment have negative effects on workers' careers. These mechanisms are the foundation for Articles 1 and 2. Subsection 3.2.2 explains how countries' economic situation and labor market policies strengthen or weaken the micro-level mechanisms, to derive hypotheses about their moderating role, which will be tested in Article 2. Table 2 provides an overview about the theoretical mechanisms and a summary of the respective hypotheses. It numbers the former from (1) to (11) and I refer to these in the following sections or subsections.

3.2.1 Micro-level mechanisms

Articles 1 and 2 examine the negative effects of job loss and unemployment on workers' careers. One explanation for these is based on human capital theory (Becker, 1993) assuming that workers who lost their job and change firms, occupations, or industries (1) lose rewards to their specific human capital. It is sometimes further argued that periods of non-employment, following from job loss, result in the depreciation of general skills. Another explanation rests on signaling theories highlighting the problem of asymmetric information between applicants and employers (Spence, 1973). Specifically, they assume that employers infer applicants' unobserved productivity from their work biographies potentially resulting in (2) unemployment stigma. It also has been highlighted that the negative signal associated with job loss and unemployment should be stronger for layoffs or dismissals, because, in contrast to plant closures, firms have some discretion about which employees are let go, offering information to future employers (Gibbons and Katz, 1991).[24] Job search theories provide an additional explanation assuming that job-seekers decision to accept or reject a job offer depends on their reservation wage (Mortensen, 1986). The latter is in part determined by search constraints including the number of incoming job offers and workers' financial resources. For example, if jobs that match their skills and qualifications are rare and benefits and private incomes run out or are used up over time, workers will reduce their reservation wage and accept lower quality job offers. Since job searches that follow after job loss are subject to (3) greater constraints than those that follow after voluntary job separation or education and inactivity the quality of the formed matches is lower.

As Table 2 summarizes, these theories suggest that job loss and unemployment have negative effects on multiple dimensions of workers careers. Specifically, Article 1 examines subsequent employment chances, different labor incomes, including earnings and wages, and non-standard employment. In this article I also test whether the effects are larger for displacements due to plant closure than dismissals. In Article 2 the focus is on unemployment and its effects on four different facets of non-monetary job quality.

3.2.2 Macro-level moderators

The literature review in section 2.1 suggested that the negative effects of job loss and unemployment on workers' subsequent careers may vary according to countries' institutional setups. Four factors that are considered theoretically and which are examined empirically in Article 2 with respect to non-monetary job quality are the economic situation, ALMPs, UBs, and EPL. Table 2 summarizes the hypotheses and their derivation is explained in the following.

[24] While applicants may not provide the information about job loss let alone the specific reason in the written applications, periods of unemployment or at least non-employment are apparent from résumés. Information about job loss and specific reasons may further be available publicly (e.g., plant closure, mass layoff), from reference letters or through inquiries from employers.

It can be assumed that a poor economic situation increases the negative effects, because (3) fewer available job offers reduce workers' reservation wages resulting in a lower job quality (e.g., Gangl, 2006). If workers must change firms, occupations, or industries to find a job, (1) the loss of rewards to specific human capital is one explanation for this. The (2) unemployment stigma mechanism is ambiguous: while a poor economic situation increases unemployment duration and, thereby, stigma, individual unemployment is also less informative about individuals' productivity in these contexts (e.g., Kroft, 2013).

Generous UBs are expected to decrease the negative effects, because according to job search theory they (3) act as a search subsidy (Burdett, 1979) that increases workers' reservation wages leading to a higher job quality. This also means that workers are less likely to (1) lose rewards to specific human capital. While generous UBs may also increase (2) unemployment stigma due to longer durations, this effect can be assumed to be weak, because employers in these countries know that prolonged job searches are not necessarily due to workers' low productivity, but rather their high bargaining power.

Support through ALMPs is assumed to buffer the negative effects of unemployment by counteracting (1) the loss of rewards to specific and the depreciation of general skills. Specific education updates workers' skills to changing demands (Gangl, 2006) reducing the losses associated with changes of occupations or industries.

General education and on-the-job training can be assumed to stop the depreciation of general skills. The effects are likely to be more beneficial if programs are tailored to unemployed persons' needs. ALMPs may also reduce (2) unemployment stigma, although it is occasionally argued that in some countries participation in specific programs rather represents a negative signal. Wage subsidies in the private sector or direct employment programs in the public sector are also expected to reduce the negative effects, because in line with job search theory they (3) increase the number of available job opportunities. Another labor market policy is EPL for regular employment which reflects the extent of job security provisions. Stricter EPL increases the costs of firing and indirectly also of hiring workers (Gangl, 2006). Job search theory states that this should (3) reduce labor market dynamics and increase unemployment durations. Prolonged unemployment increases the risk to (1) lose rewards associated with specific human capital, as it will become more difficult to locate re-employment that makes use of such skills. It will also lead to (1) a greater depreciation of general skills and (2) a stronger unemployment stigma.

Table 2 Overview and summary of the theoretical mechanisms and hypotheses

#	Theoretical mechanisms	Hypotheses about total effects and heterogeneity or mediation [explanation]
1*	(1) Loss of rewards to and/or depreciation of human capital; (2) Unemployment stigma, negative signal; (3) Lower match quality due to search constraints	Total effects: (-) employment, total labor income, earnings and wages, (+) non-standard employment Heterogeneity: [s] for dismissals than displacements [stronger (2)]
2	Micro-level: (1), (2), (3) Macro-level: Arguments about how micro-level mechanisms are strengthened or weakened based on economic situation and labor market policies	Total effects: (-) occupational status, job autonomy, job authority, job security Heterogeneity: [s] the poorer the economic situation [stronger (3) and (1)] [w] the more generous the UBs [weaker (3) and (1)] [w] the more support received through ALMPs [weaker (1), (2), and (3)] [s] the stricter the EPL for regular contracts [stronger (3), (1), and (2)]
3	Stepping-stone (SSH) and trap (TH) hypotheses interpret and weigh (dis-)advantages differently in terms of: (4) Different change in human capital, (5) Different signals, (6) On vs. off the job search	Total effects: (Adequate) employment chances Stepping-stone: (+) — Trap: (-) (4.1) Maintenance and/or acquisition — (4.2) Loss and/or depreciation, lock-in (5.1) Positive signal: motivation — (5.2) Negative signal: aspiration (6.1) On-the-job: job shopping, networks — (6.2) Off-the-job: intensity Heterogeneity: [s] stepping stone for younger than older [stronger (5.1)] [s] stepping stone for academically qualified than vocationally trained [stronger (5.1)]
4	(7) Direct and persistent health effects (8) Indirect effects via negative career consequences	Total effects: (-) health Mediation: [w] after controlling for indicators of negative career consequences [no (8)]
5	(9) Gender-neutral perspective: change in time available, change in relative sources (10) Gender-based perspective: doing gender, gender display (10.1), gender deviance neutralization (11) inertial mechanisms	Total effects: (+) on persons', (-) on partners', (+) total housework hours Heterogeneity: [s] effects for men than women [stronger (9)] [s] effects for women than men [stronger (10)] [s] effects for men (women) in male- (female-) typed tasks [stronger (9) and (10)] Only men: [s] effects the longer the non-employment duration [stronger (10)] Only men: [w] effects the longer the non-employment duration [stronger (11)]

Notes: *Article 1 does not explicitly state the listed hypotheses. Theoretical mechanisms are numbered in parentheses and referenced throughout sections 3.2 to 3.5. Hypotheses about total effects: (+) positive effects, (-) negative effects. Hypotheses about heterogeneity or mediation: [s] stronger effects, [w] weaker effects, texts in brackets explain why effects are expected to be stronger or weaker.
Sources: Own illustration.

3.3 The economic consequences of inadequate re-employment

The comparison of an overeducated re-entry and unemployment is also based on arguments from human capital, signaling, and job search theories. However, as highlighted in section 3.1 and shown in Table 2, depending on the interpretation of these theories, two opposing predictions can be distinguished. According to the stepping-stone hypothesis (SSH) taking up an overeducated job helps (4.1) maintaining and acquiring human capital. Sicherman and Galor's (1990) career mobility theory explicitly states that investments in work experience through overeducation increase internal and external promotion probabilities. In contrast the trap hypothesis (TH) suggests that an overeducated re-entry leads to (4.2) the acquisition of wrong human capital resulting in lock-in effects in inadequate employment and it even has been argued that workers adapt to lower job requirements and lose cognitive abilities (De Grip et al., 2008). With respect to signaling the SSH maintains that overeducation provides (5.1) a more positive signal than unemployment as it highlights job-seekers motivation. However, the TH states that it may also (5.2) signal workers lower aspirations (McCormick, 1990). Finally, both hypotheses differ in their assessment of the (dis-)advantages of an on- versus off-the-job search. The SSH states that (6.1) an on-the-job search is more efficient, as it provides access to more information on adequate jobs, for example, through networks. In contrast, the TH highlights (6.2) the higher search intensity of an off-the-job search. Because it can be assumed that the positive signal prevails among workers who have accumulated less labor market experience or possess less specific skills, Article 3 also examines the hypotheses that an overeducated re-entry is associated with more positive effects among younger than older unemployed as well as those with academic qualifications than those with vocational degrees.

3.4 The effects of job loss and unemployment on health and well-being

The direct or short-term effects of job loss and unemployment on health are theoretically attributed to the loss of economic and psychosocial rewards associated with employment (Nordenmark and Strandh, 1999). The loss of income is argued to restrict individuals' agency and also forces adjustments to the standard of living. This may affect physical health through increases in health-damaging and decreases in health-promoting behaviors (e.g., Bartley, 1994). Changes in mental health are explained by unemployment depriving individuals of the psychosocial rewards of employment. Moreover, jobs are often important to individuals' identity by providing a major social role. The different health consequences are also interrelated. For example, Korpi (2001) states that psychological problems can over time accumulate into physical health problems.

How can the potential long-term effects be explained? As Table 2 shows the life course perspective distinguishes two basic models (e.g., Strandh et al., 2014). The first perspective assumes that (7) the direct negative effects, which theoretically may originate from the loss of economic and psychosocial rewards, persist over time. In contrast, a second perspective argues that job loss and unemployment (8) provoke a "chronic stress process" (Burgard et al., 2007: 370) or a "social chain of risks" (Brydsten et al., 2015: 799). In this view, the negative effects are not explained by a persisting direct effect, but rather by an initial event resulting in negative economic as well as health consequences that in their interplay produce poor and declining health across the life course. This second perspective is closely linked to the idea of cumulative advantages and disadvantages. It also theoretically integrates research on the career by considering higher unemployment risks, employment instability, and lower job quality in the subsequent career as mechanisms explaining the negative effects of an early-career job loss on later life health.[25] Table 2 lists this mediation hypothesis, which is tested in Article 4, and states that the effect of job loss on health should become smaller or vanish once the negative career effects are taken into account.

3.5 The effects of job loss and unemployment on partners' division of housework

Theories about the division of housework and additional theoretical mechanisms are used in Article 5 examining couples' reallocation and changes in total housework after one partner's unemployment. Table 2 shows that these theories can be sorted into a (9) gender-neutral and a (10) gender-specific perspective. The former comprises time-availability and relative resources hypotheses that are derived differently in the literature (Bianchi et al., 2000). These state that spouses' roles in housework are due to their different time constraints and their relative resources and productivities in the market and domestic spheres. Based on these, it is expected that job loss shifts housework to the unemployed person and away from the partner, as time and relative resources change. While these arguments are per se gender-neutral, men work longer and earn more meaning that they (9) should experience greater gains in time and larger losses in relative resources. This suggests that the positive (negative) effect on the person's (partner's) housework is larger for men than women. However, a contrary hypothesis is derived from sociological doing gender theories arguing that women and men perform or avoid housework to symbolically enact their femininity and masculinity (Berk, 1985; West

[25] Moreover, this perspective assumes that the direct and ongoing negative effects on health may vice versa affect workers' lower chances for (adequate) re-employment, reinforcing the negative effects through a combination of social causation and health selection.

and Zimmermann, 1987). Two specific versions are Brines' (1994) gender display and Greenstein's (2000) gender-deviance neutralization hypotheses suggesting that (10) men who economically depend on their partners do less housework than what would be expected based on rational grounds, while breadwinning wives do more domestic work to display their gender and compensate for norm deviations. From these arguments it is expected that the positive (negative) effect of job loss on a person's (partner's) housework is larger for women than men, because for the former unemployment results in norm compliance instead of deviation. For various reasons increases in the total household production are also likely: households have less income to "outsource" housework, a more frequent and extensive use of the home increases the amount of housework, a less efficient use of persons' time due to lower opportunity costs, and the take up of tasks that were deemed unnecessary or simply neglected before (Gough and Killewald, 2011; van der Lippe et al., 2018).

Although the theoretical discussion so far already extends previous studies, two further points that have often been neglected are different tasks of housework and the role of the duration of unemployment or non-employment following a job loss. Theoretically both the gender-neutral and gender-specific perspectives provide arguments for the idea that men and women increase their housework upon job loss more strongly in male- or female-typed activities respectively. While the gender-neutral perspectives explain this by reference to a specialization argument arguing that (9) partners not only have comparative advantages with respect to paid and unpaid work but also for different tasks within the domestic sphere, the gender-specific perspective highlights that (10) societies also hold expectations about which tasks reflect masculinity or femininity next to ideas about who should perform the housework. Therefore, spouses are expected to disproportionately increase their time in those tasks that they are able to perform more efficiently or that demonstrate more clearly their belonging to a specific gender category. A last theoretical argument concerns the duration of unemployment or non-employment. Table 2 list two different mechanisms. The classical argumentation of Brines (1994) states that (10.1) prolonged unemployment increases men's distress over norm deviation such that over time they give up more and more on domestic tasks with their female partners having to take over responsibility. This is in stark contrast to another theoretical perspective that looks at the duration of non-employment and suggests that for men's job losses (11) inertial mechanisms hinder immediate changes (Gershuny et al., 2005). A lagged adaptation is more likely because habits are difficult to change, skills for housework must be built, and the gendered meaning of housework must be challenged.

4. Research designs

In this chapter I describe the research designs of the five articles of this thesis and explain the choices regarding their specific characteristics. These include the data and samples and the definition of the independent and dependent variables used (section 4.1) as well as the methods applied (section 4.2). The specific characteristics are associated with certain strength and weaknesses, which I highlight by discussing similarities and differences between the articles and by drawing comparisons to the research designs used in the respective areas of research.

4.1 Data, samples, and independent and dependent variables

Table 3 provides an overview and summary of the research designs of the five articles. It shows that all micro-data include at least basic information on the timing of the independent and dependent variables allowing for a clear temporal order and assessing the longevity of effects. However, it also makes clear that the detail of this information differs, because the data range from prospectively collected panel data (Articles 1, 3, 5), over retrospectively collected life history data (Article 4) to cross-sectional data with some retrospective questions (Article 2). In subsection 4.1.1 the choices of these data are explained against the background of the life course perspective, the research questions and theoretically derived hypotheses as well as available alternative data. Subsection 4.1.2 further highlights the advantages and disadvantages the data imply for the measurement of job loss and unemployment. Subsection 4.1.3 explains how the data allow taking account of the idea of different, but interrelated life domains and their multidimensionality as well as to examine complementarities and trade-offs. The implications of the data for the methods of causal and multi-level analysis applied are discussed in section 4.2.

4.1.1 Data and samples

Articles 1, 3, and 5 are based on the *Socio-Economic Panel (SOEP)*, which offers *prospectively collected panel data* designed to be nationally representative of the German adult population living in private households (Wagner et al., 2007). The articles draw on data from 1984 (first wave) to 2015.[26] The SOEP is well-suited for this thesis, as it is itself based on the life course perspective. It interviews all household members aged 16 years and older and offers large-scale and long-run panel data in different, but interrelated life domains. The SOEP also

[26] The articles use different waves or years of the SOEP, because not all variables are available in all waves or years.

Table 3 Overview and summary of the research designs

#	Micro-data	Sample	Independent variables	Dependent variables	Methods
1	SOEP[a], 1988-2015, Germany	20-60 years, employees, standard employment	T: job loss: displacement due plant closure or dismissal; C: no T, but other job changes	Labor market statuses (5), Labor incomes (3), Non-standard employment (3); Time: -3 to +5 waves/years	Logistic regressions; Exact and propensity score matching with fixed-effects regressions with impact function
2	ESS[b], 7 rounds, 2002-2014, 34 countries, 164 country-rounds[*]	20-64 years, employees, no armed forces	T: unemployment of three months or more in the last five years; C: no T	Occupational status, autonomy, authority, job security; Time: [0 to +5 years]	Multiple imputation; Coarsened exact matching and two stage multi-level models (with country fixed-effects)
3	SOEP[a], 1984-2012, Germany	18-54 years, unemployed persons after job loss, at least vocational degree	T: overeducated re-entry in a specific month; C: remaining unemployed and continuing job search for at least one month	Employment chances, Adequate employment chances; Time: -2 to +5 waves/years	Discrete-time hazard model with independent competing risks and a piecewise constant specification; Exact and dynamic propensity score matching
4	SHARE[c], SHARELIFE, 2008-2009, 14 countries	≥ 50 years, left education/ first job between 11/14-35 years, no self-employment or retirement in the early-career	T: job loss in the early-career: plant closure or layoff; C: no T, but other job changes (+ continuous employment)	Self-rated health; Time: [+16 to 77 years]	Logistic regressions with country fixed-effects and average marginal effects; Sequence analysis
5	SOEP[a], 1991-2015, Germany	Both partners' at most 54 years, heterosexual couples living together	T: job loss of one partner: employed to unemployed; C: continuous employment of this partner	Both partners' total and task-specific housework hours (4); -3 to +4 waves/years	Fixed-effects regressions with impact function

Notes: [a] SOEP: Socio-Economic Panel, [b] ESS: European Social Survey, [c] SHARE: Survey of Health, Ageing and Retirement in Europe. [*]This analysis also uses macro-data from different sources for 26 countries and 124 country-rounds: see Article 2 and the supplementary material for details.
Sources: Own illustration.

collects retrospective data on individuals' and families' lives in-between interviews and on the time before their participation. It further has the advantage to include detailed information about workers' careers. This allows for clear definitions of job loss and unemployment, making use of the reason for job loss (see subsection 4.1.2), and the use of an extended subjective measure of inadequate employment which resolves many of the problems of other measures (see Article 3). The SOEP further offers unusually detailed information about the dependent variables of interest such as measures of labor market status, labor income, non-standard employment or inadequate employment (see Articles 1, 3) and task-specific housework hours (see Article 5).

As all articles are based on the life course perspective, they make similar use of some of the advantages of the SOEP: First, the SOEP not only allows for a clear temporal order of the variables of interest, but also to examine the outcome dynamics for several years around job loss, unemployment, or inadequate re-employment. This is important, because it makes it possible to examine whether the effects of life events are transitory or lasting, which itself provides information about the potential for cumulative advantages and disadvantages (P4), as the longevity of effects is an important precondition (P1: time). It also allows taking account of the duration of unemployment or non-employment (P1: time). Second, all articles investigate multiple outcomes within their respective life domains (see subsection 4.1.3). Third, they all apply sophisticated methods of causal analysis by making explicit use of outcome measures before and after the life events of interest (see subsection 4.2.1).

Some differences in the use of the SOEP can be highlighted, too. While all articles are interested in job loss, they differ in their definitions. Only Article 1 distinguishes different reasons and focuses on job displacements due to plant closures and dismissals. Although there are theoretical arguments for Articles 3 and 5 to define job loss as the transition from employment to unemployment, the main reasons are practical. These articles use specific features of the SOEP (i.e., monthly data on labor market status, partner data) that make adding the information on the reason for job loss difficult, as this results in too small sample sizes. The theoretical and methodological advantages and disadvantages of different definitions will be further discussed in subsection 4.1.2.

Table 3 shows that the articles further differ in their sample restrictions. While Article 1 focuses on workers who are employed and, therefore, can lose their job, Article 3 only examines those who already had a transition from employment to unemployment, because it is interested in labor market re-entry. Article 5 extends the individual-level focus of Articles 1 and 3 by making full use of the fact that the SOEP is a household panel. Specifically, it links partners who live together via specific identifiers to construct longitudinal couple data. As it examines the gender-specific reaction to one partner's unemployment with respect to the division of housework and the total household production, the analyses are restricted to cohabiting heterosexual couples. The remaining sample restrictions are explained in the respective articles.

Article 4 uses *retrospectively collected life history data* from *SHARELIFE*, which is the third wave (2008-2009) of the Survey of Health, Ageing, and Retirement in Europe (SHARE) panel study (Börsch-Supan et al., 2013). The SHARE data are based on individual or household probability samples and the target population consist of all persons aged 50 years and over with a regular domicile. Current partners living in the same household were interviewed regardless of age. SHARELIFE retrospectively collected the life histories for all individuals who participated in the first two waves of SHARE and the following 14 countries are included: Austria, Germany, Sweden, Netherlands, Spain, Italy, France, Denmark, Greece, Switzerland, Belgium, Czech Republic, Poland and Ireland.

As my co-authors and I are interested in the long-term health effects of job loss and unemployment, SHARELIFE has a number of advantages over other data. In contrast to household panel surveys, where the long-term effects label usually implies following individuals for up to five years, SHARELIFE makes it possible to focus on job loss in the early-career and examine health, on average, for more than 30 years later allowing to take the ideas of timing and, especially, the longevity of effects (P1: time) very serious. Second, in contrast to the cohort studies used in previous research (see subsection 2.3.2), the data include information on the reason for job loss, distinguishing between displacements due to plant closures and layoffs, which has both theoretical and methodological advantages. Moreover, by covering 14 European countries instead of specific birth or school-leaver cohorts, it allows testing whether findings of earlier studies can be extrapolated to a much broader population. Third, in line with the life course perspective, SHARELIFE also includes an unusual amount of detail about workers early-life circumstances, which can be used to construct an extensive set of control variables. It also covers workers' whole careers making it possible to examine to what extent the potential negative effects of an early-career job loss are mediated through increased subsequent unemployment risks or employment instability offering some insights into processes of cumulative advantages and disadvantages (P4).

However, compared to the panel data arising from cohort studies, the use of retrospective information for 14 different countries also implies some methodological limitations for causal analysis (see subsection 4.2.1) and the need to take account of the nesting of individuals within countries in multi-level analysis (see subsection 4.2.2). Another disadvantage may be that the data only offer limited information on the dependent variable health (see subsection 4.1.3). Moreover, the implications of recall errors and the issue of survivor bias associated with retrospective data have to be considered (see section 5.1). As interest is in the effect of an early-career job loss, the sample is restricted to individuals who hold at least one job during that time (see Table 3), with the specifics being explained in Article 4.

Article 2 is based on the first seven rounds (2002-2004) of the *European Social Survey* (ESS) offering *repeated cross-sectional data with some retrospective questions*. It is a comparative survey that is collected biannually and the majority of countries participate repeatedly. In each country and round the population of interest is persons aged 15 and over living within private households and the data are based on random probability samples. The ESS was explicitly created to provide comparable data across countries and time and standardizes all important aspects of survey methodology (Fitzgerald and Jowell, 2010). The ESS has some advantages over alternative data with respect to the comparative research questions raised in Article 2. It not only provides data about past unemployment and non-monetary job quality, but also has been attested a high quality.[27] Second, the ESS provides information for a large number of countries making it possible to use a quantitative comparative approach based on multi-level analysis (see subsection 4.2.2). Specifically, I aimed at using as many country-rounds as possible, because reliable estimates of the effects of macro-level variables require a sufficient number of macro-level observations. (Bryan and Jenkins, 2016). Previous studies have used small-N panel data, such as the European Community Household Panel (ECHP) or data from different household panel surveys, and applied a qualitative comparative approach (e.g., Dieckhoff, 2011). While these studies highlight differences in the effect of unemployment for a handful of countries and offer interpretations about potential reasons for this, they are not able to quantify the moderating role of specific macro-level variables, because countries differ on more than one relevant factor.

[27] Next to winning the Descartes Price of the EU for its advancements in survey research, an evaluation of the sampling quality by Kohler (2008), comparing the ESS 2002 with four other European comparative surveys, suggest that it performed best on all of four separate criteria (i.e., documentation, sampling process, external criteria for representativity, internal criteria for representativity).

Third, the ESS has the advantage of being a repeated cross-sectional survey with most countries participating more than once. This has also been labelled "comparative longitudinal survey data" (Schmidt-Catran and Fairbrother, 2016: 23) and similar to a within-person analysis at the micro-level it allows for a within-country analysis at the macro-level strengthening the comparative analysis of Article 2 (see subsection 4.2.1).

While the ESS is well-suited to address individuals nesting in structural and institutional context (P6: historical time and place) and, thereby, provides information about the potential for preventive intervention (P7), the information on the timing on the independent variable at the micro-level only concerns a question about past unemployment. This implies some limitations for the definition of job loss and unemployment used (see subsection 4.1.2) as well as the methods of causal analysis applied (see subsection 4.2.1). Given these issues it may be asked why no large-N comparative panel or life history data were used. Unfortunately such data are not available.[28]

The ESS has been complemented with time-varying macro-data to measure countries economic situation and labor market policies. These data have been collected from different sources and the details can be found in Article 2 and, in particular, in supplementary material 3. The sample was restricted to workers in dependent employment, because the theoretical derivations do not directly apply to self-employed workers. Workers in the armed forces were excluded, because for these occupational status cannot be measured.

While the preceding discussions highlight the trade-offs in the choice of data and their respective implications by making comparisons between the articles and to the state of the research, one question remains: Why do the articles only rely on survey data, whereas the literature reviews suggest the frequent use of administrative data? As discussed in subsection 2.1.1 studies on the economic and in rare cases also the non-economic consequences of job loss can be based on both administrative and survey data and each implies advantages and disadvantages with respect to the specific characteristics of the research design (Kuhn, 2002;

[28] Only the European Union Statistics on Income and Living Conditions (EU-SILC) offers micro-level panel data (although only for up to four years) for a large number of countries. However, it has no information about non-monetary job quality and, in contrast, to comparative surveys is based on the idea of a "common framework" as opposed to a "common survey" meaning that there exist "common procedures, concepts and classifications, including harmonised lists of target variables" (Wolff et al., 2010: 40), but each country is granted flexibility in the provision of the data. For example, some countries use administrative data, while others use survey data and some use one or more existing data sources, while others implement new harmonized surveys. Accordingly, countries differ in many survey methodological aspects (e.g., sampling designs, fieldwork periods, reference periods, data collection, questionnaires) raising some issues regarding the comparability of the data.

OECD, 2013; van Wachter, 2010).[29] The main disadvantage of administrative data and the reason they were not used is the very limited information on the outcomes that are of interest, mostly because the processes they originate from (e.g., tax data, social security data, unemployment benefit data) are not aligned with researchers' interests. For example, the German administrative data do not or only recently have started to include information on labor market statuses other than dependent employment or registered unemployment. The same applies to information on income from different sources, working hours, and non-standard employment. The way the data arise further makes clear that no subjective information such as self-rated health or data about households members' time-use and specifically housework hours is available. Also quantitative comparative projects would require the harmonized use of administrative data across a large number of countries (e.g., Kuhn, 2002; OECD, 2013). Potential disadvantages of not using administrative data are the rather low sample sizes coming along with survey data. They imply a lower precision in the estimates and some additional trade-offs between different research interests.[30] A solution for future research, which is beyond the scope of this thesis, may be the combination of administrative and survey data.

4.1.2 Independent variables

The independent variables of all articles concern job loss and unemployment.[31] As mentioned previously, different definitions of job loss and unemployment have been given. Following Brand (2015), I here distinguish some ideal definitions before considering the use in the literature and this thesis. However, as Brand (2015: 360) notes different terms are often used interchangeably and "distinctions are not always made explicit in the literature and are to some degree amorphous." The subsection ends with a description of the definition of the control groups, a topic that has received very little attention so far. As suggested in section 1.1, clear definitions of treatment and control groups are not only important methodologically, but also theoretically, because individuals attach different interpretations to different life events affecting their choices and actions (P5: human agency). For example, Pearlin et al. (1981: 339) state

[29] While data on the economic consequences are readily available, information on the non-economic consequences is mostly not. However, increasingly studies based on administrative data include information on health. The latter is, for example, measured by data on medical diagnoses or hospitalizations as well as ultimately by all-cause and cause-specific mortality (e.g., Browning et al., 2006, Browning and Heinesen, 2012).

[30] Low sample sizes often preclude analysis of treatment- and effect heterogeneity. For example, Article 5 trades in differentiating the reasons for job loss against the use of partner data and the study of outcome dynamics. Article 4 trades in effect heterogeneity analyses across countries against differentiating the reasons for job loss.

[31] The exception is Article 3 examining inadequate re-employment after job loss and unemployment. Because details on different definitions of inadequate employment and overeducation as well as their measurement are provided in the article and the supplementary material, this subsection will focus on job loss and unemployment.

that the effects of life events vary by "their desirability, by the degree of control people have over their occurrence, or by whether or not they are scheduled."

Job separations can be roughly classified into *voluntary separations* that are worker-initiated (e.g., resigned, quitted) or *involuntary separations* that are employer-initiated (e.g., fired, laid off). Job separations due to health problems may be both, but are typically defined to be involuntary. There also are reasons for job separations that cannot be easily classified, for example, mutual agreements or the completion of temporary jobs.[32] According to these definitions, only involuntary separations are *job losses*. Among the latter, *job displacements*, resulting from economic and business conditions (e.g., downsizing, restructuring, relocating, plant closings) that are beyond the control of individual workers, are distinguished from other job losses, including those due to health problems and workers being fired for individual reasons. As I explain below, most surveys only partially take these differentiations into account.

Job separations must be distinguished from *unemployment* which is normally defined as individuals without a job, but who want to work and are currently available to take up employment. In surveys this definition is implemented differently. Typically respondents are asked whether they are unemployed and (not) seeking a job or whether they are registered unemployed. Comparing all the definitions it becomes clear that job loss (involuntary separation) can but must not result in unemployment. It also becomes apparent that unemployment may be due to voluntary separations and that it can follow after other labor market statuses, for example, if persons are unemployed after education or training as well as inactivity.

The different areas of research this thesis is located in have paid more or less attention to these issues. However, studies on the economic consequences mostly focus on job loss and, especially, job displacements. Among displacements they also often distinguish between plant closures and layoffs. Although layoffs are also job displacements per definition, in surveys they often cannot be distinguished from other job losses (see below).

The differentiations are made for both theoretical and methodological reasons. Theoretically, job loss should be distinguished from voluntary separation as only the former is associated with negative economic consequences. Methodologically, clearer definitions of the life event

[32] For example, mutual agreements reflect voluntary separations if they are used by employees to change jobs without consideration of the notice period. They reflect involuntary separations if they are used by employers to avoid legal and practical problems associated with dismissals. Similarly, after the completion of a temporary job, both employers and workers may decide to not renew their contract, because either is not satisfied with the other.

of interest make it easier to model the respective selection processes (see subsection 4.2.1). It can be assumed that the selection into all separations is more heterogeneous than that into job loss. Selection into job displacements is arguably easiest to model as by definition they result from economic and business conditions such that workers' characteristics are likely to be less important. This point also provides a motivation for the distinction of job displacements due plant closures and layoffs. Gibbons and Katz (1991) argue that the latter should be associated with more negative effects, because employers have some discretion about who is laid off and this information is used by prospective employers. A similar argument can be made for dismissals, for which it is unclear whether they reflect layoffs, health problems, or workers being fired. Related to these discussions, plant closure studies are often thought to have a methodological advantage, because if almost all workers are let go, selection is unlikely due to their individual characteristics. Methodologically, distinguishing job separations and, especially, job loss from unemployment is also important, because by looking at their joint occurrence one would already condition on a possible outcome of job loss such that estimates for all job losses are likely biased. Although research on the non-economic consequences and, especially, on health and well-being has a much stronger focus on unemployment, studies on the direct or short-term effects have increasingly considered the reasons for job loss.

Given these differences, which definitions are used in the articles of this thesis? Article 1 investigates the effect of job loss on workers' subsequent careers and distinguishes displacements due to plant closure and job loss due to dismissal. For dismissals it is unclear whether they should be considered as displacements, because the wording of the question does not provide any information about whether the worker was laid off, had health problems, or was fired for individual reasons. However, by making these distinctions the analyses go beyond the usual approach of grouping all job losses or examining transitions from employment to unemployment.

Article 4 investigates the effect of an early-career (involuntary) job loss on later life health.[33] It considers both plant closures and layoffs. The word layoff is used in the question wording suggesting that workers rather reported displacements than all kinds of dismissals. This is further supported by the fact that only a very small fraction of job losses had to be excluded, because they were also due to illness or disability. However, as wording differences should be

[33] Similar to other research the article refers to involuntary job loss to make clear that the reasons for job separations are considered. According to the above definitions the adjective involuntary is, however, redundant.

treated with caution, my co-authors and I also distinguished in some analyses between plant closures and layoffs as the reasons for job loss.[34] With few exceptions such differentiations are not made in this area of research.

While Articles 1 and 4 adhere to the ideal definitions, Articles 3 and 5 rely on another frequently used definition equating "job loss" with the transition from employment to unemployment (e.g., Dieckhoff, 2011; Ehlert, 2013; Gangl, 2006). Studies using this definition often argue that they want to distinguish "job loss" (transitions from employment to unemployment) from "voluntary separations" (transition from employment to inactivity) and other reasons for unemployment (e.g., transition from inactivity to unemployment). While this motivation is similar to the one described above, the differentiations are less precise and also give up the key distinction between job loss and unemployment. Article 3 focuses on inadequate re-employment, but uses an inflow sample of persons who transitioned from employment to unemployment and, thereby, considers this definition of "job loss". As explained in subsection 4.1.1 this has mostly practical reasons. Article 5 also uses this definition but next to practical reasons, there are theoretical and methodological arguments for focusing on transitions from employment to unemployment. The theories that expect individuals to change their housework hours rely on changes in relative time availability, relative resources, and norm conformity. These changes only occur if persons become unemployed and stay unemployed or non-employed, while job loss (without following unemployment) may be less relevant. Another reason for why the use of job loss and, specifically, job displacement is less relevant for this research question, is that the selection with respect to housework, in my view, can be modeled relatively well by observed variables available in the data. By focusing on transitions from employment to unemployment, Article 5 also extends the previous literature which has either used cross-sectional data or longitudinal analyses that do not distinguish between transitions into and out of unemployment (e.g., Gough and Killewald, 2011). Nevertheless, the above highlighted limitations should be considered in the interpretation of the results.

Lastly, Article 2 examines the effects of unemployment on subsequent job quality. Given the trade-offs discussed in subsection 4.1.1, this article has to rely on a coarser definition of "job loss", because workers only reported whether they have ever been unemployed and seeking work for a period of more than three months within the last five years. While the question

[34] Kuhn (2002) points out that there exist a variety of expressions (e.g., dismissed, fired, laid off, made redundant), such that different wordings in surveys may give respondents some hint about what data collectors are asking for, but should not be over-interpreted.

only ask workers for unemployment that lasted some time and they had to be searching for a job, indicating that unemployment was not voluntary, it does not offer any information about whether workers were employed before or why they were unemployed. An assumption of article 2, therefore, is that the reported unemployment mostly stems from core workers that have lost their jobs and that their unemployment was likely involuntary triggering the theoretical mechanisms that have been discussed in subsection 3.2.1. While this is considered plausible, the ambiguity in the definition misses out on the theoretical and methodological advantages described above.

A topic that is often neglected in discussions of the definition of job loss is the choice of the control group (see also subsection 2.1.1), which is, in general, used to approximate what would have happened to workers in the absence of the event (see subsection 4.2.1).[35] Article 1 uses as a control group all workers who did not lose their job at a specific time including workers who remained at their employer and those who separated for other reasons that do not indicate leaving the labor force. This contrasts with the definitions used in many previous studies requiring workers to remain at their employer or even employed throughout the whole observation window. I argue that my definition allows for a more general counterfactual and does not imply conditioning on future outcomes.[36] A similar definition is used in Article 4. To assess the relevance of the control group choice, this article also uses a second definition where persons who did not lose a job had to be continuously employed in their early career

The comparison group in Article 5 consists of persons who are continuously employed, because here the research question and the theories explicitly define a comparison with the situation of what would have happened if the person had remained employed. The limitations of the past unemployment definition in Article 2 become also (and possibly even more) apparent in the definition of the control group, which here refers to workers who do not report having been unemployed for a least three month in the past five years. Although the definition of the control group is clear in itself, the broadness implies that the counterfactual situation remains ambiguous.

[35] In longitudinal analyses one may be thinking that it is sufficient to compare workers before and after job loss. However, not using a control group may result in wrong conclusions as discussed in subsections 2.1.1 and 4.2.1.
[36] Article 3 which focuses on inadequate re-employment as compared to continued unemployed specifically addresses the problem of conditioning on future outcomes, by applying a dynamic comparison of persons who left unemployment into overeducation in a specific month to those who continued searching for at least one additional month. Here the control group was not restricted to remain unemployed for a longer period of time to avoid that they are selected in terms of their future (adequate) employment chances.

4.1.3 Dependent variables

The choice of the dependent variables of this thesis is motivated by the life course perspective. Details about their measurements can be found in each article. Following the idea of different, but interrelated life domains (P2), Articles 1, 2, and 3 consider the economic consequences in the career domain. Articles 4 and 5 focus on the non-economic consequences by examining the health and well-being and the family domains. Moreover, all articles share the idea that the effects of job loss, unemployment, and inadequate re-employment within each life domain should be measured on multiple dimensions to take account of their multidimensionality and to highlight potential complementarities and trade-offs.

For example, Article 1 measured the career consequences by looking at labor market status, labor income, and non-standard employment. This is important to highlight trade-offs and complementarities that may arise between different aspects of careers. For instance, workers may want to minimize earnings and wage losses and, thus, be willing to accept non-standard employment. Examining only one of these dimensions misses such considerations. A similar motivation underlies Article 2 that looks at four different facets of non-monetary job quality taking account of recent debates that workers' well-being cannot be measured by money alone (Muñoz de Bustillo et al., 2011). Article 3 also provides a more balanced assessment of the stepping-stone and trap hypotheses by not only considering adequate re-employment, but also investigating workers overall employment chances.

Article 5 highlights the necessity to also consider multiple dimensions of outcomes in life domains reflecting the non-economic consequences. In contrast to most previous longitudinal research it measures different housework tasks and is able to examine whether men and women, despite both adapting their housework hours when becoming unemployed, do so in different and potentially gendered spheres. This differentiation has strong theoretical implications as described in section 3.5. In contrast, Article 4 has to make some concessions with respect to the aim of considering multiple dimensions of the outcome as only one measure of health is used. This is due to the availability of suitable other indicators and the fact that Article 4 already added complexity to the analyses by differentiating the reasons for job loss and performing a mediation analysis. However, by choosing self-rated health as an outcome which considers both aspects of physical health and mental health (see Garbarski, 2016 for a research synthesis) and, in addition, is predictive of overall mortality after adjusting for specific health measures and other variables (Idler and Benyamini, 1997), it still is in line with the idea of measuring consequences holistically.[37] Moreover, the mediating variables in these analyses are measured on multiple dimensions. They not only concern the cumulated unemployment duration, the number of job ends, and the number of job losses, but also are measured by a sequence analysis that calculated a complexity index based on workers activity statuses aiming at a broader assessment of employment instability (Gabadhino et al., 2011).

4.2 Methods

This chapter provides some background to and discusses decisions about the methods used in the five articles of this thesis. In subsection 4.2.1 I offer an introduction to causal analysis using observational data and discuss three common estimators that based on different assumptions allow estimating causal effects. Because each method used in the articles is associated with a common estimator, their strength and weakness are highlighted. In subsection 4.2.2 I discuss different motivations of and approaches to multi-level analysis which are reflected in the methods used in Articles 2 and 4.

4.2.1 Causal analysis

When examining the effects of job loss, unemployment, and inadequate re-employment, it has to be considered that these events are neither randomly assigned nor happen by accident. As explained before, their occurrence rather depends on employees' and employers' decisions and the contexts in which their interactions take place. In observational data the latter are reflected in characteristics of employees, employers, and the context. For example, the risk of job loss increases in recessions, because firms lay off workers to react to changes in demand. However, employees in small firms may have a greater risk to be laid off than those in large firms, because large firms have other resources to weather a recession. Layoffs or dismissals may also be more likely among workers with low than high education, because employers want to only keep the most skilled employees. Similarly, the decision to take up a job for which one is overeducated likely depends on the time one already has been unemployed and the resources available to continue searching.

These *selection processes* imply that workers who experience job loss, unemployment, and inadequate re-employment will *systematically differ in characteristics other than the event* and if these characteristics also affect the outcome, simple comparisons of affected and unaf-

[37] Self-rated health, which strongly relates to physical health, may also be an adequate choice in the sense that many authors argue that while the short-term effects of job loss are more likely to be found for mental health, in the long run physical health problems accumulate (see subsection 3.4).

fected workers will confound outcome differences that are due to the effect with those resulting from the groups' different compositions in terms of (observed and unobserved) characteristics affecting the risk of (i.e., baseline bias) as well as their reaction to the event (i.e., differential treatment effect bias) (Morgan and Winship, 2015).

To discuss how and when causal effects can be estimated using observational data (see Gangl, 2010 for a review), I introduce the potential outcomes framework or counterfactual model of causality.[38] To keep the discussion practical I focus on the effect of job loss on income as a running example, but the same ideas apply to the other areas of research.

The potential outcome framework has been formalized by Rubin (1974), although the underlying ideas have been developed and can be found in earlier work in statistics and econometrics (see Morgan and Winship, 2015 for a review). It is particularly useful in defining causal effects and in clarifying under which assumptions they may be estimated. Specifically, I here discuss three common estimators that rely on cross-sectional or longitudinal data (Caliendo, 2006) and explain how each method used in the articles can be associated with one of these.

The potential outcome framework begins with the idea that a treatment has a specific effect for each individual.[39] In the running example, the binary treatment is job loss (D) with some workers i experiencing it ($D_i = 1$) and others not ($D_i = 0$). The observed outcome income (Y_{it}) is measured at some post-treatment time t. Specific to the potential outcome framework is the idea that each worker also has two potential outcomes indicated by superscripts: the income (Y_{it}^1) that would have been realized at t in case of job loss and the income (Y_{it}^0) that would have been realized at t in the absence of job loss. Given these, it is easy to define the individual causal effect (1) of job loss on income as the difference between the two potential outcomes for each worker (or alternatively any other comparisons such as a ratio).

(1) $$\delta_{it} = Y_{it}^1 - Y_{it}^0$$

However, as the observation rule (2) highlights the individual causal effects can never be identified, because at some specific time t one either observes the income in case of job loss

[38] The following presentation is inspired by Caliendo (2006) and Morgan and Winship (2015), but differs in notation. Specifically, I introduce the post-treatment time t at which the outcome is measured right from the beginning, because this is helpful in the discussion of estimators that are based on longitudinal data.
[39] The experimental language reflects the framework's scientific origins, but has no further implications here.

or in the absence of it. This is also called the fundamental problem of causal inference (Holland, 1986).[40]

(2) $Y_{it} = D_i Y_{it}^1 + (1 - D_i) Y_{it}^0$

Therefore, researchers focus instead on different kinds of average causal effects, which can be estimated under certain assumptions. The average treatment effect at t, $ATE_t = E(\delta_t) = E(Y_t^1 - Y_t^0) = E(Y_t^1) - E(Y_t^0)$, is defined as the expected income difference for both states for a randomly chosen worker from the population. The average treatment effect on the treated at $t, ATT_t = E(\delta_t | D = 1) = E(Y_t^1 - Y_t^0 | D = 1) = E(Y_t^1 | D = 1) - E(Y_t^0 | D = 1)$, is defined similarly but for a randomly chosen worker who experienced job loss.[41] It is this second average causal effect that is typically of interest as it reflects the consequences for those workers who are actually treated and is, thus, assumed to bear the greatest policy relevance (Caliendo, 2006). It is also the main interest of the articles and will be the focus of the remaining discussion.[42] Given the definition of the ATT_t, it becomes clear that the challenge lies in identifying the counterfactual $E(Y_t^0 | D = 1)$, that is, the expected income that workers who lost their job would have realized had they not lost their job, by maintaining plausible identifying assumptions.

The simplest way to identify the counterfactual is to use the observed income of the control group. This gives rise to the naive-estimator (NE) (3):

(3) $ATT_t^{NE} = E(Y_t^1 | D = 1) - E(Y_t^0 | D = 0)$

(4) $E(Y_t^0 | D = 1) = E(Y_t^0 | D = 0)$

However, it immediately becomes clear that the naive estimator only estimates the ATT if the identifying assumption (4) is plausible. It states that the income at t that is realized in the absence of job loss would be the same for the treated and controls. While this assumption would be likely fulfilled in an experimental study, where job loss would be randomly assigned by a coin flip, which is impartial to and independent of workers potential incomes, it is generally

[40] The observation rule implicitly includes the so-called stable unit treatment value assumption (SUTVA), stating that a unit's potential outcomes do not depend on the treatment of other units or the mechanism by which the treatment takes place (Morgan and Winship, 2015).
[41] The average treatment effect on the controls (ATC) is defined analogously, but of no further interest here.
[42] This also means that the differential treatment effect bias resulting from the treated and controls different reaction to the treatment is not a problem, because the treatment effect on the treated only concerns the former.

violated in an observational study.[43] The above discussed selection processes imply that workers who lose their job differ in observed (e.g., education) and unobserved (e.g., motivation) characteristics that affect income in the absence of job loss, resulting in baseline or confounding bias. However, the selection processes likely differ in their strength for different reasons of job loss. As explained in subsection 4.1.2, job displacement, in particular, if it is due to plant closure, is considered to be relatively exogenous meaning that selection is less likely driven by worker characteristics making (4) less implausible.

If the identifying assumption that underlies the naive estimator is not credible, how can it be relaxed? While the cross-sectional estimator (CSE) (5) appears to be similar, it differs in that the expected incomes are now conditional on a vector of pre-treatment control variables X.

(5) $ATT_t^{CSE} = E(Y_t^1|D = 1, X) - E(Y_t^0|D = 0, X)$

(6) $E(Y_t^0|D = 1, X) = E(Y_t^0|D = 0, X)$

Accordingly, the identifying assumption (6) states that within groups that are defined by the combination of the values of the control variables in X, the income at t that is realized in the absence of job loss would be the same for the treated and controls. This assumption is also called selection on observables (Caliendo, 2006) highlighting that it is only plausible if all variables that drive selection and affect the potential incomes are known to the researcher and observed. The assumption is more credible than that for the naive estimator and all baseline or confounding bias that arises from observed variables can be eliminated by conditioning. However, it still rules out that unobserved variables, such as motivation, affect the probability of job loss and the potential incomes. This may be plausible if a rich set of control variables is available or if it is combined, for example, with a relatively exogenous reason for job loss such as plant closure. If selection into the latter is driven mainly by observable employer characteristics (e.g., industry) and not by workers' characteristics, the cross-sectional estimator can yield valid approximations to the ATT_t.

Articles 2 to 4 rely on the cross-sectional estimator as they compare treated and untreated individuals but condition on pre-treatment control variables. Given that its credibility relies on

[43] Such an experiment not only implies large practical difficulties, but is also ethically unacceptable. Moreover, even experiments that randomly assign a manipulated treatment share some problems with observational studies as their practical implementation usually deviates from the ideal (Shadish et al., 2002). However, experiments on related research question are possible and promising as is illustrated by studies that send out applications with manipulated unemployment durations to study the effect on invitations to job interviews (e.g., Kroft et al., 2013).

the control variables chosen, the question arises what variables should be conditioned on. Methodologists in the potential outcomes tradition have warned that one should only control for pre-treatment variables and new research on causal graphs shows that the wrong set of control variables "may fail to remove all confounding bias or even introduce new biases through overcontrol or endogenous selection" (Elwert, 2013: 257). Therefore, I selected the set of control variables theoretically and only included pre-treatment variables that are assumed to affect the treatment and outcome. I also avoided overcontrol bias by refraining to control for variables that mediate the total effect of interest.[44] The exception is Article 4 were after estimating the total effect of job loss on health, my co-authors and I control for mediating variables to separate the direct and indirect effects.

Next to identification, issues of estimation have to be considered. Ideally one would estimate the ATT_t within groups defined by the combination of the values of the control variables in X and afterwards average over these. However, because there are usually many control variables with many values non-parametric estimation is not feasible due to problems of high dimensionality or sparseness. Therefore, the articles rely on different (semi-)parametric methods for conditioning. Specifically, they use coarsened exact matching (Iacus et al., 2012) with regression models (Article 2), dynamic propensity score matching (Sianesi, 2004) (Article 3), and logistic regression (Article 4). Irrespective of their advantages and disadvantages the different conditioning methods all rely on the assumption that all relevant control variables have been considered. A discussion of the methods and the control variables is provided in the articles, but given the above considerations the assumption is more plausible in Articles 3 and 4. Article 3 uses rich panel data which allows controlling for confounding in a detailed way and Article 4 in addition distinguishes different reasons for job loss making use of the idea that plant closures are relatively exogenous. Similar to most comparative studies on cross-sectional data, the list of available control variables in Article 2 is rather limited, making the identifying assumption less plausible.

What if the assumptions of the cross-sectional estimator are not considered sensible?[45] Longitudinal data and estimators make different assumptions that may be considered more convinc-

[44] While my choices are guided by the literature on graphical causal models (Pearl, 2009) I did not make full use of it by strictly applying, for example, the adjustment or backdoor criteria in the selection of control variables.

[45] If this assumption is considered implausible but only cross-sectional data are available, researchers often use instrumental variable estimators (Gangl, 2010) which rely on the availability of a variable Z that is associated with the treatment D (instrument relevance) and the outcome Y only through its effect on the treatment D (instrument exogeneity). For example, if interest is in the effect of unemployment on health, plant closure may be

ing. The classical estimator, which in addition to the post-treatment measure of the outcome (Y_t) also uses a pre-treatment measure ($Y_{t'}$), is the before-after estimator (BAE) (7).

(7) $\quad ATT_t^{BAE} = E(Y_t^1|D=1) - E(Y_{t'}^0|D=1)$

(8) $\quad E(Y_t^0|D=1) = E(Y_{t'}^0|D=1)$

In contrast to the cross-sectional estimator, the before-after estimator does not identify the counterfactual $E(Y_t^0|D=1)$ by means of a control group, but by the treated groups expected income $E(Y_{t'}^0|D=1)$ in the pre-treatment period t', implying a within-worker comparison over time.[46] The key advantage is that the within-worker comparison implicitly controls for all variables, unobserved or observed, that drive selection into job loss, are constant, and have constant effects over time. However, the estimator relies on the strong identifying assumption (8) that in the absence of the treatment the income would not have changed for the treated. In the running example this excludes changes in wages due to common period effects (e.g., economic shocks) as well as other growth processes (e.g., wage increases). It also precludes transitory shocks in the pre-treatment period due to anticipation (e.g., wage restraints).

Another estimator that is closely related to the before-after estimator but also makes use of the between-group comparison underlying the cross-sectional estimator is the so-called difference-in-differences estimator (DiDE) (9). It also relies on the income trend for workers who lose their job, but subtracts, in addition, the income trend in the control group.

(9) $\quad ATT_t^{DiDE} = [E(Y_t^1|D=1) - E(Y_{t'}^0|D=1)] - [E(Y_t^0|D=0) - E(Y_{t'}^0|D=0)]$

(10) $\quad E(Y_t^0|D=1) - E\left(Y_{t'}^0|D=1\right) = E(Y_t^0|D=0) - E\left(Y_{t'}^0|D=0\right)$

The identifying assumption (10) for the difference-in-differences estimator is not anymore that in the absence of job loss those who lose their job would not have experienced any changes in income, but rather "only" that they would have experienced the same change as workers who did not lose their job. This is also called the common trends assumption (Lechner, 2010). It is weaker than the one of the before-after estimator, because the difference-in-differences estimator remains unbiased if period effects or other growth processes in the ab-

used as an instrument maintaining that it is associated with unemployment and only is associated with health through its effect on unemployment. While instrument relevance is given, instrument exogeneity is not plausible, because unemployment rates (and other variables) may cause an association between plant closure and health.

[46] I here assume panel data and that the differences in pre- and post-treatment outcomes are taken within workers. The BAE and the estimator introduced below can, however, also be used with repeated cross-sectional data.

sence of job loss are the same for the treated and controls. Anticipation effects are, however, still a potential issue.

Articles 1 and 5 use fixed-effects regression models where either separate outcome trends are estimated for the treatment and control groups (Article 1) or a two-way fixed-effects model is applied that estimates period effects by using the information of the control group (Article 5). These methods apply the basic identification strategy of the difference-in-differences estimator by combining a within- and between-group comparison.[47] Article 1 extends the model further in that it combines fixed-effects regression models with a preceding propensity score matching. The idea of this approach is that the (conditional) common trends assumption is more plausible after matching on a vector of pre-treatment control variables X. Moreover, both articles not only examine how the effects develop in the post-treatment periods, but also investigate differences between treated and untreated in the pre-treatment period. If no differences are found in the latter this provides some credibility to the identifying assumption as outcome trends are similar in a period where the treatment is absent.

While all articles are interested in the effects of job loss, unemployment, or inadequate re-employment, two articles, in addition, have a multi-level structure in which workers are nested within countries. As will be discussed next, the multi-level structure has statistical implications that need to considered, but also offers additional possibilities to enrich the analyses.

4.2.2 Multi-level analysis

Articles 2 and 4 use data from individuals nested in countries.[48] This multi-level structure has to be taken into account for statistical reasons, but may also be of interest for theoretical reasons.[49] In a recent review of multi-level analysis for comparative research Bryan and Jenkins (2016) distinguish four approaches. In two of these approaches researchers are interested in the effect of individual-level variables, but have to take account of the multi-level structure for statistical reasons. This is also the motivation Article 4 is based on. The other two approaches follow from theoretical reasons, because researchers not only want to take account

[47] With balanced panel data and two periods, the difference-in-differences estimator is identical to the first-difference and fixed-effects estimators that include period effects. With unbalanced panel data or more than two periods these estimators differ in relevant aspects, but the general identification strategy remains the same. One difference is that the fixed-effects estimator rather relies on a within-person demeaning than differencing (Brüderl and Luwdig, 2015).

[48] Article 2 includes an additional country-round level. For illustrative purposes, I here focus on the common two-level case. However, at the end of this subsection I discuss the advantages of using the additional level.

[49] The following presentation and the distinction between different motivations for and approaches to multi-level analysis is inspired by Bryan and Jenkins (2016), although my notation differs.

of the statistical issues a multi-level structure raises, but are also interested in estimating the effect of country-level variables and their cross-level interactions with individual-level variables. This is the motivation underlying Article 2. In the following I explain both motivations and the respective approaches and demonstrate how the applications in Articles 4 and 2 follow from these.

The statistical reasons for using multi-level analysis are best explained by reference to the simple multi-level model described in (11-3).

(11-1) $\mathrm{Y_{ic}} = \beta_{0c} + \beta_1 X_{1ic} + \sum_{k=2}^{K} \beta_k X_{kic} + \varepsilon_{ic}$

(11-2) $\beta_{0c} = \gamma_{00} + v_{0c}$

(11-3) $\mathrm{Y_{ic}} = \gamma_{00} + \beta_1 X_{1ic} + \sum_{k=2}^{K} \beta_k X_{kic} + v_{0c} + \varepsilon_{ic}$

In the multi-level model in (11-3) the outcome Y_{ic} for an individual i in country c is determined by an intercept γ_{00}, the effect β_1 of the independent variable of interest, the effects β_k of the control variables X_{kic}, a country-level error v_{0c}, and an individual-level error ε_{ic}. The model differs from a standard linear regression model, because it includes a country-level error v_{0c} representing the effects of unobserved country-level variables. The same model is also given in (11-1) and (11-2) showing that the country-specific intercepts β_{0c} are allowed to vary from γ_{00} by v_{0c}.

Estimating model (11-3) with ordinary least-squares (OLS) based on pooled data gives unbiased estimates of the effects, but results in invalid, and usually too small, standard errors, because the independence of errors assumption is violated. This violation results from the fact that individuals from the same country will be more similar than individuals from different countries as they share the effects of unobserved country-level variables v_{0c}.

Researchers who are interested in estimating the effect of $\beta_1$but want to take account of the statistical issues can apply two approaches. The first is to use cluster-robust standard errors which control for the effects of v_{0c} and allow for a general correlation structure among individuals within countries. The second approach, the country fixed-effects model, addresses the clustering by estimating the v_{0c} by including a dummy variable for each country but one resulting in country-specific intercepts. In practice it is often recommended to combine these approaches, because cluster-robust standard errors do not fully account for the clustering of individuals in countries (e.g., Cameron and Miller, 2015) and country-fixed effect may remove confounding if the independent variable of interest is affected by the unobserved country-level variables.[50] In Article 4 the interest is in the effect of the individual-level variable job loss on health. Accordingly, my co-authors and I used cluster-robust standard errors and country fixed-effects to account for the nesting of individuals in countries.[51] We used country fixed-effects to additionally remove confounding as country-level variables such as economic situation or labor market polices affect both the risk of job loss and health.

Although the two remaining approaches, so-called mixed-effects and two-stage models, also may be used to address the statistical issues associated with clustering, they are usually motivated theoretically, because they allow estimating the effects of country-level variables and their cross-level interactions with individual-level variables.[52] Two explain these approaches it is useful to extend the model in (11-3) as follows.

(12-1) $\mathrm{Y_{ic}} = \beta_{0c} + \beta_{1c}X_{1ic} + \sum_{k=2}^{K} \beta_k X_{kic} + \varepsilon_{ic}$

(12-2) $\beta_{0c} = \gamma_{00} + \gamma_{01}Z_c + v_{0c}$

(12-3) $\beta_{1c} = \gamma_{10} + \gamma_{11}Z_c + v_{1c}$

(12-4) $\mathrm{Y_{ic}} = \gamma_{00} + \gamma_{01}Z_c + \gamma_{10}X_{1ic} + \gamma_{11}Z_cX_{1ic} + \sum_{k=2}^{K} \beta_k X_{kic} + v_{0c} + v_{1c}X_{1ic} + \varepsilon_{ic}$

In contrast to the model in (11-3), the model in (12-4) includes a country-level variable Z_c as well as the cross-level interaction between the country-level variable and the individual-level variable of interest Z_cX_{1ic}. It also adds an additional country-level error v_{1c} which is interacted with X_{1ic}. The model is equally described by (12-1) to (12-3) showing that it is similar to a standard linear regression model where the intercept β_{0c} and the slope β_{1c} are allowed to vary across countries. In contrast, to the model in (11-3) the variation in the intercept (as well as in the slope) is in part explained by the country-level variable Z_c and in part random. In Article 2 a model similar to (12-4) is used, because interest is in how the effect of the individual-level variable unemployment on non-monetary job quality is moderated by country-level variables such as economic situation and labor market policies.

[50] This is also the standard in panel data analysis (Brüderl and Ludwig, 2015). Panel data can be understood to have a multi-level structure with person-observations nested in persons. Therefore, Articles 1 and 5 use the same strategy as Article 4 by combining person fixed-effects with cluster-robust standard errors.

[51] A problem is that cluster-robust standard errors are only consistent for large numbers of clusters. For Article 4 it was verified that the conclusions do not depend on the choice for or against cluster-robust standard errors.

[52] Mixed-effects models are also referred to as random coefficients or random effects models, while two-stage models also are known as the two-step approach.

In principal the model described by (12-1) to (12-3) may be estimated simultaneously by using the mixed-effects approach or in separate steps by using the two-stage approach with each having specific advantages and disadvantages. The mixed-effects approach uses pooled data and the country-level errors v_{0c} and v_{1c} are modelled as random effects which are assumed to follow a multivariate normal distribution. As described in (12-1) to (12-3), in practice this model is usually estimated by assuming random intercepts v_{0c} for β_{0c} as well as random slopes v_{1c} for β_{1c} the effect of the independent variable of interest. However, the effects β_k of the control variables are most often assumed to be constant across countries.

An alternative to the mixed-effects approach is the two-stage approach. In this approach, in the first-stage, an individual-level model similar to (12-1) is estimated separately within each country. However, in contrast to (12-1) all parameters are allowed to vary across countries including the variance of the individual-level error. The estimated intercepts or slopes are collected and, in the second-stage, regressed on the country-level variables of interest. Specifically, if interest is in cross-level interactions, as in Article 2, the estimated slopes for the individual-level variables of interest are regressed on the country-level variables that are assumed to be moderators (12-3).

In Article 2 I chose the two-stage over the mixed-effects approach for a number of reasons. First, as it is implemented in two stages it allows applying additional analyses in each stage. For example, before estimating the effect of unemployment on non-monetary job quality within each country, I used coarsened exact matching (CEM) in Article 2. Second, the two-stage approach can be graphically illustrated by a scatter plot of the first-stage estimates against the macro-level variable of interest (see Figure 3 in Article 2). This allows analyzing and interpreting the data visually making the procedure very transparent. Third and most important, the two-stage approach allows the effects of all individual-level variables to vary across countries. This is important, because Heisig et al. (2017) show that the usual practice in mixed-effects models, to constrain the control variables to have constant effects across countries, results in omitted variable bias due to misspecification.

The two-stage approach also has some potential disadvantages. In contrast to the mixed-effects approach, which constraints some coefficients to be constant across countries and also uses between-country variation to estimate the effects of the individual-level variables, its flexible specification and the fact that only within-country variation is used to estimate the latter, makes it less efficient and possibly results in imprecise estimates. However, as in Article 2 the number of individuals per country is, on average, large, this is not considered an issue. A second problem may be that the dependent variable used in the second stage is estimated. To take account of the uncertainty in the first stage estimates, I follow the recommendation of the literature and use estimated dependent variables (EDV) regression models in the second stage. These are estimated by feasible generalized least squares (FGLS) (Lewis and Linzer, 2005). To take further account of the nesting of country-rounds in countries (see next paragraph) standard errors are clustered by country.

While I have focused on the common two-level case to explain the different reasons for and approaches to multi-level analysis as well as to demonstrate how the applications in Articles 4 and 2 follow from these, the multi-level structure in Article 2 actually includes an additional level. Because I use repeated cross-sectional data, individuals are nested in country-rounds and countries. The fact that most countries are observed repeatedly over time allows to estimate, in some analyses, the moderating effects of economic situation and labor market policies by only using within-country variation over time. As explained in section 4.1, this allows controlling for all time-constant unobserved heterogeneity between units. In this case this concerns all stable characteristics of countries such as social policy traditions or compositional differences. This strategy also reduces concerns about the cross-national comparability of non-monetary job quality measures.

5. Summary and conclusions

In section 5.1 I summarize the main findings of the articles and discuss their limitations. Based on this, in section 5.2 I draw some general conclusions and give an outlook for future research.

5.1 Main findings of the articles

In Article 1 *"Losing standard employment in Germany: The consequences of displacement and dismissal for workers' subsequent career"* I find that both displacements due to plant closures and dismissals have short- and long-term negative effects on employment. Five years after the events workers who lost their jobs have 12 to 15 percentage points lower employment chances than those who did not. The findings also suggest that their situation will only slightly improve as time passes. Although I show that the lower employment chances are mostly due to unemployment, more than a third can also be attributed to workers who have left the labor force entirely, especially into (early-)retirement. Apart from retirement, only a small increase in inactivity is found. Sensitivity analyses based on retrospectively collected monthly data confirm these results.

Article 1 also reveals that in Germany, in contrast to recent findings for the US and UK, the lasting total income losses, which are in the range between 15 and 23 percent, are to a greater extent explained by falls in employment and working hours instead of negative effects on hourly wages. However, even for hourly wages I find significant wage scars in the range between 6 to 8 percent.

I further show that job loss increases the risk of non-standard employment. For self-employment (5 to 7 percentage points) the findings are similar for both events, while for part-time somewhat smaller increases are found for displacements (2 to 4 percentage points) compared to dismissals (5 to 6 percentage points). For fixed-term contracts the effects are large in the year after job loss (20 and 25 percentage points for displacement and dismissal respectively) but decrease in the long run (6 and 11 percentage points) with the over time pattern suggesting further reductions. Sensitivity analyses on the heterogeneity within non-standard employment show that the increases in self-employment are largely due to solo self-employment, which is generally considered to be of a lower quality. For the effects on part-time work, marginal employment, which is often viewed critically, only plays a small role. Concerning temporary agency work as specific form of temporary employment, I find increases of 3 to 5 percentage points in the short run. However, these fall to zero after about five years.

Overall the findings are in line with the hypotheses about negative effects for employment, total labor income, earnings, and wages and positive effects for non-standard employment. The expectation of stronger effects for dismissals than displacements is only partly confirmed. Although the effects are more pronounced for dismissals for most outcomes, the differences to plant closures become smaller as time passes. A further interesting finding is that the negative effects on earnings are largely explained by dismissed workers' lower working hours. The results on labor market status suggest that job loss does not necessarily result in a large group of discouraged workers who are not easily re-integrated into the labor market. For labor income, Article 1 improves our understanding of explanations for total labor income losses in different welfare states by linking previous findings about larger (short-term) total losses in conservative welfare regimes (Ehlert, 2013), but stronger earnings or wage scars in liberal welfare states (Gangl, 2006). For non-standard employment, the results imply that only temporary employment is a transitory experience although even here I show higher risks of fixed-term employment five years after job loss.

Next to the disadvantages associated with using survey instead of administrative data (see subsections 2.1.1 and 4.1.1) and similar to most previous studies the analyses are subject to two specific methodological issues. First, they do not account for workers leaving employers in anticipation of job loss. If these workers are selected with respect to the outcomes of interest, this results in bias.[53] Second, the findings of the analyses that are conditional on re-employment may be biased, because they are not only affected by selection into job loss, which is thoroughly modeled, but potentially also by a dynamic selection into re-employment.[54] Third, I analyzed the risks of non-standard employment separately and it remains open how they are interrelated.

In Article 2 *"The effects of unemployment on non-monetary job quality in Europe: The moderating role of economic situation and labor market policies"* the first-stage micro-level analyses reveal that unemployment negatively affects subsequent non-monetary job quality in the majority of the 164 country-rounds analyzed. Taking account of the multidimensionality of job quality, I show that workers who have experienced unemployment in the last five years have a lower occupational prestige, less autonomy and authority, and, in particular, face greater job insecurity than those who have not. This also confirms previous findings on the scar effects of job loss and unemployment for workers' subsequent non-monetary job quality (Brand, 2006; Dieckhoff, 2011).

However, the results reveal a large heterogeneity in the effects across countries and over time. In macro-level analyses I examine whether this variation can be explained by the unemployment rate, GDP growth, UB generosity, expenditures on ALMPs and PLMPs, and EPL for regular contracts. All models included period effects. In some specifications I added country fixed-effects to only estimate the moderating effects by using within country-variation over time. Similar to previous studies (e.g., Dieckhoff, 2011), the second-stage macro-level analyses show that some of the moderating factors have effects that are in line with the theoretical

[53] However, it is also not clear how these employees report their job loss in survey data. If they leave early but nevertheless report that they lost their job due to a plant closure or dismissal the problem vanishes.

[54] If selection into re-employment is due to temporally stable worker-specific characteristics the fixed-effect panel estimators are unbiased. Studies using selection-correction models in sensitivity analyses find that the results are relatively stable (e.g., Brand, 2006; Gangl, 2006). I refrained from using these, because they rely on strong assumptions. Specifically, they require a variable that affects selection into re-employment but not the outcomes of interest.

expectations. However, as the findings are not consistent across different outcomes, model specifications, and measures of the moderators, they have to be interpreted with caution.

Overall, the results confirm Article 1 and the individual-level hypothesis suggesting that unemployment has negative effects on subsequent non-monetary job quality. It also shows that the effects vary substantially across countries and over time. As the results of the macro-level analyses are not consistent, they imply a limited role of economic situation and labor market policies in explaining this variation and at the same time point to the need for further theoretical and empirical investigations.

Some limitations have to be considered. First, the micro-level analyses may overestimate the negative effects of unemployment as they, similar to most comparative research, are based on cross-sectional data and the underlying selection on observables assumption may not be plausible given the limited number of control variables (see subsection 4.2.1). If the amount of bias in the effect estimates varies across country-rounds, this may also affect the macro-level analyses. Another critique following from the mixed findings about the importance of countries' institutional set-up concerns the theoretical expectations of this and previous studies. For some macro-level variables it is likely that they have heterogeneous effects, for example, due to differences in the design of labor market policies. If some ALMPs mitigate the negative consequences, but others result in lock-in effects or increase stigma, the overall effects may be close to zero. For other moderating variables opposing mechanisms may be at work resulting in ambiguous overall effects, too. For example, theory predicts that higher unemployment rates increase stigma by prolonging unemployment, but also decrease stigma as unemployment is less informative about individual workers' productivity. Similarly, while UBs increase unemployed workers' bargaining power, they also extend unemployment duration, potentially fostering the depreciation of human capital or unemployment stigma. A related issue is that specific policies may only work for specific population subgroups, which means that future research would benefit from more differentiated theoretical considerations.

Article 3 *"Better overeducated than unemployed? The short- and long-term effects of an overeducated labour market re-entry"* switches perspectives and focuses on the re-employment of workers after a transition from employment to unemployment. In descriptive analyses, my co-author and I show that more than a third takes up inadequate re-employment. Compared to the 18 percent of overeducated workers overall, this suggests that many use inadequate jobs to re-enter the labor market. However, the descriptive findings also indicate that overeducation is of varying importance to workers, as the unemployment duration for those with adequate re-employment is three months shorter than for those with an overeducated re-entry. This latter finding also supports the application of the dynamic propensity score matching approach, which compares an overeducated re-entry after a specific unemployment duration with the alternative of remaining unemployed and continuing the job search for at least one additional month. Our findings show that overeducation is associated with large short-term positive employment effects (61 percentage points after 6 months). Although the effects decrease over time as some of those who remained unemployed initially also take up jobs, the positive effects on employment chances range from 10 to 20 percentage points for two to five years after re-employment. Additional analyses show that these effects are to a similar extent explained by lower risks of unemployment and inactivity. However, further results reveal that the higher employment chances come at the costs of lower chances for adequate re-employment. Across the follow-up period, workers who made an overeducated re-entry have a 30 to 40 percentage points lower chance to work in a job that matches their skills and qualifications than those who did not. Overall, the results support the stepping-stone hypothesis in terms of employment but the trap hypothesis with respect to adequate employment.

My co-author and I also examined effect heterogeneity by labor market experience and educational qualification. The results are similar to the overall findings. In contrast to our expectation we find slightly higher employment effects for established workers and only small differences in adequate employment chances. With respect to education, we find stronger lock-in effects into overeducation for workers with vocational degrees lending slight support to our hypothesis, but no differences in the effect on employment chances. The similarities in the effects are potentially more striking than the small differences that are revealed.

As highlighted in subsection 4.2.1 the analyses rely on the selection on observables assumption. While we used a homogenous sample and the rich panel data allowed controlling for many observed variables and the elapsed unemployment duration, workers who take up an overeducated job may still differ on unobserved characteristics. For example, if they have a lower ability, we likely underestimate the positive employment effects and overestimate the negative effects with respect to adequate re-employment. Although we do not find any differences in employment chances before the treatment indicating that the selection on observables assumption is plausible we cannot rule out such issues. Similar studies with designs that also take account of selection on unobservable characteristics have reported results in line with ours (Baert et al., 2013). Another issue may be the measurement of overeducation with the literature being in disagreement over the best approach. We used an extension of the standard subjective measure and also tested for alternatives with qualitatively similar results. Finally, our analyses do not allow us to disentangle different mechanisms for the effects of overeducation, pointing to an avenue for future research.

In Article 4 *"The effect of an early-career involuntary job loss on later life health in Europe"* my co-authors and I focus on the non-economic long-term consequences of job loss. We find that workers who lost their jobs due to plant closures or layoffs in their early-career, defined as the first ten years after labor market entry, have an about 6 percentage points higher chance to be in fair or poor self-rated health than those who did not. Similar results are found using the five-point self-rated health scale and applying models for ordinal or continuous outcomes. The findings are also robust to the control group used as they do not change if comparisons with workers who did not lose their job and in addition were continuously employed throughout their early career are made. The results are also very similar for plant closures and layoffs. This makes the effect estimates for layoffs more plausible as they correspond with those based on a relatively exogenous reason for job loss. Furthermore, a particularly interesting finding of this article is that the total effect of job loss on health was only reduced by about 10 to 15 percent when controlling for the mechanisms of subsequent unemployment risks and employment instability. Overall the hypothesis about the negative long-term effects can be confirmed, but the results for the mediating role of economic consequences were only partly in line with our expectations.

However, there remain some caveats to the analyses. The use of retrospective data implies that measures of childhood health and childhood socio-economic status are not as specific as, for example, in prospective cohort studies. In addition, the SHARELIFE survey may be affected by recall errors, although studies that examine this issue find that respondents remember fairly well (Havari and Mazonna, 2015). Another problem may be survivor bias which can be interpreted quite literally here. As the literature review in section 2.3 emphasized job loss and unemployment may increase mortality. In our study this means that the persons who were most negatively affected by job loss may already have died or at least cannot participate in a survey. This would, however, result in an underestimation of the negative health effects in Article 4. Similar to Article 1, we cannot take into account selection arising from anticipation. If workers who leave early are positively selected with respect to health, we may overestimate the negative effects of an early-career job loss. Lastly, the finding that subsequent unemployment risks and employment instability only mediate a small share of the total effect is important, but could to some extent be specific to our sample with workers building their careers during an overall good economic situation.

In Article 5 *"Unemployment and housework in couples: Task-specific differences and dynamics over time"* my co-author and I examine another aspect of the non-economic consequences of job loss by studying how couples change their division of housework and their total household production. We find that job loss by men increases their own housework by about 2.3 to 2.5 hours and decreases their female partners' housework by 0.3 to 0.8 hours a day. This also means that job loss results in an expansion of the total household production. For job loss by women the results are very similar with slightly smaller increases of 1.9 to 2.0 hours. Male partners accordingly reduce their domestic labor by 0.3 to 0.4 hours. The similar results for men and women and, especially, the finding that men increase their housework more than women cast doubt on the gender-specific perspective. They are likely explained by different initial conditions (men earn more, mean work longer) and confirm our hypothesis on the importance of relative time availability, resources, and productivities. The relevance of different initial conditions is also supported in sensitivity analyses showing that men and women who work full-time before job loss as well as their partners show almost identical reactions. We further show that for both men's and women's unemployment couples adapt immediately providing little evidence for the hypotheses about a lagged adaptation or alternatively an intensification of the avoidance of housework by men.

However, because total housework may hide differences by specific tasks we also look at the effects for gender-neutral, female-typed, and male-typed housework. The overall results of immediate reactions and larger increases for the unemployed persons than decreases for their partner hold over the different tasks. However, in line with our expectation, we find that husbands spend relatively more additional hours on neutral and, in particular, male-typed tasks, whereas wives' extra time is to a large extent attributable to increases in routine chores. We also find slight signs of adaptation for the case of husbands' unemployment and routine housework, which is consistent with the fact that adaptation should play a role for those tasks where men have collected the fewest experiences so far or that are most at odds with their traditional gender roles.

Overall the results are, however, opposed to the gender-specific perspective showing that couples mostly follow economic rationales in reallocating their time spend on domestic work. The gender-specific reactions with respect to the tasks are in line with the doing gender approaches, but can also be explained by specialization stressing that spouses spend relatively more of their extra time in tasks they possess skills for. While this is not easily distinguished the finding that men immediately and substantially increase their routine housework, is difficult to reconcile with gender-based expectations.

Despite the article's strengths, there remain some limitations. Stylized time use data are inferior to diary methods and although fixed-effects models control for upward bias due to temporally stable over-reporting, time-varying measurement error due to respondents over-reporting more extensively after they have lost their jobs, for example, to appear productive, remains possible. Another shortcoming is that our data do not allow investigating the role of childcare in detail. While sensitivity analyses show that both men and women increase their time with children following a job loss, the type of childcare activities cannot be further distinguished.

5.2 Conclusions

Next to the main findings of the articles, this thesis also offers a number of conclusions on (research about) the economic and non-economic consequences of job loss, unemployment, and inadequate re-employment. In the following, I discuss three general conclusions that are related to the life course perspective and also refer back to the societal debates and trends described in chapter 1.

First, this thesis shows the value of a *general theoretical model* of the economic and non-economic consequences of job loss, unemployment, and inadequate re-employment. Endorsing the key principles of time (P1), cumulative advantages and disadvantages (P4), and human agency (P5), the results confirm the logical order of the central concepts and the assumed effects they have on each other. Specifically, Articles 1 to 3 show that job loss triggers unemployment, which as a mobility-reinforcing event affects individuals' chances for adequate re-employment on multiple dimensions including labor incomes, non-standard employment risks, and non-monetary job quality. The findings further illustrate the importance of counter-mobility events by highlighting their role in shaping the consequences of job loss and unemployment. Specifically, the decision to accept inadequate re-employment compared to remaining unemployed and continuing the job search affects workers' subsequent careers. Moreover, as I find large variation in the effects of unemployment on non-monetary job quality across countries and over time, I provide empirical evidence for the idea that the interactions of job searchers and employers are affected by historical time and place (P6). While this supports the life course perspective, the results indicate a limited role of economic situation and labor market policies as explanatory factors. As suggested in the last section, this points to the need for some refinements of the theoretical arguments about the moderating role of different macro-level variables. The general theoretical model is also confirmed by the effects of job loss and unemployment in the health and well-being and family domains (P2: different, but interrelated life domains). Moreover, the principle of linked lives (P3) receives support as this thesis illustrates that unemployment not only affects persons own housework hours, but also their partners' domestic activities. Finally, the findings indicate that the theoretical mechanisms put forward in one life domain are important for understanding the consequences in other life domains. For example, processes of cumulative advantages and disadvantages (P4) are revealed by showing that the economic consequences partially explain negative effects of an early-career job loss on later life health. However, as a large part of the effect still remains unexplained, some further theoretical and empirical work is warranted.

Second, this thesis illustrates the importance of implementing an *encompassing evaluation design*. The results show that job loss, unemployment, and inadequate re-employment have negative effects on different, but interrelated life domains (P2) and affect multiple outcomes within these, highlighting their multidimensionality. They also suggest that the consequences are partly shared by family members (P3: linked lives). Furthermore, complementarities are revealed in the results of lower labor incomes, higher non-standard employment risks, and lower non-monetary job quality, while trade-offs are shown for finding quick re-employment and securing a high job quality. Concerning the trends of rising job insecurity described in chapter 1, these findings have two implications: Policy-makers need to be aware that the full costs of job loss and unemployment go far beyond lower employment and earnings. Moreover, they need to balance the trade-off between fostering quick re-employment and ensuring a high job quality in reaction to the highlighted economic pressures. The usefulness of the encompassing evaluation design is further confirmed in the analyses on treatment and effect heterogeneity. The finding of similar effects for job losses due to plant closures, layoffs, and dismissals indicate that the results are not limited to specific events only. The shown similarities are further relevant for the interpretation of previous studies which have not made these differentiations. This thesis suggests that they still give a good impression of the average effects. The value of examining treatment heterogeneity is further highlighted by the results on spouses' immediate adaptation in the domestic sphere in case of one partner's unemployment. With respect to effect heterogeneity, the comparative analyses offer some implications for the societal debates (P6: historical time and place, P7: preventive intervention). In line with the few previous studies no consistent moderating role of labor market policies is found, meaning that they are not associated with unambiguously smaller or larger negative effects of unemployment on job quality. This also shows that more knowledge needs to be accumulated and that a stronger focus on specific policies that are targeted at concrete groups would be beneficial. Most importantly, the findings of the five articles show the significance of taking account of the longevity of the effects (P1: time). This thesis shows that job loss, unemployment, and inadequate re-employment in general have lasting negative effects. For most outcomes the effects are still present after about five years and in some analyses effects for more than 30 years later are found. However, the results also point out that for specific outcomes the effects may be transitory, for example, for temporary employment after job loss. The overall persistency of the consequences has important implications for the trends of rising job insecurity. Given that the interrelated structural changes are likely to intensify the differential exposure and vulnerability to job loss, the shown longevity of effects implies and accumulation of advantages and disadvantages. Therefore, job loss and unemployment are key mobility mechanisms that explain growing inequalities across different life domains and over the life course.

Third, the findings of this thesis support the view that it is important to apply *state-of-the-art methods* of causal and multi-level analysis to test theoretically derived hypotheses. In line with the focus on causal analysis based on observational data, the results of this thesis emphasize the need to consider the (dynamic) selection into life events. Descriptive results and, especially, the findings of (dynamic) (propensity score) matching analyses show that treated and untreated workers substantially differ in observed characteristics that also affect their further careers and lives. This thesis further indicates that clear definitions of the independent variables (e.g., specific reasons for job loss, focus on transitions from employment into unemployment) as well as the use of longitudinal data allow for better causal inferences. For example, I find that selection on observed characteristics is smaller for plant closures than layoffs and dismissals and analyses of pre-treatment trends in outcomes show only small differences if the focus is on specific life events. Moreover, the comparative analyses illustrate the very flexible two-stage approach to multi-level analysis. Specifically, they emphasize its transparency, for example, by visualizing the analyses and interpretations of the cross-level interactions. The comparative analyses also point out the potential of using the logic of a longitudinal within-unit analysis at the macro-level by examining changes in countries over time.

Besides these general conclusions, this thesis also has three implications for future research. First, studies would benefit from extra knowledge about and (even) more concrete definitions of job loss. While the differentiations made in this thesis already go beyond those usually applied, they are still not closely tied to the interrelated structural changes that motivate such research. Specifically, if interest is in how job loss due to technological change or international trade affects labor markets and societies, better tools to identify specific reasons in administrative data or more fine-tuned survey questions for employees and employers should be developed. For example, currently it remains unclear whether a plant closure results from larger trends or even whether it is involuntary or voluntary from the business owner's perspective.

Second, this thesis highlights another methodological issue. Research still has to apply or applies different research designs making it very difficult to integrate findings from different studies. While this is partly explained by different research questions, it mostly is due to practical limitations (e.g., available data). This issue may be addressed in two ways: Methodological studies could systematically vary specific research design choices learning how they affect results (e.g., survey vs. administrative data, definition of samples, definition of treatment and control groups, methods applied). Another solution, which may build on these methodological studies, literature reviews, and meta-analyses, would be to aim for a stronger conceptual and methodological harmonization. This could be achieved by defining best practices and likely implies the collection of new harmonized longitudinal employer-employee data. While projects of this kind have been conducted (e.g., Kuhn, 2002; OECD, 2013), they have not integrated administrative and survey data making it almost impossible to study the economic and non-economic consequences in parallel.

Third, as highlighted by this thesis researchers should continue to examine different, but interrelated life domains and multiple outcomes within these. In addition to the outcomes investigated in this thesis, future research could pay more attention to individuals' social participation, trust, and political engagement. More detailed assessments concerning the family life (e.g., family stability) including a more extensive theoretical and empirical foundation of spillover effects (e.g., partners' health, children's health) are also important avenues for future research.

Although these different ideas can be pursued independently, interdisciplinary and international research that addresses these issues jointly likely results in the greatest benefits. As has been illustrated in this thesis, the life course perspective provides a very useful guiding theme for such projects allowing for a theoretical and methodological integration, but also for making use of the insights from varying disciplines in the social sciences.

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Article 1

Losing standard employment in Germany:
The consequences of displacement and dismissal for workers' subsequent careers

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Abstract

This study examines the effects of job loss on workers' subsequent careers in Germany. To provide a systematic and comprehensive picture, I distinguish between displacement due to plant closure and dismissal, and analyze the effects on workers' subsequent labor market statuses, labor incomes, and non-standard employment risks. The results show that both events have lasting negative effects. Five years after job loss, displaced and dismissed workers have 12 and 15 percentage point lower employment chances respectively. Although this is mostly explained by higher unemployment risks, more than third is due to displaced and dismissed workers leaving the labor force entirely, especially via (early) retirement. Moreover, I find large short-term total labor income losses which are mainly explained by lower employment chances and reduced working hours, but falls in hourly wages become relatively more important as time passes. Five years after job loss, the negative effects on hourly wages still amount to 6 percent for displaced workers and 8 percent for workers who were dismissed. With respect to non-standard employment, I show that both displacement and dismissal increase the risks of self-employment, part-time employment, and temporary employment with only the latter being transitional in nature.

1. Introduction

Over the last three decades, globalization, international trade, and technological change have increased job insecurity for workers in modern market economies (Mills et al., 2006). They are at greater risk of job loss, because firms increasingly adjust to these trends by closing, downsizing, relocating, or restructuring (Brand, 2006).[1] They also often have to accept non-standard re-employment, as in many countries, governments have deregulated employment protection laws and reduced unemployment benefits to increase labor market flexibility (Hipp et al., 2015).

Although the process of job creation and destruction is an important source of productivity growth, it creates significant adjustment costs. For example, it is well-established that job loss has large and persistent negative effects on workers' economic and overall well-being (see Brand, 2015; von Wachter, 2010). Studies have also shown that the risk and consequences of job loss are socially stratified (e.g., Ehlert, 2016), implying that it is a key mobility mechanism through which workers' career experiences are divided over time (Brand, 2006; DiPrete & Eirich, 2006).

Despite extensive research documenting the negative effects of job loss on employment, earnings, and wages (e.g., Ehlert, 2013; Gangl, 2006; Ruhm, 1991), previous studies have usually looked at these outcomes in isolation or ignored important consequences altogether. Further, the use of different data, definitions of job loss, and methods makes it difficult to compare the results for different outcomes across studies (Kuhn, 2002; von Wachter, 2010). To deal with these issues, I provide a unified analysis that offers a systematic and comprehensive picture of the consequences of job loss (Oesch & Baumann, 2015; Upward & Wright, 2017). Specifically, I address the following research questions: What are the short- and long-term consequences of displacement due to plant closure as well as dismissal for workers' subsequent labor market statuses, labor incomes, and non-standard employment risks? The need for a unified analysis is also highlighted by four specific aspects that so far have received limited attention.

First, in the analyses of workers' subsequent labor market statuses, I differentiate non-employment into unemployment, education or training, retirement, and inactivity. Earlier studies have often only examined the level of non-employment, ignoring that some forms (e.g., inactivity) usually raise greater concerns among policy makers than others (e.g., educa-

[1] In the German labor market, every year more than ten percent of workers separate from their current job and about one out of three of these are displaced due to plant closure or dismissed.

tion or training) (OECD, 2013). Second, I link the literature on the total labor income losses (e.g., Ehlert, 2013; OECD, 2013) to studies on so-called earnings or wage scars (e.g., Gangl, 2006; Ruhm, 1991) by separating the former into three different sources: the loss of employment, lower working hours, and lower hourly wages. So far, such decompositions have only been reported for the flexible US and UK labor markets. I provide first analyses for the conservative welfare state of Germany which represents an interesting contrasting case. Third, as pay only partly reflects job quality, I follow the few studies that have examined a broader range of job characteristics (e.g., Brand, 2006; Farber, 1999) and provide first evidence for Germany on workers' use of self-employment, part-time employment, and temporary employment in finding a new job. I extend previous research by simultaneously looking at different types of non-standard employment and examine to which extent these are transitory or lasting. Fourth, previous studies have often grouped displacements due to plant closures with dismissals and sometimes even voluntary job separations.[2] This is unfortunate as plant closures offer a quasi-experimental strategy to estimate the effects of job loss (Brand, 2015). In contrast, for dismissals it is unclear whether or not they should be considered displacements as they may be due to layoffs but also can follow from workers being fired for individual reasons. Therefore, I examine both events separately and assess empirically to which extent they differ in their consequences.

The analyses are based on survey data from the Socio-Economic Panel (SOEP). In contrast to most administrative data, it provides detailed information on workers' labor market statuses, labor incomes, working hours, and non-standard employment allowing investigating a wide range of outcomes.[3] Moreover, the SOEP allows considering the reasons for job separations. It is also one of the longest running household panel studies, making it possible to follow workers for two years before and five years after job loss. This sheds light on the question whether the effects are "temporary blemishes" or "lasting scars" (Ruhm, 1991). In the analyses, I focus on workers who are in standard employment before displacement or dismissal such that findings of increased subsequent non-standard employment are not simply explained by voluntary labor supply decisions. Methodologically, I combine propensity score matching,

[2] Many studies define job loss as transitions from employment to unemployment. This ignores that job loss does not have to result in unemployment as well as mixes involuntary and voluntary job separations. As Brand (2015, p. 360) states "job loss is as discrete event, whereas unemployment is a state, with a great deal of heterogeneity with respect to instigation and duration."

[3] For example, administrative data usually lack information on labor market statuses other than dependent employment and registered unemployment as well as on working hours and non-standard employment.

to find comparable workers who did not experience job loss, with fixed-effects regression models to additionally remove any time-constant unobserved heterogeneity.

The analyses show three important findings: First, displacement and dismissal have persistent negative effects on employment chances. The main source of this is higher unemployment risks, but more than a third of the long-term effect is due to workers who have left the labor force entirely, especially via (early) retirement. This must be interpreted in light of Germany's traditional support of early retirement through a combination of extended unemployment benefits and generous early retirement schemes. Therefore, for many older workers early retirement represented an "attractive" alternative to long-term unemployment and a pathway out of the labor market. However, over the last two decades, these exit routes have been gradually closed such that the negative consequences of job loss for older workers have likely increased (Buchholz et al., 2013). Second, in contrast to recent findings for the US and UK, I show that in Germany the lasting total labor income losses are mostly explained by declines in employment and working hours instead of lower hourly wages. This integrates the seemingly disparate findings that total income losses are greater in conservative welfare states (Ehlert, 2013) but earnings and wage scars are more pronounced in liberal regimes (Gangl, 2006). Third, concerning non-standard employment, both displaced and dismissed workers are found to have consistently higher risk of self-employment, part-time employment, and temporary employment. More importantly, only temporary employment appears to be transitory in nature suggesting that for many workers non-standard re-employment is not a bridge into regular jobs.

2. Literature review and theoretical mechanisms

Sections 2.1 to 2.3 provide reviews of previous studies for each of the outcomes of interest and highlight some limitations that will be addressed in this study. To provide some theoretical background, section 2.4 summarizes the main mechanisms explaining why displacement and dismissal should have negative effects on workers' subsequent careers.

2.1 The effects of job loss on labor market status

Earlier studies show that displacement and dismissal have substantial negative effects on employment chances (Brand, 2015; Fallick, 1996). Some studies find that higher unemployment risks fade away after about five years (e.g., Ruhm, 1991, Upward & Wright, 2017), but other research suggests more lasting consequences (e.g., Schmieder et al., 2010). It is generally known that durations of joblessness vary greatly (Fallick, 1996). Non-employment lasts longer during recessions (e.g., Farber, 2017) or if the economic situation in workers' former industries or occupations is poor. Comparative studies highlight the importance of labor market and welfare state policies (e.g., DiPrete, 2002; Ehlert 2016; Gangl, 2004). In flexible labor markets with low out-of-work benefits workers find new jobs quicker, partly because they cannot afford a lengthy search. Moreover, layoffs lead to longer unemployment than displacements due to plant closures, as they are supposed to send a negative signal about the workers (e.g., Gibbons & Katz, 1991).

However, previous studies have strongly focused on the level of non-employment (Fallick, 1996), ignoring policy makers' interest in its composition (OECD, 2013). Workers updating their skills (i.e., education or training) or actively looking for a job (i.e., unemployed) likely have a stronger labor market attachment than inactive persons. Re-employment of the registered unemployed can also be supported by active labor market policies. In contrast, governments have little influence over discouraged workers who have left the labor force and are less likely to return (i.e., inactive). Retirement may or may not be considered problematic dependent on whether governments support early retirement to relieve the labor market in times of high unemployment (Buchholz et al., 2013). Recent research also attests to the importance of examining different forms of non-employment showing that workers leaving the labor force explains as much as unemployment of the overall non-employment effect (Oesch & Baumann, 2015; Upward & Wright, 2017). I add to these studies by looking at different reasons for job loss and examine how the effects on different forms of non-employment develop over time.

2.2 The effects of job loss on labor income

Displacement and dismissal have also been shown to cause large earnings losses (Brand, 2015; von Wachter, 2010), but estimates of the immediate (5 to 60 percent) and persistent effects (0 to 30 percent) vary greatly (e.g., Couch & Placzek, 2010; Ehlert, 2013; Gangl, 2006; Jacobsen et al., 1993; OECD, 2013; Ruhm, 1991). This is partly due to differences in worker and context-level characteristics. Earnings losses are cyclical (e.g., Farber, 2017) and larger for laid-off than displaced workers (e.g., Gibbons & Katz, 1991). Higher tenure and industrial or occupational mobility also result in larger negative effects, pointing to the loss of rewards to specific skills. However, methodological differences are another source of variation (Kuhn, 2002; von Wachter, 2010). Some studies include zero income during non-employment in their measurement of earnings, because non-employment contributes to the full costs of job loss (e.g., Ehlert, 2013; OECD, 2013). Other research has restricted their samples to workers with some positive earnings, to focus on losses due to lower working hours and hourly wages (e.g., Couch & Placzek, 2010; Jacobsen et al. 1993). Moreover, estimated earnings losses increase with the length of the reference period for the reported labor incomes (OECD, 2013) as studies examining annual or quarterly earnings are affected to a greater extent by non-employment, even if they use some kind of positive earnings restrictions (Kuhn, 2002).

Despite the fact that differences in the earnings and wage measurements used make comparisons across studies difficult, decompositions of the total labor income losses into its effects via non-employment, lower working hours, and lower hourly wages have rarely been reported. A recent exception is the study by Upward and Wright (2017) showing that in the UK the short-term total labor income losses are mostly due to higher non-employment, but that in the long-run falls in income are mainly driven by lower hourly wages than reduced hours of work. Lachowska et al. (2018) find for the US that at the time of the mass-layoff, non-employment and fewer working hours account for 80 percent of the total losses, while five years later lower wage rates are the main source. The current study adds to this research by decomposing the total labor income losses after displacement and dismissal in Germany. This not only sheds light on the unanswered question how much of the earnings losses are due to lower working hours instead of hourly wages, but also helps integrating findings from comparative research that show that total income losses are larger in conservative welfare states (e.g., Ehlert, 2013), but that conditional on employment earnings and wage scars are stronger in liberal regimes (e.g., Gangl, 2006).

2.3 The effects of job loss on non-standard employment

Non-standard employment has strongly increased over the last three decades (Hipp et al., 2015). Although some have argued that it promotes the labor market re-integration of the unemployed, it also has been questioned in terms of its job quality (e.g., OECD, 2002, 2010; OECD/European Union, 2017).[4] Despite the fact that alternative work arrangements are important re-employment routes, they rarely have been investigated as consequences of displacement or dismissal. To date Farber (1999) offers the only encompassing assessment of the effects of job loss on different forms of non-standard re-employment. Using US data, he finds

[4] To take account of the heterogeneity of non-standard employment as well as the fact that concerns about job quality vary within its specific forms, too, I distinguish solo from other self-employment, marginal from regular part-time employment, and temporary agency jobs from fixed-term contracts in the sensitivity analyses.

that the risks of part-time and temporary employment increase substantially. While these effects are shown to level off with time since job loss, these findings are not established by following the same workers over time and in some analyses no control group is used. Some other studies have focused on specific forms only (Dieckhoff, 2011; Farber, 2017; Oesch & Baumann, 2015; von Greiff, 2009). While they also find that job loss increases the risk of non-standard employment, they often follow workers for a only short period of time or do not take into account the reason for job loss. However, similar to the employment and earnings effects, workers who have been laid off may face greater non-standard re-employment risks than those who have experienced plant closure. Moreover, a number of studies have examined non-standard re-employment's role as a stepping stone towards regular employment (e.g., Gebel, 2013; Lietzmann et al. 2017). These studies emphasize that workers may not only react to financial constraints when taking up these jobs, but also consider that acquiring work experience, signaling their motivation, or searching on-the-job offers greater chances for adequate re-employment than a continued job search. If self-employment, part-time employment, and temporary employment act as stepping stones, the higher risks following displacement or dismissal should be transitory. I here follow workers for up to five years after job loss to provide some first empirical evidence for Germany.

2.4 Theoretical mechanisms for the effects of job loss

In this study I focus on estimating the effects of job loss on workers' subsequent labor market statuses, labor incomes, and non-standard employment risks. However, to provide some theoretical background, I summarize the three mechanisms that have been put forward in the literature to explain the negative effects of job loss (e.g., Brand, 2006; Gangl, 2006). The first mechanism rests on the distinction between general and specific human capital (Becker, 1993). Because the latter is not transferable across employers, displaced or dismissed workers lose rewards associated with their firm-specific skills. Comparable losses originate from workers changing industries or occupations in order to find a new job. Besides the loss of these rewards, non-employment due to job loss may also lead to the depreciation of general skills, further decreasing workers' employability.

Signaling theories present a second explanation, pointing to problems of asymmetric information (Spence, 1973). If employers infer applicants' unobserved productivity from their work biographies, job loss and the associated periods of non-employment represent a negative signal. Gibbons and Katz (1991) argue that the signal of job loss is more adverse for layoffs, because displacements due to plant closures do not carry information about workers' performances. This further highlights the importance to empirically distinguish these events. A third explanation stresses the role of search constraints in matching workers to jobs. Although workers usually change jobs voluntarily to improve their labor market position, displacement or dismissal force them to find re-employment despite being “in a poor position to perform a quality job screening” (Brand, 2006, p. 294). Specifically, job search theories argue that workers’ reservation wages decrease over time, because benefits run out and private incomes are used up (Mortensen, 1986).

Together these explanations imply that displaced and dismissed workers have difficulties to find new jobs that provide similar rewards to the one’s they lost, with more negative effects being expected for the latter. In the short-run this will lead to increased non-employment, partly because they will hold out for jobs befitting their skills and qualifications. Contingent on various factors, workers may react differently over time: Some will try to update their skills, while others may leave the labor force entirely. The majority of workers will, however, have to accept jobs of lower quality implying financial losses and increased risks of non-standard employment.

3. Data, measures, and methods

3.1 Data

The Socio-Economic Panel (SOEP) is an annual household panel survey that is designed to be nationally representative of the adult population living in private households in Germany (Wagner et al., 2007). It started in 1984 with approximately 12,000 persons living in 6,000 households. In the latest wave, in 2015, about 27,000 persons and 16,000 households were interviewed. The SOEP interviews all household members aged 16 years and over and offers detailed data about job separations and their reasons, workers’ labor market statuses, labor incomes, and job characteristics as well as rich socio-demographic information. For the analyses, I follow workers who are displaced due to plant closure or are dismissed (treatment groups) between the interviews t and $t+1$ for up to three ($t-2$) interviews before and five interviews ($t+5$) after job loss. For each treated group, a control group of workers who do not experience job loss is defined since part of the negative effects may result from foregone upward mobility. I further restrict the sample to workers aged 20-60 years in t and who are in standard employment (i.e. dependent full-time employment with a permanent contract) to focus on employees with stable careers. This also makes sure that findings of increased non-standard employment risks after job loss are not simply due to voluntary labor supply decisions. Moreover, these restrictions further reduce heterogeneity. As information on displacement due to plant closure or dismissal is only available since 1991, I use the data from the years 1988 to 2015. Applying these restrictions, the sample includes 116,417 person-spells from 21,515 persons, including 1,040 persons-spells with displacements due to plant closure and 2,641 with dismissals.

3.2 Measures

Table S1 in the supplementary material provides an overview about the measurements. The *treatment groups* displacement or dismissal between t and $t + 1$ are based on workers' reports about job separations and their reasons. Reported displacements are due to plant closure. For dismissals it remains unclear whether they are due to layoffs or workers being fired for individual reasons. While this cannot be distinguished, I go beyond most previous studies using the SOEP by explicitly separating displacements from dismissals. Workers who did not change their job are included in the control groups as are workers who resigned or had a mutual agreement. Including these voluntary and other job separations allows for a more general counterfactual than only focusing on workers who remained with their employer (Upward & Wright, 2017). Some reasons are not considered, as they are not in line with the sample restrictions (e.g., expiry of fixed-term contract, end of vocational training, closure of own business), indicate that workers leave the labor force (e.g., (early) retirement, leave of absence), or are ambiguous (e.g., other).

Three groups of *outcomes* are measured at the time of the interview: labor market status, labor income, and non-standard employment. I distinguish five labor market statuses: employment, unemployment, education or training, retirement, and inactivity. The latter three are also grouped as out of labor force. Employment includes both dependent employment and self-employment. In education and training is comprised of persons who are, in general, in vocational, or in higher education as well as those in further education or occupational retraining. Inactivity includes all other non-employed persons. In the sensitivity analyses, the proportions of months in each labor market status since the last interview are also examined.

The gross labor income in the last month excludes any extra income but includes overtime pay. The self-employed are asked to estimate their monthly income before tax. To decompose the total labor income losses into lower employment, working hours, and hourly wages three variables are used: Labor income including zero income for non-employment, labor income conditional on employment, and gross hourly wage conditional on employment. The latter is calculated by using the average actual weekly working hours including overtime. All income variables are measured in Euros and deflated to 2011 prices using the consumer price index.

Non-standard employment is defined as self-employment, part-time employment, and temporary employment. Self-employment includes farmers, freelance professionals and academics, other self-employed and family workers. In the sensitivity analyses, I distinguish the solo self-employed from all other self-employed. Part-time employment is defined as having less than 35 average actual working hours and in the sensitivity analyses marginal (0<hours<15) and regular part-time employment (15≤hours<35) are separated. Temporary employment is measured by having a fixed-term instead of a permanent contract. In the sensitivity analyses, temporary agency employment, where the de-jure and de-facto employer are not the same, is analyzed, too. Both forms of temporary employment are conditional on dependent employment.

To control for differences in observable characteristics between the treatment and control groups, I use several sets of *control variables*. These variables refer to the interview before job loss to avoid post-treatment bias and are selected theoretically assuming that they affect workers' risk to experience displacement or dismissal as well as their future careers. They include demographics (age, sex, migration background), education, detailed measures of overall (full-time, part-time, unemployment experience) and recent (number of month in different labor market statuses in the year before the interview) employment history, job characteristics (industry, firm size, occupation), firm tenure, household structure (partner or spouse, number of persons/children), health, region, and state unemployment rates. Specifically, demographics and household structure likely affect employers' firing and hiring as well as workers' labor supply decisions. Health, region, and state level unemployment affect workers' displacement or dismissal risks and represent important search constraints concerning their further career. Education, employment history, and firm tenure capture differences in human capital and indicate previous labor market attachment or performance. Industry, firm size, and the occupation are important structural determinants of the risk of job loss that will also affect workers' subsequent career paths.

3.3 Methods

To analyze the effects of job loss, matching is combined with fixed-effects regression models and all analyses are performed separately for displacement and dismissal. In a first step, matched control groups are formed by combining exact and propensity score matching. In a second step, fixed-effects regression models are run on the matched samples to estimate the effects of displacement or dismissal on those workers who have experienced the respective events.

To ensure that workers who lose their job and those who do not face the same aggregate labor market conditions, exact matching on the year before job loss is used.[5] For the remaining control variables, propensity score matching was performed with the propensity scores $p(\boldsymbol{x})$ estimated by logistic regression models of the respective treatment indicators on the vector of control variables $\boldsymbol{x}$ (Tables S2-S3 in the supplementary material). Comparing different matching algorithms, radius matching with calipers of 0.001 for displacement and 0.004 for dismissal was used, because it provided the best results in the trade-off between bias, variance, and scope of the estimator (Gangl, 2015).[6,7] Due to the exact matching on the year, common support is also checked in every year. For displacement only 12 of the 1,040 (1.2 percent) and for dismissals only 34 of the 2,641 persons-spells (2.8 percent) are off support. The matching was performed using the psmatch2 ado in Stata (Leuven & Sianesi, 2003).

Based on the matched samples, which include treated and untreated workers with similar estimated propensity scores $\hat{p}(\boldsymbol{x})$ and should, therefore, be balanced on $\boldsymbol{x}$, fixed-effects regression models are run allowing to control for any time-constant unobserved heterogeneity between the workers who lost their jobs and those who did not. The following model is fitted

$$y_{it} = a_i + \sum_{k=-1}^{5} \gamma_k T_k + \sum_{k=-1}^{5} \delta_k T_k D_i + \varepsilon_{it}$$

with y_{it} as the outcome for worker i in year t, a_i as a worker fixed effect, T_k as a set of dummy variables indicating time relative to the reference year $(t-2)$, D_i as a time-constant treatment group indicator, and ε_{it} as the idiosyncratic errors. The coefficients γ_k measure the outcome trajectory in the control group relative to the reference year $(t-2)$ and the coefficients δ_k measure the difference in the outcome trajectories between the treated and controls, providing estimates of the average treatment effect on the treated. Standard errors are clus-

[5] Exact matching on the year also ensures that workers are not matched with themselves as different person-spells of the same person may be part of the treatment group for one year and part of the control group in other years.
[6] The calipers are set to 10 percent of the standard deviation of the estimated propensity scores meaning that they are stricter than calipers that are usually employed in the literature (Gangl, 2015).
[7] King and Nielsen (2016) argue for the use of coarsened exact matching (CEM) over propensity score matching (PSM), as PSM may not necessarily decrease imbalances. I show below that PSM improves the balance from before to after matching in this study. Jann (2017) also suggests that King and Nielsen's results rest on comparing CEM to one-to-one nearest neighbor PSM without replacement. He argues that matching algorithms that do not throw away good matches such as radius matching are not affected by King and Nielsen's main critique.

tered by workers. For binary outcomes y_{it} (labor market statuses, non-standard employment), the above model is estimated as a fixed-effects linear probability model offering a clear interpretation of the coefficients δ_k as average discrete changes in the probability of the outcome (Wooldridge, 2010). Interpreting the δ_k as effects of job loss requires the assumption that in the absence of displacement or dismissal the outcome trajectory of the treated would have remained parallel to the one of the untreated workers. While this assumption cannot be tested, zero effects of displacement or dismissal in the pre-treatment periods shown below indicate its plausibility.

4. Results

4.1 Descriptive results

To provide an overview about the sample, Table 1 reports the sample size, the number of interviews, and some descriptive statistics separately for both treatment groups. The analyses are based on 1,040 persons-spells with displacement and 2,641 person-spells with dismissal and the median number of interviews is seven for both groups. The sample also holds information on 112,736 person-spells who did not experience job loss and are used to form the matched control groups. Of the 1,040 workers that are displaced between t and $t + 1$, about 84 percent are re-employed in the five years after job loss. Among the 2,641 workers who are dismissed about 77 percent are re-employed. For workers who return into the labor market median durations until re-employment are the same for both treatment groups. Interestingly, a smaller share of workers are able to find standard re-employment, with only two third of those who are displaced and six out of ten workers of those who are dismissed. The latter also need longer to find standard re-employment. These results already point out that non-standard employment is likely to be an important route back into the labor market, an issue that is examined in more detail below.

Table 1 Re-employment after displacement and dismissal

	Displacement		Dismissal	
	Percent/median	LQ, UQ	Percent/median	LQ, UQ
Re-employment?	83.5%		76.6%	
Years to re-employment	1	1, 2	1	1, 2
Standard re-employment?	66.7%		59.0%	
Years to standard re-employment	1	1, 2	2	1, 3
Number of interviews	7	6, 8	7	6, 8
N (person-spells)	1,040		2,641	
N (persons)	987		2,225	

Notes: LQ: lower quartile. UQ: upper quartile.
Sources: SOEP 1988-2015, author's calculations.

4.2 Propensity score matching

The following discussion focuses on control variable balance before and after propensity score matching to assess the matching quality. To compare the balance, Table 2 reports the means of the respective treatment groups as well as of the unmatched and matched control groups. It also shows the corresponding standardized biases. The mean (median) standardized bias for displacement reduces from 13.7 (10.8) to 0.7 (0.5) percent from before to after matching. For dismissal the respective changes are from 23.2 (21.5) to 0.8 (0.4) percent. Notably, the differences in observed characteristics before matching are much larger for dismissal than for displacement supporting the argument that plant closures represent a more exogenous reason for job loss. However, the results also show that for both groups there are relevant differences between treated and untreated workers. That the matching has been successful is not only confirmed by the very low mean (median) standardized bias, but also by the fact that for each single control variable the standardized bias is below the threshold of five percent (Gangl, 2015).

Table 2 highlights some further differences. Due to the exact matching on the year before job loss the standardized bias falls to zero after matching. Overall, the patterns for both treated groups are similar, although the differences to the control group are usually larger for dismissal. Displaced and dismissed workers are younger, less often female, and more often have a migration background. They have lower education as well as less total and recent employment and more unemployment experience. They are more likely to work in the primary and construction as well as trade industries but are less often employed in the bank and insurance as well as the services sector. They work in smaller firms, are more often blue-collar workers and less often white-collar workers or civil servants. Strong differences are also revealed for firm tenure indicating that low tenure increases the risk of job loss. Dismissed workers live less often with a spouse, while displaced workers have fewer children. Both treatment groups are less satisfied with their health and more often live in East-Germany, pointing to relevant regional differences. This is further confirmed by higher state unemployment rates for displacement and, in particular, dismissal.

Table 2 Balance of the control variables before and after matching separated for displacement and dismissal

	Displacement					Dismissal				
	Treated	Controls				Treated	Controls			
		Before		After			Before		After	
	Mean	Mean	SB	Mean	SB	Mean	Mean	SB	Mean	SB
Year before job loss	2001.49	2003.40	-28.4	2001.49	0.0	2001.85	2003.40	-23.3	2001.85	0.0
Age	41.06	41.78	-7.3	41.00	0.6	39.19	41.78	-24.9	38.99	1.8
Female	0.31	0.33	-5.0	0.31	-0.2	0.30	0.33	-6.3	0.31	-1.2
Migration background	0.25	0.21	8.3	0.25	-0.8	0.25	0.21	9.1	0.25	-0.3
Education: Less than lower secondary	0.41	0.34	14.9	0.42	-1.8	0.44	0.34	20.6	0.44	0.2
Intermediate or higher secondary	0.43	0.42	1.8	0.42	1.8	0.44	0.42	5.0	0.44	0.1
Tertiary	0.17	0.25	-20.1	0.17	0.0	0.12	0.25	-32.5	0.12	-0.4
Total experience: Full-time employment	17.94	18.34	-3.8	17.95	-0.1	16.05	18.34	-21.3	15.90	1.3
Part-time employment	1.03	1.07	-1.4	0.99	1.3	0.92	1.07	-5.0	0.93	-0.4
Unemployment	0.52	0.37	13.3	0.49	1.8	0.93	0.37	39.7	0.88	3.0
Recent experience: Full-time employment	11.16	11.29	-4.9	11.16	0.1	10.55	11.29	-25.9	10.63	-2.6
Part-time employment	0.37	0.42	-2.4	0.35	0.8	0.34	0.42	-3.7	0.34	0.2
Unemployment	0.26	0.10	16.2	0.26	0.0	0.67	0.10	38.5	0.59	4.0
Education or training	0.12	0.12	-0.6	0.13	-0.9	0.28	0.12	12.5	0.28	-0.6
Inactivity	0.08	0.07	2.3	0.09	-1.8	0.16	0.07	10.5	0.15	1.0
Industry: Primary & construction	0.26	0.18	19.6	0.27	-0.8	0.31	0.18	31.2	0.31	1.3
Manufacturing & energy	0.31	0.26	9.5	0.30	1.2	0.25	0.26	-2.2	0.25	-0.1
Trade	0.18	0.11	21.2	0.18	0.3	0.18	0.11	18.7	0.18	-0.8
Transport	0.07	0.06	5.6	0.07	1.5	0.05	0.06	-2.2	0.05	0.3
Bank & insurance	0.02	0.05	-16.0	0.02	-1.2	0.01	0.05	-21.2	0.01	-0.3
Services	0.15	0.34	-43.7	0.16	-1.4	0.19	0.34	-33.9	0.19	-0.7
Firm size: Less than 20 employees	0.33	0.17	35.7	0.33	0.1	0.42	0.17	55.5	0.42	0.6
20 to less than 200 employees	0.34	0.29	10.5	0.34	-0.2	0.34	0.29	9.2	0.34	-0.1
200 to less than 2000 employees	0.16	0.26	-22.6	0.16	0.1	0.16	0.26	-25.2	0.16	-0.5
2000 or more employees	0.17	0.28	-26.6	0.17	0.1	0.09	0.28	-49.5	0.09	-0.1
Occupation: Blue-collar worker	0.49	0.35	28.8	0.50	-0.5	0.58	0.35	46.0	0.57	0.2
White-collar worker	0.49	0.57	-16.7	0.48	1.1	0.42	0.57	-30.6	0.42	0.2

Table 2 continued

Civil servant	0.02	0.07	-28.1	0.02	-2.3	0.00	0.07	-38.2	0.00	-3.9
Firm tenure	9.51	11.94	-25.6	9.39	1.2	5.62	11.94	-72.7	5.62	0.0
Partner in household: No Partner	0.21	0.22	-2.1	0.21	0.2	0.27	0.22	11.1	0.26	1.0
Partner	0.14	0.12	5.5	0.13	1.9	0.16	0.12	12.7	0.16	0.0
Spouse	0.65	0.66	-2.1	0.66	-1.5	0.57	0.66	-18.9	0.57	-0.8
Number of persons	2.96	2.98	-1.1	2.98	-1.0	2.96	2.98	-1.2	2.96	0.0
Number of children	0.50	0.56	-7.9	0.49	0.3	0.57	0.56	0.9	0.57	0.5
Health satisfaction	6.82	7.03	-10.7	6.82	-0.3	6.73	7.03	-14.9	6.75	-1.3
Region: North Germany	0.11	0.13	-4.0	0.11	0.0	0.11	0.13	-5.5	0.11	-0.3
East Germany	0.34	0.26	18.4	0.34	0.0	0.42	0.26	35.6	0.42	-0.1
South Germany	0.30	0.36	-11.5	0.30	-0.4	0.26	0.36	-21.5	0.26	0.2
West Germany	0.24	0.26	-3.9	0.24	0.3	0.21	0.26	-11.7	0.21	0.2
State unemployment rate	11.25	10.24	22.1	11.23	0.4	12.08	10.24	39.0	12.18	-1.8

Notes: Means for the treated on-support are reported. SB: standardized bias. See Table S1 for details on the measurement of the variables.
Sources: SOEP 1988-2015, author's calculations.

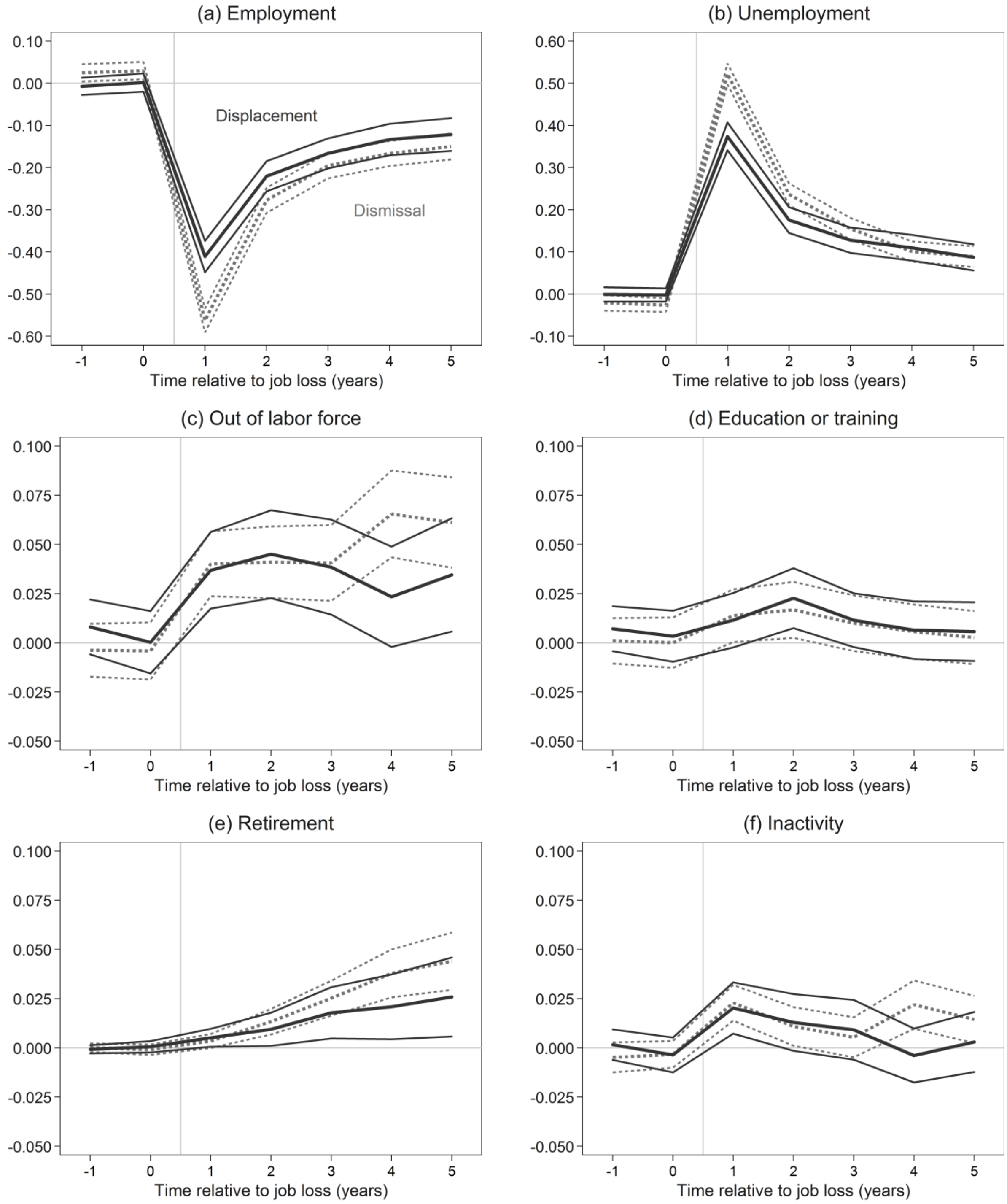
4.3 Effects of displacement and dismissal

The results of the fixed-effects regression models based on the matched samples are illustrated in Figures 1 and 2. Specifically, I plot the coefficients δ_k for the interaction between the time variable and the time-constant treatment group indicator, because these represent the estimates of the average treatment effects on the treated. The full regression tables are presented in the supplementary materials (Tables S4-S13). To help readers relating the figures and tables, I have bold-printed or highlighted some exemplary coefficients in the tables.

Figure 1 illustrates the effects of displacement (solid black line) and dismissal (dashed grey line) on workers' subsequent *labor market statuses* along with 95 percent confidence intervals. Panel (a) shows large short-term negative employment effects. A year after job loss they amount to about 41.1 (=-0.411*100, coefficient in bold, Table S4) and 56.3 (=-0.563*100, coefficient in bold, Table S6) percentage points for displacement and dismissal respectively. Five years after job loss they remain at 12.2 and 15.0 percentage points, suggesting that many workers face difficulties in finding stable re-employment or have stopped searching. From panel (b) and (c) it can be seen that in the short-run these non-employment effects are mainly due to higher unemployment risks, but in the long-run workers leaving the labor force relatively gains in importance. In the first year after displacement about 91 percent (=[0.374/0.411]*100, dark-grey shaded coefficients, Table S4) of the negative employment effect is due to unemployment, with the remaining 9 percent being explained by workers out of labor force. In the fifth year after displacement, 29 percent (=[0.035/0.122]*100, light-grey shaded coefficients, Table S4) of the employment effect is explained by workers leaving the labor force. Similar estimates are shown for dismissal with even 41 percent (=[0.061/0.150]*100, light-grey shaded coefficients in Table S6) of the negative long-run employment effect being due to workers not searching for a job.

While panel (c) combined the different out of labor force states, panels (d) to (e) show the disaggregated effects for education or training, retirement, and inactivity. The positive total out of labor force effects are in the short-run not driven by any of these single activities. For example, two years after displacement, workers are more likely to be in education or training (2.3 percentage points, =0.023*100), retirement (0.9 percentage points, =0.009*100), and inactivity (1.3 percentage points, =0.013*100) adding up to about 4.5 percentage points (coefficients in bold, Table S5). In the first years after dismissal similar effects are revealed (Table S7). However, as time passes, both treatment groups increasingly make use of retirement.

Figure 1 Effects of displacement and dismissal on labor market status (change in probability)



Notes: Matched samples. Fixed-effects estimates with 95 percent confidence intervals based on clustered standard errors for the effects of displacement (solid black lines) and dismissal (dashed grey lines). See Tables S4-S7 for the full regression models. The plotted effects are the respective interaction coefficients δ_k. Out of labor force (c) combines panels (d) Education or training, (e) Retirement, and (f) Inactivity.
Sources: SOEP 1988-2015, author's calculations.

Education or training represents only a short-term response to job loss with the largest but overall rather small positive effects observed two years after the event. The long-run effects for inactivity are somewhat more pronounced for dismissal. Overall, these findings confirm the idea of persistent negative employment effects. However, they go beyond previous studies in illustrating that although non-employment is mostly due to unemployment, workers also exit the labor market via other routes, especially (early) retirement. Given the higher short-term unemployment risks associated with dismissal, this article suggest that dismissed workers indeed take longer to find re-employment (Gibbons & Katz, 1991), although, the differences become smaller over time.

Figure 2 shows the results for the two other outcome groups of interest: labor income (panels (a) to (c)) and non-standard employment (panels (d) to (e)). For the analyses of labor income, I estimate the effects of displacement and dismissal on three different measures, to decompose the total labor income losses into three sources: lower employment, lower working hours, and lower hourly wages.[8] Because panel (a) includes zero income to measure the total labor income losses, the dependent variable is unlogged. In panels (b) and (c) logged dependent variables are used. Before comparing the effects across the measures to assess the importance of lower employment, working hours, and hourly wages, each measure of labor income is first discussed separately.

Looking at the total labor income including zero for non-employment (panel (a)), large short-term losses are revealed. In the year after job loss, total labor income falls by 1,053 Euro for displacement and 1,346 Euro for dismissal (coefficients in bold, Tables S8, S10). This amounts to losses of about 40 and 61 percent in terms of each treatment groups' average incomes in the year before job loss.[9] As time passes, these falls in income are reduced to 407 Euro (15 percent) and 514 Euro (23 percent) five years after displacement and dismissal respectively. These income losses may be due to changes compared with workers' situation before job loss or because workers miss out on income growth they would have experienced in the absence of job loss. This illustrates the importance of a control group allowing estimating the trend in labor incomes in the absence of job loss. These estimates are not shown in Figure 2, but are given by the coefficients γ_k for the time variable T_k (Tables S8, S10). Espe-

[8] The analyses rest on labor incomes from workers in dependent as well as self-employment. Excluding all person-spells with at least one person-year in self-employment does not change the results substantially.

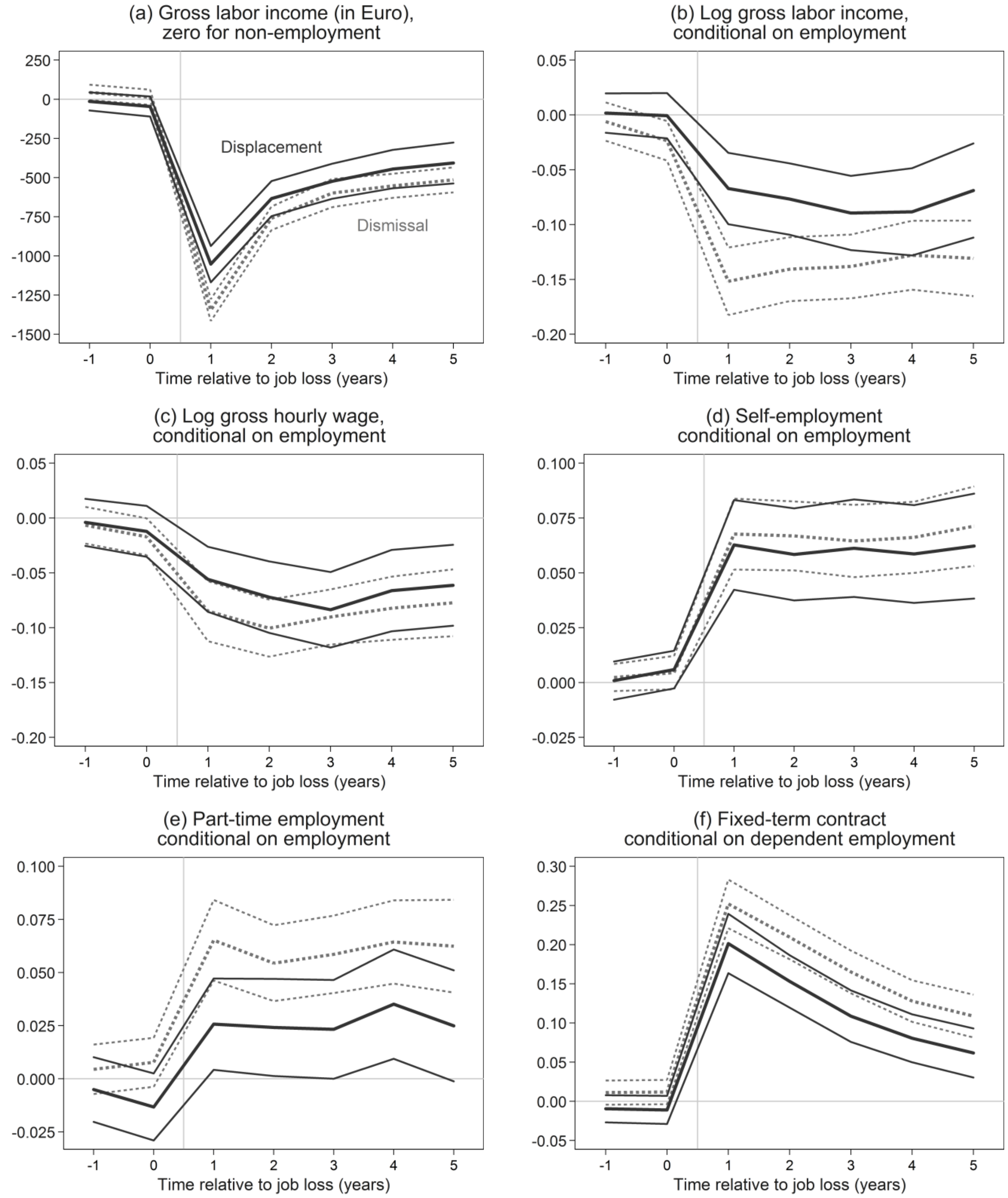
[9] The average incomes of displaced and dismissed workers in the year before job loss are 2,646 and 2,223 Euro. The percentage losses in the year after job loss are 40 (=[1,053/2,646]*100) and 61 percent (=[1,346/2,223] *100).

cially for dismissal the control group trend is positive, suggesting that some part of the total losses can be explained by foregone upward mobility.

Panels (b) and (c) in Figure 2 present the results for labor income last month as well as hourly wages conditional on employment. For the former the negative effects of displacement are relatively stable with 6.9 percent (=[exp(0.067)-1)]*100) in the first and 7.1 percent (=exp(0.069)-1) in the fifth year after job loss (coefficients in bold, Table S8). For dismissals, the estimates are larger ranging between 16.4 percent (year 1, =[exp(0.152)-1)]*100)) to 14.0 percent (year 5, =[exp(0.131)-1)]*100)) (coefficients in bold, Table S10). Looking at the growth of the monthly labor income in the respective control groups (Tables S8, S10) it becomes clear that for both events the majority of the negative effects are due to workers missing out on income growth they would have experienced in the absence of job loss. Considering hourly wages (panel (c)), losses ranging between 5.8 (year 1, =[exp(0.056)-1)]*100) and 6.3 percent (year 5, =[exp(0.061)-1)]*100) are found for displacement (coefficients in bold, Table S8). For dismissal the negative effects on hourly wages amount to 8.9 percent (=[exp(0.085)-1)]*100) in the year after the event and 8.0 percent (=[exp(0.077)-1)]*100) in the long-run (coefficients in bold, Table S10). For both events the hourly wage scars are mostly explained by the wage growth workers missed out on.

I now turn the question to what extent the total labor income losses are due to the loss of employment, lower working hours, and lower hourly wages. To make the results comparable to previous studies, I first compare panels (a) to (c) to examine the extent to which the total losses are due to non-employment and lower working hours as opposed to lower hourly wages. For displacement the total labor income losses amount to 40 percent, but hourly wages are reduced by only 5.8 percent. This means that about 86 percent (=1-(5.8/40.0)) of the total labor income losses can be attributed to non-employment or lower working hours, with the remaining 14 percent being due to lower hourly wages. For dismissal about 85 percent (=1-(8.9/61.0)) of the negative effects are due to lower employment or working hours. In the long-run hourly wage losses become more important, but the percentage of total labor income losses that is attributed to non-employment or lower working hours remains at 58 percent for displacements and 65 percent for dismissals. In contrast to findings for the US and UK (Lachowska et al., 2018; Upward & Wright, 2017), higher non-employment or lower working hours are more important in explaining the long-run negative effects of displacement and dismissal in Germany.

Figure 2 Effects of displacement and dismissal on labor income last month (a-c) (change in level or logs) and non-standard employment (d-f) (change in probability)



Notes: Matched samples. Fixed-effects estimates with 95 percent confidence intervals based on clustered standard errors for the effects of displacement (solid black lines) and dismissal (dashed grey line). See Tables S8, S10, S12, and S13 for the full regression results. The plotted effects are the respective interaction coefficients δ_k. Effects for panel (a) are measured in Euro deflated to 2011 prices and effects for panels (b) and (c) are in log labor income and log hourly wages.
Sources: SOEP 1988-2015, author's calculations.

However, these comparisons still do not assess the relative importance of lower working hours conditional on employment. Contrasting panels (b) and (c), it can be seen that losses in monthly labor income and hourly wages are very similar for workers who have been displaced. For example, in the fifth year after the event displaced workers have monthly labor income losses of about 7.1 percent and hourly wage losses of about 6.3 percent, indicating that lower working hours are relatively unimportant. In contrast, for dismissed workers five years after the event, their monthly income is reduced by 14 percent, but their hourly wage losses only amount to 8 percent. Lower working hours are, therefore, a relevant explanation for dismissed workers' total labor income losses.[10] Another comparison that is interesting is between displaced and dismissed workers. In line with Gibbons and Katz (1991), who analyze weekly earnings, I find larger negative effects for dismissal than displacement when looking at the monthly labor income conditional on employment (panel (b)). However, when investigating hourly wages these differences become much smaller suggesting that the more negative effects of dismissal are mostly due to their reduced post-job loss working hours.

Panels (d) to (e) of Figure 2 illustrate the results for workers' risk of *non-standard employment* after job loss. Panel (d) reveals an increased probability of self-employment of about 5 to 7 percentage points with very similar time paths for displacement and dismissal (Tables S12, S13). For part-time employment (panel (e)) the effects differ somewhat with 2 to 4 percentage point increases for displacement and somewhat larger positive effects of about 5 to 6 percentage points for dismissal. Concerning fixed-term contracts (panel (f)), it is shown that workers who lose their job have higher risks of only holding a fixed-term contract with a 20 (displacement) and 25 (dismissal) percentage points increase in the first year after the event. However, over time these risks decrease to 6 and 11 percentage points. Overall, this suggests that displaced and dismissed workers initially have a substantially higher likelihood for non-standard employment and that only for fixed-term contracts the time paths suggest that the effects may be transitory.

4.4 Sensitivity analyses

To address concerns that employment is only measured at one specific point in time, the analyses for labor market status are repeated using the retrospectively reported monthly status. The proportion of months a worker spent in each status since the last interview is analyzed

[10] Estimates on the effects on working hours including zero and log working hours conditional on employment are presented in Tables S9 and S11 in the supplementary material, supporting these conclusions.

and the results are illustrated in Figures S1 and S2 (Tables S14-S17) in the supplementary material. The findings closely mirror those reported above showing that the large short-term effects on employment are mainly explained by higher unemployment. In the long-run workers leaving the labor force explains a larger share of the non-employment effect with retirement being the main reason. As the monthly data also allow distinguishing part-time and full-time employment, the proportion of months in part-time conditional on employment is examined, too. Similar to the main analyses dismissed workers have somewhat greater part-time risks than displaced workers.

Additional sensitivity analyses address the heterogeneity within the forms of non-standard employment. For these analyses displacement and dismissal have been grouped as the analyzed outcomes are rare and information is not available in all years. The results are shown in Figure S3 (Tables S18-S20) in the supplementary material. Solo-self-employment and other self-employment are distinguished as it is the former that raises the greatest concerns about job quality. Panel (a) in Figure S3 reveals that the increases in self-employment are mainly due to solo self-employment which makes up for about 70 percent of the effect in all years except for the last. Panel (b) distinguishes between marginal and regular part-time employment. It reveals that the positive part-time effect is mostly due to increases in regular instead of marginal part-time employment with the latter being responsible for about 19 to 38 percent of the effect. Lastly, panel (c) provides evidence that workers who have lost their job have an increased short-term risk for temporary agency work of about 3 to 5 percentage points. As time passes, this effect becomes close to zero suggesting that agency work is only used temporarily to return into the labor market.

5. Conclusions

This article examined the effects of displacement due to plant closure and dismissal on workers' subsequent careers in Germany. Focusing on workers in standard employment, it provides comprehensive empirical evidence about the consequences of job loss on multiple career dimensions as well as how these evolve over time. Specifically, using data from the Socio-Economic Panel (SOEP), I analyzed the effects on workers' labor market statuses, labor incomes, and non-standard employment risks for up to five years after job loss.

Applying fixed-effects regression models to matched samples, three main results stand out: First, displacement and dismissal have large and persistent negative effects on employment chances. While it is shown that in the short-run these negative employment effects are mainly due to increased unemployment, over time the share that can be attributed to workers leaving the labor force rises. Especially (early) retirement seems to be a relevant response to job loss in Germany. Apart from retirement only a small positive effect on inactivity is found, suggesting that job loss does not necessarily result in a group of discouraged workers who may not easily be reintegrated into the labor market. However, while (early) retirement long represented an "attractive" alternative to long-term unemployment, the pension policy paradigm in Germany has changed over time, suggesting that for many older workers this route has gradually been closed such that more negative career consequences can be expected (Buchholz et al., 2013).

Second, similar to recent studies for the US and the UK labor market (Lachowska et al., 2018; Upward & Wright, 2017) it was revealed that the large negative short-term effects in terms of forgone total labor income are primarily due to the loss of employment and lower working hours. However, in contrast to these studies, I find that in Germany even five years after job loss the majority of the total losses can be explained by falls in employment or working hours instead of hourly wages. This improves our understanding of explanations for total labor income losses in different welfare states by linking previous findings about larger (short-term) total losses in conservative welfare regimes (Ehlert, 2013), but stronger earnings or wage scars in liberal welfare states (Gangl, 2006). The current study also examined in more detail the reason for job loss. A particularly interesting finding is that for dismissals a substantial part of the earnings scars are explained by workers not being able to find full-time re-employment.

Third, this article provides first empirical evidence on the effects of displacement and dismissal on workers' non-standard employment risks in Germany. Extending previous studies (von Greiff, 2009), I show that job loss increases the risk of self-employment and this can be mainly attributed to higher solo self-employment. In addition, displaced and, in particular, dismissed workers face higher risks of part-time employment although this seems to be largely driven by regular instead of marginal part-time. Moreover, workers who lost their job are more likely to re-enter the labor market via fixed-term contracts and temporary agency employment. While these effects decrease over time, higher risks of fixed-term employment remain even five years after job loss.

Furthermore, the results point to somewhat more negative effects for dismissals than displacements, in particular, in the short-run. However, the differences become smaller over time and the overall patters are quite similar. This is an important finding meaning that results of previous studies who have grouped these events are not driven by a single event and, therefore, offer useful summaries of the overall effects of job loss.

There remain, however, some important caveats to the current analyses. First, the smaller sample sizes of survey compared to administrative data make it difficult to study the heterogeneity in the effects of job loss across subgroups. Ehlert (2013) shows that the effects of job loss are socially stratified. If workers who already face higher risks of displacement or dismissal are also more negatively affected by these events, job loss implies an accumulation of disadvantages (DiPrete & Eirich, 2006). An important next step, therefore, is to investigate effect heterogeneity especially for outcomes that have received less attention so far.

A second limitation of the data is that displacement and dismissal are self-reported as well as that dismissals combine layoffs and workers being fired for individual reasons. Given that the recall periods for the reason of job loss at most concern the year before the interview, measurement error may be less of an issue. Regarding the second point, I separated displacements due to plant closure in all analyses from the more heterogeneous group of dismissed workers. The fact that the results for displacements, which rely on plant closures as an exogenous reason for job loss, are similar to those for dismissals also indicates that the findings for the latter are not driven by selection. Third, concerning non-standard employment I examined each form separately leaving open how they may interact (e.g., fixed-term part-time job) to create an even greater distance to the regular jobs displaced and dismissed workers held before.

The results also point to some interesting avenues for future research. The fact that in Germany, in contrast to the US or UK, the total income losses are more strongly driven by the loss of employment and lower working hours emphasizes the need for comparative research that takes into account the institutional differences more explicitly. While some studies have provided such evidence by examining transitions from employment to unemployment (e.g., Ehlert, 2013; Gangl, 2006), comparative research looking at different reasons for job loss is rare (Kuhn, 2002). Moreover, the results for non-standard employment suggest that many workers try to find quick re-employment instead of waiting for an adequate job. An issue that merits further attention concerns the implications of taking up non-standard employment for workers' careers. As Gebel (2013) points out, workers' use of non-standard employment may raise concerns if compared to standard re-employment, but displaced or dismissed workers taking up alternative work arrangements may still fare better compared to situation of remaining jobless. Finally, although this article adds to and extends the few previous studies that have documented the negative consequences of job loss for a wide range of career outcomes (Oesch &

Baumann, 2015; Upward & Wright, 2017), future research should also pay more attention to the non-economic consequences of job loss including workers' health and well-being as well as their family lives.

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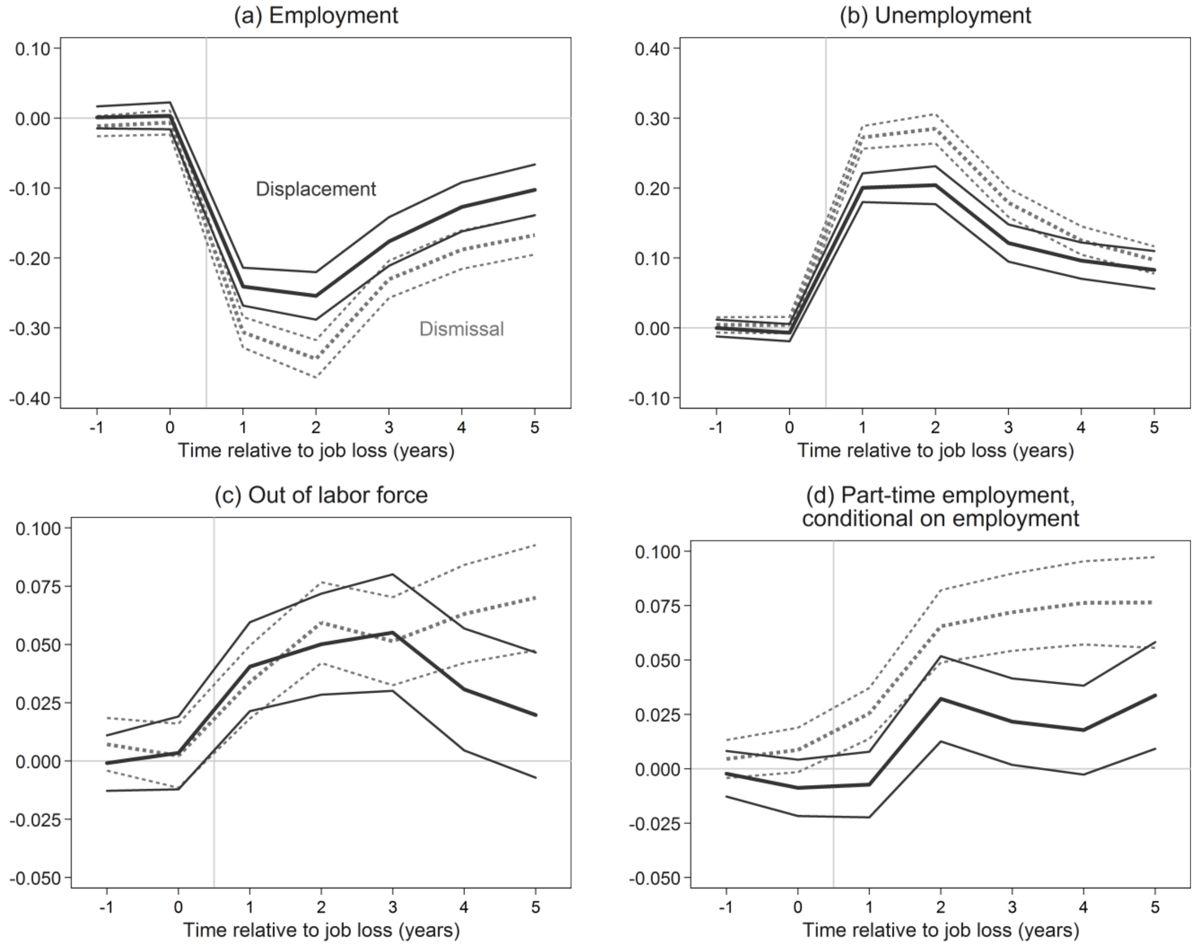
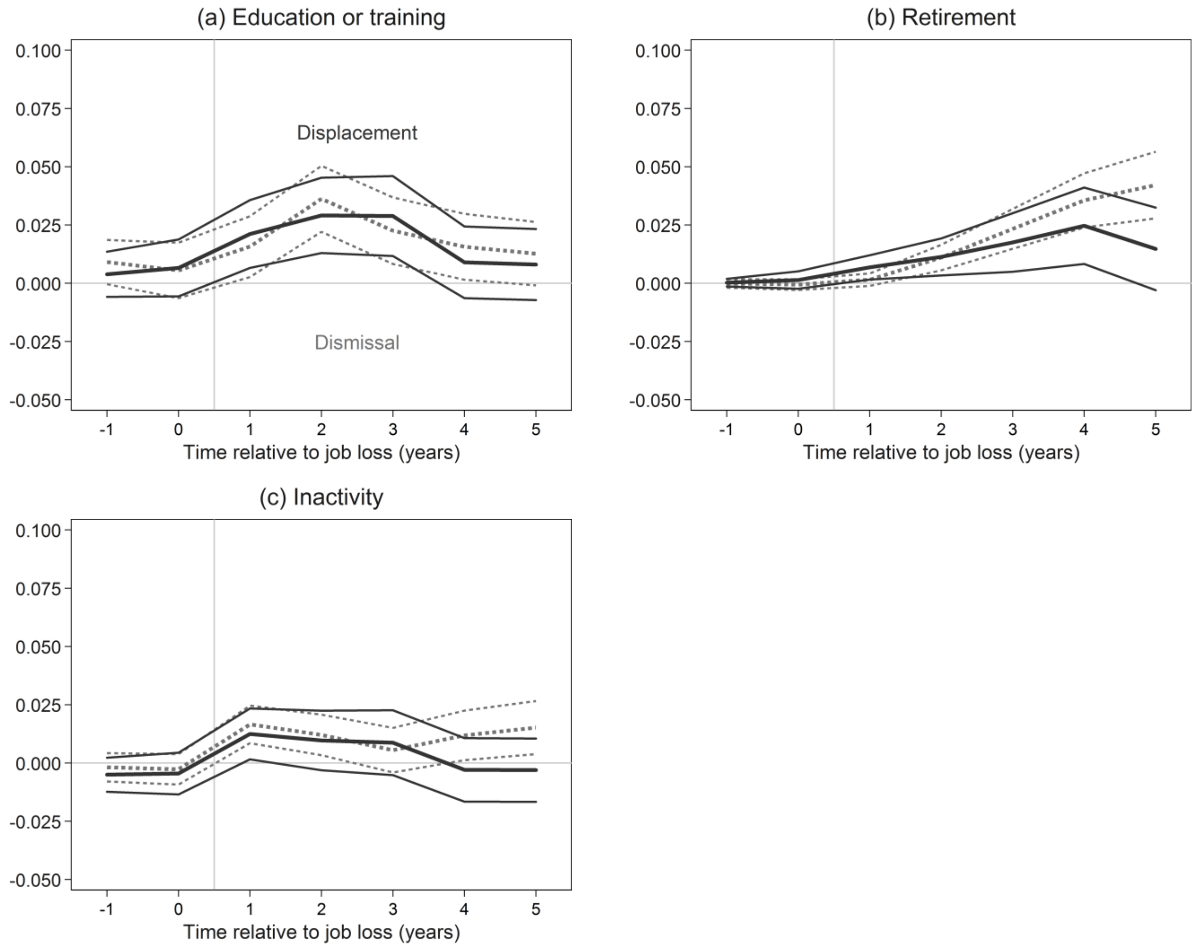
7. Supplementary material (online only)

Figure S1 Effects of displacement and dismissal on proportion of month in each labor market status (change in proportion)



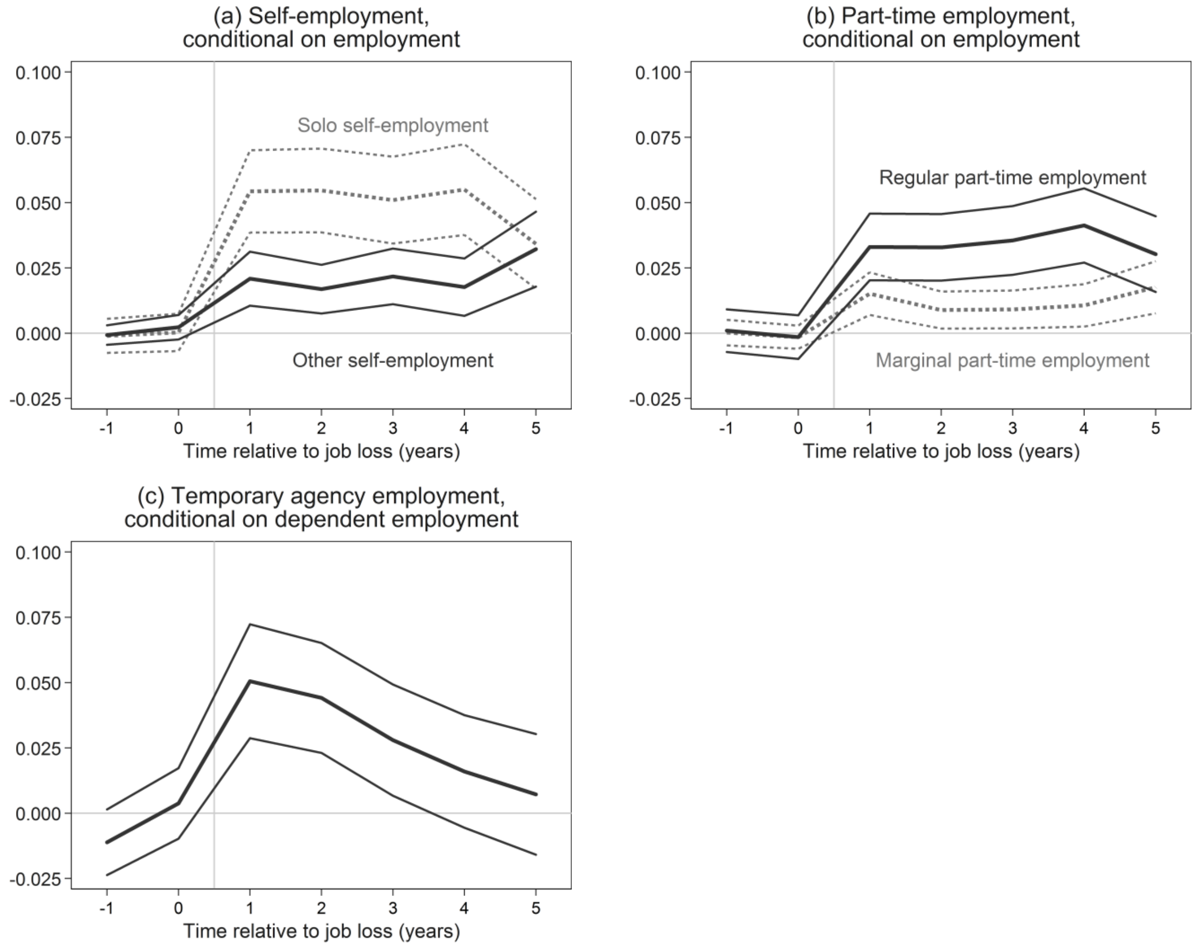
Notes: Matched samples. Fixed-effects estimates with 95 percent confidence intervals based on clustered standard errors for the effects of displacement (solid black lines) and dismissal (dashed grey lines). See Tables S14 and S16 for the full regression models. The plotted effects are the respective interaction coefficients δ_k. Out of labor force (c) combines panels (a) Education or training, (b) Retirement, and (c) Inactivity from Figure S2.
Sources: SOEP 1988-2015, author's calculations.

Figure S2 Effects of displacement and dismissal on proportion of month in each labor market status (change in proportion)



Notes: Matched samples. Fixed-effects estimates with 95 percent confidence intervals based on clustered standard errors for the effects of displacement (solid black lines) and dismissal (dashed grey lines). See Tables S15 and S17 for the full regression models. The plotted effects are the respective interaction coefficients δ_k.
Sources: SOEP 1988-2015, author's calculations.

Figure S3 Effects of job loss on non-standard employment (change in probability)



Notes: Matched samples. Fixed-effects estimates with 95 percent confidence intervals based on clustered standard errors for the effects of job loss on other self-employment/regular part-time employment/temporary agency employment (solid black lines) and solo self-employment/marginal part-time employment (dashed grey lines). See Tables S18-S20 for the full regression models. The plotted effects are the respective interaction coefficients δ_k. Results for panel (a) and (b) are based on the SOEP 2000-2015 and the SOEP 2001-2015, respectively.
Sources: SOEP 2000-2015, author's calculations.

Table S1 Measurement of variables

Variable	Measurement
<u>Dependent variables</u>	
Labor market status (1)	1=employment, 2=unemployment, 3=education or training, 4=retirement, 5=inactivity, 3-5=out of labor force, six variables for the analyses
Labor market status (2)	1=full-time employment, 2=part-time employment, 3=unemployment, 4=education or training, 5=retirement, 6=inactivity, 1-2=employment, 3-5=out of labor force, proportion of month in each labor market status since the last interview, eight variables for the analyses
Gross labor income (1)	Gross labor income last month (in Euro), deflated to 2011 prices using the consumer price index, zero income for non-employment
Gross labor income (2)	(1) but only with positive income from dependent or self-employment, logged for the analyses
Gross hourly wage	(2) divided by (average actual weekly working hours *4.33), logged for the analyses
Average actual working hours (1)	Average actual weekly working hours (in hours), zero hours for non-employment
Average actual working hours (2)	(1) but only with positive hours from dependent or self-employment, logged for the analyses
Self-employment	1=self-employment (includes family workers), 0=dependent employment
Type of employment	1=dependent employment, 2=solo self-employment (without employees), 3=other self-employment (with employees, includes family workers)
Part-time employment	1=part-time employment ($0<\text{hours}<35$), 0=full-time ($35\leq\text{hours}$)
Working time	1=marginal part-time ($0<\text{hours}<15$), 2=regular part-time ($15\leq\text{hours}<35$), 3=full-time ($35\leq\text{hours}$)
Fixed-term contract	1=fixed-term contract, 0=permanent contract, only dependent employment
Temporary agency employment	1=temporary agency employment (de-jure is not de-facto employer), 0=no temporary agency employment (de-jure is de-facto employer), only dependent employment
<u>Independent variable</u>	
Job loss (1)	1=displacement due to plant closure, 2=dismissal, 3=no job loss (no job change and job separations due to own resignation or mutual agreement)
Job loss (2)	1= job loss due to displacement due to plant closure or dismissal, 0=no job loss
<u>Control variables</u>	
Socio-demographics	
Age	In years
Female	1=female, 0=male
Migration background	1=migration background, 0=no migration background

Table S1 continued

Education	
Educational degree	1=less than lower secondary degree (CASMIN 1a-c), 2= intermediate and higher secondary degree (CASMIN 2a-c), 3=tertiary degree (CASMIN 3a-c)
Employment history	
Total experience	1=full-time employment, 2=part-time employment, 3=unemployment (in decimal years), total=complete employment history since the age of 15 years, 3 variables for the analyses
Recent experience	1=full-time employment, 2=part-time employment, 3=unemployment, 4=education or training, 5=inactivity (in months), recent=in the year before the interview, five variables for the analyses
Characteristics of the job	
Industry	1=primary & construction, 2=manufacturing & energy, 3=trade, 4=transport, 5=bank & insurance, 6=services
Firm size	1=less than 20 employees, 2=20 to less than 200 employees, 3=200 to less than 2000 employees, 4=2000 and more employees
Occupation	1=blue-collar worker, 2=white-collar worker, 3=civil servant
Firm tenure	In decimal years
Household structure	
Partner in household	1=no partner or spouse in household, 2=partner in household, 3=spouse in household
Number of persons	Number of persons living in household
Number of children	Number of children aged 0 to 14 years living in household
Health	
Health satisfaction	On a scale with 11 scale points from 0=completely dissatisfied to 10=completely satisfied
Context characteristics	
Region	1=North-Germany (Bremen, Hamburg, Lower Saxony, Schleswig-Holstein), 2=East-Germany (Berlin, Brandenburg, Mecklenburg-Vorpommern, Saxony, Saxony-Anhalt, Thuringia), 3=South-Germany (Bavaria, Baden-Württemberg, Hesse), 4=West-Germany (North-Rhine Westphalia, Rhineland-Palatinate, Saarland)
State unemployment rate	Unemployment rate of the federal state (in percent)
Year before job loss	1990-2014

Notes: All control variables refer to the interview before the job loss. The state unemployment rate is taken from the Statistics of the Federal Employment Agency (2017) and has been merged to the SOEP.
Sources: Own illustration.

Table S2 Assignment model: Logistic regression of displacement on control variables (logits)

	Displacement (=1) vs. control (=0)		
	Coef.		SE
Age	0.008		(0.01)
Female	0.027		(0.08)
Migration background	0.181	*	(0.08)
Education: Less than lower secondary			
Intermediate or higher secondary	-0.136		(0.08)
Tertiary	-0.227		(0.12)
Total experience: Full-time employment	-0.004		(0.01)
Part-time employment	0.006		(0.01)
Unemployment	-0.006		(0.03)
Recent experience: Full-time employment	-0.173		(0.15)
Part-time employment	-0.177		(0.15)
Unemployment	-0.095		(0.15)
Education or training	-0.220		(0.16)
Inactivity	-0.111		(0.16)
Industry: Primary & construction			
Manufacturing & energy	-0.105		(0.09)
Trade	-0.003		(0.10)
Transport	-0.032		(0.13)
Bank & insurance	-0.826	***	(0.24)
Services	-1.026	***	(0.11)
Firm size: Less than 20 employees			
20 to less than 200 employees	-0.377	***	(0.08)
200 to less than 2000 employees	-0.826	***	(0.10)
2000 or more employees	-0.813	***	(0.10)
Occupation: Blue-collar worker			
White-collar worker	-0.046		(0.08)
Civil servant	-0.544	*	(0.27)
Firm tenure	-0.056	***	(0.01)
Firm tenure (squared)	0.001	***	(0.00)
Partner in household: No Partner			
Partner	0.142		(0.11)
Spouse	0.074		(0.09)
Number of persons	0.019		(0.03)
Number of children	-0.123	*	(0.05)
Health satisfaction	-0.052	**	(0.02)
Region: North Germany			
East Germany	0.137		(0.14)
South Germany	0.249		(0.13)
West Germany	0.047		(0.11)
State unemployment rate	0.201	***	(0.05)
State unemployment rate (squared)	-0.006	***	(0.00)
Constant	-2.841		(1.87)
N (person-spells)	113776		

Notes: *** p<0.001, ** p<0.01,* p<0.05. Standard errors are shown in parentheses. See Table S1 for details on the measurement of the variables.
Sources: SOEP 1988-2015, author's calculations.

Table S3 Assignment model: Logistic regression of dismissal on control variables (logits)

	Dismissal (=1) vs. control (=0)		
	Coef.		SE
Age	-0.067	**	(0.02)
Age (squared)	0.001	**	(0.00)
Female	-0.005		(0.05)
Migration background	0.221	***	(0.05)
Education: Less than lower secondary			
Intermediate or higher secondary	-0.282	***	(0.05)
Tertiary	-0.563	***	(0.08)
Total experience: Full-time employment	-0.024	*	(0.01)
Full-time employment (squared)	0.001	**	(0.00)
Part-time	0.010		(0.01)
Unemployment	0.196	***	(0.03)
Unemployment (squared)	-0.011	***	(0.00)
Recent experience: Full-time employment	0.201		(0.19)
Full-time employment (squared)	-0.012	***	(0.00)
Part-time	0.039		(0.19)
Unemployment	0.108		(0.18)
Education or training	0.028		(0.19)
Inactivity	0.130		(0.19)
Industry: Primary & construction			
Manufacturing & energy	-0.329	***	(0.06)
Trade	-0.250	***	(0.07)
Transport	-0.444	***	(0.10)
Bank & insurance	-0.935	***	(0.19)
Services	-0.714	***	(0.07)
Firm size: Less than 20 employees			
20 to less than 200 employees	-0.462	***	(0.05)
200 to less than 2000 employees	-0.721	***	(0.06)
2000 or more employees	-1.089	***	(0.08)
Occupation: Blue-collar worker			
White-collar worker	-0.222	***	(0.05)
Civil servant	-2.170	***	(0.42)
Firm tenure	-0.175	***	(0.01)
Firm tenure (squared)	0.004	***	(0.00)
Partner in household: No Partner			
Partner	0.054		(0.06)
Spouse	-0.202	***	(0.06)
Number of persons	-0.033		(0.02)
Number of children	0.123	***	(0.03)
Health satisfaction	-0.081	***	(0.01)
Region: North Germany			
East Germany	0.166		(0.09)
South Germany	0.065		(0.08)
West Germany	0.002		(0.08)

Table S3 continued

State unemployment rate	0.067 ***	(0.01)
Constant	-1.369	(2.26)
N (person-spells)	115377	

Notes: *** p<0.001, ** p<0.01,* p<0.05. Standard errors are shown in parentheses. See Table S1 for details on the measurement of the variables.
Sources: SOEP 1988-2015, author's calculations.

Table S4 Effects of displacement on labor market status (1) (FE linear probability models)

	Employment		Unemployment		Out of labor force	
	Coef. (SE)		Coef. (SE)		Coef. (SE)	
Time: γ_k						
1 year before	0.035	***	-0.007	**	-0.027	***
	(0.003)		(0.002)		(0.002)	
0 year	0.090	***	-0.042	***	-0.047	***
	(0.003)		(0.003)		(0.002)	
1 year after	0.081	***	-0.037	***	-0.044	***
	(0.003)		(0.003)		(0.002)	
2 years after	0.022	***	0.000		-0.022	***
	(0.003)		(0.003)		(0.002)	
3 years after	-0.011	***	0.017	***	-0.006	**
	(0.003)		(0.003)		(0.002)	
4 years after	-0.036	***	0.024	***	0.012	***
	(0.004)		(0.003)		(0.003)	
5 years after	-0.057	***	0.028	***	0.029	***
	(0.005)		(0.004)		(0.003)	
Time x displacement: δ_k						
1 year before	-0.007		-0.001		0.008	
	(0.010)		(0.009)		(0.007)	
0 year	0.002		-0.002		0.000	
	(0.011)		(0.008)		(0.008)	
1 year after	**-0.411**	***	0.374	***	0.037	***
	(0.019)		(0.017)		(0.010)	
2 years after	-0.221	***	0.175	***	0.045	***
	(0.018)		(0.016)		(0.011)	
3 years after	-0.166	***	0.128	***	0.038	**
	(0.018)		(0.015)		(0.012)	
4 years after	-0.133	***	0.110	***	0.023	
	(0.019)		(0.016)		(0.013)	
5 years after	-0.122	***	0.087	***	0.035	*
	(0.020)		(0.016)		(0.015)	
Constant	0.910	***	0.043	***	0.047	***
	(0.006)		(0.004)		(0.004)	
N (person-spells)	97246		97246		97246	

Notes: Matched sample. *** p<0.001, ** p<0.01,* p<0.05. Standard errors clustered by worker are shown in parentheses.
Sources: SOEP 1988-2015, author's calculations.

Table S5 Effects of displacement on labor market status (1) (FE linear probability models)

	Education or training		Retirement		Inactivity	
	Coef. (SE)		Coef. (SE)		Coef. (SE)	
Time: γ_k						
1 year before	-0.023	***	0.000		-0.005	***
	(0.001)		(0.000)		(0.001)	
0 year	-0.037	***	0.001	**	-0.010	***
	(0.001)		(0.000)		(0.001)	
1 year after	-0.036	***	0.001	***	-0.009	***
	(0.001)		(0.000)		(0.001)	
2 years after	-0.033	***	0.008	***	0.002	
	(0.001)		(0.001)		(0.002)	
3 years after	-0.032	***	0.018	***	0.008	***
	(0.001)		(0.001)		(0.001)	
4 years after	-0.032	***	0.032	***	0.011	***
	(0.001)		(0.002)		(0.002)	
5 years after	-0.032	***	0.050	***	0.012	***
	(0.001)		(0.002)		(0.002)	
Time x displacement: δ_k						
1 year before	0.007		-0.001		0.002	
	(0.006)		(0.001)		(0.004)	
0 year	0.003		0.001		-0.004	
	(0.007)		(0.001)		(0.005)	
1 year after	0.012		0.005	*	0.020	**
	(0.007)		(0.002)		(0.007)	
2 years after	**0.023**	**	**0.009**	*	**0.013**	
	(0.008)		(0.004)		(0.007)	
3 years after	0.012		0.018	**	0.009	
	(0.007)		(0.007)		(0.008)	
4 years after	0.006		0.021	*	-0.004	
	(0.007)		(0.008)		(0.007)	
5 years after	0.006		0.026	*	0.003	
	(0.008)		(0.010)		(0.008)	
Constant	0.036	***	-0.001		0.012	***
	(0.003)		(0.002)		(0.002)	
N (person-spells)	97246		97246		97246	

Notes: Matched sample. *** p<0.001, ** p<0.01,* p<0.05. Standard errors clustered by worker are shown in parentheses.
Sources: SOEP 1988-2015, author's calculations.

Table S6 Effects of dismissal on labor market status (1) (FE linear probability models)

	Employment		Unemployment		Out of labor force	
	Coef. (SE)		Coef. (SE)		Coef. (SE)	
Time: γ_k						
1 year before	0.051	***	-0.005		-0.046	***
	(0.004)		(0.004)		(0.003)	
0 year	0.175	***	-0.083	***	-0.092	***
	(0.005)		(0.004)		(0.003)	
1 year after	0.161	***	-0.074	***	-0.087	***
	(0.005)		(0.004)		(0.003)	
2 years after	0.085	***	-0.023	***	-0.061	***
	(0.005)		(0.004)		(0.003)	
3 years after	0.045	***	-0.002		-0.043	***
	(0.005)		(0.005)		(0.003)	
4 years after	0.021	***	0.002		-0.023	***
	(0.006)		(0.005)		(0.004)	
5 years after	-0.003		0.009		-0.005	
	(0.006)		(0.005)		(0.004)	
Time x dismissal: δ_k						
1 year before	0.025	*	-0.021	*	-0.004	
	(0.010)		(0.009)		(0.007)	
0 year	0.030	**	-0.026	**	-0.004	
	(0.011)		(0.008)		(0.007)	
1 year after	**-0.563**	***	0.522	***	0.040	***
	(0.014)		(0.013)		(0.008)	
2 years after	-0.278	***	0.237	***	0.041	***
	(0.015)		(0.013)		(0.009)	
3 years after	-0.195	***	0.155	***	0.041	***
	(0.015)		(0.013)		(0.010)	
4 years after	-0.166	***	0.101	***	0.066	***
	(0.015)		(0.012)		(0.011)	
5 years after	-0.150	***	0.089	***	0.061	***
	(0.016)		(0.013)		(0.012)	
Constant	0.810	***	0.097	***	0.093	***
	(0.005)		(0.004)		(0.003)	
N (person-spells)	112635		112635		112635	

Notes: Matched sample. *** p<0.001, ** p<0.01,* p<0.05. Standard errors clustered by worker are shown in parentheses.
Sources: SOEP 1988-2015, author's calculations.

Table S7 Effects of dismissal on labor market status (1) (FE linear probability models)

	Education or training		Retirement		Inactivity	
	Coef. (SE)		Coef. (SE)		Coef. (SE)	
Time: γ_k						
1 year before	-0.042	***	0.000		-0.004	**
	(0.003)		(0.000)		(0.002)	
0 year	-0.076	***	0.000		-0.016	***
	(0.003)		(0.000)		(0.001)	
1 year after	-0.073	***	0.000		-0.015	***
	(0.003)		(0.000)		(0.001)	
2 years after	-0.067	***	0.006	***	-0.001	
	(0.003)		(0.001)		(0.001)	
3 years after	-0.065	***	0.014	***	0.007	***
	(0.003)		(0.001)		(0.002)	
4 years after	-0.065	***	0.031	***	0.011	***
	(0.003)		(0.002)		(0.002)	
5 years after	-0.066	***	0.050	***	0.011	***
	(0.003)		(0.003)		(0.002)	
Time x dismissal: δ_k						
1 year before	0.001		0.000		-0.005	
	(0.006)		(0.001)		(0.004)	
0 year	0.000		-0.001		-0.003	
	(0.007)		(0.001)		(0.003)	
1 year after	0.014	*	0.003		0.023	***
	(0.007)		(0.002)		(0.005)	
2 years after	0.017	*	0.013	***	0.011	*
	(0.007)		(0.003)		(0.005)	
3 years after	0.010		0.025	***	0.005	
	(0.007)		(0.005)		(0.005)	
4 years after	0.006		0.038	***	0.022	***
	(0.007)		(0.006)		(0.006)	
5 years after	0.003		0.044	***	0.014	*
	(0.007)		(0.007)		(0.006)	
Constant	0.075	***	0.001		0.018	***
	(0.003)		(0.001)		(0.002)	
N (person-spells)	112635		112635		112635	

Notes: Matched sample. *** p<0.001, ** p<0.01,* p<0.05. Standard errors clustered by worker are shown in parentheses.
Sources: SOEP 1988-2015, author's calculations.

Table S8 Effects of displacement on labor income last month (FE linear regressions)

	Gross labor income, zero for non-employment		Log gross labor income, employ-ment spells only		Log gross hourly wage, employ-ment spells only	
	Coef. (SE)		Coef. (SE)		Coef. (SE)	
Time: γ_k						
1 year before	110.968	***	0.023	***	0.016	***
	(6.43)		(0.00)		(0.00)	
0 year	270.283	***	0.048	***	0.024	***
	(6.98)		(0.00)		(0.00)	
1 year after	286.812	***	0.059	***	0.050	***
	(7.36)		(0.00)		(0.00)	
2 years after	188.214	***	0.065	***	0.062	***
	(8.29)		(0.00)		(0.00)	
3 years after	125.885	***	0.066	***	0.067	***
	(9.14)		(0.00)		(0.00)	
4 years after	75.905	***	0.066	***	0.072	***
	(10.99)		(0.00)		(0.00)	
5 years after	30.192	*	0.066	***	0.075	***
	(12.53)		(0.00)		(0.00)	
Time x displacement: δ_k						
1 year before	-14.079		0.002		-0.004	
	(29.38)		(0.01)		(0.01)	
0 year	-46.077		-0.001		-0.012	
	(32.77)		(0.01)		(0.01)	
1 year after	**-1052.524**	***	**-0.067**	***	**-0.056**	***
	(58.97)		(0.02)		(0.02)	
2 years after	-634.060	***	-0.077	***	-0.072	***
	(57.22)		(0.02)		(0.02)	
3 years after	-523.692	***	-0.089	***	-0.084	***
	(57.08)		(0.02)		(0.02)	
4 years after	-445.797	***	-0.088	***	-0.066	***
	(62.60)		(0.02)		(0.02)	
5 years after	-407.074	***	**-0.069**	**	**-0.061**	**
	(66.61)		(0.02)		(0.02)	
Constant	2436.388	***	7.772	***	2.561	***
	(18.85)		(0.00)		(0.00)	
N (person-spells)	97246		97171		97171	

Notes: Matched sample. *** p<0.001, ** p<0.01,* p<0.05. Standard errors clustered by worker are shown in parentheses. Log gross labor income and log gross hourly wage are conditional on employment.
Sources: SOEP 1988-2015, author's calculations.

Table S9 Effects of displacement on average actual working hours (FE linear regressions)

	Working hours, zero for non-employment		Log working hours	
	Coef. (SE)		Coef. (SE)	
Time: γ_k				
1 year before	1.633	***	0.007	***
	(0.13)		(0.00)	
0 year	4.464	***	0.025	***
	(0.15)		(0.00)	
1 year after	3.641	***	0.008	***
	(0.15)		(0.00)	
2 years after	1.064	***	0.003	
	(0.16)		(0.00)	
3 years after	-0.439	**	-0.001	
	(0.16)		(0.00)	
4 years after	-1.564	***	-0.005	*
	(0.19)		(0.00)	
5 years after	-2.542	***	-0.009	***
	(0.22)		(0.00)	
Time x displacement: δ_k				
1 year before	-0.151		0.006	
	(0.50)		(0.01)	
0 year	0.173		0.011	
	(0.55)		(0.01)	
1 year after	-17.253	***	-0.011	
	(0.88)		(0.01)	
2 years after	-9.295	***	-0.005	
	(0.84)		(0.01)	
3 years after	-6.984	***	-0.006	
	(0.84)		(0.01)	
4 years after	-5.957	***	-0.022	
	(0.88)		(0.01)	
5 years after	-5.062	***	-0.008	
	(0.91)		(0.01)	
Constant	39.295	***	3.745	***
	(0.27)		(0.00)	
N (person-spells)	97246		97171	

Notes: Matched sample. *** p<0.001, ** p<0.01,* p<0.05. Standard errors clustered by worker are shown in parentheses. Log working hours are conditional on employment.
Sources: SOEP 1988-2015, author's calculations.

Table S10 Effects of dismissal on labor income last month (FE linear regressions)

	Gross labor income, zero for non-employment		Log gross labor income, employment spells only		Log gross hourly wage, employment spells only	
	Coef. (SE)		Coef. (SE)		Coef. (SE)	
Time: γ_k						
1 year before	124.539	***	0.020	***	0.012	***
	(8.31)		(0.00)		(0.00)	
0 year	408.750	***	0.056	***	0.022	***
	(9.46)		(0.00)		(0.00)	
1 year after	421.746	***	0.068	***	0.054	***
	(10.11)		(0.00)		(0.00)	
2 years after	297.865	***	0.075	***	0.067	***
	(10.97)		(0.00)		(0.00)	
3 years after	237.907	***	0.079	***	0.075	***
	(12.09)		(0.00)		(0.00)	
4 years after	200.962	***	0.080	***	0.083	***
	(13.05)		(0.00)		(0.00)	
5 years after	157.800	***	0.083	***	0.090	***
	(15.53)		(0.01)		(0.00)	
Time x dismissal: δ_k						
1 year before	43.478		-0.006		-0.007	
	(25.11)		(0.01)		(0.01)	
0 year	12.698		-0.024	*	-0.017	*
	(24.69)		(0.01)		(0.01)	
1 year after	**-1345.778**	***	**-0.152**	***	**-0.085**	***
	(35.48)		(0.02)		(0.01)	
2 years after	-759.699	***	-0.141	***	-0.100	***
	(38.09)		(0.01)		(0.01)	
3 years after	-598.846	***	-0.138	***	-0.090	***
	(45.89)		(0.01)		(0.01)	
4 years after	-552.429	***	-0.128	***	-0.082	***
	(39.21)		(0.02)		(0.01)	
5 years after	-514.380	***	**-0.131**	***	**-0.077**	***
	(40.66)		(0.02)		(0.02)	
Constant	1875.981	***	7.629	***	2.414	***
	(12.73)		(0.00)		(0.00)	
N (person-spells)	112635		112464		112464	

Notes: Matched sample. *** p<0.001, ** p<0.01,* p<0.05. Standard errors clustered by worker are shown in parentheses. Log gross labor income and log gross hourly wage are conditional on employment.
Sources: SOEP 1988-2015, author’s calculations.

Table S11 Effects of dismissal on average actual working hours (FE linear regressions)

	Working hours, zero for non-employment		Log working hours	
	Coef. (SE)		Coef. (SE)	
Time: γ_k				
1 year before	2.411	***	0.007	**
	(0.18)		(0.00)	
0 year	8.378	***	0.033	***
	(0.21)		(0.00)	
1 year after	7.273	***	0.015	***
	(0.21)		(0.00)	
2 years after	3.838	***	0.008	**
	(0.22)		(0.00)	
3 years after	2.091	***	0.004	
	(0.24)		(0.00)	
4 years after	0.873	***	-0.003	
	(0.26)		(0.00)	
5 years after	-0.267		-0.008	*
	(0.29)		(0.00)	
Time x dismissal: δ_k				
1 year before	1.025	*	0.000	
	(0.48)		(0.01)	
0 year	1.186	*	-0.007	
	(0.50)		(0.01)	
1 year after	-24.909	***	-0.067	***
	(0.64)		(0.01)	
2 years after	-12.182	***	-0.040	***
	(0.68)		(0.01)	
3 years after	-9.010	***	-0.048	***
	(0.69)		(0.01)	
4 years after	-7.530	***	-0.046	***
	(0.68)		(0.01)	
5 years after	-7.016	***	-0.054	***
	(0.71)		(0.01)	
Constant	35.081	***	3.749	***
	(0.23)		(0.00)	
N (person-spells)	112635		112464	

Notes: Matched sample. *** p<0.001, ** p<0.01,* p<0.05. Standard errors clustered by worker are shown in parentheses. Log working hours are conditional on employment.
Sources: SOEP 1988-2015, author's calculations.

Table S12 Effects of displacement on non-standard employment (FE linear probability models)

	Self-employment		Part-time employment		Fixed-term contract	
	Coef. (SE)		Coef. (SE)		Coef. (SE)	
Time: γ_k						
1 year before	-0.003	***	-0.011	***	-0.013	***
	(0.001)		(0.001)		(0.002)	
0 year	-0.012	***	-0.043	***	-0.057	***
	(0.001)		(0.002)		(0.002)	
1 year after	-0.003	**	-0.014	***	-0.038	***
	(0.001)		(0.002)		(0.002)	
2 years after	0.002		-0.006	***	-0.031	***
	(0.001)		(0.002)		(0.002)	
3 years after	0.008	***	0.001		-0.024	***
	(0.001)		(0.002)		(0.002)	
4 years after	0.012	***	0.007	***	-0.022	***
	(0.002)		(0.002)		(0.002)	
5 years after	0.018	***	0.015	***	-0.020	***
	(0.002)		(0.002)		(0.002)	
Time x displacement: δ_k						
1 year before	0.001		-0.005		-0.010	
	(0.004)		(0.008)		(0.009)	
0 year	0.006		-0.013		-0.011	
	(0.004)		(0.008)		(0.009)	
1 year after	0.063	***	0.026	*	0.201	***
	(0.010)		(0.011)		(0.019)	
2 years after	0.058	***	0.024	*	0.153	***
	(0.011)		(0.012)		(0.017)	
3 years after	0.061	***	0.023	*	0.109	***
	(0.011)		(0.012)		(0.017)	
4 years after	0.059	***	0.035	**	0.080	***
	(0.011)		(0.013)		(0.016)	
5 years after	0.062	***	0.025		0.062	***
	(0.012)		(0.013)		(0.016)	
Constant	0.009	**	0.044	***	0.054	***
	(0.003)		(0.003)		(0.004)	
N (person-spells)	97171		97171		97057	

Notes: Matched sample. *** p<0.001, ** p<0.01,* p<0.05. Standard errors clustered by worker are shown in parentheses. Self-employment and part-time employment are conditional on employment. Fixed-term contract is conditional on dependent employment.
Sources: SOEP 1988-2015, author's calculations.

Table S13 Effects of dismissal on non-standard employment (FE linear probability models)

	Self-employment		Part-time employment		Fixed-term contract	
	Coef. (SE)		Coef. (SE)		Coef. (SE)	
Time: γ_k						
1 year before	-0.005	***	-0.011	***	-0.023	***
	(0.001)		(0.002)		(0.003)	
0 year	-0.017	***	-0.051	***	-0.094	***
	(0.001)		(0.002)		(0.003)	
1 year after	-0.008	***	-0.022	***	-0.063	***
	(0.001)		(0.002)		(0.003)	
2 years after	-0.001		-0.012	***	-0.055	***
	(0.001)		(0.002)		(0.004)	
3 years after	0.005	**	-0.004		-0.049	***
	(0.002)		(0.003)		(0.004)	
4 years after	0.010	***	0.003		-0.043	***
	(0.002)		(0.003)		(0.004)	
5 years after	0.014	***	0.012	***	-0.042	***
	(0.002)		(0.003)		(0.004)	
Time x dismissal: δ_k						
1 year before	0.002		0.004		0.011	
	(0.003)		(0.006)		(0.008)	
0 year	0.005		0.008		0.012	
	(0.004)		(0.006)		(0.008)	
1 year after	0.068	***	0.065	***	0.252	***
	(0.008)		(0.010)		(0.016)	
2 years after	0.067	***	0.054	***	0.210	***
	(0.008)		(0.009)		(0.014)	
3 years after	0.065	***	0.059	***	0.165	***
	(0.008)		(0.009)		(0.014)	
4 years after	0.066	***	0.064	***	0.128	***
	(0.008)		(0.010)		(0.014)	
5 years after	0.071	***	0.062	***	0.109	***
	(0.009)		(0.011)		(0.014)	
Constant	0.015	***	0.044	***	0.082	***
	(0.002)		(0.003)		(0.003)	
N (person-spells)	112464		112464		112310	

Notes: Matched sample. *** p<0.001, ** p<0.01,* p<0.05. Standard errors clustered by worker are shown in parentheses. Self-employment and part-time employment are conditional on employment. Fixed-term contract is conditional on dependent employment.
Sources: SOEP 1988-2015, author's calculations.

Table S14 Effects of displacement on proportion of month in each labor market status (2) (FE linear regression)

	Employment	Unemploy-ment	Out of labor force	Part-time employment
	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)
Time: γ_k				
1 year before	0.028 ***	-0.004 *	-0.024 ***	-0.010 ***
	(0.00)	(0.00)	(0.00)	(0.00)
0 year	0.065 ***	-0.018 ***	-0.047 ***	-0.022 ***
	(0.00)	(0.00)	(0.00)	(0.00)
1 year after	0.080 ***	-0.028 ***	-0.052 ***	-0.019 ***
	(0.00)	(0.00)	(0.00)	(0.00)
2 years after	0.043 ***	-0.007 ***	-0.036 ***	-0.013 ***
	(0.00)	(0.00)	(0.00)	(0.00)
3 years after	0.005	0.013 ***	-0.018 ***	-0.004 *
	(0.00)	(0.00)	(0.00)	(0.00)
4 years after	-0.022 ***	0.023 ***	-0.001	0.005 *
	(0.00)	(0.00)	(0.00)	(0.00)
5 years after	-0.043 ***	0.026 ***	0.017 ***	0.015 ***
	(0.00)	(0.00)	(0.00)	(0.00)
Time x displacement: δ_k				
1 year before	0.001	-0.000	-0.001	-0.002
	(0.01)	(0.01)	(0.01)	(0.01)
0 year	0.003	-0.007	0.003	-0.009
	(0.01)	(0.01)	(0.01)	(0.01)
1 year after	-0.241 ***	0.200 ***	0.040 ***	-0.007
	(0.01)	(0.01)	(0.01)	(0.01)
2 years after	-0.254 ***	0.204 ***	0.050 ***	0.032 **
	(0.02)	(0.01)	(0.01)	(0.01)
3 years after	-0.176 ***	0.121 ***	0.055 ***	0.022 *
	(0.02)	(0.01)	(0.01)	(0.01)
4 years after	-0.127 ***	0.096 ***	0.031 *	0.018
	(0.02)	(0.01)	(0.01)	(0.01)
5 years after	-0.103 ***	0.083 ***	0.020	0.034 **
	(0.02)	(0.01)	(0.01)	(0.01)
Constant	0.905 ***	0.036 ***	0.058 ***	0.052 ***
	(0.01)	(0.00)	(0.00)	(0.00)
N (person-spells)	95926	95926	95926	95766

Notes: Matched sample. *** p<0.001, ** p<0.01,* p<0.05. Standard errors clustered by worker are shown in parentheses. Part-time employment is conditional on employment.
Sources: SOEP 1988-2015, author's calculations.

Table S15 Effects of displacement on proportion of month in each labor market status (2) (FE linear regressions)

	Education or training		Retirement		Inactivity	
	Coef. (SE)		Coef. (SE)		Coef. (SE)	
Time: γ_k						
1 year before	-0.021	***	0.000		-0.003	***
	(0.00)		(0.00)		(0.00)	
0 year	-0.041	***	0.000		-0.006	***
	(0.00)		(0.00)		(0.00)	
1 year after	-0.045	***	0.001	**	-0.008	***
	(0.00)		(0.00)		(0.00)	
2 years after	-0.042	***	0.006	***	0.001	
	(0.00)		(0.00)		(0.00)	
3 years after	-0.040	***	0.015	***	0.007	***
	(0.00)		(0.00)		(0.00)	
4 years after	-0.040	***	0.028	***	0.011	***
	(0.00)		(0.00)		(0.00)	
5 years after	-0.040	***	0.045	***	0.012	***
	(0.00)		(0.00)		(0.00)	
Time x displacement: δ_k						
1 year before	0.004		0.000		-0.005	
	(0.00)		(0.00)		(0.00)	
0 year	0.007		0.001		-0.005	
	(0.01)		(0.00)		(0.00)	
1 year after	0.021	**	0.007	*	0.012	*
	(0.01)		(0.00)		(0.01)	
2 years after	0.029	***	0.011	**	0.010	
	(0.01)		(0.00)		(0.01)	
3 years after	0.029	***	0.018	**	0.009	
	(0.01)		(0.01)		(0.01)	
4 years after	0.009		0.025	**	-0.003	
	(0.01)		(0.01)		(0.01)	
5 years after	0.008		0.015		-0.003	
	(0.01)		(0.01)		(0.01)	
Constant	0.044	***	-0.001		0.015	***
	(0.00)		(0.00)		(0.00)	
N (person-spells)	95926		95926		95926	

Notes: Matched sample. *** p<0.001, ** p<0.01,* p<0.05. Standard errors clustered by worker are shown in parentheses.
Sources: SOEP 1988-2015, author's calculations.

Table S16 Effects of dismissals on proportion of month in each labor market status (2) (FE linear regression)

	Employment	Unemploy-ment	Out of labor force	Part-time employment
	Coef. (SE)	Coef. (SE)	Coef. (SE)	Coef. (SE)
Time: γ_k				
1 year before	0.044 ***	-0.005 *	-0.038 ***	-0.014 ***
	(0.00)	(0.00)	(0.00)	(0.00)
0 year	0.123 ***	-0.037 ***	-0.085 ***	-0.034 ***
	(0.00)	(0.00)	(0.00)	(0.00)
1 year after	0.160 ***	-0.062 ***	-0.097 ***	-0.031 ***
	(0.00)	(0.00)	(0.00)	(0.00)
2 years after	0.111 ***	-0.032 ***	-0.079 ***	-0.022 ***
	(0.00)	(0.00)	(0.00)	(0.00)
3 years after	0.068 ***	-0.010 **	-0.058 ***	-0.010 ***
	(0.00)	(0.00)	(0.00)	(0.00)
4 years after	0.036 ***	0.003	-0.039 ***	-0.002
	(0.01)	(0.00)	(0.00)	(0.00)
5 years after	0.016 **	0.005	-0.021 ***	0.009 *
	(0.01)	(0.00)	(0.00)	(0.00)
Time x dismissal: δ_k				
1 year before	-0.011	0.004	0.007	0.005
	(0.01)	(0.01)	(0.01)	(0.00)
0 year	-0.006	0.004	0.002	0.009
	(0.01)	(0.01)	(0.01)	(0.01)
1 year after	-0.306 ***	0.272 ***	0.034 ***	0.025 ***
	(0.01)	(0.01)	(0.01)	(0.01)
2 years after	-0.344 ***	0.285 ***	0.059 ***	0.066 ***
	(0.01)	(0.01)	(0.01)	(0.01)
3 years after	-0.230 ***	0.179 ***	0.051 ***	0.072 ***
	(0.01)	(0.01)	(0.01)	(0.01)
4 years after	-0.188 ***	0.125 ***	0.063 ***	0.076 ***
	(0.01)	(0.01)	(0.01)	(0.01)
5 years after	-0.167 ***	0.097 ***	0.070 ***	0.076 ***
	(0.01)	(0.01)	(0.01)	(0.01)
Constant	0.820 ***	0.073 ***	0.107 ***	0.054 ***
	(0.00)	(0.00)	(0.00)	(0.00)
N (person-spells)	111089	111089	111089	110905

Notes: Matched sample. *** p<0.001, ** p<0.01,* p<0.05. Standard errors clustered by worker are shown in parentheses. Part-time employment is conditional on employment.
Sources: SOEP 1988-2015, author's calculations.

Table S17 Effects of dismissal on proportion of month in each labor market status (2) (FE linear regressions)

	Education or training		Retirement		Inactivity	
	Coef. (SE)		Coef. (SE)		Coef. (SE)	
Time: γ_k						
1 year before	-0.037	***	0.000		-0.001	
	(0.00)		(0.00)		(0.00)	
0 year	-0.077	***	-0.000		-0.008	***
	(0.00)		(0.00)		(0.00)	
1 year after	-0.087	***	0.000		-0.010	***
	(0.00)		(0.00)		(0.00)	
2 years after	-0.082	***	0.005	***	-0.001	
	(0.00)		(0.00)		(0.00)	
3 years after	-0.078	***	0.012	***	0.008	***
	(0.00)		(0.00)		(0.00)	
4 years after	-0.078	***	0.026	***	0.012	***
	(0.00)		(0.00)		(0.00)	
5 years after	-0.079	***	0.044	***	0.014	***
	(0.00)		(0.00)		(0.00)	
Time x dismissal: δ_k						
1 year before	0.009		-0.000		-0.002	
	(0.00)		(0.00)		(0.00)	
0 year	0.006		-0.001		-0.003	
	(0.01)		(0.00)		(0.00)	
1 year after	0.016	*	0.002		0.017	***
	(0.01)		(0.00)		(0.00)	
2 years after	0.036	***	0.011	***	0.012	**
	(0.01)		(0.00)		(0.00)	
3 years after	0.023	**	0.023	***	0.006	
	(0.01)		(0.00)		(0.00)	
4 years after	0.016	*	0.036	***	0.012	*
	(0.01)		(0.01)		(0.01)	
5 years after	0.013		0.042	***	0.015	**
	(0.01)		(0.01)		(0.01)	
Constant	0.087	***	0.001		0.019	***
	(0.00)		(0.00)		(0.00)	
N (person-spells)	111089		111089		111089	

Notes: Matched sample. *** p<0.001, ** p<0.01,* p<0.05. Standard errors clustered by worker are shown in parentheses.
Sources: SOEP 1988-2015, author's calculations.

Table S18 Effects of job loss on type of employment (FE linear probability models)

	Self-employment		Solo self-employment		Other self-employment	
	Coef. (SE)		Coef. (SE)		Coef. (SE)	
Time: γ_k						
1 year before	-0.005	***	-0.004	**	-0.002	*
	(0.001)		(0.001)		(0.001)	
0 year	-0.017	***	-0.010	***	-0.007	***
	(0.002)		(0.001)		(0.001)	
1 year after	-0.008	***	-0.006	***	-0.002	**
	(0.002)		(0.002)		(0.001)	
2 years after	-0.002		-0.002		0.000	
	(0.002)		(0.002)		(0.001)	
3 years after	0.002		-0.000		0.002	
	(0.002)		(0.002)		(0.001)	
4 years after	0.006	*	0.003		0.003	*
	(0.002)		(0.002)		(0.002)	
5 years after	0.010	***	0.004	*	0.006	***
	(0.003)		(0.002)		(0.002)	
Time x job loss: δ_k						
1 year before	-0.002		-0.001		-0.001	
	(0.004)		(0.003)		(0.002)	
0 year	0.003		0.000		0.002	
	(0.004)		(0.004)		(0.002)	
1 year after	0.075	***	0.054	***	0.021	***
	(0.009)		(0.008)		(0.005)	
2 years after	0.072	***	0.055	***	0.017	***
	(0.009)		(0.008)		(0.005)	
3 years after	0.073	***	0.051	***	0.022	***
	(0.010)		(0.008)		(0.005)	
4 years after	0.073	***	0.055	***	0.018	**
	(0.010)		(0.009)		(0.006)	
5 years after	0.066	***	0.034	***	0.032	***
	(0.011)		(0.009)		(0.007)	
Constant	0.016	***	0.011	***	0.006	***
	(0.002)		(0.002)		(0.001)	
N (person-spells)	68498		68498		68498	

Notes: Matched sample. *** p<0.001, ** p<0.01,* p<0.05. Standard errors clustered by worker are shown in parentheses. All outcomes are conditional on employment.
Sources: SOEP 2000-2015, author's calculations.

Table S19 Effects of job loss on part-time employment (FE linear probability models)

	Part-time employment		Marginal part-time employment		Regular part-time employment	
	Coef. (SE)		Coef. (SE)		Coef. (SE)	
Time: γ_k						
1 year before	-0.011	***	-0.002	***	-0.008	***
	(0.001)		(0.001)		(0.001)	
0 year	-0.046	***	-0.008	***	-0.038	***
	(0.002)		(0.001)		(0.001)	
1 year after	-0.016	***	-0.003	***	-0.013	***
	(0.002)		(0.001)		(0.002)	
2 years after	-0.007	***	-0.002	*	-0.005	***
	(0.002)		(0.001)		(0.001)	
3 years after	0.001		-0.001		0.002	
	(0.002)		(0.001)		(0.002)	
4 years after	0.008	***	0.001		0.007	***
	(0.002)		(0.001)		(0.002)	
5 years after	0.016	***	0.003	*	0.014	***
	(0.002)		(0.001)		(0.002)	
Time x job loss: δ_k						
1 year before	0.001		0.000		0.001	
	(0.005)		(0.002)		(0.004)	
0 year	-0.003		-0.002		-0.001	
	(0.005)		(0.002)		(0.004)	
1 year after	0.048	***	0.015	***	0.033	***
	(0.007)		(0.004)		(0.007)	
2 years after	0.042	***	0.009	*	0.033	***
	(0.007)		(0.004)		(0.006)	
3 years after	0.045	***	0.009	*	0.036	***
	(0.007)		(0.004)		(0.007)	
4 years after	0.052	***	0.011	*	0.041	***
	(0.008)		(0.004)		(0.007)	
5 years after	0.048	***	0.018	***	0.030	***
	(0.009)		(0.005)		(0.007)	
Constant	0.043	***	0.008	***	0.035	***
	(0.002)		(0.001)		(0.002)	
N (person-spells)	113361		113361		113361	

Notes: Matched sample. *** p<0.001, ** p<0.01,* p<0.05. Standard errors clustered by worker are shown in parentheses. All outcomes are conditional on employment.
Sources: SOEP 1988-2015, author's calculations.

Table S20 Effects of job loss on temporary agency employment (FE linear probability model)

	Temporary agency employment	
	Coef. (SE)	
Time: γ_k		
1 year before	0.000	
	(0.002)	
0 year	-0.006	**
	(0.002)	
1 year after	-0.008	***
	(0.002)	
2 years after	-0.011	***
	(0.002)	
3 years after	-0.009	***
	(0.003)	
4 years after	-0.009	***
	(0.003)	
5 years after	-0.008	**
	(0.003)	
Time x job loss: δ_k		
1 year before	-0.011	
	(0.006)	
0 year	0.004	
	(0.007)	
1 year after	0.051	***
	(0.011)	
2 years after	0.044	***
	(0.011)	
3 years after	0.028	**
	(0.011)	
4 years after	0.016	
	(0.011)	
5 years after	0.007	
	(0.012)	
Constant	0.042	***
	(0.003)	
N (person-spells)	62500	

Notes: Matched sample. *** p<0.001, ** p<0.01,* p<0.05. Standard errors clustered by worker are shown in parentheses. Outcome is conditional on dependent employment.
Sources: SOEP 2001-2015, author's calculations.

Article 2

The effects of unemployment on non-monetary job quality in Europe: The moderating role of economic situation and labor market policies

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Abstract

Previous research has illustrated that unemployment not only has short-term, but also medium-term negative effects on workers' careers. While most studies have focused on employment chances and earnings losses, this article examines the effects of unemployment on four different facets of non-monetary job quality in Europe. Specifically, I take a comparative perspective investigating to what extent the effects of unemployment on subsequent occupational status, autonomy, authority, and job security are moderated by countries' economic situation and institutions, including active and passive labor market policies in addition to employment protection legislation. The analyses draw on micro data from round 1 to 7 (2002-2014) of the European Social Survey (ESS) including harmonized information about 125,000 workers nested in 34 countries for up to 7 rounds. Using two-stage multi-level models, the first-stage micro-level analyses reveal that unemployment has negative effects on all four facets of non-monetary job quality in the majority of the 164 country-rounds examined. Specifically, job security is negatively affected by experiences of unemployment within the last five years. However, at odds with the theoretical predictions, the second-stage macro-level analyses do not provide consistent empirical evidence for the moderating role of economic situation and labor market policies.

1. Introduction

A large literature in the social sciences has illustrated that job loss and unemployment not only have short-term, but also medium-term negative effects on workers' careers (see Brand 2015; von Wachter 2010; OECD 2013 for recent reviews). In the wake of the financial crisis in 2007-2008 and its aftermath, interest in the so-called scar effects of unemployment has been renewed among researchers and policy-makers. While most previous studies have focused on unemployment's negative effects on subsequent employment and earnings, little attention has been paid to its consequences for non-monetary facets of job quality such as occupational status, autonomy, authority, and job security (see Brand 2006; Dieckhoff 2011; Oesch and Baumann 2015 for exceptions). This is surprising, given that research on job quality has a long tradition (Jencks et al. 1988) and a growing literature across different disciplines highlights that workers' well-being cannot be measured by earnings alone (e.g., Gallie 2007; Green 2006). The need for a multidimensional analysis of job quality is also mirrored in workers' opinions on what makes a good job. For example, Muñoz de Bustillo et al. (2011: 16) find that "the most valued attribute … is job security" and that, besides a high income, workers "are also quite concerned about the nature of the job, whether it is interesting and useful to society." Accordingly, previous research reveals that a variety of bad working conditions negatively affect life satisfaction suggesting that the quality of work is essential for individuals' overall well-being (e.g., Drobnič et al. 2010).

Moreover, while researchers have started to examine how the effects of unemployment vary at the individual-level, for example by gender or immigration status (e.g., Birkelund et al. 2017; Kuhn 2002; Mooi-Reci and Ganzeboom 2015), only few studies have taken a comparative perspective, examining how macro-level factors such as countries' economic situation and institutions moderate the effects of unemployment on workers' careers. Studies that have investigated cross-country differences in the effects of unemployment point to the importance of labor market policies such as unemployment insurance, training programs, or employment protection legislation (Dieckhoff 2011; Gangl 2006). However, among the latter only Dieckhoff (2011) has focused on non-monetary job quality. Using the European Community Household Panel (ECHP) she examined the effect of unemployment on job authority, subjective and objective job security, and job satisfaction in Austria, Denmark, Spain, and the United Kingdom. The results showed that unemployment has negative effects on authority as well as subjective and objective job security for at least two years after re-employment. The revealed cross-country differences were in line with institutional variation for some facets of non-monetary job quality but not for others. The study did not address the moderating role of macro-level variables empirically. However, from a policy point of view, it is important to understand to what extent cross-country differences are shaped by economic situation and specific labor market policies.

Therefore, I address the following two separate but interrelated research questions. First, what are the effects of unemployment on four different facets of non-monetary job quality? Second, to what extent do countries' economic situation and labor market policies moderate these effects? The first question is located at the micro-level and examines the effects of unemployment on non-monetary job quality within different countries and at different points in time. It builds the foundation for the second question that asks for the role of a variety of macro-level variables in explaining the variation in the effects of unemployment.

Using micro-data from the European Social Survey (ESS), including information about 34 countries for up to 7 rounds (2002-2014), this article complements previous studies in various ways. First, expanding on studies that solely focus on employment and earnings, it examines four different facets of non-monetary job quality, including occupational status, autonomy, authority, and job security. Thus, the micro-level analyses offer a multidimensional and comprehensive analysis of the costs of unemployment in terms of job quality. These measures have been used in previous studies this topic (Brand 2006; Dieckhoff 2011) and reflect important dimensions of job quality (e.g., Findlay et al. 2013 for a review).[1] Second, complementing previous comparative studies (e.g., Dieckhoff 2011) I examine the moderating role of economic situation as well as three different labor market policies by means of two-stage multi-level models (Lewis and Linzer 2005). Specifically, this article investigates active labor market polices, unemployment insurance benefits, and employment protection legislation. Moreover, I take advantage of the fact that the ESS not only allows for comparisons between countries, but also to examine the effects of within-country changes in economic conditions and labor market policies on the effect of unemployment on non-monetary job quality. Using this within-variation by including country fixed-effects controls for time-constant unobserved heterogeneity between countries, such as social policy traditions or compositional differences, as well as reduces concerns about the cross-national comparability of job quality measures. Third, this article's comparative focus also complements research that has focused on effect

[1] While autonomy and job security figure prominently in different definitions of job quality (Findlay et al. 2013), occupational status (Brand 2006) and job authority (Brand 2006, Dieckhoff 2011) have been investigated in previous studies. Therefore, the latter measures allow discussing the results of this study in relation to previous findings.

heterogeneity at the micro-level (e.g., Birkelund et al. 2017, Mooi-Reci and Ganzeboom 2015), by investigating how the average effects for each country vary with their economic situations and institutional set-ups.[2]

The remainder of this article proceeds as follows: the next section addresses the questions why unemployment should have a negative effect on job quality (micro-level, first step) and how economic situation and labor market policies are expected to moderate this effect (macro-level, second step). The following sections present the data, measures, and analytic strategy followed by a discussion of the results. The last section summarizes the findings and offers some concluding remarks.

2. Theory and hypotheses

2.1 Micro-level: The effect of unemployment on job quality

Considering the relationship between employees and employers at the micro-level, the question arises why unemployment should have negative effects on job quality. Previous studies have identified three different, but interrelated mechanisms that explain how unemployment causes scar effects.

First, human capital theory distinguishes between general and specific skills, where the latter are not transferable across firms and only partly across occupations or industries (Becker 1993). Specifically, the longer an employee works for a firm, the more firm-specific skills are acquired and rewarded in terms of a higher job quality. Becoming unemployed and having to take employment different from one's prior line of work goes along with losing the rewards to firm-specific skills and potentially occupation- or industry-specific human capital. Corroborating empirical evidence is presented in studies that find larger earnings losses for workers with higher pre-unemployment tenure as well as for those who change occupations or industries (Brand, 2015). Moreover, it is sometimes argued that work interruptions also result in the depreciation of general human capital. For example, Edin and Gustavsson (2008: 171-175) find a positive relationship between time out of work and deprecation of general work-related skills. Given the loss of rewards to specific and the depreciation of general human capital,

[2] By average effects I mean that no subgroup differences between workers within countries are examined. For example, I do not estimate separate effects for women and men within each country. I also use regression methods that only model differences in conditional expectations/means, leaving changes in other aspects of the conditional distribution unexamined. Although this is standard in the social sciences, it should be noted that means do not necessarily represent distributions very well (e.g., Maggino 2017) (see also footnote 7).

unemployed workers will likely receive job offers of lower job quality compared with their pre-unemployment jobs.

Theories of signaling or unemployment stigma offer a second explanation for scar effects. The key idea is that employers overcome uncertainty about applicants' productivity by considering observable characteristics such as their employment history (Spence 1973). Irrespective of unemployment's actual effects on skills and knowledge, employers may view it as a signal of low productivity. This will weaken unemployed persons bargaining power and result in fewer and lower quality job offers (e.g., Lockwood 1991). In a recent field-experiment Kroft et al. (2013) manipulated the length of unemployment spells in fictitious résumés finding that the likelihood of an invitation to a job interview decreases with the signaled unemployment duration.

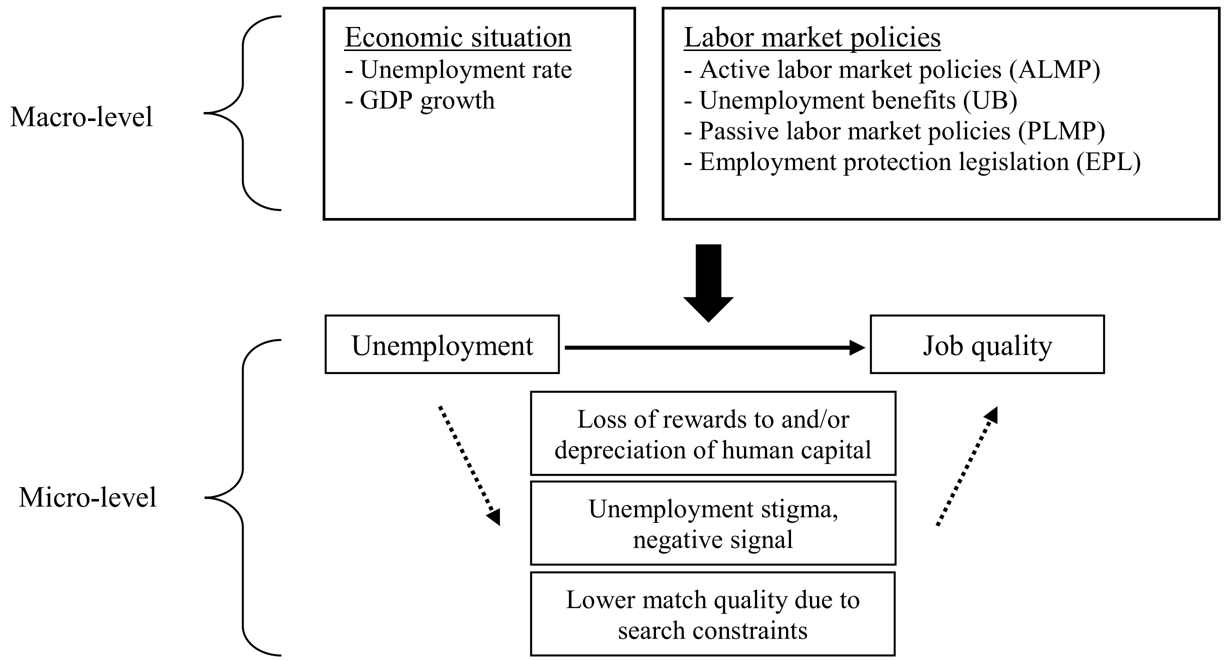
Lastly, job search theories assume that job-seekers' decisions to accept or reject a job offer are determined by their reservation wage (e.g., Mortensen, 1986). The latter reflects the lowest job quality at which a worker is willing to accept a particular job offer and is affected by job searchers' characteristics, but also by search constraints, such as the number of incoming job offers and workers' available income. Given that most unemployed persons face financial constraints, because unemployment benefits or other income sources like savings or family income are used up over time, they likely have to accept job offers that are associated with a lower job quality compared with their pre-unemployment job. In other words, due to these search constraints newly formed job matches after becoming unemployed tend to be of a lower quality than those matches the workers had formed before. Previous research also attest to the significance of search constraints by showing that unemployment has less negative effects on workers' careers if they receive unemployment benefits (e.g., Gangl 2004).

A few previous studies have also examined to what extent the effects of job loss and unemployment on job quality vary by workers' characteristics such as gender, immigration status, or parenthood. In terms of employment and wages, Kuhn (2002) reports that job loss has more negative consequences for women. Following job search theories, this may be explained by their greater search constraints concerning geographical mobility. In contrast, Mooi-Reci and Ganzeboom (2015) find that wage losses due to unemployment are more short-lived for women and strongly persistent for men. With respect to immigration status the results are also mixed. While Mooi-Reci and Ganzeboom (2015) find for the Netherlands more negative wages effects for non-Dutch workers, a recent field experiment by Birkelund et al. (2017) shows no multiplicative disadvantage for unemployed ethnic minorities in terms of callback rates for job applications. Mooi-Reci and Ganzeboom (2015) also investigated effect heterogeneity by parenthood finding, however, no statistically significant differences for either women or men. While these studies show that the effects of unemployment on subsequent job quality may vary across subgroups of workers, in this article, I focus on the average effects for workers within each country, in order to examine the moderating role of macro-level variables.

Figure 1 summarizes the arguments showing that unemployment and job quality are linked through the micro-level mechanisms of loss of rewards to and/or depreciation of human capital, unemployment stigma or negative signaling, and lower match quality due to search constraints. Based on these mechanisms, I derive the following micro-level hypothesis:

Hypothesis 1: Unemployment compared to continuous employment has a negative effect on subsequent job quality.

Figure 1 Micro-macro model regarding the effect of unemployment on subsequent job quality as well as the moderating role of economic situation and labor market policies



Sources: Own illustration.

2.2 Macro-level: The moderating role of economic situation and labor market policies

Given that job quality scars can be explained by the interaction of unemployed persons' job search and employers' hiring behaviors at the micro-level, it is expected that macro-level variables which govern these interactions, such as economic conditions and institutions, are important moderators.

With respect to economic situation, job search theory predicts that in slack labor markets, indicated, for example, by high unemployment or low growth, unemployed workers receive fewer job offers decreasing their reservation wage. In other words, workers losing their job in a poor economic situation often have to accept jobs with a lower job quality compared to their pre-unemployment job, because of the lack of better opportunities (e.g., Gangl 2006).[3] Specifically, if economic downturns push workers to accept jobs in occupations or industries they have not been trained for, the loss of specific human capital likely results in stronger negative effects on job quality. The unemployment stigma mechanism is ambiguous: while a poor economic situation increases unemployment durations, and, thereby, also stigma, individual unemployment is also less informative about individuals' productivity in these contexts (Kroft et al. 2013). Overall, the job search and human capital mechanisms are expected to dominate.[4]

Hypothesis 2: The poorer the economic situation in a country, the stronger the negative effect of unemployment on subsequent job quality.

With respect to passive labor market policies, I mainly focus on the generosity of unemployment insurance in terms of income replacement and benefit duration. Based on job search theories, Burdett (1979) describes unemployment insurance as a "search subsidy" meaning that the additional income increases workers' reservation wage. Put differently, the decommodification of workers through unemployment benefits increases their bargaining power such that they can reject poor job offers and continue the search for adequate re-employment. Consequently, workers are less likely to be pushed into occupations or industries outside of their qualifications, meaning that a complete loss of rewards to specific human capital is avoided. In summary, job search theory predicts that generous unemployment benefits extend unemployment durations and, thereby, increase the matching quality. While longer unemployment durations may also increase unemployment stigma, this effect can be assumed to be weaker, because employers in these countries know that prolonged job searches are rather due

[3] The theoretical argumentation focuses on what Gangl (2006: 990) calls the "behavioral implications" of macro-level variables. However, economic conditions and labor market policies may also affect who becomes unemployed and re-employed. For example, in countries with poor economic conditions, the re-employed may be composed of individuals who are less negatively affected by job loss.

[4] In this study, we focus on variation in the economic situation across countries and over time. Of course, differences in economic situations within countries across regions or industries may also result in different effects of past unemployment on subsequent job quality (see also footnote 10).

to workers increased bargaining power than their low productivity. Accordingly, I derive the following hypothesis.

Hypothesis 3: The more generous the unemployment benefits in a country, the weaker the negative effect of unemployment on subsequent job quality.

Besides passive labor market policies, countries also differ in their use of active labor market policies (ALMP), which can be assumed to mitigate the negative effects of unemployment by counteracting the loss of rewards to specific as well as the depreciation of general skills. Specific education updates workers' skills to changing demands (e.g., Gangl, 2006), reducing losses associated with changes of occupations or industries. General education and on-the-job training are thought to stop the depreciation of general skills. The effects are likely to be more positive if programs are tailored to job searchers' needs. While ALMP overall is expected to decrease stigma, it is occasionally argued that in some countries participating in specific programs may rather send a negative signal. The creation of private and public job opportunities is also thought to buffer the negative effects as in line with job search theory the number of available job offers increases. Overall, ALMP are assumed to moderate the negative effect of unemployment on job quality in the following way:

Hypothesis 4: The more support unemployed workers receive through ALMP in a country, the weaker the negative effect of unemployment on subsequent job quality.

Lastly, it is often argued that employment protection legislation (EPL), governing the hiring and firing of workers, moderates the scar effects of unemployment (e.g., Gangl 2006). Focusing on EPL for regular contracts, economic theory assumes that stricter job security provisions reduce labor market dynamics and, therefore, result in an involuntary extension of unemployment which, in turn, should be associated with a greater depreciation of general human capital and unemployment stigma. Given the longer unemployment duration, it is also more likely that search constraints gain in importance, pushing workers to accept jobs that have a lower job quality than their pre-unemployment position. For example, if stricter job security provisions create barriers to regular employment, due to employers risk-averse hiring, unemployed workers may be hired on temporary contracts that provide less job security, but are also often associated with lower job quality (Dieckhoff 2011).

Hypothesis 5: The stricter the employment protection legislation for regular contracts in a country, the stronger the negative effect of unemployment on subsequent job quality.

3. Data and methods

3.1 Micro data

I use comparative data of round 1 to 7 (2002-2014) of the European Social Survey (ESS) covering up to 36 countries. The ESS is collected biannually and the majority of countries participate repeatedly.[5] The data are based on random probability samples in each country and round and the population of interest is all persons aged 15 and over resident within private households. The ESS was explicitly created to provide comparable data across countries and time and standardizes all important aspects of survey methodology (Fitzgerald and Jowell, 2010). I chose the ESS not only because it is the only comparative survey that includes data on past unemployment and non-monetary job quality for a large number of countries at different points in time, but also because it has been attested a high quality. For example, next to winning the Descartes Price of the European Union for its advancements in survey research, an evaluation of the sampling quality by Kohler (2008) comparing the ESS 2002 with four other European surveys suggests that it performed best on all of four separate criteria (i.e., documentation, sampling process, external criteria for representativity, internal criteria for representativity).

For the analyses, data from all rounds are pooled and the sample is restricted to employees aged 20 to 64, who do not work in the armed forces.[6] It includes roughly 125,000 workers nested in 34 countries for up to 7 rounds. The average number of workers across all 164 country-rounds is about 762 ranging from 175 in Albania (2012) to 1302 in Germany (2014).[7] Although I aimed at using as many of the country-rounds as possible, 12 of the 176 potential country-rounds had to be excluded from the micro-level analyses, mostly because information on one or more of the key variables is not available (see supplementary material S1 for details).

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[5] I use the following integrated data files: ESS 1: Edition 6.4, ESS 2: Edition 3.4, ESS 3: Edition 3.5, ESS 4: Edition 4.3, ESS 5: Edition 3.2, ESS 6: Edition 2.2, ESS 7: Edition 2.0. For the imputation models I also use the following data from the interviewer questionnaires: ESS 1: Edition 5.1, ESS 2: Edition 3.2, ESS 3: Edition 2.0, ESS 4: Edition 2.0, ESS 5: Edition 3.0, ESS 6: Edition 2.1, ESS 7: Edition 2.0. The data are provided by the Norwegian Social Science Data Services, Norway – Data Archive and distributor of ESS data for ESS ERIC.

[6] Self-employed individuals have been excluded, because the theories that predict negative effects of past unemployment on job quality do not usually apply. Individuals reporting an occupation in the armed forces have been excluded, too, because they are not differentiated in ISCO-88.

[7] As explained in footnote 2, I do not examine subgroup differences between workers within countries. This has two reasons. First, the sample sizes for subgroups within countries are often too low to reliably estimate the effects of unemployment within these. Second, focusing on average effects makes sense as a first step, because research on the effects of unemployment on non-monetary job quality is still very scarce and the theoretical derivations presented do not suggest that the labor market policies I consider are targeted at specific subgroups.

While the ESS goes to great lengths to maximize comparability and homogenize data quality across countries and rounds, some issues remain. Specifically, exploratory analyses show that, on average, across all 164 country-rounds, 14 percent of workers have missing values on one or more variable. Furthermore, the percentage of complete cases ranges from 58 percent in the United Kingdom (2012) to 97 percent to Norway (2004). To handle missing data at the micro-level, I use multiple imputation to create and analyze ten multiply imputed data sets. Compared to other missing data techniques, it improves the accuracy and power of the analyses (Schafer and Graham 2002). Specifically, all incomplete variables are imputed under a fully conditional specification (White et al. 2011). The micro-level analyses are performed for each imputed dataset and the estimates and standard errors are combined using Rubin's rules (Rubin 1978). The imputation and analyses are done in Stata 13.1 using the mi package. Supplementary material S2 provides details about missing data, the imputation models, and diagnostics. It also shows that the estimates of the effects of unemployment on non-monetary job quality are very similar if instead complete case analyses are used.

3.2 Micro-level variables

The key independent variable is past unemployment. The respondents are asked whether they have ever been unemployed and seeking work for a period of more than three months and whether any of these periods have been within the last five years. For the analyses, I compare workers who report unemployment in the last five years with those who do not. Because unemployment periods last for at least three months and respondents report seeking work, it likely captures more involuntary than voluntary unemployment, although no information about the reason for unemployment is available. The restriction to unemployment within the last five years also increases the interpretability of the results by directing the attention to the medium-term effects of past unemployment.

The key dependent variable non-monetary job quality is measured by the following four indicators: occupational status, autonomy, authority, and job security. These measures have been used in previous studies (e.g., Brand 2006; Dieckhoff 2011) and reflect important dimensions of job quality.[8] Occupational status is measured by the standard occupational prestige scale (SIOPS) which, according to Ganzeboom and Treimann (2003: 173), reflects the "social re-

[8] Unfortunately, the data used do not include repeated measures of earnings. The data do, however, include a question on whether respondents' have taken courses or attended lectures or conferences. Although this may represent another dimension of job quality ("continued training"), the question does not allow identifying whether the training is employer-funded. It may also include participation in ALMP making it not suitable for the current analysis.

wards (approval, admiration, deference, contempt) people can expect in human interactions" based on their jobs.[9] This corresponds well with workers' appreciation of jobs that are interesting, helpful to other people, and useful to society (Muñoz de Bustillo et al. 2011). Autonomy conceptually reflects the dimension of task discretion (Findlay et al. 2013). The respective question in the ESS main questionnaire is "Please say how much the management at your work allows you to decide how your own daily work is organized?" The answers range from "0 I have no influence" to "10 I have complete control" on an 11-point scale. Another job quality dimension is authority representing workers' status within their workplace (Dieckhoff 2011). It may also be indicative of the extent to which workers can participate in relevant decision-making. In the ESS main questionnaire authority is measured by the question "In your main job, do you have any responsibility for supervising the work of other employees?" with "Yes" and "No" as possible answers. The questionnaire also makes clear the "supervising" refers to "both monitoring and being responsible for the work of others". Finally, job security has been identified as one of the most important dimensions of job quality by both researchers and workers (e.g., Munoz de Bustillo et al. 2011). I use type of contract as an objective measure defining job security as having a contract of unlimited duration compared to workers with contracts of limited duration and those who do not have a contract at all.

In general, all measures used can be considered objective in the sense that employees report their working conditions instead of their subjective satisfaction with the job or specific aspects of it. The latter measures are not used here, because they not only reflect objective working conditions but also job values making the interpretation of the results more difficult (Dieckhoff 2011). For example, if one would not find a negative effect of unemployment on job satisfaction this may either be explained by the fact that there is no effect on objective working conditions or due to a combination of changes in both working conditions and job values.

The analyses also control for a number of variables that may confound the association between past unemployment and job quality. Besides socio-demographics, such as age and sex, individuals' years of education as well as their social origin is controlled. Social origin is measured by the educational qualifications of the parents as well as the question whether or

[9] I translated the four-digit ISCO-88 (COM) and ISCO-08 codes that are provided in the ESS into SIOPS using the conversion tools of Ganzeboom and Treiman. In round 6 and 7 the ESS changed its occupational coding from ISCO-88 (COM) to ISCO-08. The ISCO-08 codes have been translated into ISCO-88 before conversion into SIOPS. For occupational codes that are unique to ISCO-88 (COM), SIOPS is assigned using two- or three-digit level information.

not the mother was working at age 14. For these control variables it is assumed that they affect both workers' unemployment risk and job quality. For example, older workers may have a lower risk of unemployment, because they have higher tenure or contracts of unlimited duration. At the same time, they may hold positions of higher job quality due to their greater work experience. Similarly, it is known that education shields workers from unemployment and that employers reward higher qualifications with higher job quality.

We explicitly do not control for variables that represent important mechanisms for the effect of unemployment on job quality. For example, a worker's current tenure, occupation, and industry will at least in part reflect the loss of rewards to specific human capital. Adjusting for these variables would induce so-called overcontrol bias in estimating the total effect of unemployment on non-monetary job quality (Elwert 2013). Details about the measurement are given in Table 1.

3.3 Macro data and macro-level variables

The ESS is complemented with time-varying macro data about unemployment rates, GDP growth, unemployment benefit generosity, active and passive labor market policies, and employment protection legislation (see supplementary material S3 for references). Because reliable estimates of the effects of macro-level variables require a sufficient number of macro-level observations, all efforts were made to assemble data for as many country-rounds as possible. However, not all indicators are available for all country-rounds. Therefore, the macro-level analyses will focus on the 26 countries and 124 country-rounds with complete data (see supplementary material S1 for details). Table 1 offers a summary of the macro-level variables and Table 2, in addition, provides descriptive statistics for each of the 26 countries.

Economic situation is measured by the harmonized unemployment rate from the Key Indicators of the Labour Market (KILM) of the International Labour Organization (ILO). For a sensitivity analysis, I also use the GDP growth per capita from the World Development Indicators (WDI) of the World Bank.[10]

Information on unemployment benefit generosity is taken from the Comparative Welfare Entitlement Data 2 (CWED 2) by Scruggs et al. (2014). These include harmonized time-varying information about net replacement rates and unemployment benefit duration in weeks.

[10] While the labor market policies are designed and implemented at the national level, it would have been interesting to examine variation in the economic situation at the regional or industry level, too (see also footnote 4). However harmonized information at these levels are not available for most of the examined countries.

Table 1 Measurement of the micro- and macro-level variables

Variable	Measurement
Micro-level variables	
Dependent variables	
Occupational prestige [a]	Standard occupational prestige scale (SIOPS) based on four-digit ISCO-88 (COM) and ISCO-08, Range: 6-78
Autonomy	Allowed to decide how own daily work is organized? Range: 0 = I have no influence - 10 = I have complete influence
Authority	1 = responsible for supervising other employees, 0 = otherwise
Job insecurity	1 = contract of unlimited duration, 0 = no contract or contract of limited duration
Independent variables	
Past unemployment	1 = experienced unemployment within the last 5 years, 0 = otherwise
Age	In years
Sex	1 = female, 0 = male
Education	Completed full-time education in years
Father's education [b]	Two categories according to ISCED: 1 = ISCED 3-6, 0 = ISCED 0-2
Mother's education [b]	Two categories according to ISCED: 1 = ISCED 3-6, 0 = ISCED 0-2
Mother's employment at age 14	1 = employed, 0 = otherwise
Macro-level variables[c]	
Unemployment rate	Unemployment rate in percent
GDP growth	GDP growth per capita in percent (constant 2005 dollars)
Benefit generosity	Index = average net replacement rate × benefit duration in percent of 48 months, Range: 0 = no benefits - 100 = full replacement for 48 months or longer
Passive labor market policies (PLMP)	Expenditure per unemployed as percent of GDP per capita
Active labor market policies (ALMP)	Expenditure per unemployed as percent of GDP per capita
Employment protection legislation (EPL)	OECD EPL for regular contracts indicator (version1), Range: 0 = unregulated - 6 = highly regulated

Notes: [a] Footnote 5 provides details about the conversion of ISCO-88 (COM) and ISCO-08 into SIOPS, [b] ISCED 3-6 reflect completed upper secondary education or higher and ISCED 0-1 reflect completed lower secondary education or less, [c] References to the macro data are given in supplementary material S3.
Sources: Own illustration.

Table 2 Descriptive statistics on the macro-level variables

Country	Code	N	Unemployment rate		GDP growth		Unemployment benefit generosity		PLMP expenditure		ALMP expenditure		EPL for regular contracts	
			Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Austria	AT	4	4.5	0.4	1.8	1.4	11.9	0.4	55.5	2.7	20.0	2.7	2.6	0.2
Belgium	BE	7	7.7	0.9	0.8	2.1	67.0	1.9	65.6	7.1	17.4	4.4	1.9	0.1
Bulgaria	BG	3	7.9	1.9	4.7	7.3	18.0	1.4	6.6	4.0	8.0	1.3	1.8	0.0
Czech Republic	CZ	5	7.3	1.3	2.2	4.6	5.9	0.7	7.3	2.7	3.2	1.2	3.2	0.1
Denmark	DK	7	5.3	1.3	0.1	2.7	54.3	12.4	76.1	22.3	51.1	10.4	2.1	0.0
Estonia	EE	4	9.4	3.9	2.9	11.7	8.2	1.8	7.4	7.9	1.2	0.5	2.9	0.1
Finland	FI	6	8.7	1.4	0.8	4.8	29.5	1.0	39.1	3.2	16.7	3.2	2.2	0.1
France	FR	5	8.4	0.5	0.2	2.1	34.3	0.7	39.0	5.1	19.1	2.3	2.4	0.0
Germany	DE	7	8.6	1.7	0.7	3.1	16.4	0.0	39.7	7.8	18.5	5.4	2.7	0.0
Greece	GR	3	10.1	1.6	1.9	1.5	13.9	0.2	8.1	0.8	3.1	1.7	2.8	0.0
Hungary	HU	3	8.2	1.6	-0.4	5.6	10.0	0.9	13.2	2.4	8.3	0.7	2.0	0.0
Ireland	IE	7	7.1	4.4	2.6	4.8	13.9	1.3	38.3	2.4	23.3	10.9	1.4	0.1
Italy	IT	2	9.7	2.8	-2.2	5.3	8.0	3.4	27.4	20.0	10.9	0.1	2.8	0.0
Latvia[a]	LV	1	10.0		11.9		13.5		4.5		2.2		2.3	
Lithuania	LT	2	9.0	6.8	-0.7	18.6	8.1	0.5	7.3	2.4	6.9	5.6	3.0	0.5
Netherlands	NL	7	3.6	0.8	1.1	2.8	37.9	2.0	84.4	25.2	46.8	19.4	2.9	0.0
Norway	NO	7	3.5	0.7	0.5	1.7	42.2	9.4	23.6	5.2	28.6	2.6	2.3	0.0
Poland	PL	6	14.3	4.9	3.8	2.0	7.7	0.9	11.0	1.3	6.6	4.0	2.2	0.0
Portugal	PT	7	7.5	3.0	0.2	2.3	40.7	4.2	30.9	9.2	14.4	6.6	4.4	0.2
Romania[a]	RO	1	7.2		4.8		12.8		11.8		3.2		1.9	
Slovak Republic	SK	5	15.2	3.5	4.1	6.1	8.5	1.9	6.0	3.1	2.0	0.4	2.3	0.1
Slovenia	SI	3	5.7	0.9	0.5	8.0	12.9	0.3	14.4	4.6	6.1	1.9	2.9	0.2
Spain	ES	7	13.4	5.0	0.9	2.8	37.9	0.8	28.7	4.9	11.4	3.5	2.3	0.1
Sweden	SE	5	7.1	1.2	0.6	3.7	20.3	2.2	25.6	11.0	22.4	5.0	2.6	0.0
Switzerland	CH	6	3.6	0.7	0.6	2.3	34.5	4.9	35.8	7.7	23.7	4.8	1.6	0.0
United Kingdom	GB	4	6.4	1.6	0.1	3.4	3.8	0.1	7.4	1.2	1.4	0.5	1.3	0.0

Notes: Table 1 provides details about the measurement, all indicators are measured with a lag of three years to approximate the economic situation or labor market policies at the time of unemployment, [a] No within-country standard deviation reported, because only one round is available.
Sources: Key Indicators of the Labour Market (KILM) of the International Labor Office (ILO), World Development Indicators (WDI) of World Bank, Comparative Welfare Entitlement Dataset 2 (CWED2) of Scruggs et al. (2014), OECD, Eurostat, EPL for regular contracts of Avdagic (2015); Supplementary material S3 provides references. Own calculations.

The latter are re-expressed in percent of 48 months meaning that countries that offer unlimited duration or a duration of 48 months or more have a value of 100. To measure benefit generosity an index was constructed that multiplies the average of the net replacement rates for two household types (i.e., single and family) with the benefit duration in percent of 48 months. This index takes values between 0 and 100. To give an example, a country with an average replacement rate of 0.7 and a benefit duration of 1 year has a value of 17.5 (=0.7*((12/48) *100)). Another measure of decommodification is the expenditure on passive labor market policies (PLMP) per unemployed as a percentage of GDP per capita. This measure is used for a sensitivity analysis. Although it covers unemployment assistance in addition to insurance as well as is more sensitive to the structure of unemployment across countries, it is less clear which aspects of decommodification are captured. Similarly, to assess how much support the unemployed receive through active labor market policies (ALMP), the expenditure on ALMP per unemployed as a percentage of GDP per capita is considered. The latter includes, for example, expenditure on training, employment incentives, and job creation. Expenditure data are taken from OECD and Eurostat.

Information on employment protection legislation (EPL) is taken from OECD (e.g., Veen 2009). For a few Central and Eastern European (CEE) countries, the OECD indicator has been complemented by information from Avdagic (2015) who scored CEE countries following the OECD approach. For the analyses, I focus on version 1 of the EPL indicator for regular contracts. The indicator is constructed from a number of items that capture different aspects of job security provisions (e.g., severance pay, advance notification) and varies between 0 (unregulated) and 6 (regulated).

3.4 Methods

Since the individual-level data from the ESS (level 1) are nested within rounds (level 2) and countries (level 3), I use a multi-level model to take account of this structure. A general three-level model can be written as follows:

(1) $$Y_{itj} = \beta_{0tj} + \sum_{p=1}^{P} \beta_{ptj} X_{pitj} + v_{itj}$$

where Y_{itj} is the job quality for a worker i nested in round t and country j. X_{pitj} are P individual-level variables such as past unemployment, age, and years of education, and v_{itj} is an individual-level error term. The variation of β_{ptj} across rounds and countries is modeled as a function of Q time-varying country-level variables Z_{qtj} and a country-level error term ε_{ptj}

(level 2 and 3) where the former include the indicators for economic situation and labor market policies.

(2) $$\beta_{ptj} = \gamma_{p0} + \sum_{q=1}^{Q} \gamma_{pq} Z_{qjt} + \varepsilon_{ptj}$$

This model can be fitted by either estimating the equations simultaneously, relying on so-called mixed-effects models, or by using a two-stage approach (Lewis and Linzer 2005). In the latter, the individual-level parameters are estimated within each country-round (first stage) and, in a second stage, the estimated coefficients are used as the dependent variable in a macro-level regression. Compared to the simultaneous estimation, the two-stage approach is very flexible and allows all individual-level coefficients to vary across countries and rounds. This is important as not only the effects of past unemployment may vary across country-rounds, but also those of the control variables. In this article, I also use the two-stage approach because it reinforces the conceptual distinction between the micro- and macro-level analyses as well as allows implementing additional steps with respect to the former.

Specifically, before estimating the effect of past unemployment on the different facets of non-monetary job quality within each country-round (first stage), I preprocess the data by using coarsened exact matching (CEM) (Iacus et al. 2012). Preprocessing the data by matching workers who experienced unemployment with those who did not on the control variables (e.g., age, sex, years of education) makes the subsequent regression analyses less model-dependent. It also restricts the data to the empirical common support in order to avoid model-dependent extrapolations. CEM was performed separately within each country-round. After coarsening the age and years of education variable, exact matching on all control variables was performed. Balance checks comparing the differences in the control variables before and after CEM show that the matching was successful (see supplementary material S4 for details). The following micro-level analyses are all performed on the matched data.

In the first-stage micro-level analyses for each country-round the respective non-monetary job quality indicator is regressed on past unemployment as well as the control variables. For the continuous control variables, age and education squared terms are included to allow for non-linear effects. For the dependent variables, occupational prestige and autonomy, I estimate linear regression models and for the binary dependent variables, authority and job security, logistic regression models are estimated and average marginal effects are calculated. I used the provided design weights to take account of unequal selection probabilities in some country-rounds.

In the second-stage macro-level analyses the estimated past unemployment effects serve as the dependent variable and are regressed on time-varying indicators of economic conditions, labor market policies, and round dummies. Including dummies for each round allows controlling for common unobserved time-varying factors (e.g., common economic shocks). To take account of the uncertainty in the first-stage estimates, I follow the recommendation of the literature and use so-called estimated dependent variables (EDV) regression models in the second stage which are estimated by feasible generalized least squares (FGLS) (Lewis and Linzer, 2005). To further consider the nesting, standard errors are clustered by countries. Moreover, the independent variables are measured with a three years lag to approximate the situation at job search. To increase the interpretability of the macro-level analyses all independent variables were centered and standardized to unit variance.

For the macro-level analyses, I estimate both EDV regression models without and with country fixed-effects. The latter models only use within-country variation over time to estimate the effects of economic situation and labor market policies on the past unemployment effect on non-monetary job quality. They serve as an important robustness check as they control for all time-constant unobserved heterogeneity between countries (e.g., social policy traditions, compositional differences) and reduce concerns about the cross-national comparability of the outcomes.

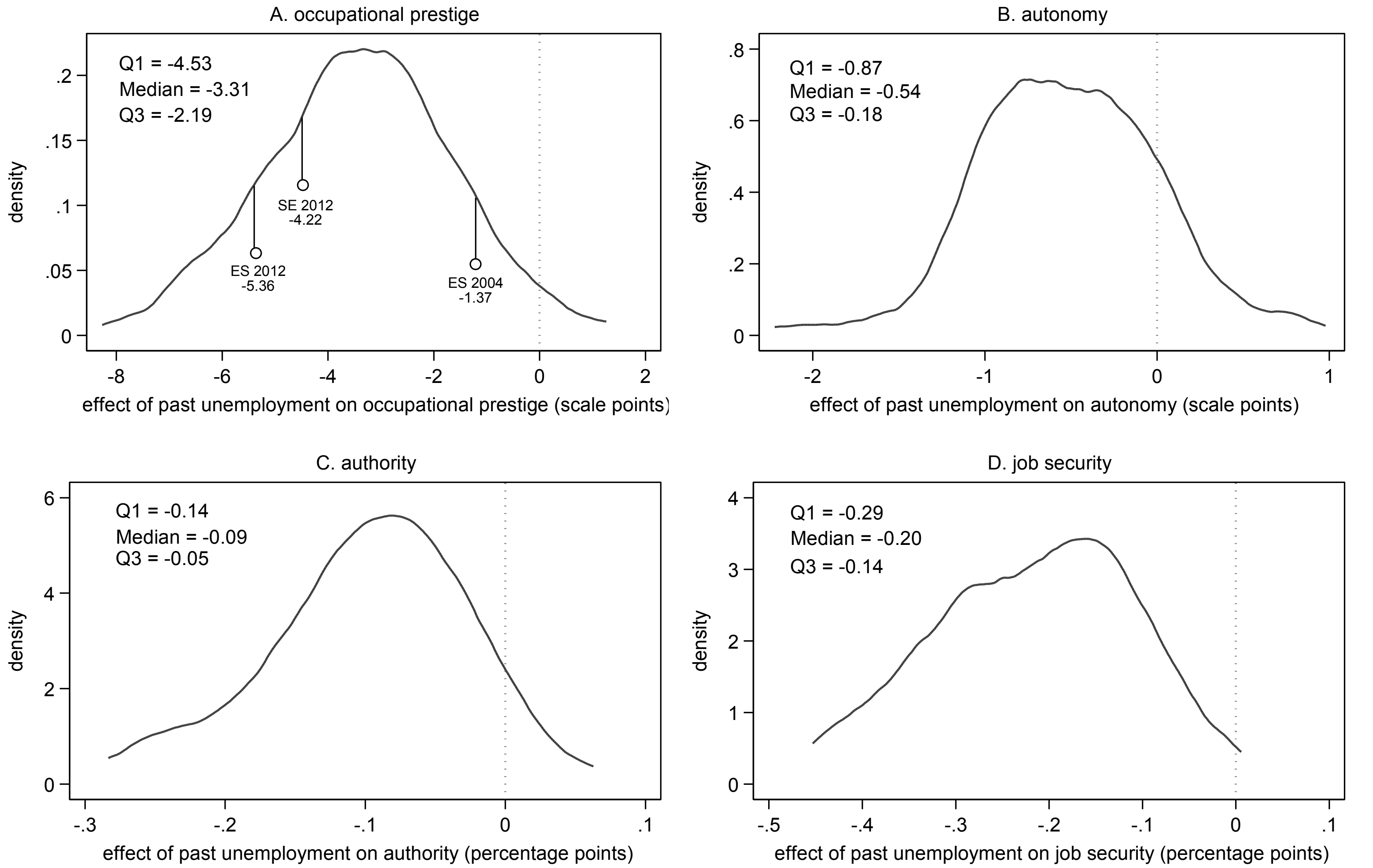
4. Results

4.1 Micro-level analyses

Figure 2 shows the results of the first-stage micro-level analyses by plotting the distributions of the estimated past unemployment effects on the four indicators of non-monetary job quality for all country-rounds (Panels A-D). These estimates are based on regression models separately fitted to the matched data within each of the 164 available country-rounds. To give an example, I highlighted the results for three country-rounds in Panel A. Using a linear regression of occupational prestige on past unemployment and the control variables, the negative effect of past unemployment is -5.36 scale points (90% CI: -7.79, -2.92) in Spain in 2012 (ES 2012). For Sweden in 2012 (SE 2012) and Spain (ES 2004) estimates of -4.22 (90% CI: -6.71, -1.72) and -1.37 (90% CI: -3.92, 1.17) are obtained.

More generally, the distribution of the estimates indicates that in the majority of the country-rounds unemployment within the last five years has a negative effect on current occupational prestige (Panel A). However, it also shows that the effects strongly vary across countries and

Figure 2 Results of micro-level analyses: Effects of past unemployment on four indicators of non-monetary job quality



Notes: Q1 = first quartile, Q3 = third quartile, distributions are smoothed by kernel density estimation with an epanechnikov kernel; ES 2012 (Spain), SE 2012 (Sweden), and ES 2004 (Spain) are only highlighted as examples of between- and within country variability (see text).
Sources: European Social Survey, round 1 to 7 (2002-2014), 164 country-rounds. Own calculations.

rounds. The median effect is -3.31 and 50 percent of the estimates range from -4.53 (first quartile) to -2.19 (third quartile).

Similar results are obtained for the other measures of non-monetary job quality. Panel B shows the distribution of the effects for autonomy. Although the median effect of -0.54 is not very large, the results reveal that the negative effect of past unemployment on autonomy varies substantially across country-rounds and that for about a quarter of the 164 available country-rounds the negative effect is at least -0.87 (first quartile). At the same time, Figure 3 shows that in a quarter of the country-rounds unemployment does not seem to have any medium-term effect on workers' opportunities to decide about the organization of their own daily work (Q3=-0.18, third quartile).

For the indicators of authority and job insecurity, I estimated logistic regression models and calculated average marginal effects. Panel C highlights that in most country-rounds workers who experienced unemployment in the last five years have a lower probability to hold a supervising position. The median of the distribution is about -9 percentage points and in about 10 percent of the country-rounds the negative effect exceeds -20 percentage points. This highlights that in some country-rounds past unemployment is associated with substantially significant losses in authority.

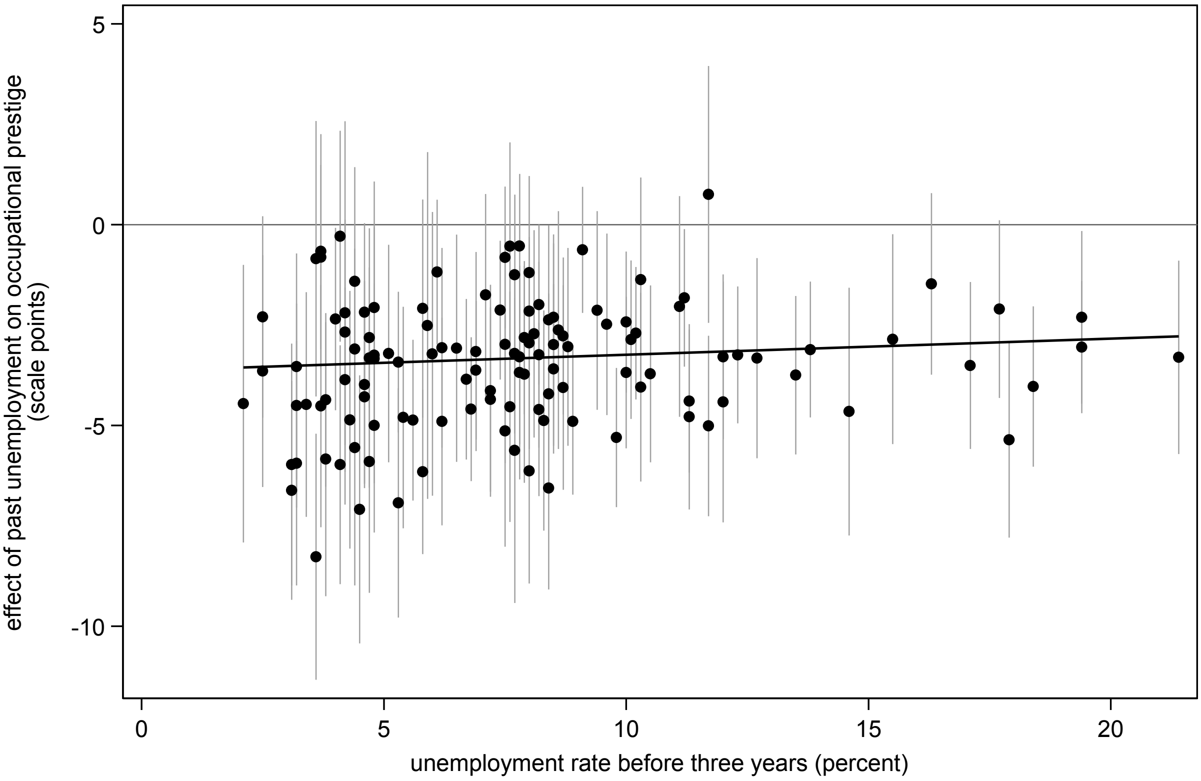
Lastly, considering job security (Panel D) the results show rather strong negative effects of past unemployment. The median across all country-rounds is -20 percentage points, indicating a substantially higher risk of having no contract at all or only a contract of limited duration. Moreover, looking at the distribution, we see that for almost all country-rounds past unemployment decreases, on average, the probability of having a secure job.

Overall, the results of the first-stage micro-level analyses are in line with hypothesis 1 as well as the findings of previous research (Brand 2006; Dieckhoff 2011), suggesting that unemployment has medium-term negative effects on a variety of job quality indicators. Moreover, the analyses reveal substantial variation in the effects of past unemployment across countries and time, begging the question to what extent this can be explained by differences in economic conditions and labor market policies.

4.2 Macro-level analyses

To examine how the effects of past unemployment on job quality are moderated by economic situation and labor market policies the estimated effects are used as the dependent variable in the second-stage macro-level analyses. For illustration, Figure 3 plots the effects of past unemployment on occupational prestige (y-axis), including the respective 90 percent confidence intervals, against the unemployment rate before three years (x-axis). It also includes the regression line from a bivariate EDV regression model of the past unemployment effects on the unemployment rate before three years.

Figure 3 Moderating effect of unemployment rate on the effect of past unemployment on occupational prestige (Bivariate EDV regression model)



Notes: Past unemployment effects on occupational prestige with 90 percent confidence intervals.
Sources: European Social Survey, round 1 to 7 (2002-2014), 124 country-rounds; Table 1 and Table 2 provide details about the macro data. Own calculations.

The results reveal that a higher unemployment rate is associated with slightly smaller negative effects of past unemployment on occupational prestige. Specifically, a five percentage point increase in the unemployment rate is predicted to decrease the negative effect by about 0.2 scale points. This bivariate finding is at odds with hypothesis 2 stating that poor economic conditions result in larger negative effects of unemployment. However, to check whether the bivariate results are robust to controlling for other macro-level variables, the following paragraphs report the findings for the multiple regression macro-level analyses.

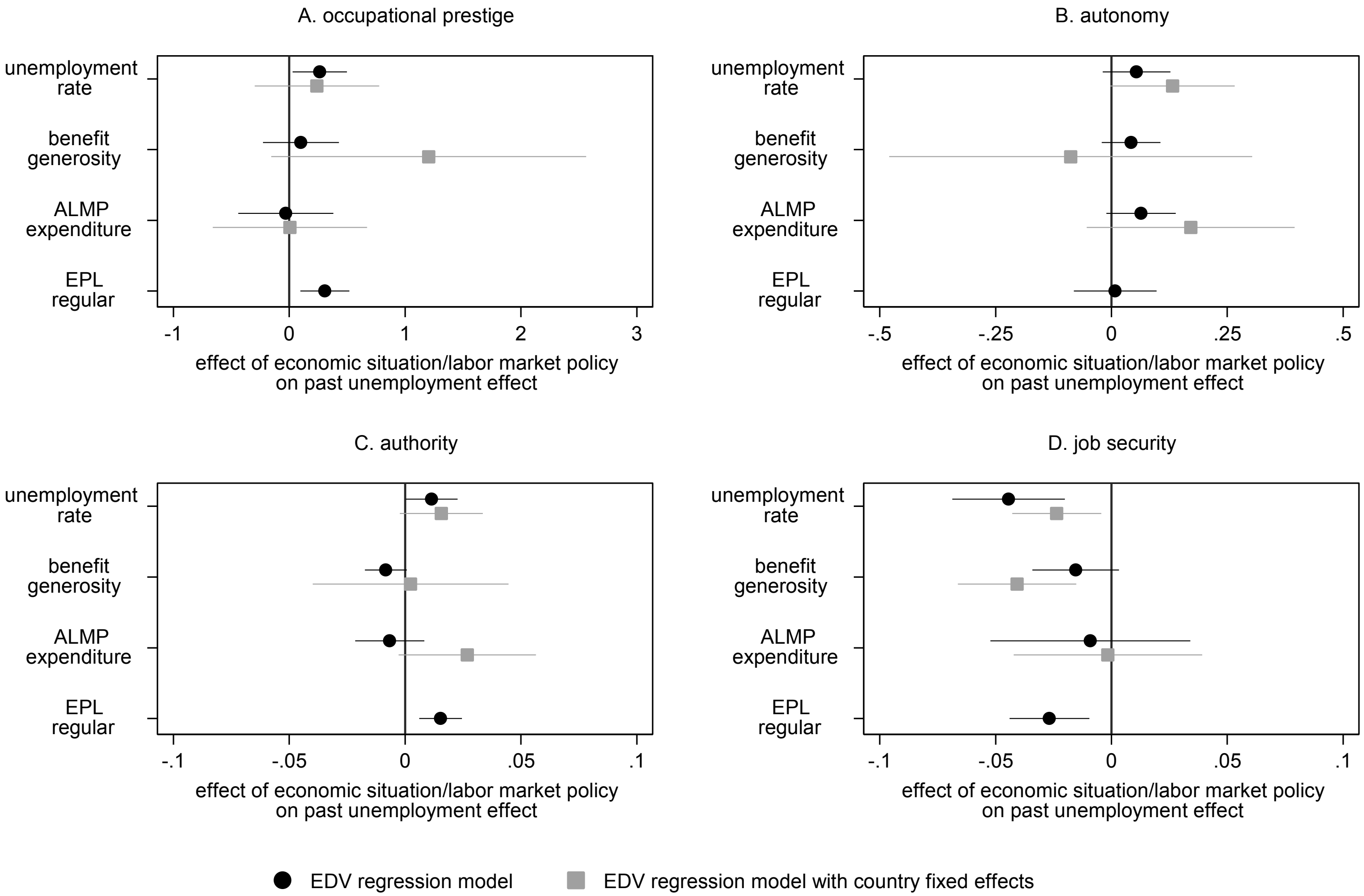
Figure 4 shows the results of the EDV regression models controlling for the other macro-level variables as well as period effects. For example, in the models for unemployment rate, I control for benefit generosity, ALMP expenditure, EPL for regular contracts, and period effects (model 1, black circle). In a second specification (model 2, grey square), country fixed-effects are added to check whether the results are robust to unobserved time-constant heterogeneity between countries. These latter models only use within-country variation in unemployment rates over time to estimate the moderating effect of unemployment rate on the effect of past unemployment on job quality. Panel A to D report the results for the different indicators of non-monetary job quality. The coefficients are standardized and accompanied by 90 percent confidence intervals. Supplementary material S5 provides the corresponding results table.

For occupational prestige (Figure 4, Panel A), the findings of the multiple regression analysis are similar to those for the bivariate analysis shown in Figure 3. Specifically, a one standard deviation increase in the unemployment rate is associated with a 0.26 scale point reduction in the negative effect of past unemployment, controlling for labor market policies and period effects (specification 1, black circle). The model that adds country fixed-effects yields a very similar estimate of 0.24 (specification 2, grey squares). At odds with hypothesis 2, these results suggest that economic conditions do not strongly affect the size of the negative effect of unemployment on occupational prestige.

For benefit generosity the results differ between the two specifications. While the first specification suggests almost no effect, the EDV regression model that includes country fixed effects shows a substantially significant positive effect. A one standard deviation increase in benefit generosity is predicted to reduce the negative effects of past unemployment on occupational prestige by 1.2 scale points. Although this finding is in line with hypothesis 3, suggesting that higher benefit generosity buffers the negative effects of unemployment, the wide 90 percent confidence interval indicates that the effect is estimated with high uncertainty. For expenditure on ALMP (hypothesis 4), neither specification suggests a moderating effect meaning that investments in ALMP do not neither weaken nor strengthen the negative effects of past unemployment on occupational prestige.

In contrast with hypothesis 5, the results for EPL for regular contracts suggest that stricter protection reduces the negative effects of unemployment. However, this effect is rather small. A one standard deviation increase in EPL for regular contracts is predicted to reduce the negative effect by about 0.31 scale points. For EPL regular no estimates are reported based on the EDV regression models including country fixed-effects (specification 2, grey squares). Due to the very little within-country variation in EPL for regular contracts over time (see Table 2), these estimates are not considered reliable.

Figure 4 Results of macro-level analyses: Moderating effects of economic situation and labor market policies on the effects of past unemployment on four indicators of non-monetary job quality (EDV regression models, 90 percent confidence intervals)



Notes: Models for unemployment rate control for labor market policies and period effects, models for labor market policies control for other labor market policies, unemployment rate, and period effects; Latvia and Romania are not used for models that include country fixed effects, because they are not observed repeatedly.
Sources: European Social Survey, round 1 to 7 (2002-2014), 26/24 countries, 124/122 country-rounds; Table 1 and Table 2 provide details about the macro data. Own calculations.

Similar results are shown for autonomy (Panel B) and authority (Panel C). For autonomy most moderating effects are small and not substantially significant (Panel B). Only for unemployment rate and ALMP expenditure the models including country fixed-effects suggest a moderating role. For unemployment rate, this result is at odds with hypothesis 2, suggesting that a one standard deviation reduces the negative effect of past unemployment on autonomy by 0.13 scale points. Moreover, a one standard deviation increase in ALMP expenditure buffers the negative effect of past unemployment on autonomy by about 0.17 scale points. However, both of these estimates are accompanied by a high uncertainty, as reflected in the wide 90 percent confidence intervals.

For authority (Panel C) only the EDV regression model including country fixed-effects reveals a moderating effect of ALMP. In line with hypothesis 3, a one standard deviation increase in ALMP expenditure is predicted to buffer the negative effect of past unemployment on authority by about 3 percentage points. Again, although small, the moderating effect of EPL for regular contracts is at odds with the expectation that higher job security provisions are associated with more negative effects of unemployment on authority.

Panel D shows the results for job security. The negative moderating effect of the unemployment rate supports the hypothesis that the negative effect of past unemployment on job security is exacerbated by poor economic conditions. Specifically, a one standard deviation increase in the unemployment rate is predicted to increase the negative effect of unemployment on job security by about 4 percentage points. The model with country fixed-effects gives a smaller, but similar estimate (2 percentage points). The results for benefit generosity are, however, not in line with the theoretical predictions as the negative effect of past unemployment on job security increases with higher benefit generosity. For ALMP expenditure the effects are small and substantially insignificant, while the results for EPL for regular contracts are in line with hypothesis 5. A one standard deviation increase in EPL for regular contracts is predicted to increase the negative effect of past unemployment on job security by about 3 percentage points.

To test the sensitivity of these results with respect to the measurement of the macro-level variables, I repeated the analyses for economic situation by using GDP growth per capita instead of unemployment rate as an alternative indicator. For benefit generosity, I repeated the analyses using PLMP per unemployed as a percentage of GDP per capita as an alternative indicator. A comparison of the results with the original and alternative indicators is given in supplementary material S6. For GDP growth per capita the conclusions are very similar to those using unemployment rates. The negative effect of economic conditions in Panel D is, however, not resembled in the sensitivity analysis. For PLMP expenditure the results are somewhat more sensitive. For example, the positive buffering effect of benefit generosity for occupational prestige or the negative moderating effect for job security cannot be reproduced using PLMP expenditure. The implications of these results will be discussed in the following section.

5. Conclusions

This article complements research on the medium-term effects of unemployment on non-monetary job quality. Specifically, it examines how past unemployment affects four different facets of non-monetary job quality and to what extent these effects are moderated by differences in economic situation and labor market policies across countries and time.

The analyses draw on data from round 1 to 7 (2002-2014) of the European Social Survey (ESS), which includes harmonized information about 125,000 workers nested in 34 countries for up to 7 rounds. Appling two-stage multi-level models, the first-stage micro-level analyses reveal that past unemployment negatively affects non-monetary job quality in the majority of the 164 country-rounds analyzed. Approaching job quality in a multidimensional way, it is found that workers who have experienced unemployment within the last five years have a lower occupational prestige, less autonomy and authority, and, in particular, face greater job insecurity compared with those who have not been unemployed. These results confirm previous findings about the scar effects of job loss and unemployment for workers' subsequent careers and job quality (e.g., Brand 2006; Dieckhoff 2011). Specifically, Dieckhoff (2011) found similar results for job authority and job security using panel data and applying a difference-in-differences estimator for four European countries. Using data for the US, Brand (2006) showed that job loss results in long-term losses of occupational status and job authority. Overall, this suggests that research that only focuses on employment and earnings likely underestimates the negative effects of unemployment on workers' subsequent careers.

Moreover, the analyses reveal that the negative effects on job quality vary substantially across countries and time. This raises the question to what extent economic situation or labor market policies shape the size of the effects of past unemployment. To address this, the ESS data have been complemented by time-varying macro data about unemployment rates, GDP growth, unemployment benefit generosity, expenditure on active and passive labor market policies, and employment protection legislation for regular contracts. The second-stage macro-level analyses show that neither economic situation nor the different labor market policies consistently moderate the negative effects of past unemployment. Similar to previous comparative research (e.g., Dieckhoff, 2011), I find that some macro-level variables have moderating effects that are consistent with expectations, but that others do not correspond to the theoretical predictions. One potential explanation is that some of the mechanisms concerning the macro-level variables neutralize each other. For example, theory predicts that higher unemployment rates increase stigma by prolonging unemployment, but also decrease stigma as unemployment is less informative about individual workers' productivity. Similarly, while unemployment benefits increase unemployed workers' bargaining power, they also extend unemployment duration, potentially fostering the depreciation of human capital or unemployment stigma. Relatedly, heterogeneity in the moderating effects may explain the findings. For example, if some ALMP buffer the negative consequences, but others result in so-called lock-in effects or increase stigma, the overall effects may be close to zero. A last explanation may be that the effects of unemployment on job quality vary across workers and that cross-country variation by labor market policies may only be revealed for specific subgroups.

Although the results indicate that economic situation and labor market policies cannot consistently explain the variation in the effects of unemployment on non-monetary job quality, the following limitations have to be considered. First, this study does not allow distinguishing different experiences of unemployment, for example, by taking into account information on the unemployment duration or the reason for unemployment. It, therefore, may underestimate the scar effects of particularly negative experiences, such as job loss coupled with long-term unemployment. Second, similar to most comparative research, the micro-level analyses are based on cross-sectional data and not all relevant confounders can be taken into account. Therefore, the estimates of the past unemployment effects may be biased. Previous research by Brand (2006), finds remarkably similar results when comparing estimates based on cross-sectional and panel data, but this study focused on job displacements. Moreover, at the macro-level the analyses include country fixed-effects allowing taking account of time-constant unobserved heterogeneity between countries. Furthermore, in line with the majority of quantitative comparative studies, we focused on the average effects of unemployment on subsequent job quality within in each country, leaving unexamined the moderating role of worker characteristics.

While the results do suggest that economic conditions or labor market policies have a limited role in moderating the negative effects of past unemployment on job quality, future research should continue investigating potential moderators, making use of both quantitative multi-level approaches as well as comparative case-studies. Specifically, effect heterogeneities at the micro-level should be taken into account as the effects of unemployment on job quality likely vary across workers and countries' economic situation and institutional set-up may have different moderating effects for different subgroups of workers within countries. With respect to the moderating role of economic situation, it also is interesting to complement cross-country comparisons by studies using variation at the regional or industry level, to examine the effects of unemployment on subsequent careers for different economic conditions employers and employees face. Given the results of this and previous studies, an additional avenue for future research is to look at a variety of objective and subjective measures of job quality as well as to examine in how far the negative effects carry on in the long-run. Specifically, it may be interesting to investigate objective and subjective measures in parallel. While this article focused on the former, a joint analysis may improve our understanding of the potential well-being consequences of poorer working conditions due to unemployment. For example, future work could examine whether objectively lower job quality also translates into a lower job satisfaction and, therefore, a lower overall well-being or whether workers are able to uphold the latter, for instance, by adapting their job values.

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7. Supplementary material (online only)

Supplementary material S1	Country-rounds available for micro- and macro-level analyses
Supplementary material S2	Missing data, imputation models, and diagnostics
Supplementary material S3	References for macro data
Supplementary material S4	Coarsened exact matching
Supplementary material S5	Results of the macro-level analyses
Supplementary material S6	Sensitivity tests for macro-level results

Supplementary material S1 Country-rounds available for micro- and macro-level analyses

For the micro-level analyses 164 country-rounds from 34 countries are available. The column "Micro data" in Table S1.1 highlights the available country-rounds in green. Four reasons of "non-availability" can be distinguished. First, the data for round 7 (2014) have not yet been released (see the text for the versions of the integrated files used) [1 country-round]. Second, the data are not part of the integrated data files [6 country-rounds]. For example, the European Social Survey has excluded the data from Italy in 2004 because they are not comparable with those of the other countries. Third, there are problems with important variables. Variables that are used for the analyses or the multiple imputation (see supplementary material S2) are either completely missing (e.g., in France in 2002 no information about the employment relation or the type of contract are available) or the variables have too many missing values (e.g., in Turkey in 2008 more than 30 percent of the data are missing for the variable "respondent understood question" of the interviewer questionnaire which is needed for the imputation) [10 country-rounds]. Fourth, there are problems with the imputation models. This refers to issues of perfect prediction in logistic regression models used for imputation that are due to very low sample sizes [2 country-rounds].

Table S1.1 also provides details about the availability of macro data. Columns (1) to (6) refer to the following macro-level variables: (1) GDP growth per capita, (2) unemployment rate, (3) unemployment benefit generosity, (4) PLMP expenditure, (5) ALMP expenditure, (6) EPL for regular contracts. See supplementary material S3 for references to the macro data and Table 1 as well as Table 2 for details about measurement and descriptive statistics. Note that the macro data are measured with a lag of three years to approximate the situation at the time of job search.

The first three columns in Table S1 highlight the 124 country-rounds that have micro and macro data and are, therefore, used for the second-stage macro-level analyses.

Table S 1.1 Available micro and macro data

Country	Code	Round, year	Micro data	Macro data (1)	(2)	(3)	(4)	(5)	(6)
Albania	AL	6, 2012							
Austria	AT	1, 2002							
Austria	AT	2, 2004							
Austria	AT	3, 2006							
Austria	AT	4, 2008	Not in integrated files						
Austria	AT	5, 2010	Not in integrated files						
Austria	AT	7, 2014							
Belgium	BE	1, 2002							
Belgium	BE	2, 2004							
Belgium	BE	3, 2006							
Belgium	BE	4, 2008							
Belgium	BE	5, 2010							
Belgium	BE	6, 2012							
Belgium	BE	7, 2014							
Bulgaria	BG	3, 2006							
Bulgaria	BG	4, 2008							
Bulgaria	BG	5, 2010							
Bulgaria	BG	6, 2012							
Croatia	HR	4, 2008							
Croatia	HR	5, 2010							
Cyprus	CY	3, 2006							
Cyprus	CY	4, 2008							
Cyprus	CY	5, 2010							
Cyprus	CY	6, 2012							
Czech Republic	CZ	1, 2002							
Czech Republic	CZ	2, 2004							
Czech Republic	CZ	4, 2008							
Czech Republic	CZ	5, 2010							
Czech Republic	CZ	6, 2012							

Table S 1.1 continued

Czech Republic	CZ	7, 2014	
Denmark	DK	1, 2002	
Denmark	DK	2, 2004	
Denmark	DK	3, 2006	
Denmark	DK	4, 2008	
Denmark	DK	5, 2010	
Denmark	DK	6, 2012	
Denmark	DK	7, 2014	
Estonia	EE	2, 2004	
Estonia	EE	3, 2006	
Estonia	EE	4, 2008	
Estonia	EE	5, 2010	
Estonia	EE	6, 2012	
Estonia	EE	7, 2014	
Finland	FI	1, 2002	Problems with important variables
Finland	FI	2, 2004	
Finland	FI	3, 2006	
Finland	FI	4, 2008	
Finland	FI	5, 2010	
Finland	FI	6, 2012	
Finland	FI	7, 2014	
France	FR	1, 2002	Problems with important variables
France	FR	2, 2004	Problems with important variables
France	FR	3, 2006	
France	FR	4, 2008	
France	FR	5, 2010	
France	FR	6, 2012	
France	FR	7, 2014	
Germany	DE	1, 2002	
Germany	DE	2, 2004	
Germany	DE	3, 2006	

Table S 1.1 continued

Germany	DE	4, 2008	
Germany	DE	5, 2010	
Germany	DE	6, 2012	
Germany	DE	7, 2014	
Greece	GR	1, 2002	
Greece	GR	2, 2004	
Greece	GR	4, 2008	
Greece	GR	5, 2010	
Hungary	HU	1, 2002	Problems with important variables
Hungary	HU	2, 2004	Problems with important variables
Hungary	HU	3, 2006	
Hungary	HU	4, 2008	
Hungary	HU	5, 2010	
Hungary	HU	6, 2012	
Hungary	HU	7, 2014	Problems with important variables
Iceland	IS	2, 2004	Problems with important variables
Iceland	IS	6, 2012	Imputation model does not work
Ireland	IE	1, 2002	
Ireland	IE	2, 2004	
Ireland	IE	3, 2006	
Ireland	IE	4, 2008	
Ireland	IE	5, 2010	
Ireland	IE	6, 2012	
Ireland	IE	7, 2014	
Israel	IL	1, 2002	
Israel	IL	4, 2008	
Israel	IL	5, 2010	
Israel	IL	6, 2012	
Israel	IL	7, 2014	
Italy	IT	1, 2002	
Italy	IT	2, 2004	Not in the integrated files

Table S 1.1 continued

Italy	IT	6, 2012	
Kosovo	XK	6, 2012	
Latvia	LV	3, 2006	Not in the integrated files
Latvia	LV	4, 2008	
Latvia	LV	7, 2014	Not yet released
Lithuania	LT	4, 2008	Not in the integrated files
Lithuania	LT	5, 2010	
Lithuania	LT	6, 2012	
Lithuania	LT	7, 2014	
Luxembourg	LU	1, 2002	
Luxembourg	LU	2, 2004	
Netherlands	NL	1, 2002	
Netherlands	NL	2, 2004	
Netherlands	NL	3, 2006	
Netherlands	NL	4, 2008	
Netherlands	NL	5, 2010	
Netherlands	NL	6, 2012	
Netherlands	NL	7, 2014	
Norway	NO	1, 2002	
Norway	NO	2, 2004	
Norway	NO	3, 2006	
Norway	NO	4, 2008	
Norway	NO	5, 2010	
Norway	NO	6, 2012	
Norway	NO	7, 2014	
Poland	PL	1, 2002	
Poland	PL	2, 2004	
Poland	PL	3, 2006	
Poland	PL	4, 2008	
Poland	PL	5, 2010	
Poland	PL	6, 2012	

Table S 1.1 continued

Poland	PL	7, 2014	
Portugal	PT	1, 2002	
Portugal	PT	2, 2004	
Portugal	PT	3, 2006	
Portugal	PT	4, 2008	
Portugal	PT	5, 2010	
Portugal	PT	6, 2012	
Portugal	PT	7, 2014	
Romania	RO	3, 2006	Not in the integrated files
Romania	RO	4, 2008	
Russian Federation	RU	3, 2006	
Russian Federation	RU	4, 2008	
Russian Federation	RU	5, 2010	
Russian Federation	RU	6, 2012	
Slovak Republic	SK	2, 2004	
Slovak Republic	SK	3, 2006	
Slovak Republic	SK	4, 2008	
Slovak Republic	SK	5, 2010	
Slovak Republic	SK	6, 2012	
Slovenia	SI	1, 2002	
Slovenia	SI	2, 2004	
Slovenia	SI	3, 2006	
Slovenia	SI	4, 2008	
Slovenia	SI	5, 2010	
Slovenia	SI	6, 2012	
Slovenia	SI	7, 2014	
Spain	ES	1, 2002	
Spain	ES	2, 2004	
Spain	ES	3, 2006	
Spain	ES	4, 2008	
Spain	ES	5, 2010	

Table S 1.1 continued

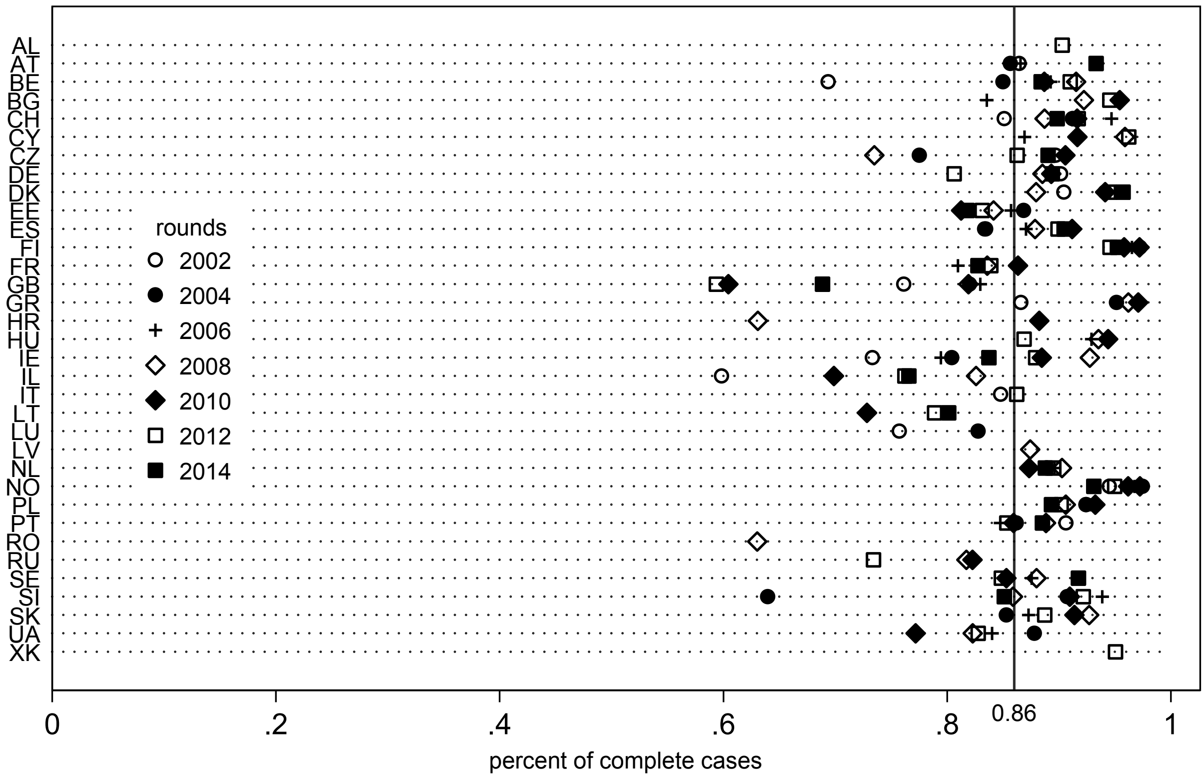
Spain	ES	6, 2012	
Spain	ES	7, 2014	
Sweden	SE	1, 2002	Problems with important variables
Sweden	SE	2, 2004	Problems with important variables
Sweden	SE	3, 2006	
Sweden	SE	4, 2008	
Sweden	SE	5, 2010	
Sweden	SE	6, 2012	
Sweden	SE	7, 2014	
Switzerland	CH	1, 2002	
Switzerland	CH	2, 2004	
Switzerland	CH	3, 2006	
Switzerland	CH	4, 2008	
Switzerland	CH	5, 2010	
Switzerland	CH	6, 2012	
Switzerland	CH	7, 2014	
Turkey	TR	2, 2004	Imputation model does not work
Turkey	TR	4, 2008	Problems with important variables
Ukraine	UA	2, 2004	
Ukraine	UA	3, 2006	
Ukraine	UA	4, 2008	
Ukraine	UA	5, 2010	
Ukraine	UA	6, 2012	
United Kingdom	GB	1, 2002	
United Kingdom	GB	2, 2004	
United Kingdom	GB	3, 2006	
United Kingdom	GB	4, 2008	
United Kingdom	GB	5, 2010	
United Kingdom	GB	6, 2012	
United Kingdom	GB	7, 2014	

Notes: See the explanations above for details about the color-coding as well as information about the data.
Sources: Own illustration.

Supplementary material S2 Missing data, imputation models, and diagnostics

Figure S2.1 shows the percentage of complete cases in the micro data for all of the 164 available country-rounds. On average, across all 164 country-rounds, 14 percent of workers have missing values on one or more variable. However, Figure S2.1 also shows that the share of complete cases varies substantially across country-rounds.

Figure S2.1 Percent of complete cases by country-round



Notes: See supplementary material 1 for the country codes.
Sources: European Social Survey, round 1 to 7 (2002-2014), 164 country-rounds. Own calculations.

The standard "missing data technique" is listwise deletion or complete case analysis. In order for complete case analysis to be unbiased, the data have to be missing completely at random (MCAR) or some special cases for regression models have to apply (Allison 2001: 6-7). Unsurprisingly, the MCAR assumption is not satisfied. This was tested by running a logistic regression of the missing indicator of a variable on the other variables. For almost all of the variables used in the micro-level analyses, the logistic regression models suggest that missingness is predicted by the other variables (results not shown).

What about the special cases? In the first special case, complete case analysis is unbiased if missingness in the covariates is independent of the outcome given the covariates. The second special case only holds for logistic regression models and assumes that the probability of missing data on any variable depends on the outcome and not on any of the independent variables. I assume that these special cases are unlikely to hold in the micro-level analyses.

Instead I use multiple imputation (MI). MI relies on the much weaker missing at random assumption (MAR) stating that the probability of missing data on a variable may depend on the other observed variables, but does not depend on the values of this variable itself, conditional on the other observed variables. Under this assumption MI is unbiased. Because MI makes use of the information available in incomplete cases, it is usually more efficient than complete case analysis.

I use the mi package in Stata 13.1 to perform multiple imputation by chained equations (MICE) and analyze the micro data. To avoid bias, the imputation model includes all variables used in the micro-level analyses (see Table 1) (White et al. 2011: 384). In addition, the model includes so-called auxiliary variables. White et al. (2011: 385) recommend including variables that predict the incomplete variables and variables that predict the missingness of the incomplete variables to make the MAR assumption more plausible and reduce bias. The following variables have been included as auxiliary variables:

Participation in training within the last 12 months (1=Yes, 0=No), father working at age 14 (1=Yes, 0=Otherwise), subjective health (1=Fair/bad/very bad, 0=Very good/good), life satisfaction (0 "Extremely dissatisfied"-10 "Extremely satisfied"), contracted working hours, firm size (4 categories), sector (3 categories), lives in big city or its' suburbs/outskirts (1=Yes, 0=No), lives with partner (1=Yes, 0=No), children at home (1=Yes, 0=No), very difficult/difficult to cope with household income (1=Yes, 0=No), an index of interpersonal trust (0-10), and four items from the interviewer questionnaire. For the latter the interviewers were asked whether the respondent asked for clarification of any questions, whether the respondent was reluctant to answer any questions, whether the respondent tried to answer the questions to the best of his or her ability, and whether the respondent understood the questions (1=never, 5=Very often). I also included the design weights variable in the imputation model as suggested by Stata (StataCorp 2013: 8).

Binary variables were imputed by logistic regression and categorical variables by multinomial logistic regression. I used Stata's augment option to avoid problems arising from perfect prediction in regression models for categorical data (White et al. 2011: 394). Ordinal and continuous variables were imputed by predictive mean matching (PMM) drawing from the 10 nearest neighbors. PMM reduces the impact of model misspecification and allows for using "the improved passive approach" to handle nonlinearities (White et al. 2011: 386). Because the analyses address the question of to what extent the effect of past unemployment on job quality varies across countries and time, these interactions also have to be considered in the imputation model. Therefore, imputation is performed separately for each country-round. The number of iterations for the burn-in period was set to 20. However, I initially used a burn-in period of 100 for one country (Czech Republic) to assess the convergence of MICE. Specifically, for each variable I plotted the mean and standard deviation of the imputed values against the iterations to see whether the imputed values follow any definite trend. These plots (not shown) do not reveal any conspicuous patterns suggesting the convergence of MICE.

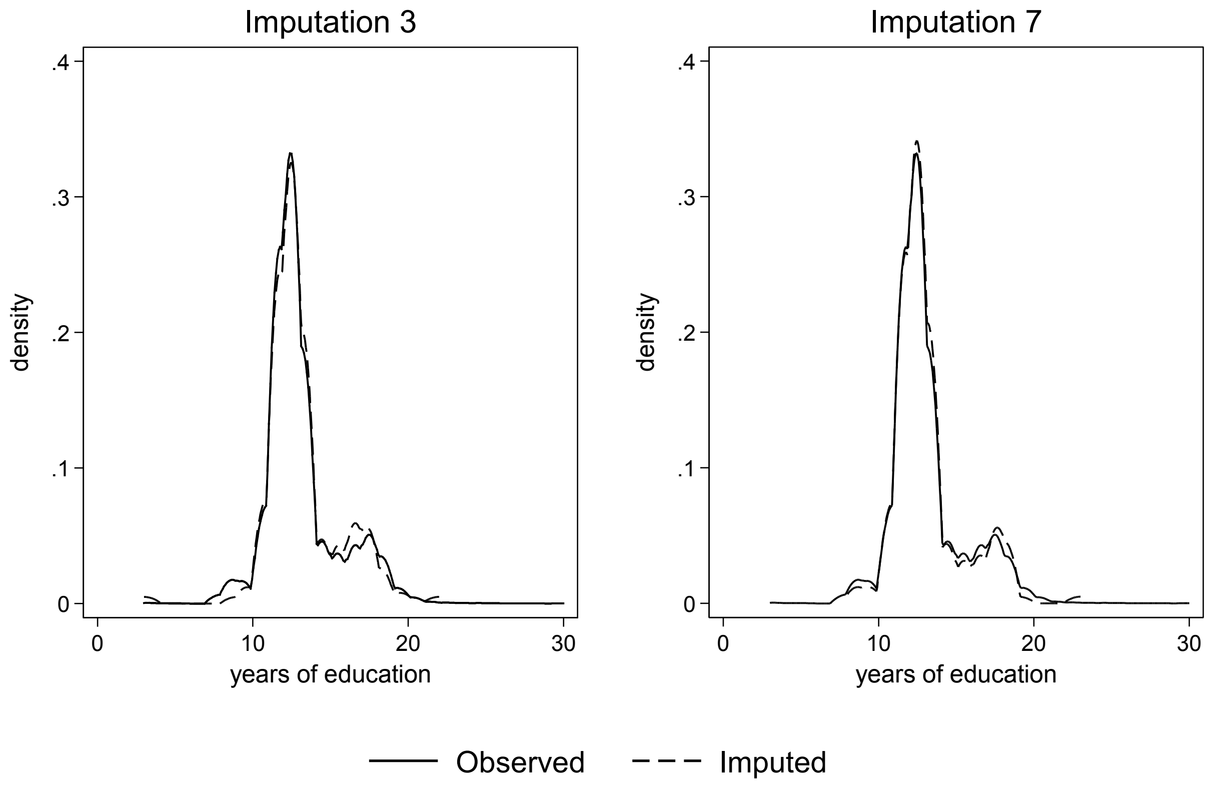
The ten multiply imputed data sets are analyzed with Stata's mi commands that combine the estimates and standard errors by applying Rubin's rules (Rubin 1987). For coarsened exact matching (CEM), I use the multiple imputations options offered within the respective Stata ado (Blackwell et al. 2009: 539-540). To combine the estimated average marginal effects after logistic regression, I apply Rubin's rules using the mimrgns Stata ado provided by Daniel Klein.

To check the fit of the imputation model, I used some diagnostics suggested by Eddings and Marchenko (2012). The basic idea is to compare the distributions of the imputed and observed values. Very large differences or implausible imputed values point to problems with the imputation model.

In general, differences between the distributions of imputed and observed values were neither very large nor systematic across imputations suggesting that the imputation model is reasonable. Figure S2.2 gives an example. It shows the imputed and observed values for the variable years of education for the third and seventh imputations across all country-round of Czech Republic. Besides some small differences in the tails of the distributions, the imputed values closely resemble the observed values.

To check whether the estimates of the effect of unemployment on non-monetary job quality depend on using multiple imputation, I repeated the analyses using complete case analyses. The correlation between the estimates based on multiple imputation and the estimates based on a complete case analyses are very strong ranging between 0.92 and 0.97 depending on the measure of non-monetary job quality examined.

Figure S2.2 Example for diagnostics concerning the imputation model



Notes: Third and seventh imputations for the variable years of education across all country-rounds of Czech Republic.
Sources: European Social Survey, round 1 to 7 (2002-2014), 164 country-rounds. Own calculations.

Supplementary material S3 References for macro data

The following macro-level variables have been used: GDP growth per capita, unemployment rate, unemployment benefit generosity, PLMP expenditure, ALMP expenditure, EPL for regular contracts.

GDP growth per capita

The data have been assembled from the Word Development Indicators (WDI) of World Bank (WB). The respective series is "GDP per capita growth (annual %)."

Source: http://databank.worldbank.org/data/reports.aspx?source=world-development-indicators

Unemployment rate

The data have been assembled from the Key Indicators of Labor Market (KILM) of International Labour Organization (ILO). I used "Table 9a. Total unemployment" from the 9th edition of the KILM interactive software.

Source: http://kilm.ilo.org/2015/install/

Unemployment benefit generosity

The data have been assembled from the Comparative Welfare Entitlement Dataset 2 (CWED2) by Scruggs et al. (2014). The CWED2 includes information about the replacement rates and benefit duration that has been used to construct the unemployment benefit generosity index. It also contains information on coverage.

Source: http://cwed2.org/

PLMP and ALMP expenditure

The data have been assembled from OECD and Eurostat. In only a few of the years missing OECD data have been complemented by Eurostat data. Expenditure in national currencies was divided by the number of unemployed taken from "Table 9a. Total unemployment" of KILM (see above) and, then, expressed as a percentage of GDP per capita. The latter has been assembled from the WDI of WB (see above). The series used is named "GDP per capita (current LCU)." Expenditure on PLMP includes categories 8 and 9 and expenditure on ALMP includes categories 2 to 7.

Sources:

http://stats.oecd.org/Index.aspx?DatasetCode=LMPEXP#,http://ec.europa.eu/eurostat/en/web/labour-market/labour-market-policy/database,
http://kilm.ilo.org/2015/install/,
http://databank.worldbank.org/data/reports.aspx?source=world-development-indicators

EPL for regular contracts

The data have been assembled from OECD and Avdagic (2015). The OECD data refers to version 1 of the EPL for regular contracts. In few years missing OECD data have been complemented by information from Avdagic (2015) who scored CEE countries following the OECD approach. The data from Avdagic have been received upon request.

Sources: http://www.oecd.org/employment/emp/EPL-timeseries.xlsx, Avdagic (2015)

Supplementary material S4 Coarsened exact matching

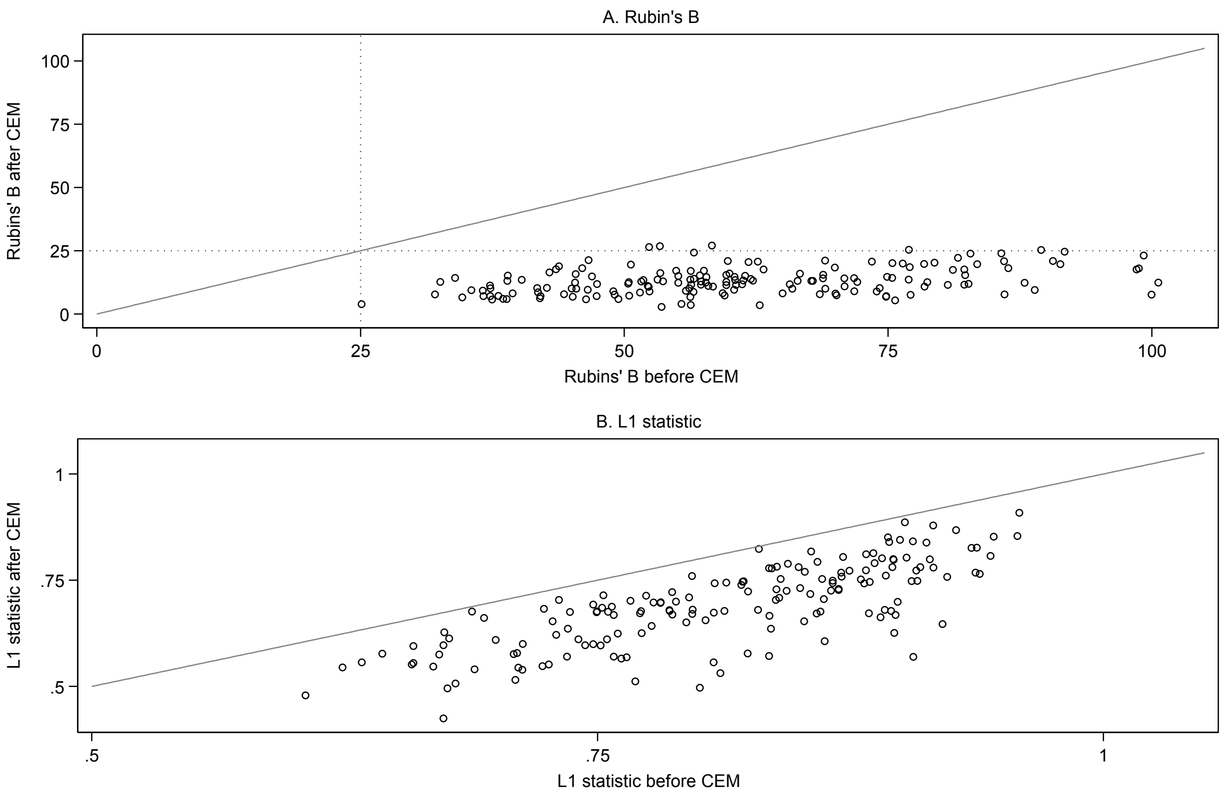
For the micro-level analyses, the data are pre-processed by using coarsened exact matching (CEM) (Blackwell et al. 2009; Iacus et al. 2012). That is, before estimating the effects of past unemployment on the different facets of job quality by using linear or logistic regression models, CEM is performed separately in each country-round.

The goal of CEM is to increase the balance on pretreatment control variables between a treatment and control group. Specifically, workers who have experienced unemployment in the last five years and those who have not are matched on the following variables: age, sex, years of education, father's education, mother's education, and mother's employment status at age 14 (see Table 1 for details). With the exception of age and years of education these are binary variables. Because exact matching on all pretreatment control variables is not possible, I use coarsened exact matching. As the name suggests, the pretreatment control variables are coarsened before exact matches are formed. The age and years of education variables are coarsened into five, respectively eight, equally sized groups. The binary variables are used in the matching as they are. Based on the coarsened variables, exact matches are formed. Because for some workers who have experienced past unemployment, the data include no workers without past unemployment and the same values on the coarsened variables, CEM results in the loss of some treatment observations that are off support. Although excluding these observations results in a redefinition of the parameter of interest (Blackwell et al. 2009: 527), restricting the analyses to the empirical common support is important to avoid model dependent extrapolations. Across the 164 country-rounds used in the micro-level analyses, the matching resulted, on average, in the loss of only about 8 percent of treatment observations.

To judge whether the CEM successfully increased balance, it is important to compare the balance before and after the matching. I use two overall measures of balance. The first measure, Rubin's B, assesses balance with respect to the absolute standardized difference of the means of the linear index of the propensity score in the treated and (matched) control group (Rubin 2001). For the groups to be considered sufficiently balanced Rubin's B is often recommended to be less than 25. However, the absolute values are less meaningful than the comparison of balance before and after matching. The second measure of balance is the so-called L1 statistic (see Blackwell et al. 2009: 530-31 for details). In contrast to Rubin's B and other conventional measures of balance, the L1 statistic assesses the global balance with respect to the full joint distribution of pretreatment control variables, including all interactions. Perfect global balance is indicated by L1 = 0 and perfect global imbalance by L1 = 1. Similar to Rubin's B, it is, however, more important to examine whether the matching substantially reduces the imbalance.

Figure S4.1 illustrates that the balance has increased from before to after matching. It shows Rubin's B (Panel A) and the L1 statistic (Panel B) before and after CEM for each of the 164 country-rounds. Panel A reveals that before the CEM all country-rounds had a balance greater than 25 and that after the matching the balance has substantially improved, with Rubin's B falling below the recommended threshold for most country-rounds. Similarly, the L1 statistic (Panel B) has decreased for all country-rounds, showing that the CEM has reduced imbalance on the pretreatment covariates. Following the idea of matching as nonparametric "preprocessing", the remaining differences are taken into account by estimating linear and logistic regression models separately for each country-round using the matched data.

Figure S4.1 Balance before and after matching assessed by the Rubin's B and the L1 statistic



Notes: See the explanations above for a definition of the measures of balance.
Sources: European Social Survey, round 1 to 7 (2002-2014), 164 country-rounds. Own calculations.

Supplementary material S5 Results of the macro-level analyses

Table S5.1 Results of the macro-level analyses: moderating effects of economic situation and labor market policies on the effect of past unemployment on four indicators of non-monetary job quality (EDV regression models)

	Effect of past unemployment on …															
	Occupational prestige				Autonomy				Authority				Job security			
	Model 1		Model 2		Model 1		Model 2		Model 1		Model 2		Model 1		Model 2	
	b	S.E.	b	S.E.	b	S.E.	b	S.E.	b	S.E.	b	S.E.	B	S.E.	b	S.E.
Unemployment rate	0.26	0.14	0.24	0.31	0.05	0.04	0.13	0.08	0.01	0.01	0.02	0.01	-0.04	0.01	-0.02	0.01
Benefit generosity	0.10	0.19	1.20	0.79	0.04	0.04	-0.09	0.23	-0.01	0.01	0.00	0.03	-0.02	0.01	-0.04	0.02
ALMP expenditure	-0.03	0.24	0.01	0.39	0.06	0.04	0.17	0.13	-0.01	0.01	0.03	0.02	-0.01	0.03	-0.00	0.02
EPL regular	0.31	0.12			0.01	0.05			0.02	0.01			-0.03	0.01		
Period fixed effects	✔		✔		✔		✔		✔		✔		✔		✔	
Country fixed effects			✔				✔				✔				✔	
N (country-rounds)	124		122		124		122		124		122		124		122	
N (countries)	26		24		26		24		26		24		26		24	

Notes: b = standardized regression coefficients, S.E. = standard errors, S.E. have been clustered by country.
Sources: European Social Survey, round 1 to 7 (2002-2014), 26/24 countries, 124/122 country-rounds; Table 1 and Table 2 provide details about the macro data. Own calculations.

Supplementary material S6 Sensitivity test of macro-level results

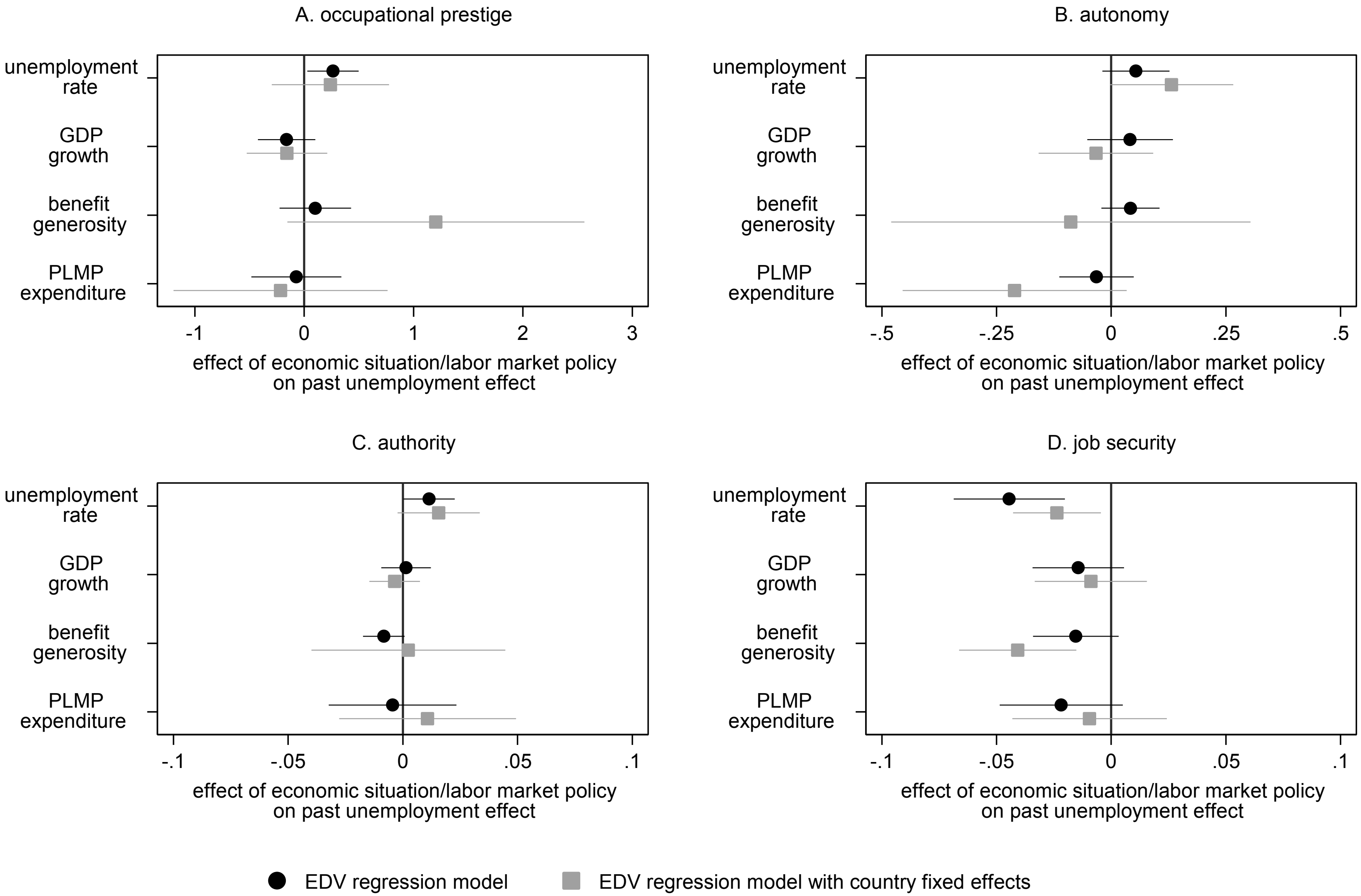
Figure S6.1 shows that the results of the second-stage macro-level analyses are somewhat sensitive to the indicators of economic situation and benefit generosity used. For each of the non-monetary job quality indicators (Panels A to D), it compares the original analyses for the moderating role of unemployment rate and unemployment benefit generosity with sensitivity analyses using alternative indicators. The alternative indicators are the GDP growth per capita and the expenditure on PLMP per unemployed as a percentage of GDP per capita.

For GDP growth per capita the results for occupational prestige, autonomy, and authority (Panels A-C) are very similar to the original analyses in that they do not change the conclusions drawn. While we find small positive moderating effects for unemployment rate, we mostly find small negative or zero effects for GDP growth per capita. For both indicators the conclusion is that macro-economic conditions to not affect the size of the negative effect of past unemployment on subsequent job quality. For job security (Panel D) the results are somewhat different. While for unemployment rate, the results are in line with the expectation that poor macro-economic conditions result in larger negative effects of past unemployment on job security, the small negative effects for GDP growth suggest no substantially significant effects of macro-economic conditions.

For benefit generosity, the results are somewhat more sensitive to measurement. For occupational prestige (Panel A), the conclusion of a buffering effect of benefit generosity is not supported by the results for PLMP expenditure and for autonomy (Panel B) the estimates for PLMP expenditure (specification 2) even suggest a negative effect of greater decommodification. For job security (Panel D), the negative effect of benefit generosity (specification 2) is not resembled by the results for PLMP expenditure.

Overall, these sensitivity analyses add to the arguments that the few results that suggest substantial moderating effects of macro-economic conditions or labor market policies should not be overinterpreted.

Figure S6.1 Sensitivity tests for the macro-level results with respect to measurement



Notes: See Figure 4.
Sources: European Social Survey, round 1 to 7 (2002-2014), 26/24 countries, 124/122 country-rounds; Table 1 and Table 2 provide details about the macro-data. Own calculations.

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Article 3

Better overeducated than unemployed?
The short- and long-term effects of an overeducated labour market re-entry

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Abstract

Previous studies have shown that overeducation is inferior to adequate employment. For example, overeducated workers have lower earnings, participate less often in continuing education and training, and are less satisfied with their jobs. This article changes perspectives by asking whether it is better for the unemployed to take up a job for which they are overeducated or to remain unemployed and continue the search for adequate employment. Theoretically, we rely on the established confrontation of the stepping-stone and trap-hypotheses which make opposing predictions in terms of long-term employment chances and job quality. Using the German Socio-Economic Panel (1984-2012) and applying a dynamic propensity score matching approach, the analyses reveal an interesting trade-off. Although an overeducated re-entry increases the long-term employment chances persistently, it also implies strong lock-in effects into overeducation for up to five years after re-employment. In sum, the results support the stepping-stone hypothesis in terms of future employment chances, but also highlight non-negligible risks of remaining trapped in a job that is below one's level of educational qualification.

1. Introduction

Numerous studies have shown that overeducation compared with adequate employment is associated with social and economic disadvantages (see McGuinness, 2006 for a review). Being employed in a job below ones level of education translates into lower earnings (Korpi and Tåhlin, 2009), lower chances of participating in continuing education and training (Büchel and Mertens, 2004), and lower job satisfaction (Verhaest and Omey, 2009). Therefore, overeducation is commonly considered inferior to adequate employment. However, comparing overeducation only to adequate employment neglects that it represents an important alternative to unemployment as well. Given the empirical evidence on unemployment scarring in terms of earnings (Gangl, 2006) and job quality (Dieckhoff, 2011), the question arises how overeducation compares to unemployment. We change perspectives and complement previous studies by asking whether it is better for the unemployed to take up a job for which they are overeducated or to remain unemployed and continue the search for adequate employment.

Theoretically, we rely on the established yet undecided confrontation of the stepping-stone and trap hypotheses (Baert *et al.*, 2013; Pollmann-Schult and Büchel, 2004a; Scherer, 2004). According to the former an overeducated re-entry increases the chances of both employment per se and adequate employment in the long run. In contrast, the trap hypothesis states that it is better to decline offers for overeducated jobs and continue the search in order to avoid disadvantages in terms of labour market integration and job quality. Empirical evidence from the few studies with a similar perspective supports the trap hypothesis, but is restricted to graduates (Baert *et al.*, 2013; Scherer, 2004) or unemployed with vocational training (Pollmann-Schult and Büchel, 2004a). We complement these studies by testing the opposing theoretical scenarios for a broader sample of unemployed. However, addressing this research question is not only interesting from a theoretical perspective, but also from a policy point of view. It informs policy-makers on strategies in fighting unemployment without losing sight of the trade-off between employment and job quality. Should policy-makers enforce acceptance of job offers irrespective of the matching between actual and required qualifications or should they weigh shortened unemployment durations against the potentially negative long-term effects of an overeducated re-entry, in particular, in terms of job quality?

Against this background, our study contributes to the overeducation literature in several ways. Theoretically, we take the widely neglected perspective of comparing an overeducated re-entry to remaining unemployed and continuing the job search. We complement the few previous studies by offering first evidence on the German labour market for all levels of education and labour market experience. Methodologically, we apply a dynamic extension of propensity score matching (Sianesi, 2004) to address the central problem of selection into overeducation. This also allows us to adequately address the problem of choosing an appropriate control group. Furthermore, we analyse the effects of an overeducated re-entry considering both employment chances and the chances of adequate employment. Thereby, we shed light on potential trade-offs or cumulative (dis-)advantages between employment chances and job quality. Lastly, using the German Socio-Economic Panel, we are able to follow the unemployed for up to five years after re-employment allowing for a comprehensive assessment of the short- and long-term effects of an overeducated re-entry. As the predictions of the stepping-stone and trap hypotheses only differ in the long run, a long-term perspective appears to be particularly important.

The remainder of this article proceeds as follows: the next section addresses the question why an overeducated re-entry should be a stepping stone or a trap for the unemployed compared with continuing the search for adequate employment. The following sections present the data, measures, and analytic strategy, followed by a discussion of the results. The last section summarises the findings and offers some concluding remarks.

2. Theory and hypotheses

Following the overeducation literature, we draw on sociological and economic micro-level theories and derive the opposing stepping-stone and trap hypotheses offering a starting point for analysing the effects of an overeducated re-entry. Unlike previous studies, which usually compare overeducation to adequate employment, the following argumentation focuses on the comparison of an overeducated re-entry to the alternative of remaining unemployed and continuing the job search.

2.1 Why should an overeducated re-entry be a stepping stone for the unemployed?

While overeducation is usually considered detrimental to individuals' careers, changing the reference from adequate employment to unemployment may alter this view. Various theories indicate that taking up an overeducated job out of unemployment may represent a bridge into employment and better jobs. For example, following signalling and statistical discrimination theories, employers may use an individual's work history and job search decisions to overcome the problem of insufficient information on a potential employee's productivity (McCormick, 1990; Korpi and Levin, 2001). Taking up overeducated employment instead of remaining unemployed will be valued positively by prospective employers, since job searchers signal their general motivation, employability, and productivity. In addition, this allows avoiding the stigma effects associated with prolonged unemployment. Recent experimental evidence supports this perspective by suggesting a larger stigma effect of unemployment than overeducation (Baert and Verhaest, 2014).

Social network and job-shopping theory offer another argument in favour of the stepping-stone hypothesis highlighting the comparative advantage of an on-the-job search (Granovetter, 1973; Johnson, 1978). These theories emphasise that work experience increases job search effectiveness by giving access to information on better (matching) vacancies. More specifically, social network theory argues that an on-the-job search allows building a social network in the respective company or industry which may be helpful in finding adequate employment. In line with the signalling arguments outlined above, the higher effectiveness of an on-the-job search may also originate from the fact that employers take the current employment status as an important productivity signal for hiring decisions (Pollmann-Schult and Büchel, 2004a). Taken together the access to more and better matching job vacancies should reduce mismatches and subsequent unemployment.

From the perspective of human capital theory, re-entering the labour market in a job for which one is overeducated will also reduce the depreciation of human capital, occupation-specific skills and general employment skills compared with remaining unemployed (Korpi and Levin, 2001). In addition, taking up an overeducated job may not only counteract the depreciation of human capital, but also allow for further investments in human capital, like general employment skills, continuing education and training or work experience. Despite of empirical evidence showing that overeducation is inferior compared with adequate employment in terms of continuing education and training (Büchel and Mertens, 2004), it may provide more and better opportunities than unemployment. Moreover, career mobility theory suggests the enhancement of both internal and external promotion probabilities by rationally choosing overeducation as an investment into work experience (Sicherman and Galor, 1990). Therefore, re-entering the labour market in an overeducated job compared with remaining unemployed and continuing the job search may not only reduce the depreciation of human capital, but also help to maintain and develop it further. In sum, the stepping-stone hypothesis links an overeducated re-entry with higher employment chances and higher job quality in the long run.

Hypothesis 1a: Taking up an overeducated job compared with remaining unemployed will increase future employment chances and chances of adequate employment.

2.2 Why should an overeducated re-entry be a trap for the unemployed?

In contrast to the argumentation above, re-entering the labour market via overeducated employment may just as well turn out to be a long-term trap, for example, by retarding transitions to adequate employment (Baert *et al.*, 2013). In the following we will sketch relevant mechanisms challenging the positive implications of the stepping-stone hypothesis.

Taking up an overeducated job may as well be perceived as a bad signal by employers and thus negatively affect the advancement to better jobs as well as the long-term employment prospects. Referring to unemployed skilled workers, McCormick (1990: 311) argues that 'low investment in search for another skilled job is used by firms as a productivity signal.' Forgiving the chance of adequate employment by re-entering the labour market into overeducation may accordingly cause serious doubts about an unemployed skilled worker's productivity and professional aspirations.

Coupled with the uncertainty about the direction of the signalling effects, some researchers also challenge the presumed comparative advantage of searching on-the-job. For example, Baert *et al.* (2013) argue that the job search intensity while searching on-the-job will hardly be comparable to an off-the-job search. Given that search intensity and success are positively related, the overeducated re-entry may be disadvantageous compared with remaining unemployed and searching off-the-job by retarding the search for adequate employment.

As for the human capital development, it was argued above that taking up an overeducated job may prevent the depreciation of human capital that is associated with unemployment. However, the overeducated re-entry could also be disadvantageous if the acquisition of job-specific human capital constitutes a long-term mobility barrier. Investing in specific skills by on-the-job training may lock employees into their suboptimal employment positions (Pissarides, 1994) and retard the advancement to adequate employment, whereas remaining unemployed always entails the possibility of directly finding an adequate job. In addition, according to the 'use-it-or-lose-it' principle overeducated employees will soon adapt to their lower job requirements and thereby lose cognitive abilities (De Grip *et al.,* 2008), whereas waiting for an adequate job offer still entails the chance of using one's skills adequately shortly after. In sum, these arguments imply that overeducated re-employment represents a trap involving lower employment chances and lower chances of advancement to adequate employment.

Hypothesis 1b: Re-entering the labour market in an overeducated job compared with remaining unemployed will decrease future employment chances and chances of adequate employment.

2.3 Effect heterogeneity

Up to now our confrontation of the stepping-stone and trap hypotheses has been relatively general although previous findings suggest that the effects of an overeducated re-entry compared with remaining unemployed differ across the unemployed (Baert *et al.,* 2013). In particular, we expect the effects to vary by labour market experience and educational qualifications.

First, we expect different effects for workers in their early career and established workers based on the assumption that information problems will be more relevant for the former (Korpi and Levin, 2001). Since early-career workers lack important productivity indicators like past employment history or employer references, their possibilities in signalling high productivity are very limited compared with established workers. Acquiring work experience therefore is rational for them (Sicherman and Galor, 1990), whereas workers with a long employment history and references from former positions can also make use of other productivity indicators. Additionally, overeducation in the early employment career may have a very different meaning than in later career phases. Considering the early career as an ongoing matching process, episodes of overeducation should not result in a stigma effect for workers at the beginning of their career. Accordingly, the unemployed in their early career are expected to benefit more from an overeducated re-entry into the labour market than those who have already acquired a lot of labour market experience.

Hypothesis 2: Unemployed in their early career should benefit more from an overeducated re-entry into the labour market than unemployed in their mid and late career.

Second, we expect different effects for individuals with vocational and academic qualifications assuming that an overeducated re-entry represents a worse signal for the former than for the latter. Two arguments support this view. First, for unemployed with vocational degrees overeducation is by definition associated with completely unskilled positions. In contrast, unemployed with academic qualifications still often hold skilled positions. The greater descent experienced by those with vocational qualifications likely conveys a worse signal to future employers than the one send by unemployed with academic qualifications. Second, an overeducated re-entry of those with vocational degrees may also send adverse signals, because they accept an unskilled position despite having an occupation-specific training. This may, however, be more important for those unemployed with less other signals, like for example, work experience. In sum, we expect an overeducated re-entry to be a worse signal for unemployed with vocational degrees than for unemployed with academic qualifications meaning that an overeducated re-entry is associated with more positive effects for the latter than for the former.

Hypothesis 3: Unemployed with academic qualifications should benefit more from an overeducated re-entry into the labour market than unemployed with vocational degrees.

3. Data and methods

3.1 Data and sample selection

We draw on data from the German Socio-Economic Panel (SOEP) for the period 1984 to 2012. The SOEP is designed to be nationally representative of German households and offers panel data on the individual and household level. It provides information on persons' education and labour market behaviour as well as retrospective data on their monthly employment status (Wagner *et al.*, 2007). Based on the yearly and monthly data we create an inflow sample into unemployment following the unemployed for up to five years after re-employment. Left-censored spells have been excluded.[1] The sample is restricted to persons of age 18-54 years who experience at least one month of unemployment between 1984 and 2011. Because respondents may report more than one employment status in each month, we use a state space to define their main employment status. Additionally, persons without a vocational or university degree can by definition not be overeducated and are excluded from the analyses. To further reduce the heterogeneity among the unemployed, we only focus on those who have lost their job. Applying these restrictions we observe 4,538 person-spells from 3,353 persons including 1,067 exits into overeducation.

3.2 Measures

3.2.1 Overeducation

The treatment of interest is exiting unemployment into a job for which one is overeducated. Overeducation is the extent to which a worker's level of education exceeds the level that is

[1] Combining the monthly and yearly data results in spells that end in employment (monthly information), but for which no yearly data is available. If a person has multiple spells in between two interviews, we only kept the spell closest to the next interview resulting in a loss of about 4 per cent of spells. For 20 per cent of spells that end in employment, information is missing, because persons are not working at the time of the interview.

typically required for their particular job (McGuinness, 2006). To measure overeducation we rely on the so-called subjective approach. The SOEP contains yearly information about a worker's highest educational qualification and the qualifications typically needed for their current job. Comparing a worker's educational qualification with the reported level of required education generates a vertical mismatch variable distinguishing between adequate employment and overeducation. Compared with other measurements the subjective approach has the advantage of 'obtaining information from the source closest to the actual job situation, taking account of all specific circumstances' (Hartog and Oosterbeek, 1988: 186).[2] However, it also has been criticised for its subjectivity, for example, allowing persons to under- or overstate the level of education typically required. For this reason, we adopt an extension of the subjective approach proposed by Büchel and Weißhuhn (1998). This approach validates workers' subjective assessments by reference to information about their occupational status. A detailed description of the measurement model is given in Supplement A. The validation of the standard subjective measurement results in two additional categories that have to be excluded from the analyses: implausible combinations (about 2 per cent) and degree of mismatch not clearly determinable (about 8 per cent). However, the major advantage of the extended subjective approach is its higher validity by drawing a sharper line between adequate employment and overeducation. Some re-entries may be misclassified because the measurement is based on the yearly data and the mismatch status may have changed between the month of exiting unemployment and the next yearly interview. However, the number of misclassifications is likely to be low.[3]

3.2.2 Employment chances and job quality

To provide a balanced picture of the effects of an overeducated re-entry, we consider the following two outcomes. First, we measure the probability of being employed, irrespective of educational (mis-)match (1=employed, 0=not employed). This outcome is measured every 6 months up to 60 months after exiting unemployment. Although it indicates a worker's short- and long-term employment chances, it does not consider subsequent job quality. An objective measure of job quality is the chance of adequate employment (1=adequately employed,

[2] Another measurement used in the literature is the realised matches approach (e.g. Verdugo and Verdugo, 1989) defining the level of required education as a one standard-deviation range around the mean level of education within an occupation (here defined by two-digit ISCO). Using a realised-matches approach or the standard subjective approach does not change the main conclusions reported below.

[3] Misclassifications are unlikely due to state dependence in educational (mis-)match. In addition, the median time between the unemployment exit and the next yearly interview is only five months. A sensitivity analysis which reduces this median time to three months gives very similar results.

0=overeducated). This outcome is measured annually one to five years after an overeducated re-entry. Analysing both employment chances and the chances of adequate employment is important to highlight potential trade-offs or cumulative (dis-)advantages. For example, an overeducated re-entry may increase the unemployed's labour market integration in the long run without being a stepping stone into adequate employment.

3.3 Methods

To assess the effects of an overeducated re-entry compared with remaining unemployed and continuing the job search, we apply a dynamic propensity score matching approach (Sianesi, 2004). It extends propensity score matching to a dynamic setting taking account of the time-varying treatment. Instead of defining the controls as those who never take up an overeducated job, it defines them as those unemployed who do not experience the treatment until a certain point in time. Simply using the first definition may bias the results, because it conditions on future outcomes. For example, if the reason for never taking up an overeducated job is that an unemployed has found adequate employment before, the estimates will be biased towards negative treatment effects (Caliendo, 2006).

For this reason, the treatment is defined by taking up an overeducated job after an elapsed unemployment duration u ($D^{(u)} = 1$, treated) compared with remaining unemployed and continuing the search for at least one month ($D^{(u)} = 0$, controls).[4] The observed outcomes of interest – future employment chances and chances of adequate employment – are defined over time t and given by $Y_t^{(u)}$. Correspondingly, $Y_t^{1(u)}$and $Y_t^{0(u)}$ define an unemployed's potential outcomes if taking up an overeducated job at u and if remaining unemployed at least up to u, respectively.

For each elapsed unemployment duration u the treatment effect of interest is defined as:

$$\Delta_t^u = E\left(Y_t^{1(u)} - Y_t^{0(u)} | D^{(u)} = 1\right) = E\left(Y_t^{1(u)} | D^{(u)} = 1\right) - E\left(Y_t^{0(u)} | D^{(u)} = 1\right)$$

for $t = u, u + 1, \dots, T$

[4] In other words, the controls are composed of those still unemployed at u, irrespective of what happens later. For example, they may later take up an overeducated job and become treated themselves. Specifically, a person may serve as both treated and control. For example, a person who enters overeducation after six months may be a control to a person who enters overeducation after three months. Because some persons contribute with spells from different periods, they may be treated or become a control after having been treated before. Consequently, the control observations exceed the treatment observations in each month u by far. Multiple spells also result in 'within-person matches', that is, the treatment and matched control observation originate from different spells of the same person. This applies to about 3 per cent of the treatment observations. Excluding these treatment observations has no substantial effect on the results.

In the following, we will focus on the average of the Δ_t^u to highlight the general trends and patterns in the treatment effects (Sianesi, 2004).[5] As the second term $E(Y_t^{0(u)} \mid D^{(u)} = 1)$ is unidentified, estimating the effects involves comparing persons who enter overeducation with similar persons who have reached the same elapsed unemployment duration but remain unemployed and continue the job search. Similarity is defined in terms of the propensity score, that is, we only compare individuals who have a similar probability of taking up an overeducated job at time u, given their observed characteristics X. The propensity score is estimated by a discrete-time hazard independent competing risk model taking into account the exit dynamics from unemployment and right-censoring.[6] Identification rests on the conditional independence assumption (CIA)

$$Y_t^{0(u)} \perp D^{(u)} | \, X = x \text{ for } t = u, u+1, \dots, T,$$

stating that conditional on observed characteristics X and the elapsed unemployment duration u, in the absence of the treatment the treated would experience the same outcome as the controls. In other words, a causal interpretation rests on the strong assumption that we observe all variables that influence both taking up an overeducated job and the respective potential outcomes. The plausibility of this assumption must be discussed in the light of the large number of covariates we control for, including socio-demographics, educational attainment, work biography, characteristics of the last job, and many more. The covariates have been selected theoretically and are measured before the treatment to avoid post-treatment bias. Table 1 reports the definition and measurement of each covariate. For example, controlling for household income or family characteristics adjusts for differences in search constraints. To capture differences in human capital and previous labour market performance, we control for educational attainment and past and recent labour market experience. In contrast to studies that compare overeducation to adequate employment, our focus on unemployed who have lost their job should reduce both observed and unobserved differences and allows for the adjustment of previous job characteristics. Matching on work biography, previous occupational class and labour income should at least partly control for variables we cannot observe. Specifically, following Sianesi (2004: 138), we think that controlling for the elapsed unemployment duration should also 'capture important unobservables' like motivation or readiness for work.

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[5] This average is calculated by weighting the individual Δ_t^u by the elapsed unemployment duration distribution of the treated. Note that the causal interpretation pertains to the individual Δ_t^u.

[6] The discrete-time hazard model is estimated by using a multinomial logistic regression based on person-month data (Allison, 1982). The four independent competing risks are: overeducated employment, adequate employment, education/training, inactivity. We used a piecewise-constant specification to model the baseline hazard.

Table 1 Definition and measurement of the covariates

Covariate	Definition and measurement
Socio-demographics	
Age	In years
Sex	1=female, 0=male
Migration background	1=first or second generation immigrant, 0=otherwise
Education	
Educational attainment	1=vocational degree, 2=technical college degree[a], 3=university degree
Work biography	
Total employment experience (2 variables)	Total full-time experience in (decimal) years, total part-time experience in (decimal) years
Recent employment experience	Number of months employed in the 12 months before unemployment
Total unemployment experience	Total unemployment experience in (decimal) years
Previous unemployment spells	Total number of previous unemployment spells since the age of 15 years
Recent unemployment experience	Number of months unemployed in the 12 months before unemployment
Characteristics of last job	
Occupational class	6 categories according to the EGP class classification: 1=higher managerial and professional workers (I), 2=lower managerial and professional workers (II), 3=routine non-manual workers (IIIa, IIIb), 4=self-employed (IVa-IVc), 5=skilled manual workers (VI), 6=unskilled manual workers (VIIa, VIIb)
Labour income	Real monthly net labour income in 1000 Euro (adjusted to 2006 Euros)
Job satisfaction	0=completely dissatisfied, 10=completely satisfied
Household characteristics	
Household income	Real annual equivalised disposable household income in 1000 Euro (adjusted to 2006 Euros)
Partner	3 categories: 1=no partner or spouse in household, 2=partner in household, 3=spouse in household
Children	Number of children from 0-14 years
Health	
Health satisfaction	0=completely dissatisfied, 10=completely satisfied
Disability	1=legally attested disability of 30 per cent or more, 0=otherwise
Context characteristics	
Year spell started	4 categories: 1=1984-1990, 2=1991-1997, 3=1998-2002, 4=2003-2011

Table 1 continued

Quarter spell started	4 categories: 1=January-March, 2=April-June, 3=July-September, 4=October-December
Region	4 categories according to the recommendation of the SOEP: 1=North-Germany (Bremen, Hamburg, Lower Saxony, Schleswig-Holstein) 2=East-Germany (Berlin, Brandenburg, Mecklenburg-Vorpommern, Saxony, Saxony-Anhalt, Thuringia) 3=South-Germany (Bavaria, Baden-Württemberg, Hesse) 4=West-Germany (North-Rhine Westphalia, Rhineland-Palatinate, Saarland)
Regional unemployment[b]	Monthly unemployment rate of the federal state
Unemployment duration (baseline hazard)	8 categories: 1=1 month, 2=2 months, 3=3 months, 4=4-6 months, 5=7-9 months, 6=10-12 months, 7=13-15 months , 8=16 months or more

Notes: [a] This degree could be acquired in the former German Democratic Republic (GDR) and its German name is 'Ingenieurs-/Fachschule'. [b] The monthly unemployment rate of the federal state is taken from Statistics of the Federal Employment Agency (2015) and the monthly published official notifications of the Federal Employment Agency (1984-1991) and has been merged to the SOEP.
Source: Own illustration.

However, if both groups differ with respect to unobserved characteristics (e.g., ability) after controlling for these covariates, the differences in outcomes are at least partly explained by unobserved heterogeneity.

To balance the treated and controls on X, we form 'statistical twins' in terms of the propensity score, that is, the discrete-time hazard. Comparing different matching algorithms, we decided for radius matching with a propensity score radius of 0.01, because it gave the best results with respect to covariate balance. Given its importance, we matched exactly on the elapsed unemployed duration, that is, we only compare treated and controls who have spent the same number of months in unemployment. Accordingly, common support is checked in every month u. In general, only about two per cent of the treated are off-support. Standard errors have been calculated using the variance approximation of Lechner (2001).[7] All matching analyses are performed using the psmatch2 ado in Stata (Leuven and Sianesi, 2003).

4. Results

4.1 Descriptive statistics

To provide an overview of the sample Table 2 displays some descriptive statistics on the number and duration of unemployment spells by exit status. Summary statistics on the covariates at inflow into unemployment are presented in Supplement B. Of the 4,538 unemployment spells, 4,038 spells are completed (89 per cent) and 500 right-censored (11 per cent). Among the completed spells 76 per cent end in employment, 13 per cent in education/training, and 11 per cent in inactivity showing that the majority of the unemployed experience re-employment. However, more than a third of the re-employed end up in jobs for which they are overeducated. Compared with the percentage of overeducated workers in the working population (18 per cent) this highlights the importance of overeducation as a route out of unemployment.[8] Table 2 also summarises the duration of the completed spells. On average, unemployment spells that end in adequate employment are about three months shorter than those that end in overeducation emphasising that an overeducated re-entry may not be relevant to some unemployed, because they find adequate employment before. Accordingly, this implies that it is important to use the dynamic definition of the controls as discussed in the previous section.

[7] Bootstrapping turned out to be too time-consuming and Lechner (2002) finds little difference between the approximated and bootstrapped standard errors.
[8] The percentage of overeducated workers in the working population has been estimated based on a sample of all workers of age 18-54 years over the period of 1984-2012 using the SOEP. It is in line with estimates of previous German studies (see Büchel, 2001: 508).

Table 2 Duration of completed spells by exit status (in months)

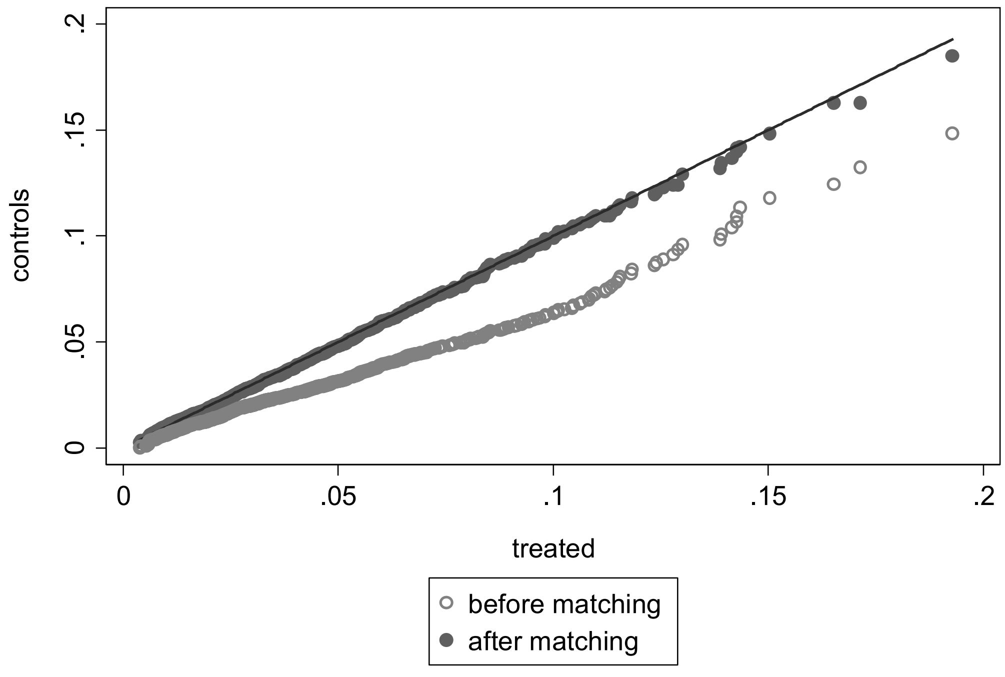
Exit to	Mean	SD	P_{25}	P_{50}	P_{75}	N (spells)
Overeducation	9.4	12.8	2	6	12	1067
Adequate employment	6.1	7.6	2	4	8	2011
Education/training	9.7	11.5	3	6	12	532
Inactive	12.0	13.3	3	8	15	428

Notes: P_{25} to P_{75} = 25th to 75th percentile of the distribution.
Source: SOEP 1984-2012, own calculations.

4.2 Propensity score matching

The following discussion focuses on a comparison of covariate balance before and after matching to assess the quality of matching. Supplement C reports the results of the discrete-time hazard competing risk model that has been used to estimate the propensity score. Figure 1 summarises the quality of the radius matching by means of an empirical quantile-quantile plot (Ho *et al.,* 2007). It plots the quantiles of the propensity score distribution of the treated against that of the controls. Before matching, the plot is consistently below the 45-degree line, indicating that the treated are substantially different from the controls. After matching, the distributions of both groups are very similar, suggesting that they have been successfully matched.

Figure 1 Quantile-quantile plot of the propensity score of the treated and controls



Notes: This plot is based on the outcome 'employment chances t+6'. The results are similar for the other outcomes.
Source: SOEP 1984-2012, own calculations.

Table 3 Balance of covariates: Means and standardised bias before and after matching

Covariates	Treated	Controls		Standardised bias	
		Before	After	Before	After
Unemployment duration (baseline hazard)	9.01	14.01	9.01	-33.2	0.0
Age	37.83	38.72	37.73	-9.6	1.1
Age squared	1522.00	1578.10	1513.60	-8.0	1.2
Female	0.48	0.52	0.49	-6.4	-1.4
First or second generation immigrant	0.17	0.11	0.16	15.9	1.7
Vocational degree	0.80	0.88	0.80	-23.7	-0.6
Technical college degree	0.08	0.03	0.07	18.8	1.7
University degree	0.13	0.08	0.13	14.2	-0.5
Total employment experience (full-time)	12.18	12.29	12.21	-1.2	-0.3
Total employment experience (part-time)	1.61	1.75	1.56	-4.0	1.2
Total employment experience (part-time) x Female	1.47	1.47	1.41	-0.2	1.6
Recent employment experience	10.50	10.66	10.54	-5.9	-1.5
Recent employment experience squared	117.11	120.37	117.84	-7.5	-1.7
Total unemployment experience	1.28	1.46	1.24	-8.1	1.6
Total unemployment experience squared	6.42	7.02	5.98	-2.3	1.7
Previous unemployment spells	1.69	1.68	1.65	0.6	2.2
Recent unemployment experience	1.24	1.07	1.19	7.3	2.3
Higher managerial and professional workers	0.04	0.06	0.04	-10.1	-0.9
Lower managerial and professional workers	0.13	0.17	0.14	-11.8	-3.2
Routine non-manual workers	0.21	0.23	0.22	-4.8	-2.3
Self-employed	0.03	0.04	0.03	-5.8	-0.1
Skilled manual workers	0.22	0.21	0.22	3.6	1.1
Unskilled manual workers	0.37	0.29	0.35	17.2	3.9
Labour income	1.07	1.09	1.07	-5.0	-1.5
Labour income squared	1.37	1.56	1.40	-7.8	-1.1
Job satisfaction	6.12	6.10	6.08	0.9	1.8
Household income	15.55	16.39	15.85	-10.0	-3.6
Household income squared	284.53	365.35	299.85	-8.0	-1.5
No partner or spouse	0.30	0.32	0.30	-3.3	1.6
Partner	0.14	0.16	0.14	-4.5	0.1
Spouse	0.56	0.52	0.56	6.3	-1.6
No partner or spouse x Female	0.13	0.15	0.13	-3.6	1.4
Partner x Female	0.07	0.07	0.07	-2.7	-0.6
Spouse x Female	0.29	0.30	0.30	-2.9	-2.3
Children	0.66	0.67	0.65	-1.0	0.6
Health satisfaction	6.94	6.60	6.88	16.1	2.9
Disability	0.02	0.04	0.02	-11.6	-1.9
Spell started in 1984-1990	0.09	0.06	0.10	11.5	-0.7
Spell started in 1991-1997	0.34	0.28	0.34	12.8	0.0
Spell started in 1998-2002	0.22	0.29	0.23	-16.4	-2.0
Spell started in 2003-2011	0.34	0.36	0.33	-4.0	2.2

Table 3 continued

Spell started in January-March	0.29	0.28	0.29	1.8	-0.4
Spell started in April-June	0.22	0.23	0.22	-3.4	-1.8
Spell started in July-September	0.24	0.26	0.24	-5.8	0.0
Spell started in October-December	0.26	0.23	0.25	7.3	2.2
North-Germany	0.13	0.12	0.13	4.2	1.0
East-Germany	0.49	0.52	0.49	-5.5	1.2
South-Germany	0.22	0.20	0.23	4.9	-1.9
West-Germany	0.16	0.16	0.16	-1.8	-0.4
Regional unemployment	13.12	13.58	13.15	-9.3	-0.7

Notes: The means of the treated that are on-support are reported.
Source: SOEP 1984-2012, own calculations.

We also calculated the means and the standardised bias before and after matching, to assess the balance on every single covariate (Table 3). Since we matched exactly on unemployment duration, the standardised bias for this variable falls to zero after matching. Table 3 highlights some other interesting differences.[9] For example, the unemployed who take up an overeducated job are overrepresented among younger workers, males and first or second generation immigrants and are also more likely to have a university degree. They have less total and recent employment experience but are more likely to have been unemployed in the 12 months before the current unemployment spell. Considering the previous job, the treated are less likely to work in higher managerial and professional occupations and are more likely to be manual workers. In addition, their labour and household income is lower than that of the controls. Comparing the mean standardised bias before and after matching, we find that matching reduces the bias below the standard threshold of 5 per cent for each covariate (Caliendo, 2006). This confirms that matching has been successful.

4.3 Effects of an overeducated re-entry over time

4.3.1 Employment chances

Figure 2 shows the probability of being employed 6 to 60 months after the treatment (time zero) for those who have taken up an overeducated job (treated) and those who have remained unemployed and continued the job search for at least one month (controls). The difference between these probabilities represents the treatment effect. The treatment effect is also shown for the two years before the treatment offering a so-called pre-treatment test of selection bias (Hagen, 2004). If selection bias is present, that is, both groups still differ on background char-

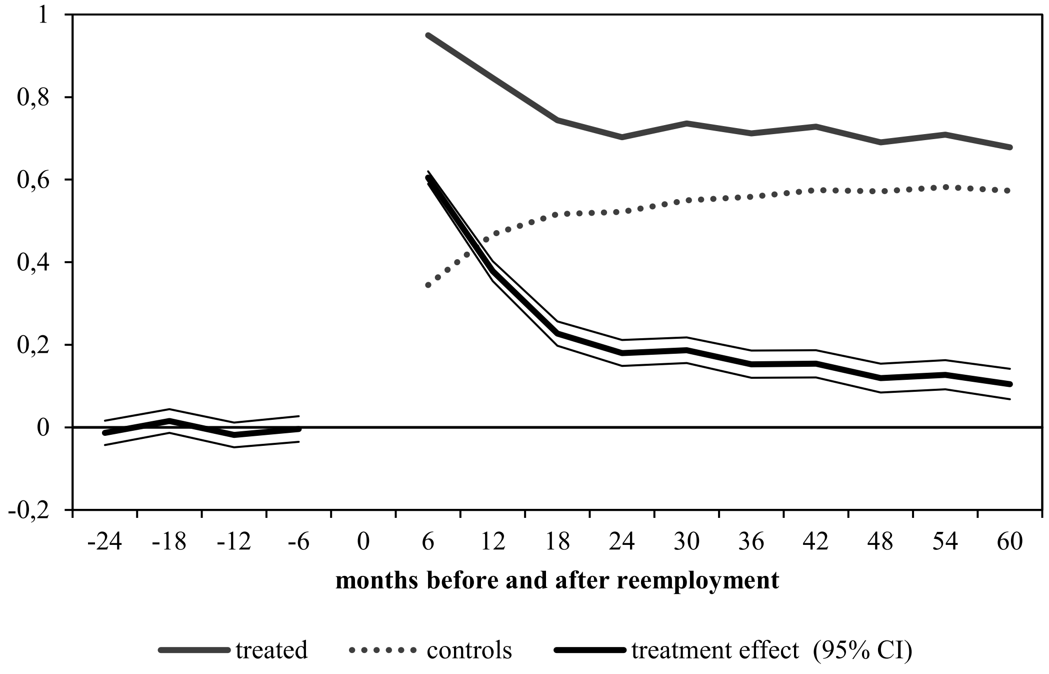
[9] We refrain from a comparison to previous studies on the determinants of overeducation, because different measurements (see Verhaest and Omey, 2010), different sample definitions, and their focus on the comparison of overeducation to adequate employment make such a comparison difficult.

acteristics, the pre-treatment effects will differ from zero. However, Figure 2 illustrates that in the 24 months before the treatment the probability of being employed is approximately the same for both groups, indicating that the CIA is plausible.

Considering the post-treatment period, we find that 6 months after the treatment 95 per cent of those who experienced an overeducated re-entry are employed compared with 34 per cent of those who have remained unemployed. This translates into a treatment effect of about 61 percentage points. To give a reading example: On average, six months after the treatment the employment chances of those re-entering the labour market in an overeducated job are 61 percentage points higher than of those who remained unemployed. Of course, in the first 12 months after re-employment large effects are not surprising given that the controls remain unemployed for some time by definition.

However, the positive employment effect is persistent, that is, even two to five years after re-employment the effects range between 10 to 20 percentage points. Additional analyses (not shown) reveal that these positive employment effects are to a similar share explained by a lower unemployment risk and a lower probability of being out of labour force. Looking at employment chances only, these results provide strong support for the stepping-stone hypothesis (1a).

Figure 2 Treatment effects: Employment chances over time (percentage points)

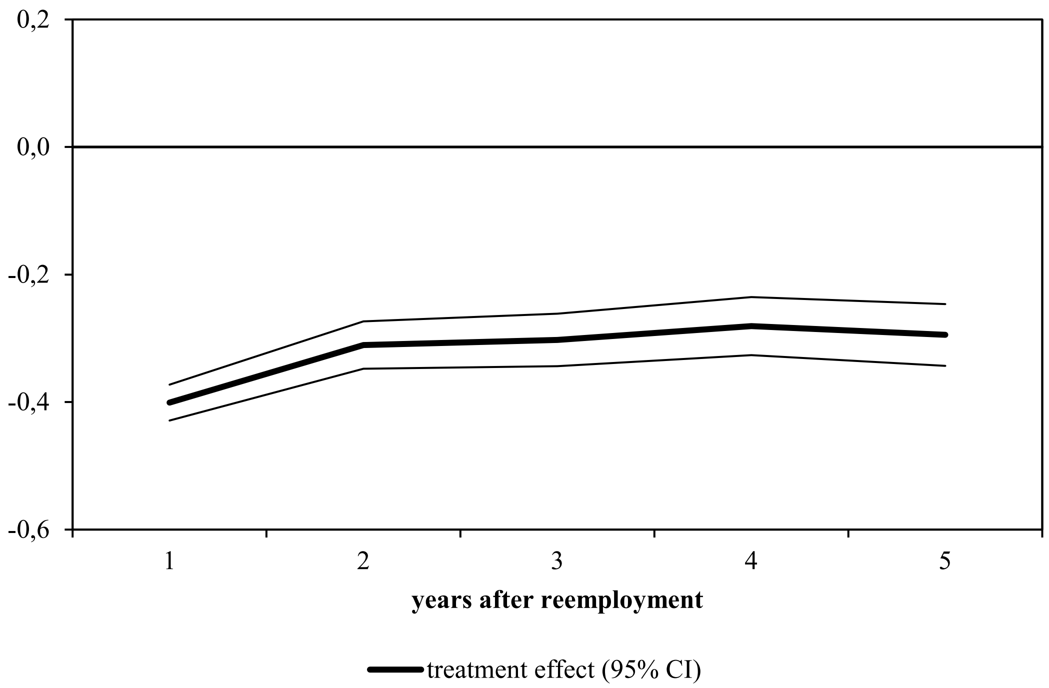


Notes: treated: probability of employment of those taking up an overeducated job, controls: probability of employment of those remaining unemployment and continuing the search, treatment effect (95% CI): average of the individual treatment effects by elapsed unemployment duration and 95% confidence interval.
Source: SOEP 1984-2012, own calculations.

4.3.2 Job quality

However, despite the positive employment effects, an overeducated re-entry does not necessarily have to be a stepping stone into adequate employment. Figure 3 shows the treatment effects for the chances of adequate employment over a period of one to five years after re-employment. Note that these analyses only compare treated and controls who are employed in the respective years. In contrast to the positive employment effects, Figure 3 shows that the unemployed who experience an overeducated re-entry face a strong negative effect in terms of chances of adequate employment. One year after re-employment, they have a 40 percentage point lower chance to work in a job that matches their educational qualifications compared with those who remained unemployed. Although the time trend suggests that some workers advance to adequate employment, the negative effects persist up to five years after re-employment, indicating a significant lock-in effect into overeducation. This result is in line with previous studies (Baert et al., 2013; Pollmann-Schult and Büchel, 2004a) showing that an overeducated re-entry delays the transition to adequate employment. It supports the trap hypothesis (1b), demonstrating that exiting unemployment into overeducation is not a stepping stone into adequate employment.

Figure 3 Treatment effects: Chances of adequate employment over time (percentage points)



Notes: treatment effect (95% CI): average of the individual treatment effects by elapsed unemployment duration and 95% confidence interval.
Source: SOEP 1984-2012, own calculations.

4.3.3 Effect heterogeneity

In order to test whether the effects vary by workers' labour market experience and educational qualification (hypotheses 2 and 3) we repeated the analyses for the respective subgroups. Table 4 and Table 5 report the treatment effects and standard errors for the two outcomes by labour market experience and educational qualification. Supplement D provides the respective figures. With respect to labour market experience, we distinguish workers with up to five years of total experience (early-career workers) from workers with more than five years (established workers) of total experience (i.e., employment and unemployment experience). With respect to educational qualification, we distinguish between workers with a vocational degree and those having a university degree where workers with a technical college degree (GDR) are included in the latter subgroup.

The general patterns and trends in the treatment effects closely resemble the results of the main analyses. We find positive employment effects and negative effects for the chance of adequate employment across all subgroups. These findings are, thus, reassuring and consistent with the main effects reported before. Considering the differences in the employment effects, we find slightly higher employment effects for established workers than for early-career workers, especially from year three onwards. As for the chances of adequate employment across subgroups, we find very similar effects for early-career workers and established workers (Table 4). Taken together these results contradict hypothesis 2 expecting that early-career workers should benefit more from an overeducated re-entry than established workers.[10]

How do the effects differ by educational qualification (Table 5)? In terms of employment chances we do not find any economically significant differences between workers with a vocational degree and workers having a university degree. Considering the chance of adequate employment, the analyses reveal a stronger lock-in effect into overeducation for workers with a vocational degree, in particular, in the first four years after re-employment. This finding lends weak support to hypothesis 3, arguing that workers with academic qualifications should benefit more from an overeducated re-entry. However, considering that these differences are small in size and not apparent in all years, hypothesis 3 has to be rejected. In sum, the results are in line with previous findings by Baert et al. (2013) also suggesting that the effects do not vary substantially across different skill groups.

[10] We also distinguished workers in their early careers and established workers by age. The results are similar to those reported, showing no substantially relevant differences between workers of age 18-29 (early-career) and those of age 30-54 (established workers) years.

Table 4 Treatment effects: Employment chances and chances of adequate employment over time by labour market experience

Month	Employment chances				Year	Chances of adequate employment			
	LME: <= 5 years		LME: > 5 years labour			LME: <= 5 years		LME: > 5 years	
	TE	SE	TE	SE		TE	SE	TE	SE
6	0.55	0.02	0.61	0.01	1	-0.43	0.04	-0.39	0.02
12	0.34	0.03	0.39	0.01	2	-0.31	0.05	-0.31	0.02
18	0.21	0.03	0.23	0.02	3	-0.31	0.06	-0.31	0.02
24	0.17	0.04	0.18	0.02	4	-0.25	0.06	-0.29	0.03
30	0.12	0.04	0.20	0.02	5	-0.31	0.06	-0.29	0.03
36	0.13	0.04	0.16	0.02					
42	0.11	0.04	0.17	0.02					
48	0.04	0.04	0.13	0.02					
54	0.06	0.04	0.14	0.02					
60	0.05	0.05	0.12	0.02					

Notes: TE: average of the individual treatment effects by elapsed unemployment duration. LME: labour market experience.
Source: SOEP 1984-2012, own calculations.

Table 5 Treatment effects: Employment chances and chances of adequate employment over time by educational qualification

Month	Employment chances				Year	Chances of adequate employment			
	Vocational degree		University degree			Vocational degree		University degree	
	TE	SE	TE	SE		TE	SE	TE	SE
6	0.61	0.01	0.58	0.02	1	-0.44	0.02	-0.29	0.03
12	0.38	0.01	0.38	0.03	2	-0.33	0.02	-0.24	0.04
18	0.22	0.02	0.25	0.03	3	-0.32	0.02	-0.27	0.04
24	0.17	0.02	0.21	0.03	4	-0.31	0.03	-0.21	0.05
30	0.18	0.02	0.20	0.03	5	-0.29	0.03	-0.29	0.04
36	0.16	0.02	0.11	0.04					
42	0.17	0.02	0.10	0.04					
48	0.12	0.02	0.13	0.04					
54	0.14	0.02	0.09	0.04					
60	0.11	0.02	0.09	0.04					

Notes: TE: average of the individual treatment effects by elapsed unemployment duration.
Source: SOEP 1984-2012, own calculations.

5. Conclusions

This article sought to reveal whether it is better for the unemployed to take up a job for which they are overeducated or to remain unemployed and continue the search for adequate employment. To test the opposing predictions of the stepping-stone and trap hypotheses, we examined the effects of an overeducated re-entry on the long-term employment chances and chances of adequate employment. Because it is likely that these effects vary across the unemployed, we also performed subgroup analyses by labour market experience and educational qualification. Using the SOEP (1984-2012), our analyses are based on a dynamic propensity score matching approach comparing unemployed who take up an overeducated job to similar unemployed who continued the job search for at least one month. This dynamic extension of propensity score matching allows avoiding potential bias due to an inappropriate definition of controls and addressing the key methodological issue of selection into overeducation.

The empirical results reveal that taking up an overeducated job is associated with long-run positive employment effects. Even five years after re-employment, the unemployed who take up an overeducated job have a 10 percentage point higher chance to be employed. Taken by itself, this result speaks in favor of the stepping-stone hypothesis, suggesting that policy-makers should enforce the acceptance of overeducated jobs. However, looking at the chances of adequate employment, we find strong negative effects ranging from 30 to 40 percentage points. The latter analysis also shows that only a minority of those who take up an overeducated job advance to adequate employment. These results are in line with previous studies (Baert *et al.*, 2013; Pollmann-Schult and Büchel, 2004a), illustrating that an overeducated re-entry is not a stepping stone into adequate employment. More generally, they are also supportive of studies showing that overeducation is not just a temporary problem (e.g., Pollmann-Schult and Büchel, 2004b). Considering the subgroup analyses, we find that the effects are rather similar for early-career and established workers as well as for workers with a vocational and university degree.

The following limitations should, however, be considered. First, although we used a very homogenous sample and controlled for many observed differences as well as elapsed unemployment duration, our estimates are biased if treated and controls still differ on unobserved characteristics. For example, if the overeducated were less able, we would underestimate the positive employment effect and overestimate the negative effect in terms of adequate employment. Although a pre-treatment test suggests that the selection on observables assumption is reasonable, we cannot rule out the influence of unobserved heterogeneity. However, Baert *et al.* (2013) report similar results taking account of selection on unobservables by using the timing-of-events approach. Against this background, we think it is unlikely that our findings are explained by unobserved heterogeneity. Second, the literature on overeducation has not yet agreed on a standard measurement. Despite using an extension of the standard subjective approach and testing for alternative measurements, measurement error may still be present. Third, the analyses do not allow identifying the relative importance of different mechanisms. Although lower job search intensity appears to be the most likely explanation of the lock-in effect into overeducation, we cannot disentangle different mechanisms empirically. One promising approach to address these questions is the use of field experiments (see Baert and Verhaest, 2014).

What broader conclusions can be drawn from the results? From a policy-point of view, the analyses point to an important trade-off between employment chances and the chances of adequate employment. For this reason, policies that force the unemployed into overeducation, for example, by tightening benefit eligibility or revising regulations for the type of work the jobless have to accept, may cause persistent qualification mismatches that are costly for the individual and the society as a whole (Baert *et al.*, 2013). To allow for a more targeted design of policies, future research should investigate under which circumstances an overeducated reentry represents a stepping stone or a trap, respectively. Although this article shows that the effects vary little by labour market experience and educational qualification, it is likely that the above described trade-off turns out to be positive for some unemployed and negative for others. Follow-up studies could examine how the results vary by unemployment duration or the degree of mismatch. The former analysis would, for example, help to estimate the unemployment duration, at which remaining unemployed only has negative effects. In general, changing perspective and asking how overeducation compares to unemployment seems to be a valuable avenue of future research providing a more comprehensive picture on the impact of overeducation on individuals' careers.

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7. Supplementary material (online only)

Supplement A.	Measurement model of overeducation
Supplement B.	Descriptive statistics at the inflow into unemployment
Supplement C.	Discrete-time hazard competing risk duration model (Average marginal effects multiplied by 100)
Supplement D.	Figures for effect heterogeneity analyses (Table 4 and Table 5)

Supplement A. Measurement model of overeducation

Job requirement level	Occupational status	Classification with regard to the degree of congruence between job and education – Qualification level gained: Vocational degree[1]	Engineering, technical school (East Germany)	University or post-secondary technical college degree[2]
No special training required/ only a short introduction on the job	Unskilled/ semi-skilled worker	**ov**	**ov**	**ov**
	Skilled worker/ foreman/ master craftsman	*	-	-
	White-collar worker in an unskilled job	**ov**	**ov**	**ov**
	White-collar worker in a skilled job	*	*	*
	White-collar worker in a highly skilled job	-	-	-
	Self-employed person	**ov**	**ov**	**ov**
	Civil servant	-	-	-
A lengthy period of coaching at my place	Unskilled/ semi-skilled worker	**ov**	**ov**	**ov**
	Skilled worker/ foreman/ master craftsman	*	-	-
	White-collar worker in an unskilled job	**ov**	**ov**	**ov**
	White-collar worker in a skilled job	*	*	*
	White-collar worker in a highly skilled job	*	*	*
	Self-employed person	**ov**	**ov**	**ov**
	Civil servant	*	-	-
Attendance at special theoretical or practical courses/ A certificate of vocational training	Unskilled/ semi-skilled worker	*	**ov**	**ov**
	Skilled worker/ foreman/ master craftsman	**ad**	**ov**	**ov**
	White-collar worker in an unskilled job	**ad**	**ov**	**ov**
	White-collar worker in a skilled job	**ad**	**ov**	**ov**
	White-collar worker in a highly skilled job	**ad**	**ad/*** [3]	**ad/*** [3]
	Self-employed person	**ad**	**ov**	**ov**
	Civil servant	**ad**	**ad/*** [3]	**ad/*** [3]
Engineering, technical school required (East Germany)	Unskilled/ semi-skilled worker	-	-	-
	Skilled worker/ foreman/ master craftsman	-	-	-
	White-collar worker in an unskilled job	-	*	**ov**
	White-collar worker in a skilled job	**ad**	**ad**	**ov**
	White-collar worker in a highly skilled job	**ad**	**ad**	**ad**
	Self-employed person	**ad**	**ad**	**ad**
	Civil servant	-	**ad**	**ov**
A university or post-sec. technical college degree	Unskilled/ semi-skilled worker	-	-	-
	Skilled worker/ foreman/ master craftsman	-	-	-
	White-collar worker in an unskilled job	-	-	-
	White-collar worker in a skilled job	-	-	*
	White-collar worker in a highly skilled job	**ad**	**ad**	**ad**
	Self-employed person	**ad**	**ad**	**ad**
	Civil servant	-	**ad**	**ad**

Notes: ad=adequate employment, ov=overeducation, *=degree of mismatch not clearly determinable, -=implausible combination

1) including: apprenticeship, vocational school, health care school, technical school, civil service training, other training, master craftsman, engineering, technical degree.

2) including: technical college, university, technical university, dissertation, habilitation.

3) Until 1993 respondents are adequately qualified; after 1993 degree of mismatch is not clearly determinable due to changes in the questionnaire.

Source: Own illustration adapted from Büchel and Weißhuhn (1998).

Supplement B. Descriptive statistics at the inflow into unemployment

Covariates	Mean/Per cent	SD
Age	36.1	(9.4)
Female	46.8	
First or second generation immigrant	12.9	
Vocational degree	84.9	
Technical college degree (former GDR)	3.3	
University degree	11.8	
Total employment experience (full-time)	11.4	(9.0)
Total employment experience (part-time)	1.6	(3.5)
Recent employment experience	10.7	(2.4)
Total unemployment experience	1.1	(1.9)
Previous unemployment spells	1.5	(1.9)
Recent unemployment experience	0.9	(2.1)
Higher managerial and professional workers	7.3	
Lower managerial and professional workers	17.9	
Routine non-manual workers	20.8	
Self-employed	3.6	
Skilled manual workers	24.0	
Unskilled manual workers	26.4	
Labour income	1.2	(0.6)
Job satisfaction	6.1	(2.5)
Household income	17.4	(9.1)
No partner or spouse	31.6	
Partner	15.9	
Spouse	52.4	
Children	0.6	(0.9)
Health satisfaction	6.8	(2.1)
Disability	3.0	
Spell started in 1984-1990	8.8	
Spell started in 1991-1997	28.2	
Spell started in 1998-2002	24.1	
Spell started in 2003-2011	38.9	
Spell started in January-March	26.6	
Spell started in April-June	21.7	
Spell started in July-September	25.0	
Spell started in October-December	26.8	
North-Germany	13.0	
East-Germany	44.9	
South-Germany	24.2	
West-Germany	17.9	
Regional unemployment	12.7	(5.0)
N (spells)	4538	
N (persons)	3353	

Notes: See Table 1 for a detailed description of the definition and measurement of the covariates.
Source: SOEP 1984-2012, own calculations.

Supplement C. Discrete-time hazard competing risk duration model (Average marginal effects multiplied by 100)

Exit to	Overeducation		Adequately employed		Education/training		Inactivity	
	AME x 100		AME x 100		AME x 100		AME x 100	
Ref.: 1 month								
2 months	-0.251	(0.349)	-0.185	(0.501)	0.240	(0.221)	0.167	(0.194)
3 months	0.049	(0.377)	-0.113	(0.533)	-0.029	(0.218)	-0.142	(0.184)
4-6 months	-0.430	(0.302)	-1.402 ***	(0.427)	0.567 ***	(0.199)	0.147	(0.166)
7-9 months	-0.401	(0.333)	-2.522 ***	(0.452)	0.514 **	(0.224)	0.321 *	(0.194)
10-12 months	-0.207	(0.374)	-2.506 ***	(0.496)	0.777 ***	(0.267)	0.632 ***	(0.237)
13-15 months	-0.012	(0.440)	-3.951 ***	(0.510)	0.489 *	(0.297)	0.590 **	(0.277)
16 months or more	-1.218 ***	(0.294)	-5.233 ***	(0.386)	-0.093	(0.185)	0.372 **	(0.183)
Age	-0.041 *	(0.022)	-0.163 ***	(0.033)	-0.034 **	(0.017)	-0.027 *	(0.014)
Female (Ref.: male)	0.132	(0.187)	-1.508 ***	(0.302)	-0.008	(0.165)	0.792 ***	(0.125)
First or second generation immigrant (Ref.: otherwise)	0.855 ***	(0.281)	-1.586 ***	(0.284)	-0.344 **	(0.165)	-0.107	(0.151)
Ref.: Vocational degree								
Technical college degree (former GDR)	5.675 ***	(0.892)	-3.781 ***	(0.356)	0.789 **	(0.355)	-0.724 ***	(0.167)
University degree	4.768 ***	(0.680)	-0.172	(0.403)	0.210	(0.252)	0.485 *	(0.283)
Total employment experience (full-time)	0.044 **	(0.022)	0.043	(0.032)	-0.017	(0.017)	-0.013	(0.014)
Total employment experience (part-time)	-0.183 **	(0.074)	0.082	(0.074)	0.020	(0.035)	-0.048	(0.030)
Recent employment experience	-0.076	(0.086)	-0.469 ***	(0.127)	-0.124 **	(0.060)	-0.100 *	(0.053)
Total unemployment experience	-0.235 ***	(0.090)	-1.228 ***	(0.167)	-0.125	(0.086)	-0.081	(0.062)
Previous unemployment spells	0.133 **	(0.064)	0.517 ***	(0.095)	0.045	(0.055)	-0.070	(0.050)
Recent unemployment experience	0.171 **	(0.076)	-0.407 ***	(0.099)	-0.116 **	(0.047)	-0.076 *	(0.042)
Ref.: Higher managerial and professional workers								
Lower managerial and professional workers	0.846 ***	(0.238)	-1.214 **	(0.591)	0.175	(0.249)	0.464 **	(0.193)
Routine non-manual workers	1.454 ***	(0.259)	-2.377 ***	(0.605)	0.345	(0.262)	0.484 **	(0.195)
Self-employed	0.865 **	(0.372)	-2.746 ***	(0.730)	-0.129	(0.347)	0.802 **	(0.356)
Skilled manual workers	1.768 ***	(0.281)	-1.799 ***	(0.617)	0.200	(0.265)	0.376 *	(0.211)
Unskilled manual workers	2.517 ***	(0.285)	-3.400 ***	(0.601)	-0.058	(0.254)	0.623 ***	(0.207)

Supplement C. continued

Labour income	-0.303	(0.237)	0.340	(0.259)	0.396 **	(0.159)	-0.003	(0.144)
Job satisfaction	-0.025	(0.034)	0.089 *	(0.047)	-0.098 ***	(0.023)	0.004	(0.021)
Household income	-0.039 **	(0.018)	0.075 ***	(0.017)	0.041 ***	(0.012)	0.002	(0.008)
Ref.: No partner or spouse								
Partner	0.318	(0.250)	0.756 **	(0.313)	-0.141	(0.178)	0.158	(0.141)
Spouse	0.578 ***	(0.209)	1.346 ***	(0.288)	-0.097	(0.161)	0.487 ***	(0.132)
Children	-0.134	(0.107)	-0.402 ***	(0.147)	-0.005	(0.079)	-0.082	(0.074)
Health satisfaction	0.164 ***	(0.042)	0.157 ***	(0.057)	-0.024	(0.028)	-0.066 ***	(0.025)
Disability	-1.249 ***	(0.329)	-1.342 **	(0.525)	-0.181	(0.288)	1.005 ***	(0.388)
Ref. Spell started in 1984-1990								
Spell started in 1991-1997	-0.669 *	(0.405)	-0.920 **	(0.425)	0.743 ***	(0.242)	-0.501 **	(0.233)
Spell started in1998-2002	-1.353 ***	(0.408)	-0.047	(0.450)	0.570 **	(0.250)	-0.541 **	(0.237)
Spell started in 2003-2011	-1.037 ***	(0.394)	0.253	(0.424)	-0.371 *	(0.217)	-0.301	(0.231)
Ref.: Spell started in January-March								
Spell started in April-June	-0.160	(0.231)	-0.446	(0.307)	-0.018	(0.155)	0.320 **	(0.140)
Spell started in July-September	-0.400 *	(0.215)	-0.876 ***	(0.287)	0.233	(0.158)	0.237 *	(0.131)
Spell started in October-December	-0.024	(0.227)	0.068	(0.309)	0.061	(0.161)	0.401 ***	(0.148)
Ref.: North-Germany								
East-Germany	-0.192	(0.348)	-0.090	(0.495)	0.469 *	(0.256)	-0.609 **	(0.239)
South-Germany	0.031	(0.331)	-0.745 *	(0.394)	-0.078	(0.200)	1.079 ***	(0.402)
West-Germany	-0.331	(0.301)	-0.918 **	(0.379)	-0.045	(0.196)	0.651 **	(0.274)
Regional unemployment	-0.052	(0.034)	-0.120 **	(0.047)	-0.016	(0.025)	0.088 ***	(0.023)
N (person-months)	40888							
N (spells)	4538							
N (persons)	3353							

Notes: Standard errors in parentheses; *** p<0.05, ** p<0.05, * p<0.10; AME and SE are multiplied by 100; Variables specified with quadratic terms are represented by one AME (Age, recent employment experience, Total unemployment experience, Labour income, household income); Coefficients of interactions are not displayed (Female x Total employment experience (part-time), Female x Partner).
Source: SOEP 1984-2012, own calculations.

Supplement D. Figures for effect heterogeneity analyses (Table 4 and Table 5)

Figure D1. Treatment effects: Employment chances over time by labour market experience (percentage points)

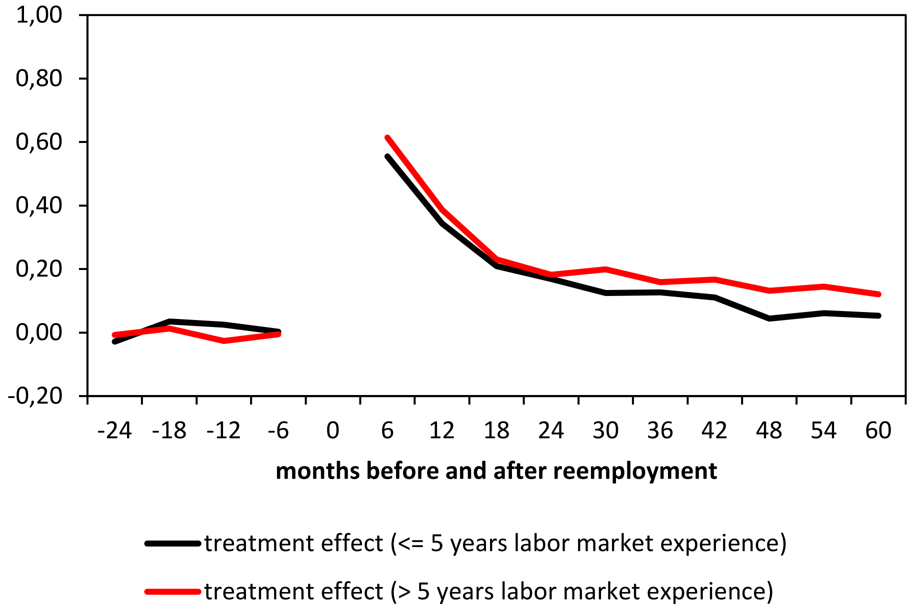
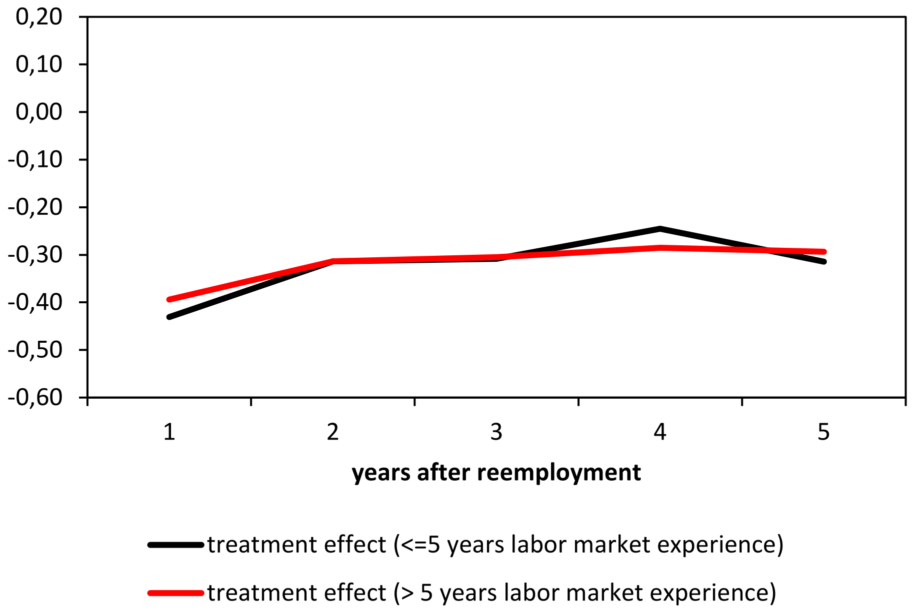


Figure D2. Treatment effects: Chances of adequate employment over time by labour market experience (percentage points)



Notes: Treatment effect: average of the individual treatment effects by elapsed unemployment duration, see Table 4 for the treatment effects and standard errors.
Source: SOEP 1984-2012, own calculations.

Figure D3. Treatment effects: Employment chances over time by educational qualification (percentage points)

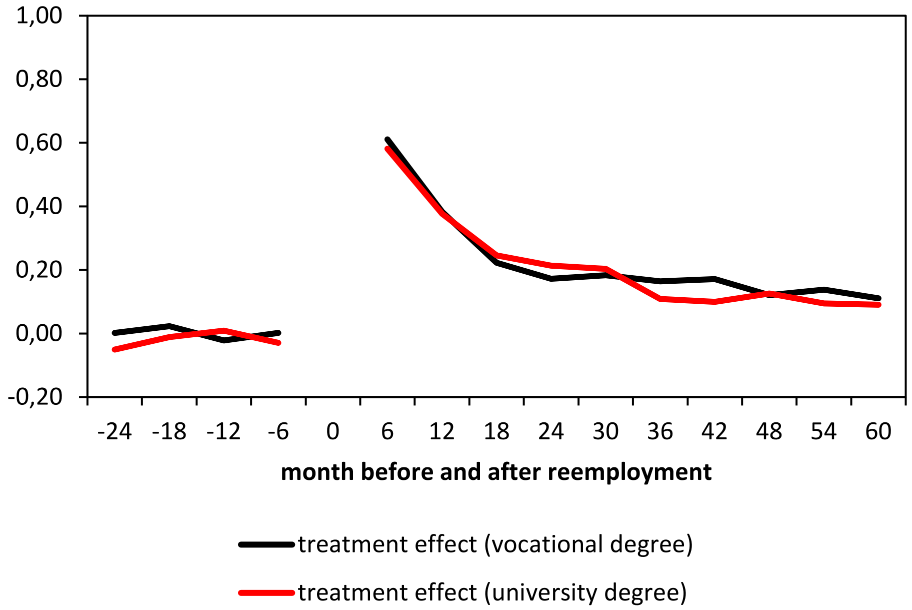
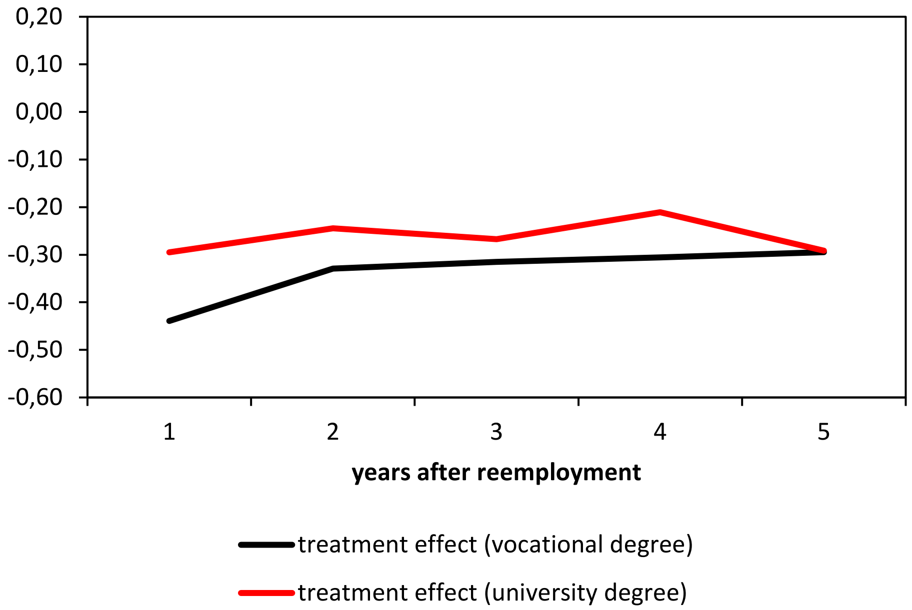


Figure D4. Treatment effects: Chances of adequate employment over time by educational qualification (percentage points)



Notes: Treatment effect: average of the individual treatment effects by elapsed unemployment duration, see Table 5 for the treatment effects and standard errors.
Source: SOEP 1984-2012, own calculations.

Article 4

The effect of an early-career involuntary job loss on later life health in Europe

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Abstract

Recent years have witnessed an increase in interest towards the long-term health consequences of early-career job loss and youth unemployment. Relying on detailed retrospective data from the third wave (2008/09) of the Survey of Health, Ageing and Retirement in Europe (SHARE) this paper investigates whether an involuntary job loss in the first 10 years after labour market entry has lasting negative effects on health more than 30 years later. The results show that an early-career involuntary job loss due to a layoff or plant closure increases the probability of fair or poor self-rated health in late life by about 6 percentage points. Moreover, examining the mechanisms behind this relationship, the analysis reveals that the subsequent unemployment risks and employment instability only explain a small share of the total effect. In line with previous studies, these findings highlight the importance of early career experiences for workers' later life health.

1. Introduction

Research and policy debates over the long-term health consequences of job loss and unemployment, in particular for young people, have intensified over the last ten years. For example, in the aftermath of the financial crisis of 2007/08 and the subsequent economic recession some authors have argued that youth has "suffered disproportionately" in the recession (Bell & Blanchflower, 2011, p. 241) and others have raised concerns about a "lost generation" (Scarpetta et al., 2010, p. 4). Although many studies show that job loss and unemployment have negative direct effects on health (e.g. Burgard et al., 2007; Strully, 2009), surprisingly little is known about the potential long-term consequences, especially for workers who experience negative labour market events in their early-career.

While the long-term consequences for young people who lost their jobs over the course of the financial crisis of 2007/08 cannot yet be analysed, the data from older birth cohorts allows investigating the potential for lasting health effects of job loss and unemployment.[1] A few previous studies on the long-term health consequences mostly examine the general population (Daly & Delaney, 2013; Schröder, 2013). However, recent evidence from long-run cohort studies suggests that young people may be particularly vulnerable to the negative effects of unemployment (Bell & Blanchflower, 2011; Strandh et al., 2014). There are a number of potential explanations for these findings. For example, Brydsten et al. (2015) highlight that the early-career represents a sensitive period in the life course as well as that young workers lack experience and resources to cope with and overcome unemployment.

Moreover, past research primarily focuses on the long-term effects of unemployment which may be either voluntary or involuntary. However, from a substantial as well as a methodological point of view, additional data on the reason for job loss provide rich information (Schröder 2013). Specifically, research on life events emphasises that their effects on health vary by 'their desirability, by the degree of control people have over their occurrence, or by whether or not they are scheduled' (Pearlin et al., 1981, p. 339), making it likely that only involuntary job losses will have lasting negative effects. In fact, voluntary job changes may even improve health as they either reflect an 'escape from a … stressful role situation' (Wheaton, 1990, p.

[1] Previous studies mostly use data from two longitudinal cohort studies: The National Child Development Study (NCDS), following a sample of persons born in 1958 in Great Britain (e.g. Bell & Blanchflower, 2011; Daly & Delaney, 2013) or the Northern Swedish Cohort (NSC) following all pupils in their last year of compulsory school in a medium-sized industrial town in Sweden in 1981 (Brydsten et al., 2015; Strandh et al., 2014). Following Schröder (2013), we draw on the retrospective life history data from the SHARELIFE survey.

2010) or upward socio-economic mobility (Schmelzer, 2012). In contrast, studies focusing on unemployment examine a state that mixes very heterogeneous experiences (Brand, 2015).

Analysing involuntary job losses also helps addressing the methodological challenges of health selection and confounding (Burgard et al., 2007). They may be considered more of an exogenous shock than unemployment. In particular, job losses due to plant closures can be thought to be 'largely beyond the control of the individual worker' (Brand, 2015, p. 360), making it unlikely that they are determined by workers' health or other observed and unobserved characteristics (Baumann et al., 2016, p.161). For these reasons, this article focuses on involuntary job losses in the first 10 years after the labour market entry and distinguishes layoffs from plant closures.[2]

Following recent interest in the mechanisms behind the negative long-term effects of job loss and unemployment on health (e.g. Strandh et al., 2014), we additionally examine how the subsequent unemployment risks and employment instability mediate this relationship. Although the previous empirical evidence is scarce (see Brydsten et al., 2015 for an exception), theoretically, a life course perspective is often employed arguing that initial disadvantages produce further relative disadvantages over time (e.g. DiPrete & Eirich, 2006). Specifically, the negative effects of an early-career job loss may work through channels of increased subsequent unemployment risks (e.g. Brandt & Hank, 2014), employment instability (e.g. Manzoni & Mooi-Reci, 2011) and lower job quality (e.g. Brand, 2006; Dieckhoff, 2011; Gangl, 2006).

This paper focuses on the following research questions. First, what are the long-term effects of an early-career involuntary job loss on self-rated health in late life? Second, do these effects differ for job losses due to layoff or plant closure? Third, to what extent do the subsequent unemployment risks and employment instability mediate the potential negative effects? To answer these questions, data from the third wave (2008/09) of the Survey of Health, Ageing and Retirement in Europe (SHARE) are used. The SHARELIFE survey collected retrospective life histories of elderly Europeans from 14 countries. It provides information on self-rated health at the time of the interview as well as details about respondents' work histories, childhood health and childhood socio-economic status. As Daly and Delaney (2013) empha-

[2] Although researchers emphasise that the school-to-work transition rather represents a process than a transition (Brzinsky-Fay, 2014), they have yet to agree on a common definition of the early-career. Because definitions of the school-to-work process often cover 5 years, we define the early-career to include the first 10 years of the career.

sise, data on these early-life circumstances are important to take into account selection into job loss and unemployment.

The remainder of the article is structured as follows: the next section provides theoretical considerations on why an involuntary early-career job loss can have negative effects on health in late life and how unemployment and employment instability following the job loss may mediate this effect. The next section presents the data, measures and methods followed by a discussion of the results. The last section summarizes the findings and offers some concluding remarks.

2. Theory and hypotheses

The life course perspective distinguishes two basic models explaining how early socio-economic conditions, in general, as well as job loss and unemployment, specifically, affect future health (e.g. Strandh et al., 2014).

The first model (I) assumes that the direct negative effect of an early-career job loss on health, once it occurred, for the most part persists over time. The direct effect itself may be explained by the deprivation of economic and psychosocial rewards that are associated with employment (see Nordenmark and Strandh, 1999 for a theoretical synthesis). Specifically, the loss of economic rewards requires individuals' to adjust their living conditions as well as restricts the control over their lives and ability to plan ahead. Besides its financial consequences, an involuntary job loss may also deprive workers' of psychosocial rewards of employment (Jahoda, 1982) as well as entail the loss of a major social role and identity. This deprivation of the rewards of employment can both negatively affect mental health and over time accumulate and manifest into poorer physical health. For example, physical health may not only be affected through changes in living standards, but also increases in health-damaging and decreases in health-promoting behaviour (Nizalova and Norton, 2017). Lastly, previous studies highlight that negative psychological effects over time can translate into physical health problems (see Korpi, 2001 for a detailed discussion).

In contrast, the second model (II) supposes that an early-career involuntary job loss negatively affects later life health, because it elicits a 'chronic stress process' (Burgard et al., 2007, p. 370; Pearlin et al., 1981) or a 'social chain of risks' (Brydsten et al., 2015, p. 799). This perspective also echoes a key argument from life course sociology stating that trigger events, such as an early-career involuntary job loss, set young people on trajectories that negatively affect their health throughout their life. According to this cumulative (dis-) advantage framework (e.g. DiPrete & Eirich, 2006), initial disadvantages may produce further relative disadvantages resulting in greater inequalities over time. In this model, the negative effect of an early-career involuntary job loss on health is assumed to be mostly operating indirectly; for example, through channels such as increased unemployment risks, employment instability and lower job quality over the course of the subsequent career.

Moreover, the direct negative effect of an early-career involuntary job loss on health may partly explain why workers have difficulties in finding re-employment as well as jobs that match their pre-unemployment positions. Put differently, the long-term negative effects on health may be reinforced over the life course through an additive and sequential interplay of processes of health selection and social causation (West, 1991).

The specific mechanisms of increased unemployment risks, employment instability and lower job quality can be derived from economic and sociological labour market theories and have been attested in numerous empirical studies on the so-called scar effects of job loss and unemployment (e.g. Brand, 2006; Brandt & Hank, 2014; Dieckhoff, 2011; Gangl, 2006; Manzoni & Mooi-Reci, 2011). For instance, Becker's (1993) human capital theory argues that job losses result in the loss of specific as well as the depreciation of general human capital, which, in turn, entails fewer and lower quality job offers by prospective employers. Another explanation is based on theories of unemployment stigma and signalling (e.g. Spence 1973). Because employers have to overcome uncertainty about applicants' productivity, they make use of observable characteristics such as their employment history. Job losses and periods of unemployment will likely signal job searchers' 'doubtful quality' and '[create] scepticism about their merit' (e.g. Young, 2012), weakening their bargaining position.

To explain why increased subsequent unemployment risks and employment instability negatively affect health across the life course, one can draw on the mechanisms for the direct negative effects as described above for the first model (I). In addition, work stress theories, such as the demand-control model or the effort-reward imbalance model, predict that reduced job quality over one's career likely has negative effects on health, too (e.g. Wahrendorf et al., 2013). Against this theoretical background, it is assumed that the potential negative long-term effects of an early-career involuntary job loss on later life health are partially mediated through these channels. Accordingly, the following hypotheses are derived:

Hypothesis 1: An early-career involuntary job loss has a negative effect on later life health.

Hypothesis 2: The total negative effect of an early-career involuntary job loss is partially mediated by increased unemployment risks and employment instability across workers' subsequent careers.[3]

3. Data, measures and methods

3.1 Data

This article used data from SHARELIFE, the third wave (2008/09) of the Survey of Health, Ageing and Retirement in Europe (SHARE). The target population consists of all persons aged 50 years and over at the time of sampling who have their regular domicile in a respective SHARE country. Persons who are incarcerated, hospitalized, out of the country during the entire survey period, unable to speak the country's language or who have moved to unknown addresses are excluded. In all countries, the data were collected based on individual or household probability samples with the use of computer assisted personal interviews (CAPI). Current partners living in the same household were interviewed regardless of age. The average response rate in wave 1 was 62 percent and the average individual retention rates were 73 percent for wave 2 and 77 percent for wave 3 (Börsch-Supan et al., 2013).[4]

SHARELIFE collected retrospective life histories for all individuals who participated in wave 1 or 2 of SHARE (see Schröder, 2011 for methodological details). The following 14 countries were included: Austria, Germany, Sweden, Netherlands, Spain, Italy, France, Denmark, Greece, Switzerland, Belgium, Czech Republic, Poland and Ireland. The data provided information on self-rated health at the time of the interview as well as details about respondents' work histories, childhood health and childhood socio-economic status, making it well-suited for the analysis of the long-term consequences of life course events (Brandt & Hank, 2014; Schröder, 2013).

To warrant high quality retrospective data, a life grid was used (Blane, 1996), taking respondents step by step through the questionnaire, starting with modules on children and partners. In the CAPI, dates of births and marriages were represented in a calendar, serving as anchor points for further questions (Schröder, 2013). The work history module collected data for every job lasting at least six month including the start and end, job characteristics and the reason

[3] Theoretically, the job quality is also of interest. However, the data only allow for tests concerning unemployment as well as employment instability across workers' subsequent career. This issue is also revisited in the conclusion.

[4] In wave 2, three new countries entered SHARE and refreshment samples were drawn. The respective average response rates were 61 percent and 54 percent. As SHARE is a multinational survey the sampling procedures differ between countries (see Börsch-Supan et al., 2013 for details).

for its end. If the gap between two jobs was six months or longer, respondents were asked for their activity status in between. Moreover, the activity statuses between leaving education and the first job as well as after the last job were reported.

For the analyses, the sample was restricted to persons 50 years and over reporting at least one job spell. Because the analyses focused on early-career involuntary job losses, only persons who started their first job between the ages 14 to 35 years were considered.[5] In addition, persons who were self-employed or retired in their early-career were excluded. Job losses of these persons are unlikely to be exogenous (Schröder, 2013). Moreover, information on whether any early-career job ended due to an illness or disability was exploited (see Burgard et al., 2007 for a similar approach). Excluding such health-induced job losses further addresses concerns about health selection issues.[6] Lastly, only workers with an interview at least 15 years after the early-career were considered, to ensure the analyses' focus on the long-term effects of job loss on health. The resulting sample provided complete information on 16,826 persons, including 946 early-career involuntary job losses.

3.2 Measures

Table S1 in the supplementary materials provides details about the measurement. Tables 1 and 2 offer descriptive statistics. The *independent variable* early-career involuntary job loss was defined as any job loss due to layoff or plant closure within the first 10 years after the labour market entry (see footnote 2). The other reasons for separation either reflected voluntary job changes (i.e. resignation) or ambiguous situations (i.e. mutual agreement, temporary job completion, other). Because previous studies used different 'control groups', persons with involuntary job losses were compared with two different groups. First, they were compared with persons who did not experience any involuntary job loss in the first 10 years after starting their first job. Second, a comparison group of persons who did not experience involuntary job loss and, in addition, was continuously employment throughout their early-career was used.

The *dependent variable* self-rated health is an overall measure of health status with answers ranging from 1 'excellent' to 5 'poor' on a five-point scale. For the analyses, the responses were grouped into a binary variable in order to focus on the key contrast between bad ('fair', 'poor') and good health ('excellent', 'very good', 'good'). A sensitivity analysis using the

[5] Moreover, only persons who left education between the ages 11 to 35 years were considered, because some control variables only pertained to persons who were in school at age 10 years.
[6] Of the reported early-career involuntary job losses less than 1 percent were also due to illness or disability.

five-point self-rated health scale is reported in Table S4 in the supplementary materials. Respondents have been found to take into account a wide range of health factors, including both aspects of physical as well as mental health, in forming an assessment of their own health (see Gabarski, 2016 for a research synthesis). Moreover, it has been shown that self-rated health is a valid and reliable measure of overall health and is predictive of mortality even after adjusting for specific health measures and other covariates (e.g. Idler & Benyamini, 1997), making it well-suited for our analyses.

To further address the issues of health selection and confounding, several sets of *control variables* were selected assuming that they affect both the risk of involuntary job loss and the late life health. In the displacement literature, for example, poor health or work performance are discussed as factors that drive the selection into involuntary job loss and, in particular, layoffs (e.g. Baumann et al., 2016). Variables that are consequences of job loss were not considered as controls, but are discussed as mechanisms below. Besides country of residence, age at the interview, sex, and years of education, childhood health (age 0 to 15 years) and childhood socio-economic status (age 10 years) were considered relevant (e.g. Daly & Delaney, 2013). Specifically, respondents' reported whether they were in fair or poor health, stayed for one month or more in hospital, or had one or more illnesses according to two check lists of illnesses during childhood.[7] In addition, they indicated whether their parents smoked or drank heavily. Controlling for these factors should, in particular, reduce concerns about health selection. Furthermore, childhood socio-economic status was measured by whether the home was poorly equipped, the number of books that were available as well as the number of persons per room and the occupation of the main breadwinner. A home was poorly equipped if it had none of the following five amenities: a fixed bath, cold or hot running water, an inside toilet, central heating. The occupation of the main breadwinner was based on the four skill levels associated with the ISCO-88 major groups (ILO, 1990). Furthermore, respondents reported their relative position in math and language at age 10 compared to their classmates. Next to the socio-demographic variables, childhood socio-economic status and school performance are important factors for individuals' career choices and, thus, their risk of involuntary job loss. They are also likely factors that determine later life health. The last set of control varia-

[7] The illnesses on list 1 included: infectious disease (e.g. measles, rubella, chickenpox, mumps, tuberculosis, diphtheria, scarlet fever), polio, asthma, respiratory problems other than asthma, allergies (other than asthma), severe diarrhoea, meningitis/encephalitis, chronic ear problems, speech impairment, difficulty seeing even with eyeglasses. The illnesses on list 2 included: severe headaches or migraines, epilepsy, fits or seizures, emotional, nervous, or psychiatric problem, broken bones, fractures, appendicitis, childhood diabetes or high blood sugar, heart trouble, leukaemia or lymphoma, cancer or malignant tumour (excluding minor skin cancers), other serious health condition.

bles concerned characteristics at the first job. Because these characteristics precede any involuntary job loss, they allow further reducing heterogeneity between those workers who experience a job loss and those who do not. The importance of these confounding variables is highlighted by studies suggesting that layoffs, but also plant closures occur more often in industries "that are more vulnerable to economic and structural problems" (Baumann et al., 2016, p. 161). In our data, the type of employment, the occupation, the sector and working time were reported allowing us to control for some associated factors. In addition, measures of whether the person lived with a partner or had children at the first job were constructed. Lastly, the age at the first job and the labour market entry cohort were controlled for.

To assess workers' subsequent careers, the following *mediating variables*, concerning the years 11 to 25 of the careers, were used. Subsequent unemployment risks were measured by the cumulated unemployment duration in years and employment instability was indicated by the number of job ends and the number of involuntary job losses. Because previous research suggests that career complexity is best measured by analyses of holistic trajectories as compared to single states (Manzoni & Mooi-Reci, 2011), in addition, a complexity index based on a sequence analysis of workers' activity statuses in years 11 to 25 of their careers was constructed. The complexity index proposed by Gabadinho et al. (2011) ranges from zero (no complexity) to one (maximum complexity) and is a composite measure of the number of transitions and the longitudinal entropy. The latter measures the diversity of states within a sequence meaning that, overall, careers with more transitions and greater variety in activity statuses were considered more complex.[8]

3.3 Methods

To estimate the effect of an early-career involuntary job loss on later life health, logistic multiple regression models with cluster-robust standard errors by country were used.[9] Because the coefficients of logistic regression models cannot be easily interpreted and can also not be compared across nested models (Mood, 2010), average marginal effects (AME) are reported. Three different models were fitted. Model 1 was a bivariate regression of fair or poor health

[8] The following activity statuses were used: unemployed, retired, training and education, domestic work, other and working. Overlaps were solved using the listed order. For less than 1 percent of persons, gaps have been filled using the preceding activity status (Kröger, 2015). The complexity index was constructed using the TraMiner library in R (Gabadinho et al., 2011). The data preparation and all analyses were performed in Stata 14.

[9] As the SHARELIFE data include variables about self-rated health in both childhood and adulthood, it may be asked why the advantages of panel data regression models were not utilised (i.e. differencing out time-constant unobserved heterogeneity using, for example, change score models). Although the measures of self-rated health concerning early and later life are similar, they differ in their reference period as well as the allowed responses meaning that we do not measure the same dependent variable over time.

on early-career involuntary job loss. Model 2 added the control variables and Model 3, in addition, included the mediating variables to examine to what extent the total effect (Model 2) was operating through channels of increased subsequent unemployment risks and employment instability. The analyses were first performed for all involuntary job losses combined. In addition, they were repeated using an indicator variable to distinguish layoffs and plant closure.

4. Results

4.1 Descriptive findings

Table 1 provides details about the early-career involuntary job losses. Of the 946 involuntary job losses about 70 percent (N=657) were due to layoffs while the remaining 30 percent (N=289) followed plant closures. A minority of job losses were experienced by civil servants. The median job loss concerned the first job, the fifth year of the career and happened at the age of 22 years in 1969, showing that the analysed job losses pertain to workers' early careers. The median time to the interview was 39 years emphasising the analyses' focus on the long-term effects of job loss on health.

Table 1 Descriptive statistics on early-career involuntary job losses

	Median/Percent	N	Minimum	Maximum
Reason for job loss				
Layoff	69.5%	657		
Plant closure	30.5%	289		
Type of employment				
Employee	95.8%	906		
Civil servant	4.2%	40		
Job	1		1	6
Year in the early career	5		1	10
Age	22		14	40
Year	1969		1931	1992
Time to interview (in years)	39		16	77

Notes: See Table S1 in the supplementary material for further details on the measurement.
Sources: SHARELIFE, authors' calculations.

Table 2 offers descriptive statistics on the dependent, control and mediating variables separating workers who experienced a job loss and those who did not. Persons who involuntarily lost a job in their early career more often reported to be in fair or poor health at the time of the interview, illustrating the potential for negative long-term effects.

For the majority of the control variables, the mean standardized difference between the two groups was small to moderate. However, for some variables, relevant differences were revealed. Specifically, persons with involuntary job losses were younger and less educated. Although they also consistently reported worse childhood health, these differences were small to moderate. Moreover, the parents of persons with involuntary job losses were more likely to smoke and drink heavily during the respondents' childhood. Regarding the childhood socio-economic status, persons with a job loss more often lived in households with none or very few books. The respective main breadwinners were less likely to hold a medium-skilled (skill level 2) and more likely to hold a lower-skilled (skill level 1) occupation. Persons with an involuntary job loss also reported to be worse or much worse compared to their classmates in math and languages. Moreover, they were less likely to be civil servants as well as less likely to be in higher-skilled (skill level 4) and more likely to be in lower-skilled (skill level 1) occupations at their first job. Their first job was also more likely in the manufacturing and energy sectors and less likely in the public, health or education sectors. Finally, they were younger at the time of labour market entry and were (under-) overrepresented in (older) younger labour market entry cohorts. Overall, the descriptive findings suggest that persons who experienced a job loss and those who did not differ on a number of characteristics that have to be controlled for to ensure that the estimated associations between job losses and health are not spurious.

Furthermore, Table 2 shows the differences in the mediating variables concerning years 11 to 25 of workers' careers. As expected, early-career involuntary job losses were associated with longer subsequent unemployment durations, more job ends, more involuntary job losses, and a higher career complexity. However, Table 2 also reveals that the careers in the sample analysed were overall quite stable and that the differences by involuntary job loss were moderate.

Table 2 Descriptive statistics on the dependent, control, and mediating variables

	Early-career involuntary job loss?			
	No (N=15880)		Yes (N=946)	
	Mean/Percent	SD	Mean/Percent	SD
<u>Dependent variable</u>				
Fair or poor self-rated health (at the interview)	34.9%		42.1%	
<u>Control variables</u>				
Demographics and education				
Age at interview (in years)	64.89	9.09	63.56	9.62
Female	53.0%		52.7%	
Education (in years)	13.68	3.64	13.00	3.31
Childhood health (age 0-15)				
Fair or poor self-rated health	7.9%		8.7%	
1+ month in hospital	6.5%		8.1%	
1+ illness(es) from list 1	84.6%		86.7%	
1+ illness(es) from list 2	25.1%		26.2%	
Parents health behaviour				
Parents smoked	63.0%		71.9%	
Parents drank heavily	7.5%		12.5%	
Childhood socio-economic status (age 10)				
Poorly equipped home	21.6%		22.0%	
Number of books				
None or very few	33.5%		40.8%	
Enough to fill one shelf	25.3%		22.2%	
Enough to fill one bookcase	25.7%		22.9%	
Enough to fill two or more book cases	15.5%		14.1%	
Persons per room				
≤ 1 person	29.7%		29.9%	
> 1 and ≤2 persons	48.5%		48.3%	
> 2 persons	21.8%		21.8%	
Occupation breadwinner				
Major group 1 (no skill level)	5.3%		4.3%	
Major group 2 (skill level 4)	4.7%		3.7%	
Major group 3 (skill level 3)	5.6%		6.1%	
Major group 4-8 (skill level 2)	65.7%		59.8%	
Major group 9 (skill level 1)	15.4%		22.5%	
Major group 0 (no skill level)	1.9%		1.6%	
No breadwinner	1.5%		1.9%	
Relative position in math				
Much better	12.0%		11.0%	
Better	26.6%		23.0%	
About the same	50.0%		50.6%	
Worse or much worse	11.4%		15.3%	
Relative position in language				
Much better	12.0%		11.0%	
Better	28.9%		25.3%	
About the same	48.5%		48.5%	

Table 2 continued

Worse or much worse	10.6%		15.2%	
Characteristics (at) first job				
Civil servant	10.6%		3.7%	
Occupation				
Major group 1 (no skill level)	2.0%		0.5%	
Major group 2 (skill level 4)	10.0%		5.6%	
Major group 3 (skill level 3)	9.5%		7.1%	
Major group 4-8 (skill level 2)	58.8%		55.1%	
Major group 9 (skill level 1)	18.3%		30.5%	
Major group 0 (no skill level)	1.5%		1.2%	
Sector				
Primary sector	8.1%		7.2%	
Manufacturing and energy	24.0%		32.3%	
Construction	7.6%		8.7%	
Services	35.5%		38.9%	
Public sector, health, and education	24.8%		12.9%	
Full-time	96.7%		97.3%	
Living with a partner	12.7%		12.2%	
Children	5.7%		6.8%	
Labour market entry				
< 1950	11.5%		13.3%	
1950-59	23.2%		17.5%	
1960-69	38.4%		36.2%	
≥ 1970	26.9%		33.0%	
Age at first job	18.69	3.75	18.05	3.58
Mediating variables				
Unemployment duration (in years)	0.18	1.15	0.74	2.39
Employment instability (year 11-25)				
Number of job ends	0.65	0.97	0.93	1.15
Number of involuntary job losses	0.08	0.30	0.23	0.54
Complexity index (0-1)	0.04	0.08	0.06	0.10

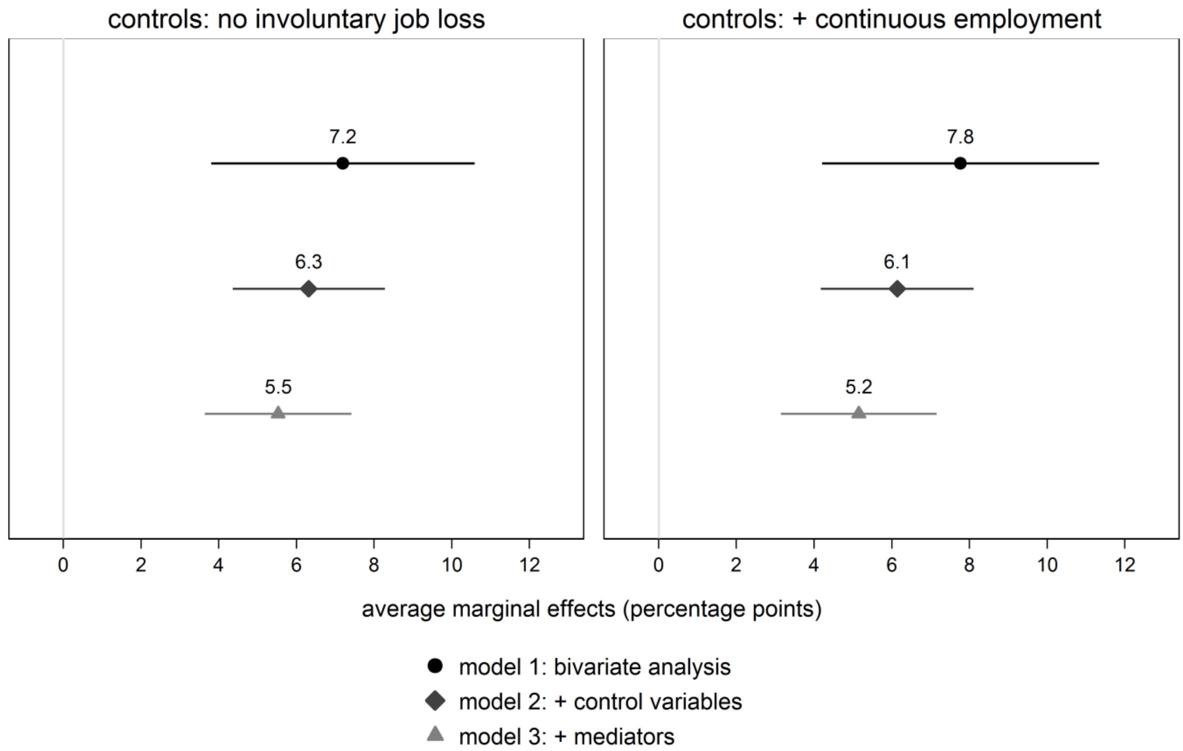
Notes: See Table S1 in the supplementary material for further details on the measurement.
Sources: SHARELIFE, authors' calculations.

4.2 Multivariate findings

The results of the bivariate and multiple logistic regression analyses are illustrated graphically and average marginal effects (AME) with 90 percent confidence intervals are presented (Jann, 2014). The full regression tables are provided in the supplementary materials. Model 1 in Figure 1 (left side, Table S2.1) shows that an involuntary job loss compared to no job loss in the early-career was associated with a 7.2 percentage point higher probability to report fair to poor self-rated health. After adjusting for country of residence, demographics and education, childhood health, parents' health behaviour, childhood socio-economic status and characteristics at the first job, the estimated total effect was reduced to 6.3 percentage points (Model 2).

This suggests that despite extensive controls for health selection and confounding, an early-career involuntary job loss is negatively associated with later life health.

Figure 1 Effects of an early-career involuntary job loss on fair or poor self-rated health in late life (average marginal effects, 90% confidence intervals)



Notes: See Table S2.1 (left side) and Table S2.2 (right side) in the supplementary material for the full logistic regression models.
Sources: SHARELIFE (N=16,826), authors' calculations.

Similar results were found using the five-point self-rated health scale and applying models for ordinal or continuous dependent variables. Table S4 in the supplementary materials compares the results for Model 2 using a binary logistic regression (see Figure 1) to the findings from an ordinal logistic and a linear regression model. In Model 3, the mediating variables indicating subsequent unemployment risks and employment instability were added, to test to what extent the negative effect of job loss was explained by these factors. Although the total effect further diminished to 5.5 percentage points (direct effect), the proportion mediated (PM = (6.3-5.5)/6.3 = 0.127) only amounted to about 13 percent.[10]

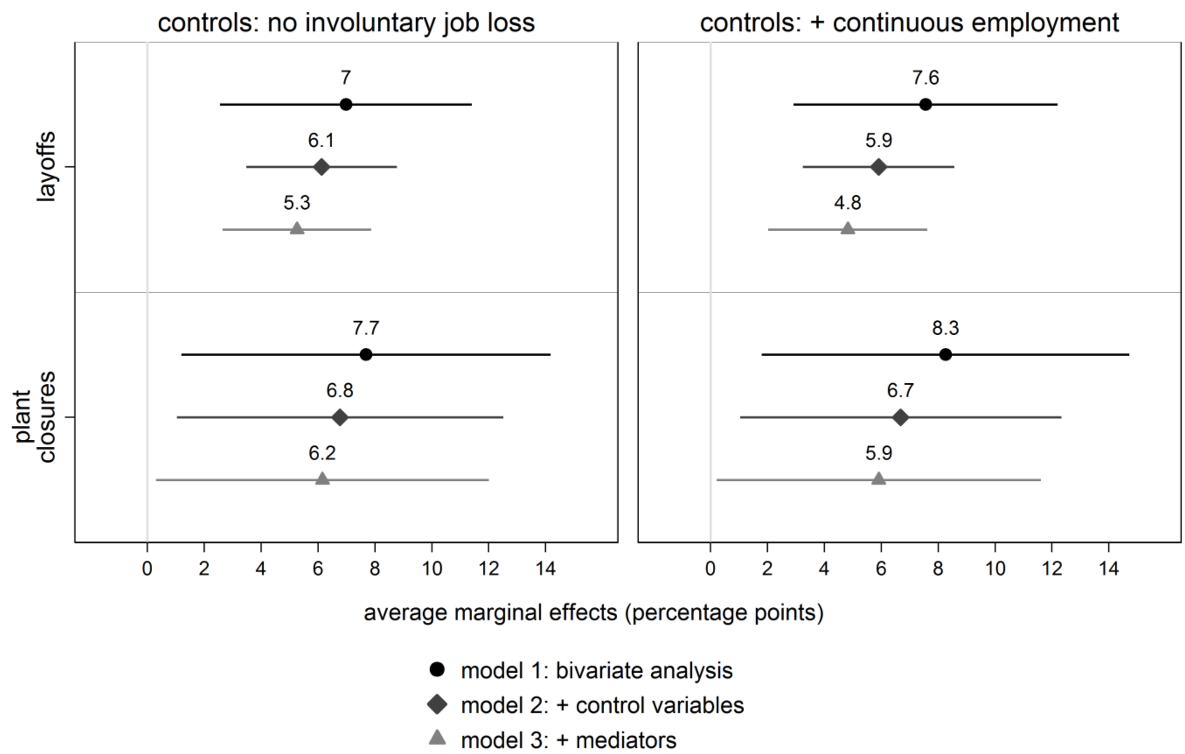
On the right side of Figure 1 (Table S2.2), the results are shown for the analyses using an alternative control group of persons who did not experience job loss and were, in addition, con-

[10] The variables measuring employment instability may be more meaningful if the employment duration in years 11-25 is controlled. The results were very similar if in addition to the mediating variables the employment duration is included in Model 3.

tinuously employed throughout their early career. Overall the estimated average marginal effects were very similar, showing that the findings are not sensitive to the definition of the control group. The estimated total and direct effects of 6.1 and 5.2 percentage points were slightly smaller and the proportion mediated was about 15 percent. Even if compared to continuously employed persons, the subsequent unemployment risks and employment instability only explained a small share of the total effect of job loss on later life health. In summary, these results support hypothesis 1. Hypothesis 2 can only partially be confirmed, as the subsequent unemployment risks and employment instability seem to play a limited role in the mediation of the total effect.

Figure 2 shows the results of the analyses using an indicator variable to further distinguish layoffs from plant closures. The upper and lower parts show the results for layoffs and plant closures, respectively. As stated before, job losses due to plant closure can be considered to be 'largely beyond the control of individual workers' (Brand, 2015, p. 360) and, thus, provide a good sensitivity test for the above reported findings.

Figure 2 Effects of an early-career involuntary job loss on fair or poor self-rated health in late life by reason for job loss (average marginal effects, 90% confidence intervals)



Notes: See Table S3.1 (left side) and Table S3.2 (right side) in the supplementary material for the full logistic regression models.
Sources: SHARELIFE (N=12,135), authors' calculations.

On the left side of Figure 2 (Table S3.1), results are shown for the control group of persons who did not experience involuntary job loss in their early career. Overall, the effects were very similar to those for the combined analyses and the differences between layoffs and plant closures were relatively small, with somewhat higher point estimates for the latter. Wald tests comparing the effects of job losses due to layoff and plant closure revealed that the differences were not statistically significant (model 1: p=0.89, model 2: p=0.88, model 3: p=0.84). Moreover, given the fewer job losses due to plant closure, the respective effects were estimated with greater uncertainty, as indicated by the wide 90 percent confidence intervals. The proportion mediated was estimated to be 13 percent for layoffs and 9 percent for plant closures, confirming the previous findings that the subsequent unemployment risks and employment instability only mediate a small share of the total effect.

The right side of Figure 2 (Table S3.2) shows the results for the analyses using the alternative control group. As for the combined analyses, the results were very similar for the different control groups, with somewhat smaller estimates for the total and direct effects of job loss and slightly larger estimates for the proportion meditated (i.e. 19 percent for layoffs, 12 percent for plant closures). Again, Wald tests of whether the effects of layoffs and plant closures differ were not statistically significant (model 1: p=0.89, model 2: p=0.86, model 3: p=0.81). Overall, the results suggest that an early-career job loss had a moderate negative effect on the probability to be in fair or poor health in late life. This finding was robust to the control group used as well as the reasons for involuntary job loss considered. The latter result is particularly relevant, as the use of plant closures should further reduce concerns about health selection and confounding.

5. Conclusions

This article contributes to the growing literature on the long-term effects of job loss and unemployment on health (Daly & Delaney, 2013; Schröder, 2013) and, in particular, complements previous studies on the lasting negative health consequences of youth unemployment (Bell & Blanchflower, 2011; Brydsten et al., 2015; Strandh et al., 2014). Specifically, it examines the effects of involuntary job loss in the first ten years after labour market entry on self-rated health measured for more than 30 years later. In addition, it takes up the increased interest in the mechanisms behind this relationship (e.g. Strandh et al., 2014) by analysing to what extent the negative effects are mediated through channels such as increased subsequent unemployment risks and employment instability.

Using detailed retrospective data from the third wave (2008/09) of the Survey of Health, Ageing and Retirement in Europe (SHARE) the analysis shows that workers' who involuntarily lost a job in their early career have on average a 6 percentage point higher probability to report fair or poor health at the time of the interview. Comparing the size of this effect to other effects in the same model, it is about as large as the effect of having five years less of education.[11] These results are based on analyses that carefully control for health selection and confounding. Specifically, health-induced job losses due to illness or disability were excluded beforehand and the regression models adjusted for demographics and education, childhood health, childhood socio-economic status as well as characteristics at the first job. Moreover, it was revealed that the effects are very similar for involuntary job losses due to layoffs and plant closures. As in plant closures almost all workers are let go, it is less likely that the corresponding job loss is due to health or other observed and unobserved characteristics (Strully, 2009). This should further reduce concerns that the revealed associations are spurious. The finding of moderate negative long-term consequences is also in line with the recent evidence from British and Swedish cohort studies showing negative effects of youth unemployment on functional somatic symptoms, life satisfaction, mental health, and self-rated health (Bell and Blanchflower, 2011; Brydsten et al., 2015; Strandh et al., 2014).

Moreover, this article also contributes to our understanding of the mechanisms behind these long-run relationships. Additional analyses controlling for indicators of unemployment risks and employment instability concerning workers' subsequent careers showed that the total effect of an early-career involuntary job loss was only reduced by about 10 to 15 percent. This is consistent with findings by Brydsten et al. (2015), showing that the association between youth unemployment and later health remained similar after adjusting for later unemployment.

There remain, however, some important caveats to the current analyses. First, this article used retrospective data, implying, that measures of childhood health and childhood socio-economic status are not as specific as, for example, in prospective cohort studies. In addition, although the SHARELIFE survey was carefully planned and techniques to improve data quality, such as the life grid, were used (Schröder, 2011), the results may be affected by recall errors. However, a recent study by Havari and Mazzonna (2015) assesses the internal and external con-

[11] Although such a comparison may help in judging the size of effects, we note, that it must be interpreted with caution as for involuntary job loss a total effect is estimated, while for years of education a direct effect is estimated as the model already controls for variables that mediate the effect of education on health.

sistency of the SHARE data, offering empirical evidence about the importance of this issue. They conclude that respondents remember childhood health and living conditions fairly well.

Second, because panel data with a comparable time window are not available, the analyses addressed issues of health selection and confounding by adjusting for a large number of observed variables. Moreover, the use of involuntary job losses and analyses taking into account the reason for job loss provide some evidence that the found associations are not spurious. However, even for involuntary job losses due to plant closures, selection may still be a problem as closures are anticipated to some extent and specific workers' may (be) select(ed) out of plants before they close (Baumann et al., 2016). For example, Schwerdt (2011, p. 93) describes two scenarios: one in which positively selected workers leave early and one in which negatively selected workers are dismissed by the management. Using Austrian administrative data he finds support for the former scenario by examining pre- and post-separation labour market outcomes. As we cannot observe such strategic behaviour in our data, the current analyses rest on the assumption, that these issues are sufficiently addressed by controlling for observed variables. If, however, 'more qualified and adaptive employees' leave early (Brand, 2015, p. 362) and these workers are also more healthy, our results may overestimate the long-term negative health effects of job loss.[12]

Lastly, our results, in particular, the finding that the subsequent unemployment risks and employment instability only mediated a small share of the total effect of job loss on health, may not necessarily generalise beyond the cohorts represented in the data.[13] Most of the respondents in the SHARELIFE survey entered the labour market under good economic conditions implying that it was easier to avoid long interruptions after a job loss, as a sufficient number of adequate jobs for reemployment were available (Schröder 2013). However, the results at least suggest that concerns about the potential long-term consequences should be taken seriously and more empirical evidence needs to be accumulated.

Overall, this article points to the importance of workers' early career experiences for later life health, highlighting the potential long-term costs of involuntary job loss. The finding that the unemployment risks and employment instability across workers' subsequent careers played only a limited role in explaining the negative health effects, merits attention in future re-

[12] The selection out of plants before they close may, however, be less of a problem in survey data as workers who leave early are likely to report that their job ended due to an (upcoming) plant closure.

[13] As highlighted in the introduction current studies on the long-term consequences necessarily have to use data about older birth cohorts.

search. Specifically, an investigation on whether this result generalises to other cohorts as well as whether other mechanisms, such as the subsequent job quality, are important, seems worth examining.

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7. Supplementary material (online only)

Table S1	Measurement of variables
Table S2.1	Logistic regression models for Figure 1 (left side), fair or poor self-rated health, average marginal effects
Table S2.2	Logistic regression models for Figure 1 (right side), fair or poor self-rated health, average marginal effects
Table S3.1	Logistic regression models for Figure 2 (left side), fair or poor self-rated health, average marginal effects
Table S3.2	Logistic regression models for Figure 2 (right side), fair or poor self-rated health, average marginal effects
Table S4	Sensitivity analyses using the five-point self-rated health scale and applying ordinal logistic as well as linear regression (model 2 in Table S2.1)

Table S1 Measurement of variables

Variable	Measurement
<u>Dependent variable</u>	
Fair or poor self-rated health	1 = fair, poor, 0 = excellent, very good, good; at the SHARELIFE interview
<u>Independent variable</u>	
Early-career involuntary job loss	
Control 1: no involuntary job loss	1 = at least one job left due to layoff or plant closure, 0 = no job left due to layoff or plant closure, jobs left due to resignation, mutual agreement, temporary job completion, and other reasons are allowed; in the first 10 years after labour market entry
Control 2: + continuous employment	Control 1 + continuous employment; in the first 10 years after labour market entry
Layoffs vs. plant closures	The same measurements as above, but layoffs and plant closures are separate categories
<u>Control variables</u>	
Demographics and education	
Age at interview	Age in years; at the SHARELIFE interview
Female	1 = female, 0 = male
Education	Education in years
Childhood health	
Fair or poor self-rated health	1 = fair, poor, 0 = excellent, very good, good, varied a great deal; age 0-15
1+ month in hospital	1 = in hospital for one month or more, 0 = otherwise; age 0-15
1+ illness(es) from list 1[a]	1 = one or more illness(es) from list 1, 0 = otherwise; age 0-15
1+ illness(es) from list 2[b]	1 = one or more illness(es) from list 2, 0 = otherwise; age 0-15
Parents health behaviour	
Parents smoked	1 = parents smoked, 0 = otherwise; age 0-15
Parents drank heavily	1 = parents drank heavily, 0 = otherwise; age 0-15
Childhood socio-economic status	
Poorly equipped home	1 = no fixed bath, cold or hot running water supply, inside toilet, and central heating, 0 = otherwise; age 10
Number of books	1 = none or very few (0-10 books), 2 = enough to fill one shelf (11-25 books), 3 = enough to fill one bookcase (26-100 books), 4 = enough to fill two (or more) book cases (101-200 books, more than 200 books); age 10
Persons per room	1 = ≤ 1 person, 2 = > 1 and ≤ 2 persons, 3 = > 2 persons

Table S1 continued

Occupation breadwinner	Based on ISCO-88 major groups (mg) and skill levels (sl), 1 = mg 1 (no sl), 2 = mg 2 (sl 4), 3 = mg 3 (sl 3), 4 = mg 4-8 (sl 2), 5 = mg 9 (sl 1), 6 = mg 0 (no sl), 7 = no breadwinner; age 10
Relative position in math	Compared to classmates, 1= much better, 2 = better, 3 = about the same, 4 = (Much) worse
Relative position in language	Compared to classmates, 1= much better, 2 = better, 3 = about the same, 4 = (Much) worse
Characteristics (at) first job	
Civil servant	1 = civil servant, 0 = employee; first job
Occupation	Based on ISCO-88 major groups (mg) and skill levels (sl), 1 = mg 1 (no sl), 2 = mg 2 (sl 4), 3 = mg 3 (sl 3), 4 = mg 4-8 (sl 2), 5 = mg 9 (sl 1), 6 = mg 0 (no sl); first job
Sector	1 = primary sector, 2 = manufacturing and energy, 3 = construction, 4 = services, 5 = public sector, health, and education; first job
Full-time	1 = full-time job, 0 = part-time job; first job
Living with a partner	1 = Living with a married or unmarried partner, 0 = otherwise; at the first job
Children	1 = At least one natural or adopted child, 0 = otherwise; at the first job
Labour market entry	1 = < 1950, 2 = 1950-1959, 3 = 1960-1969, 4 = ≥ 1970
Age at first job	Age in years; at the first job
<u>Mediating variables</u>	
Unemployment duration	Unemployed duration in years; in year 11 to 25
Employment instability	
Number of job ends	Number of jobs that ended; in year 11 to 25
Number of involuntary job losses	Number of involuntary job losses (layoff, plant closure); in year 11 to 25
Complexity index	Gabadinho et al.'s (2010) complexity index (0-1); for year 11-25

Notes: a Illnesses on list 1: infectious disease (e.g. measles, rubella, chickenpox, mumps, tuberculosis, diphtheria, scarlet fever), polio, asthma, respiratory problems other than asthma, allergies (other than asthma), severe diarrhoea, meningitis/encephalitis, chronic ear problems, speech impairment, difficulty seeing even with eyeglasses; b Illnesses on list 2: severe headaches or migraines, epilepsy, fits or seizures, emotional, nervous, or psychiatric problem, broken bones, fractures, appendicitis, childhood diabetes or high blood sugar, heart trouble, leukaemia or lymphoma, cancer or malignant tumour (excluding minor skin cancers), other serious health condition.
Sources: Own illustration.

Table S2.1 Logistic regression models for Figure 1 (left side), fair or poor self-rated health, average marginal effects

	Model 1		Model 2		Model 3	
	AME	SE	AME	SE	AME	SE
Independent variable						
Early-career involuntary job loss (Ref. no involuntary job loss)	0.072 ***	(0.02)	0.063 ***	(0.01)	0.055 ***	(0.01)
Control variables						
Country (Ref. Austria)						
Germany			0.129 ***	(0.01)	0.125 ***	(0.01)
Sweden			-0.001	(0.01)	-0.005	(0.01)
Netherlands			0.002	(0.01)	-0.001	(0.01)
Spain			0.106 ***	(0.01)	0.108 ***	(0.01)
Italy			0.044 ***	(0.01)	0.046 ***	(0.01)
France			0.054 ***	(0.01)	0.053 ***	(0.01)
Denmark			-0.031 ***	(0.01)	-0.036 ***	(0.01)
Greece			-0.100 ***	(0.01)	-0.098 ***	(0.01)
Switzerland			-0.075 ***	(0.01)	-0.075 ***	(0.01)
Belgium			-0.015 **	(0.01)	-0.014 **	(0.01)
Czech Republic			0.140 ***	(0.01)	0.147 ***	(0.01)
Poland			0.290 ***	(0.01)	0.288 ***	(0.01)
Ireland			-0.085 ***	(0.00)	-0.088 ***	(0.00)
Demographics and education						
Age at interview (in years)			0.006 ***	(0.00)	0.007 ***	(0.00)
Female (Ref. male)			0.022 *	(0.01)	0.007	(0.01)
Education (in years)			-0.012 ***	(0.00)	-0.011 ***	(0.00)
Childhood health (age 0-15)						
Fair or poor self-rated health (Ref. otherwise)			0.173 ***	(0.02)	0.171 ***	(0.02)
1+ month in hospital (Ref. otherwise)			0.036 **	(0.01)	0.036 **	(0.01)
1+ illness(es) from list 1 (Ref. otherwise)			0.008	(0.01)	0.008	(0.01)

Table S2.1 continued

1+ illness(es) from list 2 (Ref. otherwise)	0.024	(0.01)	0.023	(0.01)
Parents health behaviour				
Parents smoked (Ref. otherwise)	0.019 *	(0.01)	0.019 *	(0.01)
Parents drank heavily (Ref. otherwise)	0.058 ***	(0.01)	0.055 ***	(0.01)
Childhood socio-economic status (age 10)				
Poorly equipped home (Ref. otherwise)	0.001	(0.02)	0.001	(0.02)
Number of books (Ref. None or very few)				
Enough to fill one shelf	-0.020 **	(0.01)	-0.020 **	(0.01)
Enough to fill one bookcase	-0.027 **	(0.01)	-0.027 **	(0.01)
Enough to fill two or more book cases	-0.017	(0.01)	-0.018	(0.01)
Persons per room (Ref. ≤ 1 person)				
> 1 and ≤2 persons	0.004	(0.01)	0.003	(0.01)
> 2 persons	0.028 ***	(0.01)	0.026 ***	(0.01)
Occupation breadwinner (Ref. major group 1 (no skill level))				
Major group 2 (skill level 4)	0.004	(0.03)	0.005	(0.03)
Major group 3 (skill level 3),	0.014	(0.02)	0.016	(0.02)
Major group 4-8 (skill level 2)	0.034 *	(0.01)	0.036 **	(0.01)
Major group 9 (skill level 1)	0.047 *	(0.02)	0.048 **	(0.02)
Major group 0 (no skill level)	0.071 *	(0.03)	0.072 **	(0.03)
No breadwinner	0.016	(0.03)	0.016	(0.03)
Relative position in math (Ref. much better)				
Better	0.006	(0.01)	0.005	(0.01)
About the same	0.021	(0.01)	0.020	(0.01)
Worse or much worse	0.046 **	(0.02)	0.045 **	(0.02)
Relative position in language (Ref. much better)				
Better	-0.011	(0.02)	-0.010	(0.02)
About the same	-0.003	(0.02)	-0.002	(0.02)
Worse or much worse	0.001	(0.02)	0.002	(0.02)

Table S2.1 continued

Characteristics (at) first job					
Civil servant (Ref. employee)		-0.008	(0.01)	-0.007	(0.01)
Occupation (Ref. major group 1 (no skill level))					
Major group 2 (skill level 4)		0.015	(0.02)	0.016	(0.02)
Major group 3 (skill level 3)		0.034	(0.03)	0.034	(0.03)
Major group 4-8 (skill level 2)		0.047	(0.03)	0.045	(0.03)
Major group 9 (skill level 1)		0.068 **	(0.03)	0.064 *	(0.03)
Major group 0 (no skill level)		0.058	(0.05)	0.054	(0.05)
Sector (Ref. primary sector)					
Manufacturing and energy		-0.028	(0.02)	-0.029	(0.02)
Construction		-0.001	(0.02)	-0.002	(0.01)
Services		-0.018	(0.02)	-0.018	(0.02)
Public sector, health, and education		-0.032 *	(0.01)	-0.030 *	(0.01)
Full-time (Ref. part-time)		0.016	(0.02)	0.016	(0.02)
Living with a partner (Ref. otherwise)		0.029 *	(0.01)	0.031 *	(0.01)
Children (Ref. otherwise)		0.005	(0.02)	0.005	(0.02)
Labour market entry (Ref. < 1950)					
1950-59		-0.041 *	(0.02)	-0.042 **	(0.02)
1960-69		-0.050 *	(0.02)	-0.051 *	(0.02)
≥ 1970		-0.091 **	(0.03)	-0.091 **	(0.03)
Age at first job		0.002	(0.00)	0.002	(0.00)
<u>Mediating variables</u>					
Unemployment duration (in years) (year 11-25)				0.003	(0.00)
Employment instability (year 11-25)					
Number of job ends				-0.002	(0.01)
Number of involuntary job losses				0.010	(0.01)
Complexity index (0-1)				0.298 ***	(0.06)
Observations	16826	16826		16826	

Notes: See Table S1 for further details on the measurement, *** p < 0.001, ** p < 0.01, * p < 0.05, cluster-robust standard errors in parentheses.
Sources: SHARELIFE, authors' calculations.

Table S2.2 Logistic regression models for Figure 1 (right side), fair or poor self-rated health, average marginal effects

	Model 1		Model 2		Model 3	
	AME	SE	AME	SE	AME	SE
Independent variable						
Early-career involuntary job loss (Ref. no involuntary job loss + continuous employment)	0.078 ***	(0.02)	0.061 ***	(0.01)	0.052 ***	(0.01)
Control variables						
Country (Ref. Austria)						
Germany			0.110 ***	(0.01)	0.107 ***	(0.01)
Sweden			-0.010	(0.01)	-0.009	(0.01)
Netherlands			-0.006	(0.01)	-0.008	(0.01)
Spain			0.089 ***	(0.01)	0.089 ***	(0.01)
Italy			0.028 ***	(0.01)	0.029 ***	(0.01)
France			0.040 ***	(0.01)	0.042 ***	(0.01)
Denmark			-0.036 ***	(0.01)	-0.035 ***	(0.01)
Greece			-0.113 ***	(0.01)	-0.112 ***	(0.01)
Switzerland			-0.085 ***	(0.01)	-0.083 ***	(0.01)
Belgium			-0.019 *	(0.01)	-0.018 *	(0.01)
Czech Republic			0.123 ***	(0.01)	0.131 ***	(0.01)
Poland			0.272 ***	(0.01)	0.271 ***	(0.01)
Ireland			-0.089 ***	(0.00)	-0.095 ***	(0.00)
Demographics and education						
Age at interview (in years)			0.007 ***	(0.00)	0.007 ***	(0.00)
Female (Ref. male)			0.023 *	(0.01)	0.008	(0.01)
Education (in years)			-0.012 ***	(0.00)	-0.012 ***	(0.00)
Childhood health (age 0-15)						
Fair or poor self-rated health (Ref. otherwise)			0.165 ***	(0.03)	0.165 ***	(0.03)
1+ month in hospital (Ref. otherwise)			0.032	(0.02)	0.032	(0.02)
1+ illness(es) from list 1 (Ref. otherwise)			0.013	(0.01)	0.014	(0.01)

Table S2.2 continued

1+ illness(es) from list 2 (Ref. otherwise)	0.022	(0.02)	0.022	(0.02)
Parents health behaviour				
Parents smoked (Ref. otherwise)	0.029 **	(0.01)	0.029 **	(0.01)
Parents drank heavily (Ref. otherwise)	0.053 ***	(0.01)	0.051 **	(0.02)
Childhood socio-economic status (age 10)				
Poorly equipped home (Ref. otherwise)	-0.010	(0.02)	-0.010	(0.02)
Number of books (Ref. None or very few)				
Enough to fill one shelf	-0.022	(0.01)	-0.021	(0.01)
Enough to fill one bookcase	-0.031 *	(0.01)	-0.029 *	(0.01)
Enough to fill two or more book cases	-0.021	(0.01)	-0.021	(0.01)
Persons per room (Ref. ≤ 1 person)				
> 1 and ≤2 persons	0.011	(0.01)	0.011	(0.01)
> 2 persons	0.035 ***	(0.01)	0.034 ***	(0.01)
Occupation breadwinner (Ref. major group 1 (no skill level))				
Major group 2 (skill level 4)	-0.015	(0.03)	-0.013	(0.03)
Major group 3 (skill level 3),	-0.012	(0.03)	-0.011	(0.03)
Major group 4-8 (skill level 2)	0.004	(0.02)	0.005	(0.02)
Major group 9 (skill level 1)	0.016	(0.03)	0.017	(0.03)
Major group 0 (no skill level)	0.063	(0.03)	0.063 *	(0.03)
No breadwinner	-0.003	(0.04)	-0.002	(0.04)
Relative position in math (Ref. much better)				
Better	0.019	(0.02)	0.019	(0.02)
About the same	0.030 **	(0.01)	0.029 **	(0.01)
Worse or much worse	0.069 ***	(0.02)	0.067 ***	(0.02)
Relative position in language (Ref. much better)				
Better	-0.010	(0.02)	-0.008	(0.02)
About the same	0.008	(0.02)	0.007	(0.02)
Worse or much worse	0.006	(0.03)	0.007	(0.03)

Table S2.2 continued

Characteristics (at) first job					
Civil servant (Ref. employee)		-0.018	(0.01)	-0.019	(0.01)
Occupation (Ref. major group 1 (no skill level))					
Major group 2 (skill level 4)		0.014	(0.03)	0.015	(0.03)
Major group 3 (skill level 3)		0.025	(0.04)	0.026	(0.04)
Major group 4-8 (skill level 2)		0.046	(0.04)	0.046	(0.04)
Major group 9 (skill level 1)		0.061	(0.03)	0.059	(0.04)
Major group 0 (no skill level)		0.074	(0.05)	0.070	(0.05)
Sector (Ref. primary sector)					
Manufacturing and energy		-0.046 **	(0.01)	-0.047 **	(0.01)
Construction		-0.017	(0.02)	-0.019	(0.02)
Services		-0.032	(0.02)	-0.033	(0.02)
Public sector, health, and education		-0.055 **	(0.02)	-0.052 **	(0.02)
Full-time (Ref. part-time)		0.009	(0.03)	0.008	(0.03)
Living with a partner (Ref. otherwise)		0.011	(0.01)	0.014	(0.01)
Children (Ref. otherwise)		0.016	(0.02)	0.016	(0.02)
Labour market entry (Ref. < 1950)					
1950-59		-0.020	(0.02)	-0.020	(0.02)
1960-69		-0.037	(0.03)	-0.037	(0.03)
≥ 1970		-0.074 *	(0.04)	-0.073 *	(0.04)
Age at first job		0.003	(0.00)	0.003	(0.00)
<u>Mediating variables</u>					
Unemployment duration (in years) (year 11-25)				0.004	(0.00)
Employment instability (year 11-25)					
Number of job ends				-0.006	(0.01)
Number of involuntary job losses				0.005	(0.02)
Complexity index (0-1)				0.336 ***	(0.09)
Observations	12135	12135		12135	

Notes: See Table S1 for further details on the measurement, *** p < 0.001, ** p < 0.01, * p < 0.05, cluster-robust standard errors in parentheses.
Sources: SHARELIFE, authors' calculations.

Table S3.1 Logistic regression models for Figure 2 (left side), fair or poor self-rated health, average marginal effects

	Model 1		Model 2		Model 3	
	AME	SE	AME	SE	AME	SE
Independent variable						
Early-career involuntary job loss – layoff (Ref. no involuntary job loss)	0.070 **	(0.03)	0.061 ***	(0.02)	0.053 ***	(0.02)
Early-career involuntary job loss – plant closure	0.077	(0.04)	0.068	(0.03)	0.062	(0.04)
Control variables						
Country (Ref. Austria)						
Germany			0.129 ***	(0.01)	0.125 ***	(0.01)
Sweden			-0.001	(0.01)	-0.005	(0.01)
Netherlands			0.002	(0.01)	-0.001	(0.01)
Spain			0.106 ***	(0.01)	0.108 ***	(0.01)
Italy			0.044 ***	(0.01)	0.046 ***	(0.01)
France			0.054 ***	(0.01)	0.054 ***	(0.01)
Denmark			-0.031 ***	(0.01)	-0.035 ***	(0.01)
Greece			-0.100 ***	(0.01)	-0.098 ***	(0.01)
Switzerland			-0.074 ***	(0.01)	-0.075 ***	(0.01)
Belgium			-0.015 **	(0.01)	-0.014 **	(0.01)
Czech Republic			0.140 ***	(0.01)	0.147 ***	(0.01)
Poland			0.290 ***	(0.01)	0.288 ***	(0.01)
Ireland			-0.085 ***	(0.00)	-0.088 ***	(0.00)
Demographics and education						
Age at interview (in years)			0.006 ***	(0.00)	0.007 ***	(0.00)
Female (Ref. male)			0.022 *	(0.01)	0.007	(0.01)
Education (in years)			-0.012 ***	(0.00)	-0.011 ***	(0.00)
Childhood health (age 0-15)						
Fair or poor self-rated health (Ref. otherwise)			0.173 ***	(0.02)	0.171 ***	(0.02)
1+ month in hospital (Ref. otherwise)			0.037 **	(0.01)	0.036 **	(0.01)
1+ illness(es) from list 1 (Ref. otherwise)			0.008	(0.01)	0.008	(0.01)

Table S3.1 continued

1+ illness(es) from list 2 (Ref. otherwise)	0.024		(0.01)	0.023		(0.01)
Parents health behaviour						
Parents smoked (Ref. otherwise)	0.019	*	(0.01)	0.019	*	(0.01)
Parents drank heavily (Ref. otherwise)	0.058	***	(0.01)	0.055	***	(0.01)
Childhood socio-economic status (age 10)						
Poorly equipped home (Ref. otherwise)	0.001		(0.02)	0.001		(0.02)
Number of books (Ref. None or very few)						
Enough to fill one shelf	-0.020	**	(0.01)	-0.020	**	(0.01)
Enough to fill one bookcase	-0.027	**	(0.01)	-0.027	**	(0.01)
Enough to fill two or more book cases	-0.017		(0.01)	-0.018		(0.01)
Persons per room (Ref. ≤ 1 person)						
> 1 and ≤2 persons	0.004		(0.01)	0.003		(0.01)
> 2 persons	0.028	***	(0.01)	0.026	***	(0.01)
Occupation breadwinner (Ref. major group 1 (no skill level))						
Major group 2 (skill level 4)	0.004		(0.03)	0.005		(0.03)
Major group 3 (skill level 3),	0.014		(0.02)	0.016		(0.02)
Major group 4-8 (skill level 2)	0.034	*	(0.01)	0.036	**	(0.01)
Major group 9 (skill level 1)	0.047	*	(0.02)	0.048	**	(0.02)
Major group 0 (no skill level)	0.071	*	(0.03)	0.072	**	(0.03)
No breadwinner	0.016		(0.03)	0.016		(0.03)
Relative position in math (Ref. much better)						
Better	0.006		(0.01)	0.005		(0.01)
About the same	0.021		(0.01)	0.020		(0.01)
Worse or much worse	0.046	**	(0.02)	0.045	**	(0.02)
Relative position in language (Ref. much better)						
Better	-0.011		(0.02)	-0.009		(0.02)
About the same	-0.003		(0.02)	-0.002		(0.02)
Worse or much worse	0.001		(0.02)	0.002		(0.02)

Table S3.1 continued

Characteristics (at) first job						
Civil servant (Ref. employee)			-0.008	(0.01)	-0.007	(0.01)
Occupation (Ref. major group 1 (no skill level))						
Major group 2 (skill level 4)			0.015	(0.02)	0.016	(0.02)
Major group 3 (skill level 3)			0.034	(0.03)	0.034	(0.03)
Major group 4-8 (skill level 2)			0.047	(0.03)	0.045	(0.03)
Major group 9 (skill level 1)			0.068 **	(0.03)	0.064 *	(0.03)
Major group 0 (no skill level)			0.058	(0.05)	0.054	(0.05)
Sector (Ref. primary sector)						
Manufacturing and energy			-0.028	(0.02)	-0.029	(0.02)
Construction			-0.001	(0.02)	-0.002	(0.01)
Services			-0.018	(0.02)	-0.018	(0.02)
Public sector, health, and education			-0.032 *	(0.01)	-0.030 *	(0.01)
Full-time (Ref. part-time)			0.016	(0.02)	0.016	(0.02)
Living with a partner (Ref. otherwise)			0.029 *	(0.01)	0.031 *	(0.01)
Children (Ref. otherwise)			0.005	(0.02)	0.005	(0.02)
Labour market entry (Ref. < 1950)						
1950-59			-0.041 *	(0.02)	-0.042 **	(0.02)
1960-69			-0.050 *	(0.02)	-0.051 *	(0.02)
≥ 1970			-0.091 **	(0.03)	-0.091 **	(0.03)
Age at first job			0.002	(0.00)	0.002	(0.00)
Mediating variables						
Unemployment duration (in years) (year 11-25)					0.003	(0.00)
Employment instability (year 11-25)						
Number of job ends					-0.002	(0.01)
Number of involuntary job losses					0.010	(0.01)
Complexity index (0-1)					0.298 ***	(0.06)
Observations	16826		16826		16826	

Notes: See Table S1 for further details on the measurement, *** p < 0.001, ** p < 0.01, * p < 0.05, cluster-robust standard errors in parentheses.
Sources: SHARELIFE, authors' calculations.

Table S3.2 Logistic regression models for Figure 2 (right side), fair or poor self-rated health, average marginal effects

	Model 1		Model 2		Model 3	
	AME	SE	AME	SE	AME	SE
Independent variable						
Early-career involuntary job loss - layoff (Ref. no involuntary job loss + continuous employment)	0.076 **	(0.03)	0.059 ***	(0.02)	0.048 **	(0.02)
Early-career involuntary job loss – plant closure	0.083 *	(0.04)	0.067	(0.03)	0.059	(0.03)
Control variables						
Country (Ref. Austria)						
Germany			0.110 ***	(0.01)	0.107 ***	(0.01)
Sweden			-0.009	(0.01)	-0.008	(0.01)
Netherlands			-0.006	(0.01)	-0.007	(0.01)
Spain			0.089 ***	(0.01)	0.089 ***	(0.01)
Italy			0.028 ***	(0.01)	0.029 ***	(0.01)
France			0.040 ***	(0.01)	0.043 ***	(0.01)
Denmark			-0.036 ***	(0.01)	-0.035 ***	(0.01)
Greece			-0.113 ***	(0.01)	-0.112 ***	(0.01)
Switzerland			-0.085 ***	(0.01)	-0.083 ***	(0.01)
Belgium			-0.019 *	(0.01)	-0.017 *	(0.01)
Czech Republic			0.123 ***	(0.01)	0.131 ***	(0.01)
Poland			0.272 ***	(0.01)	0.272 ***	(0.01)
Ireland			-0.089 ***	(0.00)	-0.095 ***	(0.00)
Demographics and education						
Age at interview (in years)			0.007 ***	(0.00)	0.007 ***	(0.00)
Female (Ref. male)			0.023 *	(0.01)	0.008	(0.01)
Education (in years)			-0.012 ***	(0.00)	-0.012 ***	(0.00)
Childhood health (age 0-15)						
Fair or poor self-rated health (Ref. otherwise)			0.165 ***	(0.03)	0.165 ***	(0.03)
1+ month in hospital (Ref. otherwise)			0.032	(0.02)	0.032	(0.02)
1+ illness(es) from list 1 (Ref. otherwise)			0.013	(0.01)	0.014	(0.01)

Table S3.2 continued

1+ illness(es) from list 2 (Ref. otherwise)	0.022	(0.02)	0.022	(0.02)
Parents health behaviour				
Parents smoked (Ref. otherwise)	0.029 **	(0.01)	0.029 **	(0.01)
Parents drank heavily (Ref. otherwise)	0.053 ***	(0.01)	0.051 **	(0.02)
Childhood socio-economic status (age 10)				
Poorly equipped home (Ref. otherwise)	-0.010	(0.02)	-0.010	(0.02)
Number of books (Ref. None or very few)				
Enough to fill one shelf	-0.022	(0.01)	-0.021	(0.01)
Enough to fill one bookcase	-0.031 *	(0.01)	-0.029 *	(0.01)
Enough to fill two or more book cases	-0.021	(0.01)	-0.021	(0.01)
Persons per room (Ref. ≤ 1 person)				
> 1 and ≤2 persons	0.011	(0.01)	0.011	(0.01)
> 2 persons	0.035 ***	(0.01)	0.034 ***	(0.01)
Occupation breadwinner (Ref. major group 1 (no skill level))				
Major group 2 (skill level 4)	-0.015	(0.03)	-0.013	(0.03)
Major group 3 (skill level 3),	-0.012	(0.03)	-0.011	(0.03)
Major group 4-8 (skill level 2)	0.004	(0.02)	0.005	(0.02)
Major group 9 (skill level 1)	0.016	(0.03)	0.017	(0.03)
Major group 0 (no skill level)	0.063	(0.03)	0.063 *	(0.03)
No breadwinner	-0.003	(0.04)	-0.001	(0.04)
Relative position in math (Ref. much better)				
Better	0.019	(0.02)	0.019	(0.02)
About the same	0.030 **	(0.01)	0.029 *	(0.01)
Worse or much worse	0.069 ***	(0.02)	0.067 ***	(0.02)
Relative position in language (Ref. much better)				
Better	-0.009	(0.02)	-0.008	(0.02)
About the same	0.008	(0.02)	0.008	(0.02)
Worse or much worse	0.006	(0.03)	0.007	(0.03)

Table S3.2 continued

Characteristics (at) first job						
Civil servant (Ref. employee)			-0.018	(0.01)	-0.019	(0.01)
Occupation (Ref. major group 1 (no skill level))						
Major group 2 (skill level 4)			0.014	(0.03)	0.015	(0.03)
Major group 3 (skill level 3)			0.025	(0.04)	0.026	(0.04)
Major group 4-8 (skill level 2)			0.046	(0.04)	0.045	(0.04)
Major group 9 (skill level 1)			0.061	(0.03)	0.059	(0.04)
Major group 0 (no skill level)			0.074	(0.05)	0.070	(0.05)
Sector (Ref. primary sector)						
Manufacturing and energy			-0.046 **	(0.01)	-0.047 **	(0.01)
Construction			-0.017	(0.02)	-0.019	(0.02)
Services			-0.032	(0.02)	-0.033	(0.02)
Public sector, health, and education			-0.055 **	(0.02)	-0.052 **	(0.02)
Full-time (Ref. part-time)			0.009	(0.03)	0.008	(0.03)
Living with a partner (Ref. otherwise)			0.011	(0.01)	0.014	(0.01)
Children (Ref. otherwise)			0.016	(0.02)	0.016	(0.02)
Labour market entry (Ref. < 1950)						
1950-59			-0.020	(0.02)	-0.020	(0.02)
1960-69			-0.037	(0.03)	-0.037	(0.03)
≥ 1970			-0.074 *	(0.04)	-0.073 *	(0.04)
Age at first job			0.003	(0.00)	0.003	(0.00)
Mediating variables						
Unemployment duration (in years) (year 11-25)					0.004	(0.00)
Employment instability (year 11-25)						
Number of job ends					-0.006	(0.01)
Number of involuntary job losses					0.005	(0.02)
Complexity index (0-1)					0.336 ***	(0.09)
Observations	12135		12135		12135	

Notes: See Table S1 for further details on the measurement, *** p < 0.001, ** p < 0.01, * p < 0.05, cluster-robust standard errors in parentheses.
Sources: SHARELIFE, authors' calculations.

Table S4 Sensitivity analyses using the five-point self-rated health scale and applying ordinal logistic as well as linear regression (model 2 in Table S2.1)

	Logistic regression (1=fair, poor health; 0=excellent, very good, good health)			Ordinal logistic regression (1=poor, 2=fair, 3=good, 4=very good, 5=excellent health)			Linear regression (1=poor, 2=fair, 3=good, 4=very good, 5=excellent health)	
		AME	SE		AME	SE	b	SE
Independent variable								
Early-career involuntary job loss (Ref. no involuntary job loss)	1=	0.063 ***	(0.01)	1=	0.020 **	(0.01)	-0.12 **	(0.04)
				2=	0.025 ***	(0.01)		
				3=	-0.008	(0.00)		
				4=	-0.023 ***	(0.01)		
				5=	-0.015 **	(0.00)		
Control variables		✓			✓		✓	
Observations		16826			16826		16826	

Notes: See Table S2.1 for the full model 2 including a list of the control variables, See Table S1 for further details on the measurement, *** p < 0.001, ** p < 0.01, * p < 0.05, cluster-robust standard errors in parentheses.
Sources: SHARELIFE, authors' calculations.

Article 5

Unemployment and housework in couples:
Task-specific differences and dynamics over time

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Abstract

Unemployment not only affects individuals but also their families. Using panel data from Germany this article examines the consequences of job loss for couples' division of housework and total household production. Fixed-effects models reveal that increases in unemployed spouses' total housework hours are not offset by decreases in partners' time implying an expansion of total production next to a reallocation of housework. Supporting time availability and relative resources hypotheses, the authors find larger increases for unemployed husbands than wives, casting doubt on the idea that men "do gender" by refusing additional housework. However, task-specific estimates show that husbands spent more of their extra time on male-typed activities, whereas wives increase their hours more through routine chores. Additionally, this study shows that couples react immediately to unemployment, challenging arguments that spouses need time to adapt to new employment constellations or that men withdraw from housework the longer they remain non-employed.

1. Introduction

Employment is of primary importance in Western societies providing individuals with money and identity. Accordingly, extensive research has shown that job loss not only has negative consequences for workers' long-term employment and earnings prospects, but also affects a wide range of non-economic outcomes such as individuals' social integration or health (Brand, 2015). In the last decade, a growing body of literature has also established that the negative effects of unemployment are likely to have repercussions for other family members, too. For example, job loss may impact on couples' fertility and marital stability as well as negatively affect spouses' psychological well-being and children's socio-economic outcomes (Brand, 2015; Ström, 2003). The topic that has received most attention in this respect concerns how spouses of unemployed individuals increase their labor supply to at least partly offset the falls in family income associated with job loss (e.g., Stephens, 2002). Empirical evidence for this so-called "added-worker effect" is mixed with the results depending on the countries investigated underlining the importance of various contextual factors (e.g., Bredtmann, Otten, & Rulff, 2017).

Whereas changes in paid labor in response to one spouse's unemployment have been extensively researched, much less is known about how couples' division of housework and total household production is affected. This is surprising as investigating changes in unpaid labor not only sheds light on how families alter their daily routines in reaction to a disruptive life event, but also informs us about how households make specialization decisions in general (e.g., Gough & Killewald, 2011; van der Lippe, Treas, & Norbutas, 2017). Although women's increased participation in education and the labor market as well as demographic changes have led to some convergence in time use, the division of labor remains gendered and the relevance of different explanations is highly contested (e.g., Bianchi, Milkie, Sayer, & Robinson, 2000; Sayer 2005).

In an innovative study Gough and Killewald (2011), therefore, used panel data from the US to examine how spouses reallocate their housework as well as change their total household production while facing unemployment. They argued that investigating job loss, as an exogenous shock to working hours, allows for a more rigorous test of competing theories. Specifically, whereas formally gender-neutral time-availability or relative resources hypotheses assume that specialization follows economic rationales (Bianchi et al., 2000; England & Farkas, 1986) predicting similar effects for job loss by husbands and wives, theories that stress how women and men "do gender" by performing or avoiding housework (Berk, 1985; West & Zimmermann, 1987) state that men will not increase or even decrease their share following a job loss. An investigation of the gender-specific reactions to unemployment, thus, also addresses recent debates about the strength of the evidence for mechanisms of gender display (Brines, 1994) or gender-deviance neutralization (Greenstein, 2000) as a number of recent studies have used sophisticated research designs to reexamine these hypotheses (Auspurg, Iacovou, & Nicoletti, 2017; Hook, 2017; see Sullivan, 2011 for a review). Accordingly, the first research questions we address are: What are the effects of unemployment on couples' reallocation of housework and total household production and how do they vary by the gender of the unemployed spouse?

Although Gough and Killewald (2011) extended the few existing cross-sectional or short-run longitudinal studies (Brines, 1994; Shamir, 1986; Ström, 2002; van der Lippe et al., 2017) by applying sophisticated methods to large-scale panel data, some important aspects still remain unanswered. Specifically, as their housework measure emphasized routine chores it is unclear whether their finding that wives increase their housework hours more than men can be generalized to all domestic tasks or whether men may have increased their time more in neutral (e.g., errands and shopping) or male-typed activities (e.g., repairs and garden work) which were not inlcuded in their data. Distinguishing different housework tasks is also informative about arguments of specialization and gender construction (e.g., Coltrane, 2000). In the specific case of job loss we expect women to invest relatively more time in female-typed activities while men should perform more additional hours in male-typed domains. Our second research question, therefore, is: How do the effects of unemployment on couples' reallocation of housework and total household production vary by the specific tasks considered?

Another issue that is often raised in the literature (e.g., van der Lippe et al., 2017) concerns how the effects of unemployment on housework develop over time. Several authors have argued for a lagged adaption because habits must be constantly challenged, skills have to be acquired, and gender norms need to be weakened for couples to overcome inertia (e.g., Gershuny, Bittmann, & Brice, 2005). Whereas a recent qualitative study supports this view showing that couples attempt to maintain the status quo in their division of labor as long as possible following unemployment (Gush, Scott, & Laurie, 2015, p. 713), we are not aware of any quantitative evidence based on large-scale and long-run panel data.

Moreover, in her seminal study on gender display Brines (1994) showed that it is the long-term jobless men who withdraw from housework to reassert their masculinity offering an alternative prediction about changes over time. The importance of a dynamic perspective is also reaffirmed by studies of other life events such as childbirth or retirement suggesting potential for lead and lag effects (Kühhirt, 2012; Leopold & Skopek, 2015). Therefore, a third, contribution of this article is to examine how couples' reallocation of housework and their total household production changes across the transition from employment to unemployment over several years.

To address these questions, we use longitudinal data from the German Socio-Economic Panel (SOEP) spanning the period 1991-2015 and apply fixed-effects models. Our data are unique in that they offer disaggregated information on specific tasks (i.e., errands and shopping; washing, cleaning, cooking; repairs and garden work; childcare) as well as allow to follow couples for several years before and after job loss. This offers insights into whether households' adaptation is immediate or lagged as well as whether potential processes of gender display become overt the longer men stay out of the labor force.

2. Background

To derive hypotheses about couples' reaction to unemployment, we review two opposing perspectives concerning how households make specialization decisions in general (Bianchi et al., 2000; Coltrane, 2000). The first perspective comprises formally gender-neutral theories emphasizing the role of spouses' different time constraints, relative resources, and relative productivities in the market and domestic sphere. In contrast, the second, gender-based perspective argues that housework allows for a symbolic enactment of gender relations with women and men performing gender-appropriate tasks relatively independent of economic considerations. We also review additional arguments to explain how the effects of unemployment may differ for women and men according to the specific tasks considered as well as form expectations about the post-unemployment dynamics in housework over time.

2.1 Gender-neutral perspective

Within the gender-neutral perspective two hypotheses are usually distinguished: time availability and relative resources. The time-availability hypothesis states that couples rationally allocate housework based on partners' relative market hours and the total household production that needs to be completed (Bianchi et al., 2000; England & Farkas, 1986). Although it has received some support in empirical studies that model housework hours by using spouses' time in market work as predictors (e.g., Bianchi et al., 2000; Bittmann et al., 2003; Brines, 1994), Gough and Killewald (2011) caution that these cross-sectional studies implicitly assume that couples first decide on their paid hours and then allocate their unpaid labor accordingly. If households, however, jointly decide on market and domestic hours, the latter are endogenous to the former. They instead suggest using panel data and examine job loss, as changes in housework in this case can be assumed to follow from an exogenous shock to working hours.

The relative resource hypothesis argues that spouses allocate housework based on their relative earnings such that the partner who contributes the larger share to the family income, performs less domestic tasks. This hypothesis can be derived from Becker's (1981) microeconomic model about households' specialization decisions or from social exchange theories (Blood & Wolf, 1960; Brines, 1994). The former assumes that a gendered division of labor arises from returns to specialization, as spouses' different relative productivities in paid and unpaid labor allow maximizing their joint utility by capitalizing on their comparative advantages. Whereas the original work emphasizes women's lower human capital and their higher efficiency in childcare and domestic activities, any other factors leading women to have lower earnings potentials reproduce a gendered division of labor as well (Auspurg et al., 2017).

Although social exchange or bargaining theories challenge the assumption of a joint utility maximization (England & Folbre, 2005), they arrive at the same hypothesis. In this view, the relative resources, however, establish a power relation with the breadwinning partner having a better bargaining position in negotiations over undesirable housework (Blood & Wolf, 1960). A variant of this theme argues that spouses (mostly wives) take the responsibility for household chores, because they are economically dependent and cannot bargain out (Brines, 1994). Empirical studies have tended to support the relative resources hypothesis showing that spouses' time in housework is negatively associated with their earnings relative to their partners', although not necessarily across the full range (e.g., Bianchi et al., 2000; Bittmann et al., 2003; Brines, 1994; Evertsson & Nermo, 2004). Therefore, it is predicted that job loss shifts the housework to the unemployed spouse and away from the partner, as time and relative resources change in favor or disfavor of the former.

Hypotheses 1: Unemployment increases the person's housework hours and decreases the partner's housework hours.

While the presented arguments are formally gender-neutral and unemployment of husbands and wives should, therefore, have similar consequences, gender differences in the "initial conditions" (Brines, 1994, p.654) suggest that men experience greater gains in time and larger losses in relative resources, given that they usually work longer and earn more than women. Men's larger losses in bargaining power can also be explained from a gender-based variant of the argument stating that women's resources are discounted in societal contexts of male domination such that the same resources have a different value depending on gender (e.g., Fuwa, 2004). Given that unemployment by men results in more additional time and a larger loss of resources with a potentially greater reduction in the associated bargaining power, the following gender-specific reactions are expected.

Hypotheses 2a: The positive effect of unemployment on the person's and the negative effect on the partner's housework hours are larger for men than women.

2.2 Gender-based perspective

Criticizing the neglect of gender, sociologists have emphasized the role of norms in the emergence of a gendered division of labor. In the doing gender approach households are arenas for the symbolic enactment of femininity and masculinity with spouses performing or avoiding domestic tasks to facilitate the reproduction of gender relations (Berk, 1985; West & Zimmermann, 1987). Accordingly, nontraditional arrangements threaten individuals "gender accountability" in terms of how they are viewed by their partners, friends, and themselves. Specific versions of this approach are Brines' (1994) gender display and Greenstein's (2000) gender-deviance neutralization hypotheses arguing that economically dependent husbands underperform and breadwinning wives overperform housework to display their gender and compensate for the deviation from expectations about their "normal" roles. While these hypotheses have received support beyond the original research (e.g., Bittmann et al., 2003, Evertsson & Nermo, 2004), the validity of the results has been questioned in a number of studies, too (e.g., Hook, 2017; Killewald & Gough, 2010). A review by Sullivan (2011) concluded that for women findings of gender display or deviance neutralization were mainly due to model misspecification, whereas for men these mechanisms are limited to small and specific subgroups. With respect to unemployment the gender-based perspective makes an opposite prediction to the above theories stating that husbands refuse to increase or even decrease their share following a job loss. Their wives are assumed to "do gender" by not relinquishing responsibilities for domestic activities. In contrast, for women becoming unemployed is in line with their traditional role and their male partners can improve their "gender accountability" by reducing their hours.

Hypotheses 2b: The positive effect of unemployment on the person's and the negative effect on the partner's housework hours are larger for women than men.

Besides theoretical assumptions about the reallocation of housework among partners, previous research has also put forward several arguments for why the total household production increases in case of unemployment. These include the loss of financial resources restricting couples' opportunities to "outsource" domestic tasks and buy substitutes, the fact that additional housework accrues as the home is used more often and extensively, spouses being less efficient in their time use, and the idea that couples take up so-far neglected or unnecessary tasks (Gough & Killewald, 2011; Gush et al., 2015; van der Lippe et al., 2017).

2.3 Different tasks and dynamics over time

So far the role of different tasks has been neglected in empirical studies on the effect of unemployment (e.g., Gough & Killewald, 2011; van der Lippe et al. 2017), but theoretically it is plausible that women's and men's reactions depend on the activities considered. Although the gender-neutral perspective makes no direct mention of different housework tasks, the underlying specialization argument (Becker, 1981) may suggest that spouses increase their domestic activities in areas where they possess a comparative advantage. As women not only do more housework, but especially perform the majority of the less optional, less postponable, and less enjoyable routine chores (Coltrane, 2000), it can be assumed that they have a higher relative productivity in this domain. In contrast, men may possess comparative advantages in non-routine tasks such as repairs and garden work. Despite of the different theoretical foundation a similar prediction is derived from the gender-based perspective as cultural expectations may not only concern who should do the paid and unpaid labor but also which domestic activities reflect femininity and masculinity (Berk, 1985). Therefore, spouses may not only construct gender by increasing or decreasing their total housework hours in case of unemployment, but also by reallocating their additional time relatively more to those tasks that demonstrate that they are a productive member of a specific gender category (Coltrane, 2000).

Hypotheses 3: The effects of unemployment differ between men and women with respect to the specific tasks. Women and men becoming unemployed are expected to increase their time more in female- and male-typed tasks respectively with the according changes in their partner's housework hours.

Despite being often highlighted as a research gap (e.g., van der Lippe et al., 2017), the post-unemployment dynamics in couples' housework hours have been rarely studied. A qualitative study found that they mainly carried on as usual and that both men and women refused to swap roles (Gush et al., 2015). The authors explain this by socio-psychological motives emphasizing the status quo as something desirable. Moreover, the study showed that couples considered the reemployment prospects with husbands spending their time on job search and wives supporting these efforts instead of focusing on their own employment. This suggests that changes may not occur immediately but only over longer durations of men's non-employment. These descriptions fit well with three "inertial mechanisms" that have been elaborated by Gershuny et al. (2005, p. 658) predicting a lagged adaption, because (1) changing habits requires continuous effort, (2) the acquisition of housework skills needs time, and (3) the gendered meaning associated with domestic activities may inhibit women to decrease and men to increase their housework hours in nontraditional arrangements. Note that these mechanisms can be expected to be more important for unemployed men than women, since women do more (time-consuming) housework even if they are employed. Accordingly, for women changes in housework following a job loss can be expected to take place immediately.

Hypotheses 4a: The positive effect of unemployment on the husband's and the negative effect on their wife's housework hours increase the longer the husband remains non-employed.

While Gershuny et al. (2005) argue that the gendered meaning of paid and unpaid labor together with the other inertial processes results in a lagged adaptation, Brines (1994, p.672) makes an even stronger prediction stating that "joblessness may have a negative effect on husbands' housework time – particularly among the long-term unemployed, whose prolonged experience might intensify any distress over lost claims to male accountability." As the deviance from the male breadwinner norm growths with men's time out of the labor force, the gender-based perspective suggests that while they may take up some additional housework initially unemployed men will relinquish their domestic responsibilities over time with the according changes observed for their wives.

Hypotheses 4b: The positive effect of unemployment on the husband's and the negative effect on their wife's housework hours decrease the longer the husband remains non-employed.

3. Methods

3.1 Data and sample

We use data from the 1991-2015 waves of the Socio-Economic Panel (SOEP, version 32.1, doi: 10.5684/soep.v32.1). The SOEP is an annual household panel survey designed to be nationally representative of the German adult population living in private households (Wagner, Frick, & Schupp, 2007). In the latest wave, 2015, about 27,000 persons living in 16,000 households were interviewed. For our purposes, the SOEP has three advantages. First, it is one of the longest running household panel surveys allowing us to explore the dynamics in couples' division of housework and total household production in the years preceding and following one spouse's job loss. Second, because all household members aged 16 years and over are interviewed, each partner provides separate information about their time use and labor market status. Third, the SOEP offers disaggregated information on housework making it possible to examine how the effects of unemployment differ for time spent on neutral, female-, and male-typed tasks as well as to distinguish housework from childcare. For the analyses, we select heterosexual couples where both spouses participate in the personal interview. To rule out effects of (early) retirement (e.g., Leopold & Skopek, 2015), we censor couples once one partner exceeds the age of 54 years. For simplicity, we refer to all partners as "spouses," "wives," and "husbands" although our sample includes cohabiting couples, too. Only waves from 1991 onwards are used, because the time use data collected in previous years are not comparable. As we study families' responses to a husband's or wife's job loss we restricted the sample to couples where at least one partner is at risk of job loss during the window of observation. This includes couples where the husband or the wife experience at least one transition from employment to unemployment (unemployment spell) as well as those were at least one member is continuously employed (employment spell). The later couples provide information about changes in housework hours in the absence of one spouse's job loss and serve as a control group contributing to the estimation of the effects of the control variables in the fixed-effects models described below (e.g., Brüderl & Ludwig, 2015, p. 346). Finally, we restricted the sample to couples observed at least twice in the panel, leaving us with 75,700 couple-years from 12,183 couples. For 302 of these couples job loss is observed for both partners, whereas in 1,036 couples only the husband and in 802 couples only the wife experiences the transition from employment to unemployment.

3.2 Measures

3.2.1 Dependent variables

Since 1991 the SOEP repeatedly asks respondents about how many hours they spent on an average weekday on seven different activities including childcare and the following three domestic tasks: (1) "errands (shopping, procurements, trips to government agencies)," (2) routine "housework (washing, cooking, cleaning)," and (3) "repairs at the house, in the flat, of the car, garden work." After deleting observations with implausible values of more than 24 hours on all seven activities, we define four outcomes for each spouse: the total housework hours, that is, the sum of all domestic tasks as well as the hours for each separate activity. In line with previous studies, we consider errands (1) as neutral tasks, whereas routine housework (2) as well as repairs and garden work (3) reflect female- and male-typed tasks respectively (Coltrane, 2000). We do not include childcare hours as Sullivan (2013, p. 74) notes that time spent with children is at least to some extent "perceived rewarding and enjoyable" such that theoretical arguments assuming housework to be undesirable are only partly applicable. However, as care work also represents a relevant arena for "doing gender" (Berk, 1985; West & Zimmerman, 1987), some findings for childcare are reported in the sensitivity analyses. Following standard practice in the literature, we use absolute hours, because we are interested in changes of couple's division of housework as well as their total household production (Gough & Killewald, 2011; van der Lippe et al., 2017). Relative measures such as the husband's share conflate changes in husband's and wife's behavior not telling us who in the household reacted to job loss in which way (e.g., Hook, 2017). Using absolute hours, the estimated effects for total housework, in addition, can be decomposed into the effects for each specific task. In the sensitivity analyses we report robustness checks for relative measures.

Although stylized questions as compared to diary methods are susceptible for over-reporting (e.g., Juster, Ono, & Stafford, 2003), previous studies suggest that effect estimates based on stylized and diary data are quite similar in sign and statistical significance (Kan & Pudney, 2008). Stylized estimates also have been shown to adequately reflect trends over time (Juster et al., 2003) and Gough and Killewald (2011, p.1090) note that fixed-effects models (see below) are not only unaffected by classical measurement error, but also net out any time-constant (upward) bias in couples' reports on housework hours.

3.2.2 Independent variables

We define job loss by the transition from employment to unemployment from one interview t to the next interview $t + 1$. Each unemployment spell also consists of the years in employment preceding and the years in unemployment and inactivity following the job loss. In line with the theoretical arguments, we follow persons throughout non-employment instead of only unemployment, because their situation remains comparable with respect to the gains in time, the loss in relative resources, and the departure from the breadwinner role. Unemployment spells are, however, censored if individuals find reemployment or take up education or training as these transitions result in a new situation. Employment spells are defined by being continuously employed throughout consecutive interviews. To capture the dynamics across the transition we use a series of dummy variables that indicate relative time to job loss. As interviews in the SOEP are about one year apart the observations in t and $t + 1$ approximately reflect the situation half a year before and after job loss. The following seven dummy variables are included in the models: a) 2.5 years before, b) 1.5 years before, c) 0.5 years before, d) 0.5 years after, e) 1.5 years after, f) 2.5 years after, g) 3.5 years or more after job loss with all observations 3.5 years or more before the event serving as the reference category.

3.2.3 Controls

As we are interested in the total effects of unemployment, we only control for time-varying covariates that are assumed to affect one spouse's unemployment risk as well as both partners' housework hours in order to avoid overcontrol bias (Elwert & Winship, 2014; Gough & Killewald, 2011). Variables that are assumed to lie on the causal path from unemployment to housework and are, therefore, not controlled for include spouses' health satisfaction, household income, and home properties which may change due to relocations. The same holds true for partner's employment status as the added-worker effect proposes that partners adapt their labor supply in response to their spouse's job loss (Stephens, 2002).

Among the variables we consider exogenous to unemployment are husband's or wife's age (squared) as well as year dummy variables accounting for ageing effects and common trends in housework time. Moreover, couples' residence in East- or West-Germany and the state unemployment rate is included as regional labor market conditions constrain employment prospects and also affect household labor decisions (Burda & Hammermesh, 2010). Finally, we control for couple's care work responsibilities, because these likely influence specialization decisions concerning paid and unpaid labor and also have been shown to increase housework time, particularly for women (Kühhirt, 2012). Specifically, we add the number of children aged 0-14 years as well as five dummy variables indicating the presence of at least one child in a specific age range (0-1, 2-4, 5-7, 8-10, 11-14 years). To also consider other care work we added a dummy variable indicating that a person in need of care lives in the household. We check the robustness of our results by adding different sets of covariates in the sensitivity analyses.

3.3 Fixed-effects models

To examine the effects of job loss on spouses' housework hours we estimate the following fixed-effects models separately for husbands and wives

$$\mathrm{y_{it}} = \mathrm{a_i} + \sum_{k=-2.5}^{\geq +3.5} \gamma_k T_k + \beta X_{it} + \varepsilon_{it}$$

with y_{it} measuring husband or wife i's own or their partner's hours in year t, a_i as a couple- and spell-specific fixed-effects, T_k as seven dummy variables indicating time relative to the reference category (i.e., ≥ 3.5 years before job loss), X_{it} as a vector of control variables, and ε_{it} as idiosyncratic errors. The coefficients γ_k reflect the time path in the effect of job loss on housework hours. Standard errors have been clustered at the couple-level. By couple- and spell-specific we mean that the fixed-effects are only fixed within the context of a specific couple and unemployment or employment spell such that repeated spells within one couple or subsequent relationships are treated as separated observations (Gough & Killewald, 2011). As fixed-effects models only use within-couple variation any time-constant heterogeneity, even if unobserved, is rendered irrelevant (Brüderl & Ludwig, 2015) including, for example, stable preferences for paid and unpaid labor as well as standards for housework.

4. Results

4.1 Descriptive findings

Table 1 shows descriptive statistics for the independent and dependent variables for three periods: 1991-1999, 2000-2009, and 2010-2015. Note that these refer to the couples in our sample and are presented to highlight relevant differences in husbands' and wives' labor market status, working hours, and labor incomes as well as to show that the observed patterns and trends in paid and unpaid labor are consistent with what is known from previous research. The majority of husbands are employed (92-95%) or searching for a job (2-5%) and the average working hours of employed men marginally decreased from 9.7 to 9.5 hours on an average weekday. Their monthly net labor income (in 2011 prices) somewhat increased ranging from €2,210 in 1991-1999 to €2,370 in 2010-2015. In contrast, their wives have considerably lower employment rates which, however, significantly increased from 65% in the early to 79% in the late period. An opposite trend is observed for inactivity rates. The average working hours of employed women decreased from 7.3 to 6.8 hours on an average weekday. Next to a general decline in working hours this reflects that women's increased labor force participation is associated with a growing share of part-time employment. The wives' monthly net labor income if employed remained relatively stable ranging from €1,120 to €1,190. Looking at the employment and earnings patterns in our sample two aspects are particularly noteworthy. First, wives work about 2.5 to 3.0 hours less per average weekday than husbands implying

Table 1 Descriptive statistics

	1991-1999		2000-2009		2010-2015	
	Mean	*SD*	Mean	*SD*	Mean	*SD*
Husband						
Age (in years)	39.51	8.19	41.62	7.34	43.43	7.37
Employed (1=yes, 0=no)	0.94	0.23	0.92	0.27	0.95	0.22
Unemployed (1=yes, 0=no)	0.03	0.18	0.05	0.23	0.02	0.15
Education or training (1=yes, 0=no)	0.01	0.11	0.01	0.07	0.01	0.08
Inactive (1=yes, 0=no)	0.01	0.11	0.02	0.14	0.02	0.14
Monthly labor income/1000 (in 2011 €)[a]	2.21	1.14	2.26	1.30	2.37	1.47
Working hours [ab]	9.72	1.68	9.67	1.84	9.53	1.90
Total housework hours[b]	2.18	1.70	2.22	1.74	2.14	1.61
Neutral housework hours[b]	0.69	0.73	0.69	0.70	0.67	0.69
Female-typed housework hours[b]	0.57	0.79	0.67	0.82	0.73	0.76
Male-typed housework hours[b]	0.92	0.99	0.86	0.97	0.73	0.86
Childcare hours[b]	0.82	1.35	0.84	1.42	0.91	1.46
Wife						
Age (in years)	37.00	8.00	39.34	7.50	41.13	7.67
Employed (1=yes, 0=no)	0.65	0.48	0.72	0.45	0.79	0.41
Unemployed (1=yes, 0=no)	0.06	0.23	0.05	0.22	0.03	0.16
Education or training (1=yes, 0=no)	0.02	0.13	0.02	0.13	0.02	0.14
Inactive (1=yes, 0=no)	0.28	0.45	0.22	0.41	0.17	0.37
Monthly labor income/1000 (in 2011 €) [a]	1.12	0.64	1.08	0.75	1.19	0.82
Working hours [ab]	7.27	2.75	6.73	2.91	6.76	2.85
Total housework hours[b]	4.99	2.48	4.50	2.27	3.97	2.06
Neutral housework hours[b]	1.35	0.80	1.24	0.71	1.14	0.70
Female-typed housework hours[b]	3.09	1.86	2.69	1.63	2.28	1.44
Male-typed housework hours[b]	0.56	0.78	0.57	0.77	0.55	0.77
Childcare hours[b]	3.05	3.87	2.97	3.83	2.78	3.68
Household						
Number of children aged 0-14 years	0.84	0.96	0.76	0.92	0.75	0.93
Child aged 0-1 years (1=yes, 0=no)	0.07	0.25	0.05	0.22	0.05	0.22
Child aged 2-4 years (1=yes, 0=no)	0.17	0.37	0.14	0.35	0.14	0.35
Child aged 5-7 years (1=yes, 0=no)	0.18	0.38	0.16	0.37	0.17	0.37
Child aged 8-10 years (1=yes, 0=no)	0.18	0.38	0.17	0.38	0.16	0.37
Child aged 11-14 years (1=yes, 0=no)	0.17	0.38	0.18	0.38	0.17	0.37
Person in need of care (1=yes, 0=no)	0.02	0.13	0.02	0.14	0.02	0.13
East-Germany (1=yes, 0=no)	0.19	0.39	0.18	0.38	0.18	0.38
State unemployment rate (%)	10.42	3.87	10.67	4.41	7.69	2.72

Note: Socio-Economic Panel, v32.1, 1991-2015, weighted. N = 12,183 couples and 75,700 couple-years.
[a] Monthly labor income/1000 and working hours are reported for the employed. [b] Working hours and housework hours refer to an average weekday.

smaller gains in time following a job loss. Second, given husbands' considerably higher labor incomes, their relative resources and, correspondingly, their bargaining power drops to a larger degree if they become unemployed. These differences in the initial conditions support the reasoning underlying Hypotheses 2a.

Looking at the time husbands and wives spent on domestic tasks and childcare, the sample shows some convergence in time use but also highlights that the division of paid and unpaid labor remains highly gendered (e.g., Bianchi et al. 2000; Sayer, 2005). Whereas men's average total housework hours per day remained quite stable, small increases are revealed for time spent on routine housework and childcare, with corresponding reductions in hours spent on repairs and garden work. However, the large changes took place among wives who reduced their total housework from about 5.0 hours in the early period to roughly 4.0 hours in the late period, mainly through spending less time on routine chores. Their hours in childcare slightly decreased over time, but the average number of children and the share of couple's with young children (0-1, 2-4 years) are also lower in the later period.

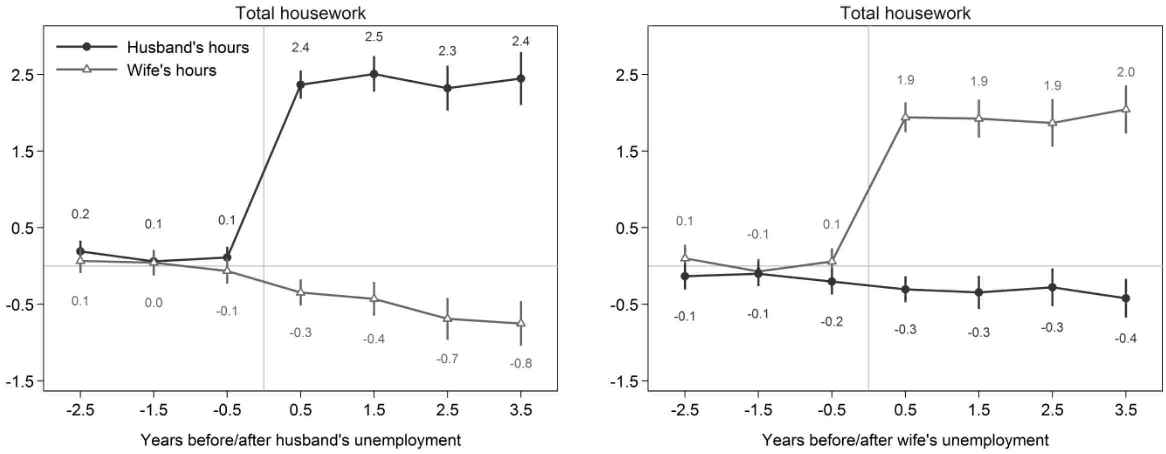
Moreover, in line with previous studies (Coltrane, 2000), we see that women shoulder the major share of the total housework. Even in the latest period (2010-2015), wives contribute about two thirds to couples' total hours. This is even more evident for the more time consuming female-typed tasks such as washing, cooking, and cleaning where women contribute more than 80% of the time. In contrast, men perform a somewhat greater share (57%) of the less time consuming male-typed tasks. Women also completed the larger share of neutral tasks including errands and shopping as well as spent considerably more time on childcare underlining the uneven distribution of housework and carework.

4.2 Multivariate findings

Figure 1 and 2 provide answers to our hypotheses. The respective coefficients of the fixed-effects models are presented in Table A1 and A2 in the appendix. Figure 1 shows the effects of husband's (left panel) and wife's unemployment (right panel) on both spouses total housework hours on an average weekday including 95% confidence intervals. Looking at the left panel, we find that following a job loss husbands increase their total hours for domestic tasks by about 2.3 to 2.5 hours with the changes taking place immediately after their transition. Even though their wives' hours are reduced – in particular, the longer the husbands remain jobless – the effects range from about 0.3 to 0.8 hours and do not match up the additional time husbands spent.

The findings for wife's unemployment are similar (Figure 1, right panel) although the increases in total housework after job loss (1.9 to 2.0 hours) are somewhat smaller. Their husbands reduce their daily time by only about 0.3 to 0.4 hours, accordingly. In case of wives' unemployment the changes also happen in the year of job loss, but their spouses reactions seem not to depend on the duration of their non-employment. For both partners' job losses there is little evidence for changes preceding unemployment, for example, as couples may anticipate the event and already adapt their division of housework beforehand.

Figure 1 Effects of husband's (left) and wife's (right) unemployment on their total housework hours on an average weekday



Notes: Socio-Economic Panel, v32.1, 1991-2015. N = 11,790/72,977 couples/couple-years for husband's and 9,339 couples/53,685 couple-years for wife's unemployment.
The estimates for husband's and wife's unemployment from the fixed-effects models are reported in the Appendix in Table A1, Model 1 (husband's hours) and Model 5 (wife's hours) and Table A2, Model 1 (wife's hours) and Model 5 (husband's hours). Spikes show 95% confidence intervals based on clustered standard errors.

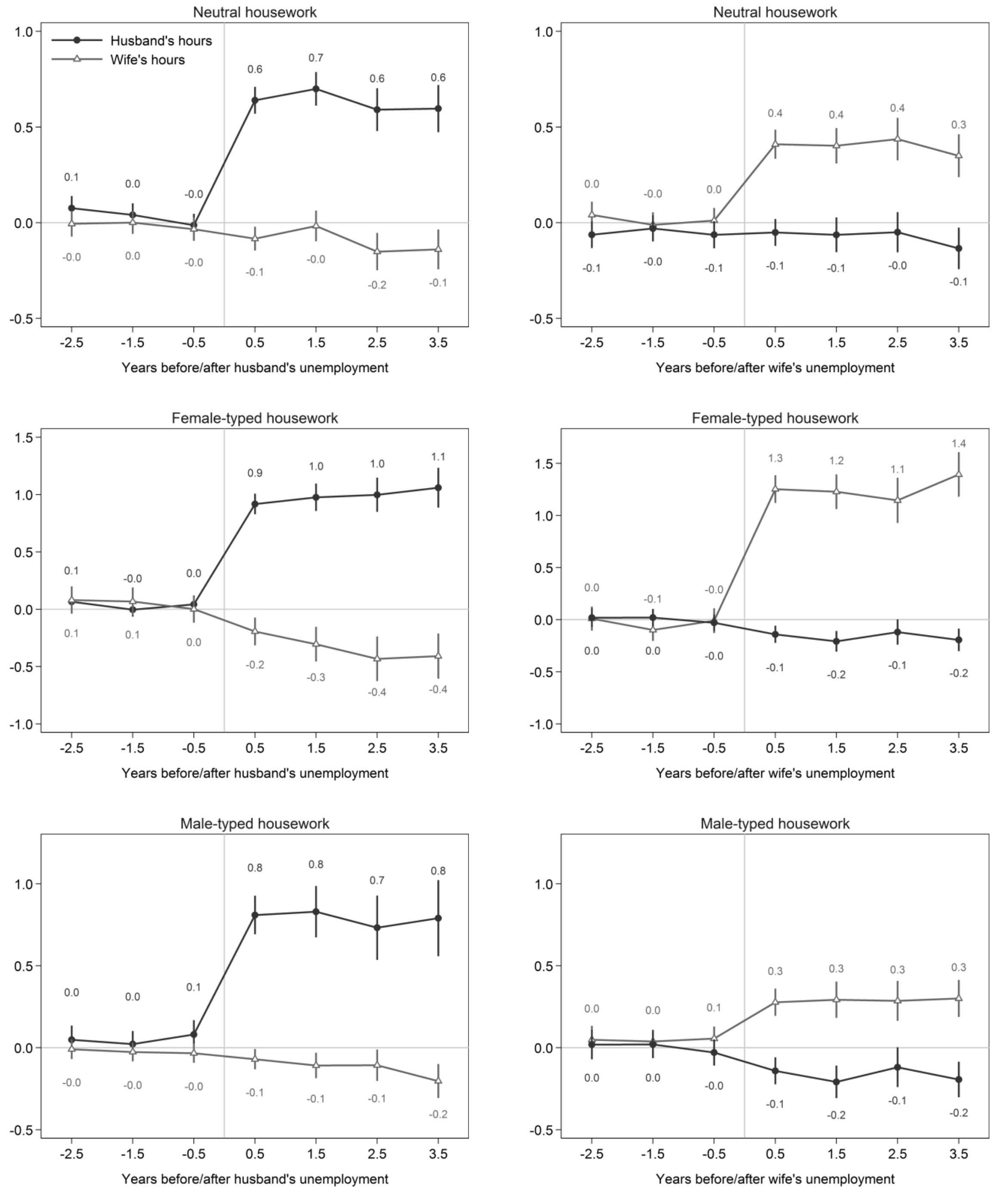
Concerning couples' overall reaction it can be stated that unemployment by either the husband or the wife results in an expansion of the total household production. For example, in case of husband's unemployment half a year after job loss couples increase their total housework by 2.1 hours (=2.4-0.3). This supports previous findings (Gough and Killewald, 2011; van der Lippe et al. (2017) suggesting that couples extend their home production, because they have less income for outsourcing or buying substitutes, more housework accrues if one spouse becomes unemployed, they undertake previously neglected tasks or simply use their time less efficiently. Concerning the derived hypotheses, our findings support Hypothesis 1 suggesting that couples adapt to changes in spouses' relative time constraints and resources by shifting household chores to the unemployed spouse and away from the partner.

Consistent with a gender-neutral perspective that takes account of the gender-specific initial conditions (Hypothesis 2a) men are shown to increase their housework hours somewhat more than women with the according reactions by their partners. This finding is likely due to men experiencing greater time gains and relative resource losses as the descriptive findings showed that they work longer and earn more than women. We revisit this explanation in the sensitivity analyses by only comparing job losses of full-time employed husbands and wives. No support is found for the gender-based arguments that women would increase their total housework to a larger extent (Hypothesis 2b) mainly because men would refuse additional domestic tasks or even decrease their involvement following a job loss. Interestingly, we find little signs of a lagged adaptation after men's unemployment (Hypothesis 4a) or an intensification of their avoidance of housework (Hypothesis 4b) the longer they remain out of labor force. These results are at odds with the qualitative study by Gush et al. (2015) who showed that couples aimed at maintaining their status quo. The only trend discernable in Figure 1 concerns wife's increasing relief from housework after their husband's job loss suggesting that it takes some time before they are able or willing to dispense domestic tasks.

However, the aggregated assessment of total housework may hide differences by specific tasks which are associated with a gendered meaning and may exhibit different dynamics over time. Therefore, Figure 2 presents the results of task-specific models. The left and right panels show the results for husband's and wife's unemployment respectively. Overall the results resemble those for total housework. For all tasks we find that the unemployed spouse increases his or her time to a larger extent than the partner reduces their hours meaning that the total production expands across all domains. We also see that the majority of the change happens in the year of the transition from employment to unemployment and that the time paths with the exception of female-typed activities suggest that couples react more or less immediately.

However, Figure 2 also reveals some interesting task-specific differences. While both men and women increase their time after becoming unemployed in all three activities, we find that husbands spent relatively more additional hours on neutral and, in particular, male-typed tasks, whereas wives' extra time is to a large extent attributable to increases in routine chores. These findings highlight the importance of a disaggregated examination of changes in housework hours and confirm Hypotheses 3 which predicted a gender-specific reaction in the kind of domestic work performed. Slight signs of adaptation are apparent for the case of husbands'

Figure 2 Effects of husband's (left) and wife's (right) unemployment on their task-specific housework hours on an average weekday



Notes: Socio-Economic Panel, v32.1, 1991-2015. *N* = 11,790/72,977 couples/couple-years for husband's and 9,339 couples/53,685 couple-years for wife's unemployment.
The estimates for husband's and wife's unemployment from the fixed-effects models are reported in the Appendix in Table A1, Models 2-4 (husband's hours) and Models 6-8 (wife's hours) and Table A2, Models 2-4 (wife's hours) and Models 6-8 (husband's hours). Spikes show 95% confidence intervals based on clustered standard errors.

unemployment and routine housework (Hypothesis 4a), but not for job losses by women or any other tasks. This is consistent with the arguments by Gershuny et al. (2005) as for women's job loss the inertial processes are theoretically less important and for men adaptation should be more apparent in tasks they do not perform routinely, they have the least skills in, and that carry the strongest gender meaning. However, given that these small changes cannot be estimated very precisely, the stark immediate changes after unemployment are perhaps more striking.

Do the task-specific results change the assessment of the gender-neutral and gender-based perspectives? As men also increase their routine housework following a job loss, the findings are not in line with a strict interpretation of gender display or deviance neutralization, predicting that men do not change or even decrease their share (Hypothesis 2b) as well as withdraw from housework with ongoing non-employment (Hypothesis 4b). In the gender-based perspective wives should also not decrease their routine housework over time. Nevertheless, the finding of slightly larger increases in time on female-typed tasks for wives' job loss confirms previous results pointing to some "stickiness" in the division of labor either due to gender norms or task-specific skills (Gough & Killewald, 2011, p. 1089; van der Lippe et al., 2017).

4.3 Sensitivity analyses

In the following we summarize the results of sensitivity analyses checking the robustness of our findings to decisions about measurement and model specification as well as providing some further insights. The respective tables are available in the online supplementary material.

Although we believe that absolute hours are more appropriate for our study, we estimated models using the share of total or task-specific housework hours performed by the spouse who becomes unemployed as the dependent variable (see Tables S1.1 and S1.2). The results confirm the main analyses as both men and women immediately increase their share of total and task-specific housework hours following a job loss with larger effects for men than women.

We also checked whether including additional covariates alters our findings, as decisions about which control variables to include are not without ambiguity. For example, whether to control for spouses' health hinges on the question whether it is more plausible that health affects job loss or the other way around. Holding constant both spouses' health satisfaction leaves the estimated unemployment effects virtually unchanged (see Tables S2.1 and S2.2).

Adding partner employment status, we find slightly larger reductions in the partners' housework hours, while the estimates for the unemployed spouses remain about the same (see Tables S3.1 and S3.2). The former finding is at odds with the idea of an added worker effect as decreases in partners' housework should be smaller once their employment status is held constant. Furthermore, we controlled for some couple characteristics such as marital status (see Tables S4.1 and S4.2) and properties of the home (size, owner, garden, house) (see Tables S5.1 and S5.2) which each had little impact on the estimated unemployment effects. Finally, we added the household income (squared) to the original specification, finding some small increases in partners' reductions of housework (see Tables S6.1 and S6.2). Holding families' financial resources fixed, the positive effect on the total home production reduces, supporting the idea that the expansion in total production is partly due to the fact that they have less opportunities for "outsourcing" tasks. However, as these changes are rather small, other mechanisms likely play an important role, too.

As we highlighted above childcare is conceptually and analytically distinct from housework (Sullivan, 2013). We here provide some findings to check whether the effects of job loss on housework and childcare are similar, as previous studies often cannot rule out that respondents report childcare within measures of housework (e.g., Gough & Killewald, 2011). For these analyses, we restricted our sample to couples who have at least one child below the age of 14 living in their home. Tables S7.1 and 7.2 reveal that if spouses lose their job they increase their childcare hours with women spending, however, more additional time, particularly, the longer they remain non-employed.

Finally, we only focused on transitions from full-time employment to unemployment adjusting for differences in the initial conditions between men and women (see Tables S8.1 and S8.2). The results concerning total housework time strengthen the gender-neutral perspective, in that, the reaction to unemployment by husbands and wives in case of losing a full-time job is almost the same.

5. Discussion

In this study we examined how couples react to one spouse's job loss by reallocating their division of housework and adjusting their total household production. Using long-run panel data from Germany, our results are consistent with previous longitudinal studies (Gough & Killewald, 2011) showing that couples mostly follow economic rationales in reallocating their time spent on domestic work. Specifically, we find that both husbands and wives increase their total housework hours with larger increases by men being explained by differences in the initial conditions reflecting that they gain more time or lose more resources following a job loss.

Moreover, the reallocation of housework is "only half of the change" observed (Gough & Killewald, 2011, p.1097; van der Lippe et al., 2017) as increases in unemployed spouses' hours are not completely offset by decreases in their partners' time implying an expansion of the total household production. Although our data do not allow disentangling all potential explanations falls in family income appear not to be the main reason. Therefore, other mechanisms including a more frequent and extensive usage of the home, partners' less efficient time use, or the completion of previously neglected tasks deserve more attention in future research.

The present study addressed two additional research gaps. First, we examined how households' responses may change with the non-employment duration of the unemployed spouse as theoretical arguments (Gershuny et al., 2005) as well as qualitative evidence (Gush et al., 2005) suggest that couples' only slowly adapt, especially following job losses by men. Our results challenge this view showing that spouses mostly react within the year of unemployment with little signs of a lagged adaption and no evidence for husband's withdrawal from housework responsibilities over time. Second, our results are helpful in contextualizing previous findings, showing that wives increase their housework hours more than husbands when using a measure emphasizing routine chores (Gough & Killwald, 2011). This study may have underestimated men's additional housework as our task-specific analyses reveal that unemployed men increase their domestic work more through spending additional time on errands and shopping as well as repairs and garden work. In contrast, extra time by women is to a larger extent attributable to female-typed tasks including washing, cooking, and cleaning.

Although these gender-specific reactions in the task-specific estimates are consistent with a gender-based perspective arguing that men and women do gender by performing domestic tasks that are culturally defined to be masculine or feminine (e.g. Berk, 1985, West & Zimmermann, 1987) rather than by doing more or less total housework, they are also in line with the specialization perspective stressing that spouses spent relatively more of their extra time in tasks they possess skills for (Gough & Killwald, 2011). While we cannot distinguish these explanations, overall our results rather support time availability and relative resources hypotheses about couples' division of housework. The findings that men increase their total housework more than women as well as that they also immediately and substantially increase their routine housework hours, are difficult to reconcile with gender display (Brines, 1994) and deviance neutralization (Greenstein, 2000) accounts. Our findings are consistent with previous studies (Shamir, 1986; Ström, 2002) as well as support recent critiques towards the gender deviance neutralization argument (e.g., Auspurg et al., 2017; Hook, 2017). However, it is important to note that some recent cross-sectional studies report results in line with gendered arguments (e.g., van der Lippe et al., 2017) as well as that housework is not the only area in which women and men may do gender (Hook, 2017).

Despite the present study's strengths, there remain some important limitations. First, stylized time use data are inferior to diary methods. Although the fixed-effects models guard us from any upward bias in estimates due to a stable over reporting of housework hours, time-varying measurement error due to respondents over reporting more extensively after they have lost their jobs to appear, for example, productive remains possible. Another shortcoming is that our data do not allow us to investigate in detail the role of childcare. Although a sensitivity analysis shows that both women and men spend additional time with their children following a job loss, it is known that women do more of the less enjoyable tasks (e.g., changing nappies, preparing children for school), while men spent more time on interactive and fun activities with children (Sullivan, 2013) something we cannot differentiate. Finally, our data do not include measures of gender ideology, allowing us examining whether job loss may provoke stronger gender-specific reactions in couples holding more traditional attitudes.

Overall, this article highlights that unemployment substantially alters families' daily routines with domestic tasks being shifted to the unemployed spouse and away from the partner as well as an accompanying increase in couples' total household production. Two avenues for future research include the question of why the total home production is extended as well as how couples adapt their care work in response to unemployment. Specifically, longitudinal diary data on couples' time use would be helpful, allowing for a more precise and fine grained measure of activities shedding light, for example, on the questions whether men and women spent additional time with their children differently or whether couples take up previously neglected tasks. On a more general note, our findings support previous studies (e.g., Brand, 2015; Ström, 2003) that it is important to study the effects of job loss as well as other life events from the perspective of families including not only partners but also parents, children, and siblings as their lives are linked and effects spread through family networks and beyond.

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7. Appendix

Table A1 Fixed-effects models for the effects of husband's unemployment on husband's and wife's total and task-specific housework hours on an average weekday

	Husband's hours				Wife's hours			
	Total	Neutral	Female-typed	Male-typed	Total	Neutral	Female-typed	Male-typed
Years before/after unemployment (Reference group: ≤ -3.5 years)								
-2.5 years	0.19**	0.08*	0.07	0.05	0.07	-0.01	0.08	-0.01
	(0.07)	(0.03)	(0.04)	(0.04)	(0.08)	(0.03)	(0.06)	(0.03)
-1.5 years	0.06	0.04	-0.00	0.02	0.04	0.00	0.07	-0.03
	(0.07)	(0.03)	(0.03)	(0.04)	(0.09)	(0.03)	(0.06)	(0.03)
-0.5 years	0.11	-0.01	0.04	0.08	-0.07	-0.03	0.00	-0.03
	(0.07)	(0.03)	(0.03)	(0.04)	(0.08)	(0.03)	(0.06)	(0.03)
0.5 years	2.37***	0.64***	0.92***	0.81***	-0.35***	-0.08**	-0.19**	-0.07*
	(0.09)	(0.04)	(0.05)	(0.06)	(0.09)	(0.03)	(0.06)	(0.03)
1.5 years	2.51***	0.70***	0.98***	0.83***	-0.43***	-0.02	-0.30***	-0.11**
	(0.12)	(0.04)	(0.06)	(0.08)	(0.11)	(0.04)	(0.08)	(0.04)
2.5 years	2.32***	0.59***	1.00***	0.73***	-0.69***	-0.15**	-0.43***	-0.11*
	(0.15)	(0.06)	(0.08)	(0.10)	(0.14)	(0.05)	(0.10)	(0.05)
≥ 3.5 years	2.45***	0.60***	1.06***	0.79***	-0.75***	-0.14**	-0.41***	-0.20***
	(0.18)	(0.06)	(0.09)	(0.12)	(0.15)	(0.05)	(0.10)	(0.05)
Constant	3.02*	0.22	1.15	1.64*	-2.96	-0.11	-2.29	-0.55
	(1.19)	(0.61)	(0.59)	(0.73)	(1.59)	(0.64)	(1.25)	(0.57)

Note: Socio-Economic Panel, v32.1, 1991-2015. *N* = 11,790 couples and 72,977 couple-years.
Models include husband's age and age squared, the number of children aged 0-14 years, dummy variables for children aged 0-1, 2-4, 5-7, 8-10, and 11-14 years, person in need of care, East-Germany, and the year as well as the state unemployment rate. Standard errors are clustered by couple and shown in parentheses.
**p<.05, ** p<.01, *** p<.001.*

Table A2 Fixed-effects models for the effects of wife's unemployment on wife's and husband's total and task-specific housework hours on an average weekday

	Wife's hours				Husband's hours			
	Total	Neutral	Female-typed	Male-typed	Total	Neutral	Female-typed	Male-typed
Years before/after unemployment (Reference group: ≤ -3.5 years)								
-2.5 years	0.10	0.04	0.01	0.05	-0.13	-0.06	0.02	-0.09
	(0.09)	(0.04)	(0.06)	(0.04)	(0.09)	(0.04)	(0.05)	(0.06)
-1.5 years	-0.07	-0.01	-0.10	0.04	-0.10	-0.03	0.02	-0.09
	(0.08)	(0.03)	(0.05)	(0.04)	(0.08)	(0.03)	(0.04)	(0.05)
-0.5 years	0.06	0.01	-0.01	0.06	-0.20*	-0.06	-0.03	-0.11*
	(0.09)	(0.03)	(0.06)	(0.04)	(0.09)	(0.04)	(0.04)	(0.06)
0.5 years	1.94***	0.41***	1.25***	0.28***	-0.31***	-0.05	-0.14***	-0.11*
	(0.10)	(0.04)	(0.07)	(0.04)	(0.09)	(0.04)	(0.04)	(0.06)
1.5 years	1.92***	0.40***	1.23***	0.29***	-0.35**	-0.06	-0.21***	-0.07
	(0.13)	(0.05)	(0.08)	(0.06)	(0.11)	(0.05)	(0.05)	(0.07)
2.5 years	1.87***	0.44***	1.14***	0.29***	-0.28*	-0.05	-0.12	-0.11
	(0.16)	(0.06)	(0.11)	(0.06)	(0.13)	(0.05)	(0.06)	(0.08)
≥ 3.5 years	2.04***	0.35***	1.39***	0.30***	-0.42**	-0.13*	-0.19***	-0.09
	(0.16)	(0.06)	(0.11)	(0.06)	(0.13)	(0.06)	(0.06)	(0.08)
Constant	0.49	-0.10	0.77	-0.18	2.38	-0.27	1.12	1.53
	(1.61)	(0.76)	(1.13)	(0.66)	(1.62)	(0.75)	(0.85)	(0.92)

Note: Socio-Economic Panel, v32.1, 1991-2015. N = 9,339 couples and 53,685 couple-years.
Models include wife's age and age squared, the number of children aged 0-14 years, dummy variables for children aged 0-1, 2-4, 5-7, 8-10, and 11-14 years, person in need of care, East-Germany, and the year as well as the state unemployment rate. Standard errors are clustered by couple and shown in parentheses.
$^{*}p<.05$, $^{**}p<.01$, $^{***}p<.001$.

8. Supplementary material (online only)

Table S1.1 Fixed-effects models for the effects of husband's unemployment on husband's share of total and task-specific housework hours on an average weekday

	Husband's share			
	Total	Neutral	Female-typed	Male-typed
Years before/after unemployment (Reference group: ≤ -3.5 years)				
-2.5 years	0.02^{*}	0.03^{*}	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.02)
-1.5 years	0.01	0.01	-0.00	0.02
	(0.01)	(0.01)	(0.01)	(0.02)
-0.5 years	0.01	0.01	0.01	0.02
	(0.01)	(0.01)	(0.01)	(0.02)
0.5 years	0.20^{***}	0.21^{***}	0.18^{***}	0.14^{***}
	(0.01)	(0.01)	(0.01)	(0.02)
1.5 years	0.21^{***}	0.21^{***}	0.19^{***}	0.16^{***}
	(0.01)	(0.01)	(0.01)	(0.02)
2.5 years	0.21^{***}	0.21^{***}	0.22^{***}	0.17^{***}
	(0.01)	(0.02)	(0.01)	(0.02)
≥ 3.5 years	0.22^{***}	0.22^{***}	0.22^{***}	0.20^{***}
	(0.01)	(0.02)	(0.02)	(0.02)
Constant	0.74^{***}	0.10	0.56^{***}	1.63^{***}
	(0.13)	(0.22)	(0.14)	(0.30)
N couples	11,778	11,488	11,733	8,938
N couple-years	72,832	69,985	72,466	48,735

Note: Socio-Economic Panel, v32.1, 1991-2015.
Table A1 lists included control variables. Standard errors are clustered by couple and shown in parentheses.
**p<.05,* ** *p<.01,* ****p<.001.*

Table S1.2 Fixed-effects models for the effects of wife's unemployment on wife's share of total and task-specific housework hours on an average weekday

	Wife's share			
	Total	Neutral	Female-typed	Male-typed
Years before/after unemployment (Reference group: ≤ -3.5 years)				
-2.5 years	0.03*	0.03*	0.00	0.03
	(0.01)	(0.01)	(0.01)	(0.02)
-1.5 years	0.01	0.00	-0.01	0.02
	(0.01)	(0.01)	(0.01)	(0.02)
-0.5 years	0.03*	0.02	0.01	0.04*
	(0.01)	(0.01)	(0.01)	(0.02)
0.5 years	0.11***	0.07***	0.09***	0.10***
	(0.01)	(0.01)	(0.01)	(0.02)
1.5 years	0.11***	0.08***	0.09***	0.09***
	(0.01)	(0.02)	(0.01)	(0.02)
2.5 years	0.10***	0.07***	0.08***	0.08**
	(0.01)	(0.02)	(0.01)	(0.03)
≥ 3.5 years	0.12***	0.09***	0.09***	0.11***
	(0.01)	(0.02)	(0.01)	(0.03)
Constant	0.40*	1.13***	0.62**	-0.12
	(0.17)	(0.28)	(0.20)	(0.37)
N couples	9,331	9,085	9,291	7,131
N couple-years	53,441	51,187	53,105	36,249

Note: Socio-Economic Panel, v32.1, 1991-2015.
Table A2 lists included control variables. Standard errors are clustered by couple and shown in parentheses.
$^{*}p<.05$, $^{**}p<.01$, $^{***}p<.001$.

Table S2.1 Fixed-effects models for the effects of husband's unemployment on husband's and wife's total and task-specific housework hours on an average weekday (additional control for husband's and wife's health)

	Husband's hours				Wife's hours			
	Total	Neutral	Female-typed	Male-typed	Total	Neutral	Female-typed	Male-typed
Years before/after unemployment (Reference group: ≤ -3.5 years)								
-2.5 years	0.19**	0.07*	0.07	0.05	0.07	-0.01	0.08	-0.01
	(0.07)	(0.03)	(0.04)	(0.04)	(0.08)	(0.03)	(0.06)	(0.03)
-1.5 years	0.06	0.04	-0.00	0.02	0.04	0.00	0.07	-0.03
	(0.07)	(0.03)	(0.03)	(0.04)	(0.09)	(0.03)	(0.06)	(0.03)
-0.5 years	0.11	-0.02	0.04	0.08	-0.07	-0.03	0.00	-0.04
	(0.07)	(0.03)	(0.03)	(0.04)	(0.08)	(0.03)	(0.06)	(0.03)
0.5 years	2.37***	0.64***	0.92***	0.81***	-0.35***	-0.08**	-0.19**	-0.07*
	(0.09)	(0.04)	(0.05)	(0.06)	(0.09)	(0.03)	(0.06)	(0.03)
1.5 years	2.50***	0.70***	0.98***	0.83***	-0.43***	-0.02	-0.30***	-0.11**
	(0.12)	(0.04)	(0.06)	(0.08)	(0.11)	(0.04)	(0.08)	(0.04)
2.5 years	2.32***	0.58***	1.00***	0.73***	-0.69***	-0.15**	-0.43***	-0.11*
	(0.15)	(0.06)	(0.08)	(0.10)	(0.14)	(0.05)	(0.10)	(0.05)
≥ 3.5 years	2.44***	0.59***	1.06***	0.79***	-0.75***	-0.14**	-0.41***	-0.21***
	(0.18)	(0.06)	(0.09)	(0.12)	(0.15)	(0.05)	(0.10)	(0.05)
Constant	3.04*	0.25	1.16*	1.63*	-2.95	-0.11	-2.30	-0.55
	(1.19)	(0.61)	(0.59)	(0.73)	(1.59)	(0.64)	(1.25)	(0.57)

Note: Socio-Economic Panel, v32.1, 1991-2015. N = 11,790 couples and 72,977 couple-years.
Table A1 lists included control variables. Models in addition include husband's and wife's health satisfaction (0-10). Standard errors are clustered by couple and shown in parentheses.
$^{*}p<.05$, $^{**}p<.01$, $^{***}p<.001$.

Table S2.2 Fixed-effects models for the effects of wife's unemployment on wife's and husband's total and task-specific housework hours on an average weekday (additional control for husband's and wife's health)

	Wife's hours				Husband's hours			
	Total	Neutral	Female-typed	Male-typed	Total	Neutral	Female-typed	Male-typed
Years before/after unemployment (Reference group: ≤ -3.5 years)								
-2.5 years	0.10	0.04	0.01	0.05	-0.13	-0.06	0.02	-0.09
	(0.09)	(0.04)	(0.06)	(0.04)	(0.09)	(0.04)	(0.05)	(0.06)
-1.5 years	-0.07	-0.01	-0.10	0.04	-0.10	-0.03	0.02	-0.09
	(0.08)	(0.03)	(0.05)	(0.04)	(0.08)	(0.03)	(0.04)	(0.05)
-0.5 years	0.06	0.01	-0.01	0.06	-0.20*	-0.06	-0.03	-0.11*
	(0.09)	(0.03)	(0.06)	(0.04)	(0.09)	(0.04)	(0.04)	(0.06)
0.5 years	1.94***	0.41***	1.25***	0.28***	-0.31***	-0.05	-0.14***	-0.11*
	(0.10)	(0.04)	(0.07)	(0.04)	(0.09)	(0.04)	(0.04)	(0.06)
1.5 years	1.92***	0.40***	1.23***	0.29***	-0.35**	-0.07	-0.21***	-0.07
	(0.13)	(0.05)	(0.08)	(0.06)	(0.11)	(0.05)	(0.05)	(0.07)
2.5 years	1.87***	0.44***	1.14***	0.29***	-0.28*	-0.05	-0.12	-0.11
	(0.16)	(0.06)	(0.11)	(0.06)	(0.13)	(0.05)	(0.06)	(0.08)
≥ 3.5 years	2.04***	0.35***	1.39***	0.30***	-0.42**	-0.14*	-0.19***	-0.09
	(0.16)	(0.06)	(0.11)	(0.06)	(0.13)	(0.06)	(0.06)	(0.08)
Constant	0.49	-0.08	0.74	-0.17	2.42	-0.23	1.13	1.52
	(1.61)	(0.76)	(1.13)	(0.66)	(1.62)	(0.75)	(0.85)	(0.92)

Note: Socio-Economic Panel, v32.1, 1991-2015. N = 9,339 couples and 53,685 couple-years.
Table A2 lists included control variables. Models in addition include husband's and wife's health satisfaction (0-10). Standard errors are clustered by couple and shown in parentheses.
$^{*}p<.05$, $^{**}p<.01$, $^{***}p<.001$.

Table S3.1 Fixed-effects models for the effects of husband's unemployment on husband's and wife's total and task-specific housework hours on an average weekday (additional control for wife's employment status)

	Husband's hours				Wife's hours			
	Total	Neutral	Female-typed	Male-typed	Total	Neutral	Female-typed	Male-typed
Years before/after unemployment (Reference group: ≤ -3.5 years)								
-2.5 years	0.19**	0.08*	0.07	0.05	0.04	-0.01	0.06	-0.01
	(0.07)	(0.03)	(0.03)	(0.04)	(0.08)	(0.03)	(0.06)	(0.03)
-1.5 years	0.07	0.04	0.00	0.02	-0.03	-0.01	0.02	-0.03
	(0.07)	(0.03)	(0.03)	(0.04)	(0.08)	(0.03)	(0.06)	(0.03)
-0.5 years	0.12	-0.01	0.05	0.08	-0.10	-0.04	-0.02	-0.04
	(0.07)	(0.03)	(0.03)	(0.04)	(0.08)	(0.03)	(0.06)	(0.03)
0.5 years	2.38***	0.64***	0.93***	0.81***	-0.43***	-0.10**	-0.25***	-0.08**
	(0.09)	(0.04)	(0.05)	(0.06)	(0.08)	(0.03)	(0.06)	(0.03)
1.5 years	2.52***	0.70***	0.98***	0.83***	-0.50***	-0.03	-0.35***	-0.12**
	(0.12)	(0.04)	(0.06)	(0.08)	(0.10)	(0.04)	(0.07)	(0.04)
2.5 years	2.34***	0.59***	1.01***	0.73***	-0.79***	-0.17***	-0.50***	-0.12*
	(0.15)	(0.06)	(0.08)	(0.10)	(0.13)	(0.05)	(0.09)	(0.05)
≥ 3.5 years	2.45***	0.60***	1.07***	0.79***	-0.82***	-0.16**	-0.45***	-0.21***
	(0.18)	(0.06)	(0.09)	(0.12)	(0.14)	(0.05)	(0.10)	(0.05)
Constant	3.08**	0.23	1.20*	1.65*	-3.46*	-0.19	-2.69*	-0.58
	(1.19)	(0.61)	(0.59)	(0.73)	(1.55)	(0.64)	(1.21)	(0.57)

Note: Socio-Economic Panel, v32.1, 1991-2015. *N* = 11,790 couples and 72,977 couple-years.
Table A1 lists included control variables. Models in addition include indicators for wife's employment status. Standard errors are clustered by couple and shown in parentheses.
$^{*}p<.05$, $^{**}p<.01$, $^{***}p<.001$.

Table S3.2 Fixed-effects models for the effects of wife's unemployment on wife's and husband's total and task-specific housework hours on an average weekday (additional control for husband's employment status)

	Wife's hours				Husband's hours			
	Total	Neutral	Female-typed	Male-typed	Total	Neutral	Female-typed	Male-typed
Years before/after unemployment (Reference group: ≤ -3.5 years)								
-2.5 years	0.10	0.04	0.01	0.05	-0.12	-0.06	0.02	-0.09
	(0.09)	(0.04)	(0.06)	(0.04)	(0.08)	(0.03)	(0.04)	(0.06)
-1.5 years	-0.07	-0.01	-0.10	0.04	-0.14	-0.04	0.00	-0.10*
	(0.08)	(0.03)	(0.05)	(0.04)	(0.08)	(0.03)	(0.04)	(0.05)
-0.5 years	0.06	0.01	-0.01	0.06	-0.20*	-0.06	-0.03	-0.11*
	(0.09)	(0.03)	(0.06)	(0.04)	(0.08)	(0.03)	(0.04)	(0.05)
0.5 years	1.95***	0.41***	1.26***	0.28***	-0.38***	-0.07*	-0.17***	-0.14*
	(0.10)	(0.04)	(0.07)	(0.04)	(0.08)	(0.03)	(0.04)	(0.06)
1.5 years	1.93***	0.40***	1.23***	0.29***	-0.42***	-0.08	-0.24***	-0.10
	(0.13)	(0.05)	(0.08)	(0.06)	(0.10)	(0.04)	(0.05)	(0.07)
2.5 years	1.88***	0.44***	1.15***	0.29***	-0.41***	-0.09	-0.17**	-0.15*
	(0.16)	(0.06)	(0.11)	(0.06)	(0.12)	(0.05)	(0.06)	(0.08)
≥ 3.5 years	2.05***	0.35***	1.40***	0.30***	-0.48***	-0.15**	-0.22***	-0.11
	(0.16)	(0.06)	(0.11)	(0.06)	(0.11)	(0.05)	(0.05)	(0.08)
Constant	0.49	-0.10	0.78	-0.19	2.27	-0.31	1.06	1.51
	(1.61)	(0.76)	(1.13)	(0.66)	(1.56)	(0.74)	(0.82)	(0.91)

Note: Socio-Economic Panel, v32.1, 1991-2015. N = 9,339 couples and 53,685 couple-years.
Table A2 lists included control variables. Models in addition include indicators for husband's employment status. Standard errors are clustered by couple and shown in parentheses.
*$p<.05$, **$p<.01$, ***$p<.001$.

Table S4.1 Fixed-effects models for the effects of husband's unemployment on husband's and wife's total and task-specific housework hours on an average weekday (additional control for marital status)

	Husband's hours				Wife's hours			
	Total	Neutral	Female-typed	Male-typed	Total	Neutral	Female-typed	Male-typed
Years before/after unemployment (Reference group: ≤ -3.5 years)								
-2.5 years	0.19**	0.08*	0.07	0.05	0.06	-0.01	0.08	-0.01
	(0.07)	(0.03)	(0.04)	(0.04)	(0.08)	(0.03)	(0.06)	(0.03)
-1.5 years	0.06	0.04	-0.00	0.02	0.04	0.00	0.07	-0.03
	(0.07)	(0.03)	(0.03)	(0.04)	(0.09)	(0.03)	(0.06)	(0.03)
-0.5 years	0.11	-0.01	0.04	0.08	-0.07	-0.03	0.00	-0.03
	(0.07)	(0.03)	(0.03)	(0.04)	(0.08)	(0.03)	(0.06)	(0.03)
0.5 years	2.37***	0.64***	0.92***	0.81***	-0.35***	-0.08**	-0.20**	-0.07*
	(0.09)	(0.04)	(0.05)	(0.06)	(0.09)	(0.03)	(0.06)	(0.03)
1.5 years	2.51***	0.70***	0.98***	0.83***	-0.43***	-0.02	-0.31***	-0.11**
	(0.12)	(0.04)	(0.06)	(0.08)	(0.11)	(0.04)	(0.08)	(0.04)
2.5 years	2.33***	0.59***	1.00***	0.73***	-0.69***	-0.15**	-0.44***	-0.11*
	(0.15)	(0.06)	(0.08)	(0.10)	(0.14)	(0.05)	(0.10)	(0.05)
≥ 3.5 years	2.45***	0.60***	1.06***	0.79***	-0.75***	-0.14**	-0.41***	-0.20***
	(0.18)	(0.06)	(0.09)	(0.12)	(0.15)	(0.05)	(0.10)	(0.05)
Constant	2.88*	0.16	1.07	1.65*	-2.82	-0.11	-2.18	-0.54
	(1.19)	(0.61)	(0.59)	(0.73)	(1.59)	(0.64)	(1.25)	(0.57)

Note: Socio-Economic Panel, v32.1, 1991-2015. N = 11,790 couples and 72,977 couple-years.
Table A1 lists included control variables. Models in addition include an indicator for couple's marital status. Standard errors are clustered by couple and shown in parentheses.
*$p<.05$, **$p<.01$, ***$p<.001$.

Table S4.2 Fixed-effects models for the effects of wife's unemployment on wife's and husband's total and task-specific housework hours on an average weekday (additional control for marital status)

	Wife's hours				Husband's hours			
	Total	Neutral	Female-typed	Male-typed	Total	Neutral	Female-typed	Male-typed
Years before/after unemployment (Reference group: ≤ -3.5 years)								
-2.5 years	0.10	0.04	0.01	0.05	-0.13	-0.06	0.02	-0.09
	(0.09)	(0.04)	(0.06)	(0.04)	(0.09)	(0.04)	(0.05)	(0.06)
-1.5 years	-0.07	-0.01	-0.10	0.04	-0.10	-0.03	0.02	-0.09
	(0.08)	(0.03)	(0.05)	(0.04)	(0.08)	(0.03)	(0.04)	(0.05)
-0.5 years	0.06	0.01	-0.01	0.06	-0.20*	-0.06	-0.03	-0.11*
	(0.09)	(0.03)	(0.06)	(0.04)	(0.09)	(0.04)	(0.04)	(0.06)
0.5 years	1.94***	0.41***	1.25***	0.28***	-0.30***	-0.05	-0.14***	-0.11*
	(0.10)	(0.04)	(0.07)	(0.04)	(0.09)	(0.04)	(0.04)	(0.06)
1.5 years	1.92***	0.40***	1.22***	0.29***	-0.34**	-0.06	-0.21***	-0.07
	(0.13)	(0.05)	(0.08)	(0.06)	(0.11)	(0.05)	(0.05)	(0.07)
2.5 years	1.86***	0.44***	1.14***	0.29***	-0.28*	-0.05	-0.12	-0.11
	(0.16)	(0.06)	(0.11)	(0.06)	(0.13)	(0.05)	(0.06)	(0.08)
≥ 3.5 years	2.04***	0.35***	1.39***	0.30***	-0.42**	-0.13*	-0.19***	-0.09
	(0.16)	(0.06)	(0.11)	(0.06)	(0.13)	(0.06)	(0.06)	(0.08)
Constant	0.63	-0.10	0.90	-0.17	2.21	-0.39	1.04	1.56
	(1.61)	(0.76)	(1.13)	(0.67)	(1.62)	(0.75)	(0.85)	(0.92)

Note: Socio-Economic Panel, v32.1, 1991-2015. N = 9,339 couples and 53,685 couple-years.
Table A2 lists included control variables. Models in addition include an indicator for couple's marital status. Standard errors are clustered by couple and shown in parentheses.
* *p<.05,* ** *p<.01,* *** *p<.001.*

Table S5.1 Fixed-effects models for the effects of husband's unemployment on husband's and wife's total and task-specific housework hours on an average weekday (additional control for home characteristics)

	Husband's hours				Wife's hours			
	Total	Neutral	Female-typed	Male-typed	Total	Neutral	Female-typed	Male-typed
Years before/after unemployment (Reference group: ≤ -3.5 years)								
-2.5 years	0.20**	0.08*	0.06	0.06	0.08	-0.01	0.08	-0.00
	(0.07)	(0.03)	(0.04)	(0.04)	(0.08)	(0.03)	(0.06)	(0.03)
-1.5 years	0.07	0.04	-0.00	0.04	0.05	0.00	0.07	-0.02
	(0.07)	(0.03)	(0.03)	(0.04)	(0.09)	(0.03)	(0.06)	(0.03)
-0.5 years	0.13	-0.01	0.04	0.10*	-0.05	-0.03	0.00	-0.02
	(0.07)	(0.03)	(0.03)	(0.04)	(0.08)	(0.03)	(0.06)	(0.03)
0.5 years	2.39***	0.64***	0.92***	0.83***	-0.33***	-0.08**	-0.19**	-0.05
	(0.09)	(0.04)	(0.05)	(0.06)	(0.09)	(0.03)	(0.06)	(0.03)
1.5 years	2.54***	0.70***	0.97***	0.87***	-0.40***	-0.02	-0.30***	-0.08*
	(0.12)	(0.04)	(0.06)	(0.08)	(0.11)	(0.04)	(0.08)	(0.04)
2.5 years	2.36***	0.59***	1.00***	0.78***	-0.64***	-0.15**	-0.42***	-0.07
	(0.15)	(0.06)	(0.08)	(0.10)	(0.14)	(0.05)	(0.10)	(0.05)
≥ 3.5 years	2.51***	0.59***	1.05***	0.86***	-0.68***	-0.14**	-0.39***	-0.15**
	(0.18)	(0.06)	(0.09)	(0.12)	(0.15)	(0.05)	(0.10)	(0.05)
Constant	3.35**	0.21	1.14	2.00**	-2.68	-0.10	-2.25	-0.32
	(1.19)	(0.61)	(0.59)	(0.73)	(1.59)	(0.64)	(1.25)	(0.57)

Note: Socio-Economic Panel, v32.1, 1991-2015. N = 11,790 couples and 72,977 couple-years.
Table A1 lists included control variables. Models in addition include home size/10 and home size/10 squared (in square meters) as well as indicators for whether home is owned, home has a garden, and home is a house. Standard errors are clustered by couple and shown in parentheses.
**p<.05*, ***p<.01*, ****p<.001*.

Table S5.2 Fixed-effects models for the effects of wife's unemployment on wife's and husband's total and task-specific housework hours on an average weekday (additional control for home characteristics)

	Wife's hours				Husband's hours			
	Total	Neutral	Female-typed	Male-typed	Total	Neutral	Female-typed	Male-typed
Years before/after unemployment (Reference group: ≤ -3.5 years)								
-2.5 years	0.11	0.04	0.01	0.06	-0.12	-0.06	0.02	-0.08
	(0.09)	(0.04)	(0.06)	(0.04)	(0.09)	(0.04)	(0.05)	(0.06)
-1.5 years	-0.05	-0.01	-0.10	0.05	-0.08	-0.03	0.02	-0.07
	(0.08)	(0.03)	(0.05)	(0.04)	(0.08)	(0.03)	(0.04)	(0.05)
-0.5 years	0.07	0.01	-0.01	0.07	-0.19*	-0.06	-0.03	-0.09
	(0.09)	(0.03)	(0.06)	(0.04)	(0.09)	(0.04)	(0.04)	(0.06)
0.5 years	1.95***	0.41***	1.26***	0.29***	-0.29***	-0.05	-0.14***	-0.10
	(0.10)	(0.04)	(0.07)	(0.04)	(0.09)	(0.04)	(0.04)	(0.06)
1.5 years	1.94***	0.40***	1.23***	0.31***	-0.33**	-0.06	-0.21***	-0.06
	(0.13)	(0.05)	(0.08)	(0.06)	(0.11)	(0.05)	(0.05)	(0.07)
2.5 years	1.88***	0.44***	1.15***	0.30***	-0.27*	-0.05	-0.12*	-0.09
	(0.16)	(0.06)	(0.11)	(0.06)	(0.13)	(0.05)	(0.06)	(0.08)
≥ 3.5 years	2.06***	0.35***	1.39***	0.32***	-0.40**	-0.13*	-0.20***	-0.07
	(0.16)	(0.06)	(0.11)	(0.06)	(0.13)	(0.06)	(0.06)	(0.08)
Constant	0.61	-0.08	0.78	-0.10	2.42	-0.27	1.07	1.62
	(1.61)	(0.76)	(1.13)	(0.66)	(1.62)	(0.75)	(0.85)	(0.91)

Note: Socio-Economic Panel, v32.1, 1991-2015. N = 9,339 couples and 53,685 couple-years.
Table A2 lists included control variables. Models in addition include home size/10 and home size/10 squared (in square meters) as well as indicators for whether home is owned, home has a garden, and home is a house. Standard errors are clustered by couple and shown in parentheses.
$^{*}p<.05$, $^{**}p<.01$, $^{***}p<.001$.

Table S6.1 Fixed-effects models for the effects of husband's unemployment on husband's and wife's total and task-specific housework hours on an average weekday (additional control for household income)

	Husband's hours				Wife's hours			
	Total	Neutral	Female-typed	Male-typed	Total	Neutral	Female-typed	Male-typed
Years before/after unemployment (Reference group: ≤ -3.5 years)								
-2.5 years	0.19**	0.08*	0.07	0.05	0.05	-0.01	0.07	-0.01
	(0.07)	(0.03)	(0.04)	(0.04)	(0.08)	(0.03)	(0.06)	(0.03)
-1.5 years	0.06	0.04	-0.00	0.02	0.02	-0.00	0.05	-0.03
	(0.07)	(0.03)	(0.03)	(0.04)	(0.08)	(0.03)	(0.06)	(0.03)
-0.5 years	0.11	-0.01	0.04	0.08	-0.09	-0.04	-0.02	-0.04
	(0.07)	(0.03)	(0.03)	(0.04)	(0.08)	(0.03)	(0.06)	(0.03)
0.5 years	2.37***	0.64***	0.92***	0.81***	-0.44***	-0.10**	-0.27***	-0.08*
	(0.09)	(0.04)	(0.05)	(0.06)	(0.09)	(0.03)	(0.06)	(0.03)
1.5 years	2.50***	0.70***	0.98***	0.83***	-0.53***	-0.03	-0.38***	-0.12**
	(0.12)	(0.04)	(0.06)	(0.08)	(0.11)	(0.04)	(0.08)	(0.04)
2.5 years	2.32***	0.59***	1.00***	0.73***	-0.81***	-0.17***	-0.52***	-0.12*
	(0.15)	(0.06)	(0.08)	(0.10)	(0.14)	(0.05)	(0.10)	(0.05)
≥ 3.5 years	2.45***	0.60***	1.06***	0.79***	-0.88***	-0.16**	-0.51***	-0.21***
	(0.18)	(0.06)	(0.09)	(0.12)	(0.15)	(0.05)	(0.10)	(0.05)
Constant	3.03*	0.23	1.14	1.66*	-1.97	0.04	-1.53	-0.47
	(1.19)	(0.61)	(0.59)	(0.73)	(1.58)	(0.64)	(1.24)	(0.57)

Note: Socio-Economic Panel, v32.1, 1991-2015. N = 11,790 couples and 72,977 couple-years.
Table A1 lists included control variables. Models in addition include equivalised disposable household income/1000 and household income/1000 squared (in 2011 Euro). Standard errors are clustered by couple and shown in parentheses.
$^{*}p<.05$, $^{**}p<.01$, $^{***}p<.001$.

Table S6.2 Fixed-effects models for the effects of wife's unemployment on wife's and husband's total and task-specific housework hours on an average weekday (additional control for household income)

	Wife's hours				Husband's hours			
	Total	Neutral	Female-typed	Male-typed	Total	Neutral	Female-typed	Male-typed
Years before/after unemployment (Reference group: ≤ -3.5 years)								
-2.5 years	0.10	0.04	0.01	0.05	-0.14	-0.07	0.02	-0.09
	(0.09)	(0.04)	(0.06)	(0.04)	(0.09)	(0.04)	(0.05)	(0.06)
-1.5 years	-0.07	-0.01	-0.10	0.04	-0.11	-0.03	0.02	-0.09
	(0.08)	(0.03)	(0.05)	(0.04)	(0.08)	(0.03)	(0.04)	(0.05)
-0.5 years	0.05	0.01	-0.01	0.06	-0.22^{*}	-0.07	-0.04	-0.12^{*}
	(0.09)	(0.03)	(0.06)	(0.04)	(0.09)	(0.04)	(0.04)	(0.06)
0.5 years	1.93^{***}	0.41^{***}	1.24^{***}	0.28^{***}	-0.36^{***}	-0.07	-0.16^{***}	-0.13^{*}
	(0.10)	(0.04)	(0.07)	(0.04)	(0.09)	(0.04)	(0.04)	(0.06)
1.5 years	1.91^{***}	0.40^{***}	1.21^{***}	0.29^{***}	-0.41^{***}	-0.08	-0.24^{***}	-0.09
	(0.13)	(0.05)	(0.09)	(0.06)	(0.11)	(0.05)	(0.05)	(0.07)
2.5 years	1.85^{***}	0.43^{***}	1.12^{***}	0.29^{***}	-0.36^{**}	-0.07	-0.16^{*}	-0.13
	(0.16)	(0.06)	(0.11)	(0.06)	(0.13)	(0.05)	(0.06)	(0.08)
≥ 3.5 years	2.02^{***}	0.35^{***}	1.37^{***}	0.30^{***}	-0.52^{***}	-0.16^{**}	-0.24^{***}	-0.12
	(0.16)	(0.06)	(0.11)	(0.06)	(0.13)	(0.06)	(0.06)	(0.08)
Constant	0.55	-0.09	0.83	-0.19	2.63	-0.20	1.23	1.60
	(1.61)	(0.76)	(1.13)	(0.66)	(1.61)	(0.75)	(0.84)	(0.92)

Note: Socio-Economic Panel, v32.1, 1991-2015. N = 9,339 couples and 53,685 couple-years.
Table A2 lists included control variables. Models in addition include equivalised disposable household income/1000 and household income/1000 squared (in 2011 Euro). Standard errors are clustered by couple and shown in parentheses.
**p<.05*, ***p<.01*, ****p<.001*.

Table S7.1 Fixed-effects models for the effects of husband's unemployment on husband's and wife's childcare hours on an average weekday

	Husband's hours	Wife's hours
Years before/after unemployment (Reference group: ≤ -3.5 years)		
-2.5 years	-0.01	0.03
	(0.09)	(0.23)
-1.5 years	-0.06	0.32
	(0.10)	(0.23)
-0.5 years	-0.11	0.11
	(0.11)	(0.24)
0.5 years	1.66***	-0.24
	(0.14)	(0.24)
1.5 years	1.65***	-0.32
	(0.18)	(0.30)
≥ 2.5 years	1.74***	-0.31
	(0.25)	(0.37)
Constant	-0.24	4.58
	(1.68)	(4.21)

Note: Socio-Economic Panel, v32.1, 1991-2015. *N* = 5,343 couples and 24,485 couple-years.
Table A1 lists included control variables. Standard errors are clustered by couple and shown in parentheses.
$^{*}p<.05$, $^{**}p<.01$, $^{***}p<.001$.

Table S7.2 Fixed-effects models for the effects of wife's unemployment on husband's and wife's childcare hours on an average weekday

	Wife's hours	Husband's hours
Years before/after unemployment		
(Reference group: ≤ -3.5 years)		
-2.5 years	-0.12	-0.10
	(0.21)	(0.16)
-1.5 years	0.00	-0.18
	(0.24)	(0.16)
-0.5 years	-0.02	-0.24
	(0.25)	(0.16)
0.5 years	1.64***	-0.45**
	(0.27)	(0.17)
1.5 years	2.10***	-0.42
	(0.33)	(0.22)
≥ 2.5 years	2.36***	-0.33
	(0.39)	(0.23)
Constant	13.40**	4.29
	(5.09)	(2.93)

Note: Socio-Economic Panel, v32.1, 1991-2015. *N* = 4,090 couples and 17,013 couple-years.
Table A2 lists included control variables. Standard errors are clustered by couple and shown in parentheses.
* *p<.05,* ** *p<.01,* *** *p<.001.*

Table S8.1 Fixed-effects models for the effects of husband's unemployment out of full-time employment on husband's and wife's total and task-specific housework hours on an average weekday

	Husband's hours				Wife's hours			
	Total	Neutral	Female-typed	Male-typed	Total	Neutral	Female-typed	Male-typed
Years before/after unemployment (Reference group: ≤ -3.5 years)								
-2.5 years	0.19*	0.08*	0.07*	0.04	0.08	0.01	0.09	-0.03
	(0.07)	(0.03)	(0.04)	(0.05)	(0.09)	(0.04)	(0.06)	(0.03)
-1.5 years	0.07	0.04	0.01	0.02	0.00	-0.00	0.06	-0.05
	(0.07)	(0.03)	(0.03)	(0.04)	(0.09)	(0.03)	(0.07)	(0.03)
-0.5 years	0.07	-0.02	0.04	0.05	-0.08	-0.03	-0.00	-0.05
	(0.07)	(0.03)	(0.03)	(0.05)	(0.09)	(0.03)	(0.06)	(0.03)
0.5 years	2.44***	0.67***	0.95***	0.82***	-0.37***	-0.09**	-0.21**	-0.08*
	(0.10)	(0.04)	(0.05)	(0.06)	(0.09)	(0.03)	(0.06)	(0.03)
1.5 years	2.57***	0.73***	1.01***	0.84***	-0.43***	-0.01	-0.30***	-0.12**
	(0.12)	(0.05)	(0.06)	(0.08)	(0.12)	(0.04)	(0.08)	(0.04)
2.5 years	2.48***	0.62***	1.06***	0.80***	-0.74***	-0.14**	-0.46***	-0.14**
	(0.16)	(0.06)	(0.08)	(0.11)	(0.14)	(0.05)	(0.10)	(0.05)
≥ 3.5 years	2.54***	0.63***	1.10***	0.81***	-0.80***	-0.13*	-0.43***	-0.24***
	(0.18)	(0.07)	(0.09)	(0.12)	(0.16)	(0.05)	(0.11)	(0.05)
Constant	3.39**	0.41	1.20*	1.78*	-3.01	-0.06	-2.45	-0.50
	(1.20)	(0.62)	(0.60)	(0.74)	(1.61)	(0.65)	(1.27)	(0.58)

Note: Socio-Economic Panel, v32.1, 1991-2015. N = 11,334 couples and 70,213 couple-years.
Table A1 lists included control variables. Standard errors are clustered by couple and shown in parentheses.
**p<.05,* ** *p<.01,* ****p<.001.*

Table S8.2 Fixed-effects models for the effects of wife's unemployment out of full-time employment on wife's and husband's total and task-specific housework hours on an average weekday

	Wife's hours				Husband's hours			
	Total	Neutral	Female-typed	Male-typed	Total	Neutral	Female-typed	Male-typed
Years before/after unemployment (Reference group: ≤ -3.5 years)								
-2.5 years	0.02	0.08	-0.04	-0.03	-0.13	-0.02	0.03	-0.14
	(0.11)	(0.05)	(0.08)	(0.05)	(0.14)	(0.06)	(0.08)	(0.09)
-1.5 years	0.02	0.02	-0.02	0.02	-0.25*	-0.07	-0.02	-0.16*
	(0.11)	(0.04)	(0.07)	(0.05)	(0.12)	(0.05)	(0.07)	(0.08)
-0.5 years	0.04	0.04	0.01	-0.01	-0.29*	-0.07	-0.01	-0.20*
	(0.11)	(0.05)	(0.08)	(0.05)	(0.12)	(0.05)	(0.06)	(0.08)
0.5 years	2.48***	0.55***	1.64***	0.29***	-0.55***	-0.12*	-0.22***	-0.21*
	(0.14)	(0.05)	(0.09)	(0.06)	(0.12)	(0.05)	(0.06)	(0.08)
1.5 years	2.43***	0.54***	1.60***	0.30***	-0.53***	-0.09	-0.34***	-0.10
	(0.18)	(0.07)	(0.12)	(0.08)	(0.16)	(0.07)	(0.08)	(0.11)
2.5 years	2.44***	0.64***	1.51***	0.28***	-0.55***	-0.16*	-0.23**	-0.15
	(0.23)	(0.08)	(0.16)	(0.08)	(0.16)	(0.07)	(0.09)	(0.10)
≥ 3.5 years	2.59***	0.50***	1.78***	0.31***	-0.57**	-0.23**	-0.21*	-0.12
	(0.22)	(0.07)	(0.16)	(0.08)	(0.18)	(0.08)	(0.08)	(0.12)
Constant	-0.02	0.44	-0.75	0.29	3.10	0.02	2.66*	0.42
	(2.01)	(1.04)	(1.30)	(0.88)	(2.18)	(1.04)	(1.20)	(1.25)

Note: Socio-Economic Panel, v32.1, 1991-2015. N = 4,661 couples and 24,258 couple-years.
Table A2 lists included control variables. Standard errors are clustered by couple and shown in parentheses.
$^{*}p<.05$, $^{**}p<.01$, $^{***}p<.001$.