# Essays on Structural Change and Social Integration

Inauguraldissertation zur Erlangung des Doktorgrades des
Fachbereichs Wirtschaftswissenschaften der
Ruprecht-Karls-Universität Heidelberg

vorgelegt von
Boris IVANOV

Heidelberg
Oktober 2022

# Acknowledgements

During the work on this dissertation I received financial support from the Leibniz Professorship for Applied Labor Economics at the University of Heidelberg (P56/2017), the SEEK-Project 2016: Fiscal and Labor Policy Institutions and Inequality at ZEW Mannheim (chapters 1 and 2) and the research project 'Evaluation of the Federal Program 'Social Integration within the Labor Market" (SILM) financed by the Federal Ministry of Labor and Social Affairs (chapter 3).

I would like thank my co-authors Melanie Arntz, Friedhelm Pfeiffer and Laura Pohlan for their contributions to the first and third chapter of this dissertation and Martin Brussig and the entire team of the SILM evaluation consortium. I greatly enjoyed our collaboration and a good part of what I have learnt over the past years, I owe to them.

Great thanks go to my advisor Christina Gathmann for her feedback and guidance throughout the making of this dissertation. Especially her continuous and patient support during my search for a suitable solo authored project is very much appreciated. Her focus on the clarity of arguments has greatly improved each of the chapters of this dissertation.

I further thank my colleagues at ZEW Mannheim and especially those of the research unit 'Labor Markets and Social Insurance'. Whenever I was in need of a quick brainstorming or a discussion of arguments or methods there was someone I could turn to. It is this atmosphere of openness and cooperativeness that I appreciate the most about this institute. To name only a few, I would like to thank Eduard Brüll for his more than generous attitude towards sharing code or data and his passion for discussing research of any kind anytime. I also thank Yasemin Karamik, Caroline Knebel, Laura Arnemann and Jan Köhler of the Scrumademics crew who helped me to structure my tasks and to commit also to the less beloved ones. Special credit goes to Marius Berger, Marius Alt and Janna Axenbeck, who introduced me to bouldering – the only kind of regular physical activity I managed to maintain, as it turned out. I thank Kornelia Peter as the representative of our colleagues in the works council for their commitment and for the lessons I learned when I was a member myself.

I thank my friends and especially my parents and brothers for their unconditional support and the good times we are having.

Above all, I am grateful to you, Lisa.
Your encouragement, wisdom and love have made this possible.

# Contents

# List of Tables

# List of Figures

# Executive Summary

Work is not only the most important source of income for the majority of people, it is an integral part of everyday culture, it defines identities and shapes political beliefs. And more than anything, it is subject to change. Thanks to its ingenuity, humankind has generated a steady stream of tools to replace itself in an ever increasing number of tasks. For example, the power loom almost entirely automated human labor inputs to weaving during the industrial revolution (Burwick, 2015). Over the past decades, automation and globalisation have substituted many middle-paying jobs in manufacturing and clerical occupations by both low and high skilled service jobs (Autor et al., 2003; Goos et al., 2014). In the future, artificial intelligence may take over many other tasks as well, like driving cars, giving legal advice or even composing pop songs (Susskind and Susskind, 2015; Heaven, 2020).

Of course, we still have not run out of work so far, but technological progress alters the employment and earnings prospects of workers with different kinds of skills. Providing workers with the means to benefit from technological progress is central to maintaining social cohesion during structural transformation (see e.g, Caprettini and Voth, 2018). It is therefore important to understand how individuals adjust to changes in the task structure and skill requirements of work.

The present dissertation contributes to this effort by studying the role of job loss during times of structural change and how policy interventions can improve the well-being of unemployed individuals. The first chapter explores how local labor market conditions and job loss interact to shape the careers of workers in declining manufacturing occupations. The second chapter examines how workers are affected if they lose their job after the task requirements of their occupation have changed. The third chapter evaluates whether subsidized jobs can improve the social integration and well-being of unemployed workers with little access to regular jobs.

As a common feature, these chapters study what determines the costs of job loss for individual workers. The first two chapters focus on the economic costs of job loss during structural transformation. The third chapter takes a different angle by considering the psychosocial costs of unemployment and whether public policy measures can help to ease them.

In what follows, I will summarize the results of each chapter and provide a brief conclusion.

### Regional Structural Change and the Effects of Job Loss

The first chapter explores how regional differences in the exposure to structural change affect individual workers.

In this joint work with Melanie Arntz and Laura Pohlan, we use two decades of West German administrative data to document the significance of such regional variation: Even though routine-manual intensive (RM) manufacturing occupations have been generally declining since the 1980s, particular local labor markets were very differently affected. While RM jobs plummeted in urban industrial centres, they grew alongside with interactive and cognitive service jobs in upcoming rural regions.

Based on this finding, we compute the local ten-year growth rate of RM jobs as an indicator of local exposure to structural change. We take this indicator to individual social security records to study how local structural change affects the careers of RM workers. For that purpose, we identify workers who were displaced during plant closures and mass-layoffs, because job loss during such events is arguably unrelated to individual productivity. For each displaced worker we match a comparable non-displaced control worker in order to obtain a credible counterfactual for how careers would have evolved in absence of job loss. We then apply a novel matched Difference-in-Differences approach to estimate and compare the causal effect of job loss for workers in different regions.

Our findings show that even in the most affected regions, structural change does not necessarily impair the careers of RM workers – unless they are hit by an unexpected layoff. In this case, RM workers suffer substantially larger employment losses in regions where RM jobs have been on a steeper decline over the past decade. In these regions, more workers switch occupations, which is associated to large and persistent wage losses. Such moves do not only involve task-specific human capital losses, but also larger reductions in firm wage premia. Therefore, displaced RM workers in strongly exposed regions are more likely to relocate or start commuting. If they do, their wage losses are low and comparable to workers in more prosperous regions. However, for many workers regional mobility costs seem to be restrictive. As a consequence, RM workers are substantially more likely to remain unemployed even after six years if they happen to be displaced in a region with a declining demand for their type of occupations.

These results suggest that even strong changes in regional occupation structures do not necessarily harm workers. As long as individuals are in a stable employment relationship they are shielded from the impact of local transformation. However, if workers are hit by an unexpected shock that terminates their current job, mobility costs and regional lock-in prevent many from taking advantage of job offers elsewhere. Therefore, the interaction of long-term structural changes and individual level shocks contributes to regional skills mismatch and labor market inequality.

### Changes in Occupational Tasks and the Effects of Job Loss

In the second chapter of my dissertation I study how changes in the task requirements of occupations affect the careers of incumbent workers.

Technological innovations constantly reshape what people do at work. Since these changes are often gradual, workers may adjust their skills on the job. However, if they are laid-off during this process, they may not yet have fully adjusted their skills to the current requirements of their occupation. Especially older workers with higher learning costs and lower returns may be slower to adapt and struggle to compete for jobs in their original occupation.

In this chapter I use a novel task dataset that allows me to consistently trace the task composition of occupations in Western Germany since the 1970s. I merge this data to social security records, in which I follow individual careers over time. In the administrative data, I identify workers who lose their jobs during plant closures. In such occasions, all workers of a plant are laid off at once, regardless of how up-to-date their skills are. Therefore, such separations are arguably unrelated to how well workers have adjusted to changes in tasks. I then compare the outcomes of workers who have been exposed to different degrees of changes in tasks since they entered their occupation.

However, workers with a greater exposure also differ in characteristics other than task change itself. For example, more tenured workers are systematically more likely to have experienced task restructuring. Therefore, they may have experienced different earnings losses after displacement even if their occupation had not changed at all. To take account of this bias, I use non-displaced workers with a similar exposure to task change as an additional control group in a Triple-Differences design.

I find that workers in the top quartile of task change experience about 90% higher earnings reductions after job loss than workers in the bottom quartile with almost zero task change. About half of these additional earnings losses are explained by a persistently lower re-employment probability of more exposed workers. However, also the earnings of workers who return to employment are lower if their occupation changed in the past. These losses are mainly driven by occupation switchers, but even if workers enter the same occupation again, they suffer larger earnings losses after a period of task change. Especially older workers are less likely to return to employment if their occupation changed strongly prior to displacement.

These results suggest that individual skills depreciate during periods of occupational task change. Indeed, many workers do not seem to fully update their skills to the current requirements of their occupations. Since switching occupations involves high average earnings losses, less adaptable individuals remain unemployed longer or exit the labor force. Therefore, changes in occupational tasks are an important source of post-displacement earnings losses.

### Do Job Creation Schemes Improve Social Integration and Well-being?

The third chapter of my dissertation turns the focus on long-term unemployed individuals who have already fallen behind the requirements of the regular labor market.

A large body of literature shows that sustained unemployment threatens mental well-being and poses a serious risk factor for social exclusion (see e.g., Paul and Moser, 2009; Frey and Stutzer, 2002). Beyond generating income, work satisfies psychosocial needs like maintaining a regular day structure or engaging in purposeful activity with others (Jahoda, 1981).

In this joint work with Friedhelm Pfeiffer and Laura Pohlan, we assess whether subsidized employment can substitute for these functions and improve the social integration and well-being of long-term unemployed individuals. For that purpose, we use the introduction of SILM (*'Social Integration within the Labor Market'*) – a recent German job creation scheme (JCS) that provided subsidized employment for long-term unemployed individuals with severe employment impediments.

The economic evaluation literature mainly concludes that JCSs reduce search efforts during participation without improving the job prospects afterwards. However, such lock-in effects may be less of a concern for hard-to-place individuals, who would be less likely to find a job in absence of participation. At the same time, these individuals are particularly exposed to the strains of unemployment and may therefore benefit more in terms of well-being. And yet, only few studies have explored the effect of subsidized employment on such 'soft' outcomes. One reason is that even large-scale surveys that include measures of well-being do not cover enough program participants to conduct quantitative analyses.

We therefore construct a novel dataset that links the administrative records of participants with a panel telephone survey. This survey provides us with information about the individual program experience and self-assessed well-being and social integration. The administrative data allows us to match each program participant with an observationally similar non-participant that is also included in the survey. Even though we cannot condition on pre-treatment outcomes, we control for variables constructed from the entire history of employment and past program participation to account for self-selection. Moreover, we provide numerous robustness checks that support our main conclusion:

Participation in the program significantly increases self-assessed life satisfaction and mental health, as well as social belonging and social status. Six months after entering the program, participants reach similar average levels of life satisfaction and social belonging like individuals in regular employment. Even though the average program effect declines over time, we show that this is explained by compositional changes in the treatment and control group. Over time, an increasing share of control individuals enter regular employment and therefore attain similar levels of well-being as participants. Among the participants, more individuals drop-out of the program and return to the well-being levels of unemployed individuals. Taking out these composition changes reveals that the effect of active participation remains stable over time.

We conclude that a well-designed JCS can substantially improve the well-being and social integration of long-term unemployed individuals. However, lock-in effects are important: Even in this potentially disadvantaged group, a sizeable share of individuals may find regular employment in the medium run and thus achieve similar levels of well-being. Therefore, such programs should strictly target workers with currently low employment prospects and frequently assess job chances on the regular labor market.

## Conclusion

Taken together, these results highlight two aspects of individual adjustment to structural change: Even profound shifts in the task structure of occupations or local labor markets do not necessarily threaten the careers of incumbent workers. The risks associated to structural change only take effect when workers are hit by an individual-level shock that interrupts an otherwise stable job match.

A stable job match serves as an insurance against long-term structural change, especially in a context of strong employment protection laws like in Germany or many other European countries. However, it is hard to insure against idiosyncratic risks that may terminate a job match, like plant closures or health shocks. Both long-term structural change and job loss are hard to predict when workers make long-lasting career decisions. Hence, there could be scope for welfare improving policy interventions. For example, subsidizing job training for older workers may improve their resilience against career shocks. On the other hand, not all risk factors can be safeguarded. Some individuals are subject to personal restrictions like health impairments or private obligations, that provide substantial barriers to regular employment. In these cases, publicly subsidized jobs can substitute for integral psychosocial functions of work, but they should strictly target individuals without access to the regular labor market.

Looking to the future, digitization will alter job descriptions in ways that were hard to imagine only a few years ago. At the same time, demographic change will increase the adjustment costs of an ageing population and aggravate already existing skill shortages. Supporting 'lifelong learning' and exploring alternative work arrangements for individuals who cannot keep pace will likely become even more policy relevant topics.

# 1. Regional Structural Change and the Effects of Job Loss

*With Melanie Arntz and Laura Pohlan*[1]

## 1.1. Introduction

In many advanced economies, automation and the relocation of production to low-cost countries have substituted for workers in routine-intensive tasks, while spurring demand for labor in other complementary tasks (Autor et al., 2003; Autor et al., 2013a; Goos et al., 2014). Within a given country, however, these structural changes are far from uniform across regions (Autor, 2019; Davis et al., 2020). Under imperfect mobility, such regional differences may lead to spatial skills mismatch and increase the risk of sustained unemployment. This should be particularly relevant when individuals are hit by an unexpected job loss that terminates a previously stable employment relationship. Yet, little is known about how individual-level shocks and long-run structural change interplay to shape workers' career paths.

In this paper, we use two decades of administrative data for West German regions and individuals to add novel evidence on this matter. We focus on workers displaced during mass layoffs and plant closures, because such separations are plausibly unrelated to individual employment and earnings prospects. We also document that these events are not systematically more common in regions with a stronger long-term decline in routine occupations. From the workers' point of view, job displacement can therefore be considered as an unexpected individual shock that exposes them to different degrees of local structural change. Comparing displaced workers' outcomes between regions while controlling for differences in worker composition allows us to analyze how local structural change and job loss interact to shape individual employment and earnings trajectories. We also study whether occupational and regional mobility serve as individual adjustment devices and identify worker groups that are most vulnerable to structural change.

In the first part of our analyses, we show that between 1990 and 2010, employment losses in West Germany were strongly concentrated in initially routine manual (RM) intensive occupations. The extent of these losses, however, varied greatly between regions and was most concentrated in urban centers with high initial employment shares in large manufacturing firms. Job growth in non-routine occupations and the service sector, in turn, was driven by more rural and initially less productive regions.

[1] Earlier versions of this chapter have been circulated as discussion papers (Arntz et al. 2021a, 2021b and 2021c).

In the second part of the paper, we take this regional variation to an administrative dataset of displaced workers. In order to identify the causal effects of job loss, we match each displaced worker with an observationally similar non-displaced worker from the same pre-displacement task specialization and from a region with a similar long-term structural change pattern. We then apply both an event study and a matched difference-in-differences (DiD) approach in the spirit of Schmieder et al. (2020). The first method focuses on how the costs of job loss within a specific occupation and region type change over time and provides results that are easily comparable to the job displacement literature. The matched DiD approach allows us to study effect heterogeneity along the entire distribution of regional structural change.

We obtain three key findings: First, our results show that even in the most exposed regions, workers specialized in RM tasks (henceforth: RM workers) are shielded from the potentially adverse effects of structural change unless they are hit by job loss. Upon displacement, however, RM workers' outcomes strongly depend on local structural change: One year after job loss, RM workers who got displaced in regions with the strongest decline in RM jobs have a 10pp lower re-employment probability and 14pp higher wage losses than comparable workers in regions where RM occupations grow the most. This regional gap remains significant even after six years. Workers with a task focus other than RM also suffer significant employment and wage losses upon displacement, but these losses are generally lower and not systematically related to RM-biased structural change.

Second, the wage losses of RM workers are closely linked to switching occupations. RM workers who take up an occupation with a different main task suffer almost 50% higher initial wage losses than those who return to RM jobs. Again, these losses are strongly concentrated in regions with strongly declining RM employment. Our results suggest that this regional gap is driven by losses in establishment premia rather than losses in task-specific human capital.

Third, regional mobility allows workers to re-enter an RM occupation by leaving strongly exposed regions. However, especially older and less skilled workers are locked in regions with poor RM job prospects and are thus more prone to long-term unemployment. For these workers, the regional context strongly determines the costs of job loss. This suggests that the costs of regional and occupational mobility are restrictive for many workers, resulting in local skills mismatch and diverging career paths.

Our paper contributes to several strands of the economic literature. It relates to the literature on the impact of local labor demand shocks on labor market outcomes. Such shocks have been found to have long-run effects on local employment rates due to sluggish out-migration responses (see e.g. Bound and Holzer, 2000; Amior and Manning, 2018; Bartik, 2021), resulting also in higher inactivity levels (e.g. Bound and Holzer, 2000; Autor et al., 2013a; Yagan, 2019). We provide a complementary angle by studying how long-term shifts in the local employment structure affect workers who are hit by an individual-level displacement shock. While the existing literature suggests that aggregate shocks can have persistent negative labor market effects, our findings indicate that the persistence of individual shocks depends on local structural change. Moreover, our results show that job loss – and subsequent economic inactivity – is an important adjustment margin.

This paper also relates to numerous studies documenting that job displacement causes substantial and persistent individual earnings and employment losses (see e.g. Ruhm, 1991a; Ruhm, 1991b; Jacobson et al., 1993 for the U.S. and Eliason and Storrie, 2006; Huttunen et al., 2011; Schmieder et al., 2010; Schmieder et al., 2020 for Europe). Common explanations put forward are the loss of industry or occupation-specific human capital (e.g. Neal, 1995; Kletzer, 1996), and regional or occupational mobility (e.g. Poletaev and Robinson, 2008; Gathmann and Schoenberg, 2010; Fackler and Rippe, 2017; Huttunen et al., 2018; Gathmann et al., 2020).[2] We show that local exposure to task-biased structural change is an important driver of the effects of job displacement. This extends the findings of earlier studies that acknowledge the role of regional labor markets: Jacobson et al. (1993) find that in the 1980s, displacement effects in the U.S. vary with the local unemployment rate at the time of job loss. Haller and Heuermann (2020) show that local labor market thickness affects post-displacement outcomes in Germany. Gulyas and Pytka (2019) document that losses in firm wage premia and the (non-)availability of well-paying jobs in the local labor market are the two most important factors for post-displacement earnings losses. These studies focus on the role of business-cycle fluctuations for the costs of job loss, while we add new insights on the impact of long-term shifts in the structure of local labor demand.[3] In particular, we are able to use regional variation in the exposure to task-biased structural change as quasi-experiment as we show that these shifts are not systematically related to the local incidence of displacement events.

Recently, Blien et al. (2021) and Goos et al. (2020) have studied the relationship between post-displacement outcomes and the routine intensity of the pre-displacement occupation. They consider the higher wage losses among routine workers to reflect the impact of routine-replacing technological change, but they do not establish any direct link between structural change and displacement effects.

Our analysis thus also speaks to recent evidence on the regional heterogeneity of routine-biased structural change. Autor (2019) shows that in the U.S. both the substitution of mid-wage routine jobs and the growth of technical and service jobs was most pronounced in urban centers. Davis et al. (2020) provide similar evidence for France. Our results confirm that routine-biased structural change in West Germany was also far from uniform across regions, but we also describe some interesting differences: job losses in RM manufacturing occupations were mainly concentrated in urban industrial centers, while non-routine and cognitive service jobs were created in more rural regions. This is in line with other studies about the geography of sectoral composition shifts in West Germany (Findeisen and Suedekum, 2008; Dauth and Suedekum, 2016; Margarian and Hundt, 2019).

By exploiting differences in regional structural change, we also contribute to the debate to what extent structural change poses a threat for incumbent workers. Recent studies show that

[2] Carrington and Fallick (2017) provide a review of the literature about the theory and evidence of different sources of post-displacement earnings losses.

[3] A few earlier papers analyzed how the costs of displacement are related to the recent regional industry or occupation structure (Neal, 1995; Neffke et al., 2018; Macaluso, 2019).

workers in routine occupations experience lower wage growth (Cortes, 2016), job stability (Edin et al., 2019; Bachmann et al., 2019) and job finding probabilities after job loss (Schmidpeter and Winter-Ebmer, 2021). Moreover, evidence from the U.S. suggests that the disappearance of routine intensive jobs mainly occurs during economic downturns (Jaimovich and Siu, 2020) and is driven by lower return rates from unemployment or non-participation into these occupations (Cortes et al., 2020). This suggests that job displacement might be particularly disruptive if it exposes routine workers to a labor market with a decreasing demand for their specific skill set. In line with this, routine workers are generally more likely to experience sustained unemployment and larger earnings losses after displacement (Blien et al., 2021; Goos et al., 2020; Dauth et al., 2021). Complementing this evidence, we find that the detrimental effects of structural change are confined to individuals who are displaced from their current jobs and that the associated costs are strongest in regions hit hardest by structural change. In concordance with previous studies for the U.S. (Cortes, 2016; Cortes et al., 2017), we find that low-skilled and older workers are affected most by task-biased structural change. This suggests that despite Germany's much stronger employment protection institutions, individual-level shocks still provide an important risk factor during structural change.

The rest of the paper is structured as follows. Section 1.2 describes the particular RM task-bias of structural change in West Germany between 1990 and 2010 and how it varies across local labor markets. Section 1.3 introduces our sample of displaced workers and their matched controls for the subsequent event study and matched DiD estimations. Section 1.4 presents results on how the displacement effects on employment and wages differ with local structural change, while Section 1.5 looks at patterns of regional and occupational mobility. Section 1.6 discusses our results and concludes.

## 1.2. Structural Change in West Germany

### 1.2.1. Data

For the analysis of regional structural change, we draw on data from Dauth (2014), which measures employment by local labor market regions and occupations on June 30 in 1990, 2000 and 2010 as recorded in the Employment History File (BeH). The BeH is an administrative dataset of the German Federal Employment Agency that covers information on all German employees subject to social security contributions and thus represents about 80% of the German labor force (Dustmann et al., 2009). After excluding employees in agriculture, mining and the public sector, each original cross section encompasses around 16 million regular employees in West Germany.[4] The data is aggregated to full-time equivalent employment in 315 KldB-1988 3-digit occupations at the level of 203 local labor market regions that correspond to major commuting zones. We further aggregate occupations to 52 occupational fields that are most similar in terms of their

[4]The data also excludes self-employed persons, civil servants and military personnel as well as interns and employees in vocational training or partial retirement. East Germany is excluded due to its unique structural change after the fall of the Iron Curtain.

task structure.[5] Moreover, we use five waves of the German Qualifications and Career Surveys (GQCS) between 1986 and 2012 to characterize the time-varying task content of occupations.[6] For that purpose, we follow the literature and distinguish between routine manual, non-routine manual, routine cognitive, non-routine interactive and non-routine analytical tasks (e.g. Autor et al., 2003, Spitz-Oener, 2006). For most of our analyses, we will distinguish occupations by their broad main-tasks according to the task structure in the 1986 wave, i.e. prior to the structural shifts that our analysis focuses on and prior to major shifts related to computerization and globalization. Merging this information to the region-occupation-level employment data allows us to describe the task-bias of shifts in the overall West German occupation structure and how these shifts vary across regions.[7]

### 1.2.2. Routine Manual Bias of Structural Change

Figure 1.1 plots the employment growth rate of occupations between 1990 and 2010 aggregated over West German labor market regions and weighted by the initial employment shares in 1990. The colors of the bars mark the occupations' main tasks as given by the GQCS 1986.

About half of all declining occupations were initially dominated by RM tasks. This is especially true for occupations with the strongest employment contraction (see list of occupations in Table A.2.1 in Appendix B.1 for further details). Most of the declining occupations were low- and mid-wage manufacturing or construction occupations, representing about 65% of total employment in 1990. In contrast, almost all growing occupations were mid- or high-wage technical (e.g. engineers, IT specialists, natural scientists) or service occupations (e.g. health care, office occupations, management). In 1986, most of the growing occupations were specialized in analytical and interactive tasks and only some in non-routine manual tasks.

The shift away from RM tasks did not only take place between, but also within occupations. Figure A.1 in Appendix B.1 plots how the average task composition (weighted by 1990 employment shares) of growing and declining occupations changed over time. Growing occupations reduced their intensity in RM and routine cognitive tasks and intensified their initial focus on non-routine analytical and interactive tasks. Declining occupations evolved from a strong specialization in RM tasks to a more diverse task composition with an increasing focus on analytical and interactive tasks.

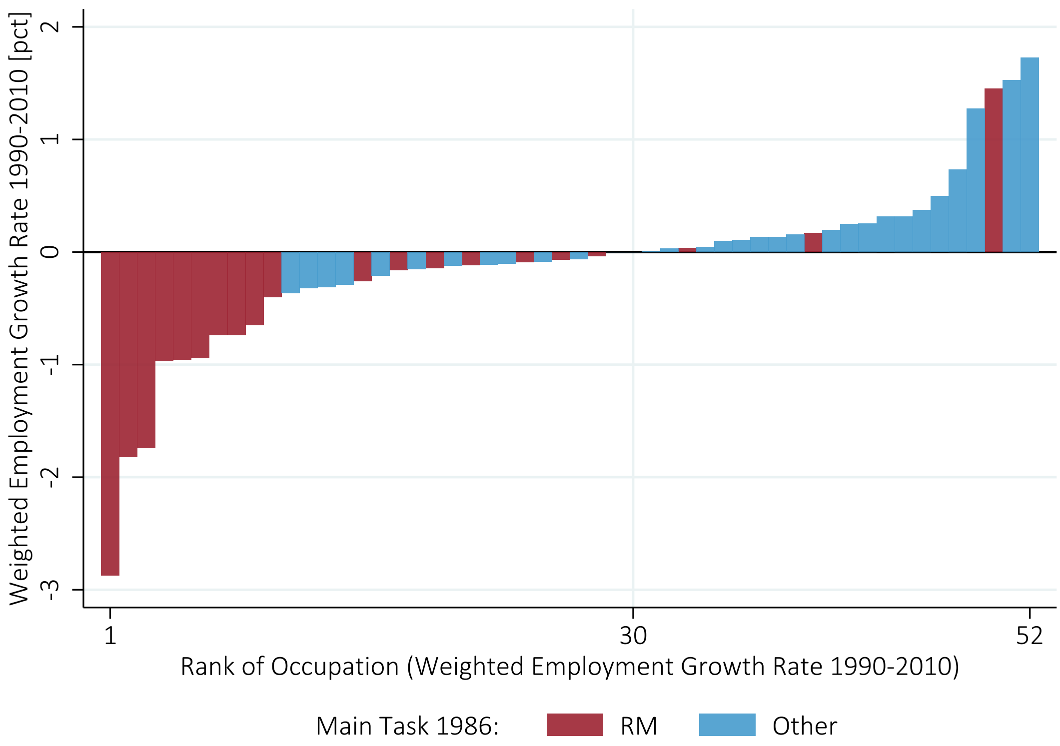
We conclude that structural change in West Germany was mainly biased against RM tasks rather than routine tasks per se. The demand for RM tasks declined both within and between occupations resulting in potentially worse career prospects for workers specialized in these tasks. By contrast,

[5]See BBSR (2021) for the mapping of counties to labor market regions and Tiemann et al. (2008) for the mapping of KldB occupations to occupational fields.

[6]BIBB/IAB and BIBB/BAuA Erwerbstätigenbefragung (Qualification and Career Survey, GQCS), waves from 1979 to 2012, DOI: http://dx.doi.org/doi:10.4232/1.1243, http://dx.doi.org/doi:10.42, http://dx.doi.org/doi:10.4232/1.2565, http://dx.doi.org/doi:10.4232/1.12247, http://dx.doi.org/doi:10, http://dx.doi.org/doi:10.7803/501.12.1.1.40.

[7]For a more detailed description of how we prepare and combine the BeH and GQCS in order to construct indicators of local structural change, see Appendix A.1.1.

**Figure 1.1.:** Aggregate Occupational Change in West Germany 1990-2010




*Notes:* RM = Occupations with mainly routine manual tasks, Other = Occupations with other main tasks (GQCS 1986). Growth rates are weighted by the occupations' initial employment share in 1990 (see the formula for $grRM_r^{LR}$ in Appendix A.1.2 where $r$ is set to the West German aggregate). These weighted growth rates can be interpreted as each occupation's contribution to overall employment growth. The vertical line at rank 30 marks the occupation with just slightly above zero growth.
*Data:* BeH, GQCS.

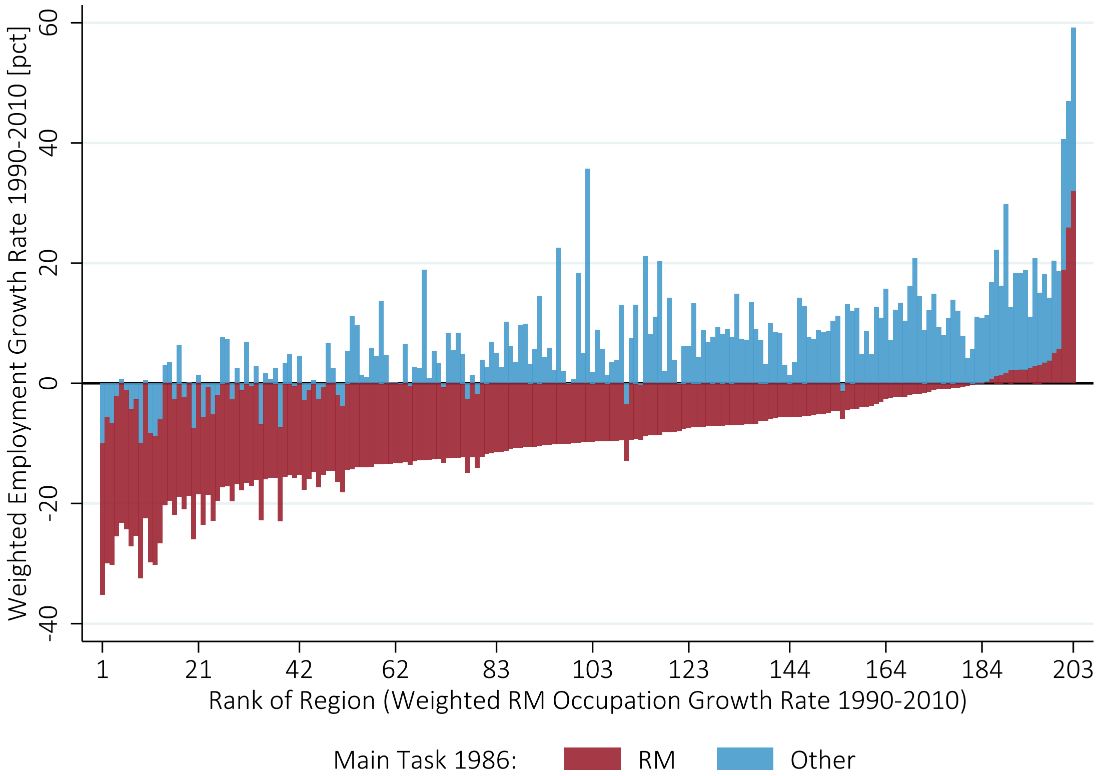
workers specialized in other main tasks have either seen stable or an increasing demand for their task-specific skills. We will therefore focus on workers from initially RM-intensive occupations and compare them to workers from occupations with other main tasks.

### 1.2.3. Regional Heterogeneity in RM-Biased Structural Change

Figure 1.2 demonstrates that task-biased structural change was far from uniform across West German regions. For each of the 203 West German local labor market regions, the figure shows the local growth rate of RM occupations (dark red bars) and all other types of occupations (light blue bars) between 1990 and 2010, ranked by the red bars. We take the red bars as a measure of the intensity of long-run **RM**-**B**iased **S**tructural **C**hange across regions and we will refer to the corresponding distribution as the RMBSC distribution.

Regions at the lower end of the RMBSC distribution experienced a strong decline in RM occupations, but only limited growth in other occupations. Overall job creation, which corresponds

**Figure 1.2.:** Occupational Change across West German Regions 1990-2010




*Notes:* RM = Occupations with mainly routine manual tasks, Other = Occupations with other main tasks (GQCS 1986). The red and blue bars represent the weighted employment growth rates of RM and other occupations between 1990 and 2010 in local labor market regions. Growth rates are weighted by the occupations' initial employment share in 1990 (see the formula for $grRM_r^{LR}$ in Appendix A.1.2).
*Data:* BeH, GQCS.

to the sum of both bars, was mostly negative or low.[8] Moving up the distribution, job decline in RM occupations becomes less severe and tends to be compensated by job growth in other occupations. At the very top, RM occupations even grew along with the other occupations. Hence, structural shifts and overall job growth are closely related (correlation $\rho = 0.93$), a finding that is in line with other studies of structural change and regional development (e.g. Glaeser, 2005, Duranton, 2007, Findeisen and Suedekum, 2008, Dauth and Suedekum, 2016).

To illustrate how regions differ along the RMBSC distribution, the top row of Figure 1.3 shows the initial (1990) industry and establishment size structure for the deciles of the distribution. The bottom row shows the corresponding growth rates between 1990 and 2010 (weighted by the 1990 shares). Regions with the strongest decline in RM jobs, i.e. the lower deciles of the RMBSC distribution, started out with a larger metal/machinery/automotive sector and a much

[8]The bars sum to total employment growth, because growth rates are weighted by the occupations' initial employment shares in 1990. At the West German aggregate, social security employment in full-time equivalents decreased by 2% between 1990 and 2010 (based on our BeH data). In headcounts, social security employment grew by about 4.7% over this period (estimate based on data of the Statistical Office of the Federal Employment Agency).

higher share of employment in large establishments with more than 250 employees. Over time, however, these regions also experienced strong employment losses in large companies and in manufacturing. For regions ranked higher in the RMBSC distribution, both the initial share and the subsequent employment decrease in the manufacturing sector and in large establishments were lower, while employment in services and retail grew more strongly. Note, however, that the initial share of RM occupations was quite similar along the RMBSC distribution (see Figure A.2(c) in Appendix A.2.2). We also find that RM job losses were more pronounced in urban areas with a higher initial labor productivity (see Figure A.2(a) and (b)). In contrast, regions at the top of the distribution were more rural and less productive in 1990, but also experienced stronger productivity and population growth in the two subsequent decades.

These stylized facts suggest that, in Western Germany, many of the RM jobs were lost in former industrial centres, where large manufacturing establishments dominated the local economy. New jobs were created in rising, innovative and more rural areas with a higher share of small and medium-sized establishments.[9] This pattern is in line with Findeisen and Suedekum (2008) who show that growing regions in West Germany rapidly transformed towards a modern industry structure, while turnover in declining regions was often driven by the disappearance of old industries. Consistent with this, a region's initial industry structure and corresponding exposure to import competition has been identified to affect regional transformation (Dauth and Suedekum, 2016). Technological change may have been another contributor to this development. Firms may have had a stronger incentive to substitute labor with automation machinery if import-exposure raised cost pressures. New tasks and jobs, on the other hand, may have been created in regions where investments were guided towards developing new products and services, rather than realizing cost savings.[10]

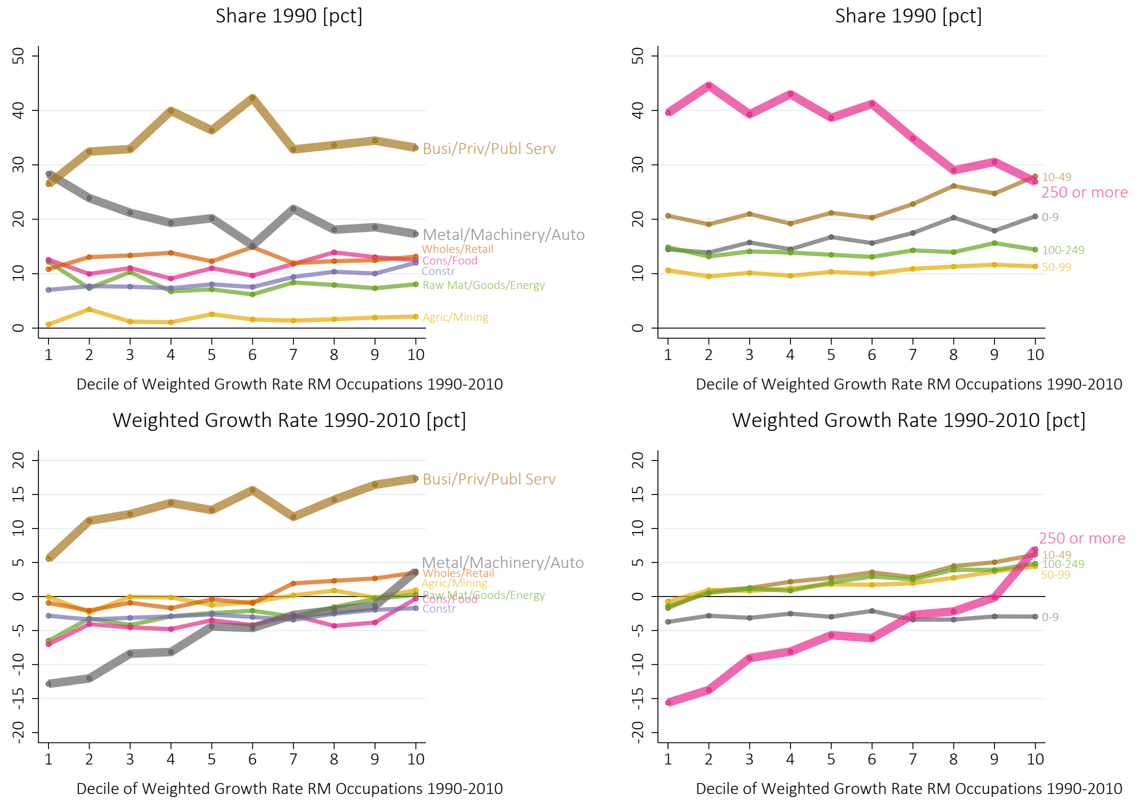
Overall, we thus find strong differences in RMBSC between West German regions. Although RMBSC is closely related to overall job growth, the RM task bias underlying these differential growth patterns implies that workers specialized in RM tasks should be affected differently than other workers. In the subsequent analysis, we will therefore focus on how the exposure to structural change affects RM and other workers by estimating post-displacement effects along the RMBSC distribution.

[9] A map of West German labor market regions distinguished by deciles of the RM and other occupation growth rate can be found in Figure **??** in the Appendix.

[10] Acemoglu and Restrepo (2018) discuss that technologies may have a replacing or reinstating effect, i.e. they may cause job and task destruction or creation. Autor et al. (2021) pick up this idea and show that job creation is strong in occupations with new augmentation technologies, while job growth is weak in occupations with innovations in automation technologies. Empirical evidence to what extent there may be regional differences in automation and augmentation innovations is missing yet, but could be an additional driver of regional structural change.

**Figure 1.3.:** Initial Industry and Establishment Size Structure and Growth over Time

**(a)** Industries: **(b)** Establishment Size:




*Notes:* Agric/mining = Agriculture, mining; Raw mat/goods/energy = Raw material, goods, energy; Metal/machinery/auto = Metal, machinery, automotive; Cons/food = Consumption goods, food; Constr = Construction; Wholes/retail = Wholesale, retail; Busi/priv/publ serv = Business, private, public services. Residual category "Other industries" omitted from the graph for ease of display. The x-axis refers to the deciles of the weighted regional growth in RM occupations between 1990 and 2010 (i.e. the ‘red bars’ in Figure 1.2, see also the formula for $grRM_r^{LR}$ in Appendix A.1.2).

*Data:* BeH, GQCS.

## 1.3. Displacement Sample and Empirical Strategy

Our analysis aims to identify the causal effect of job loss along the regional RMBSC distribution for different types of workers. This requires several conditions:

First, displaced workers should not be selected on characteristics that would influence their employment and earnings prospects also in absence of job loss, like e.g. individual productivity. For that purpose, we consider only workers who were laid off during mass-layoffs or plant closures and who had stable employment relationships preceding these events. During such events a large fraction or the entire workforce of a plant is laid off such that those affected are unlikely selected on unobservables. Conditioning on stable employment relationships ensures that workers were attached to their original plant and would probably not have left soon anyway. Second, we need to find non-displaced control workers to approximate the counterfactual situation of keeping one's job. In particular, displaced workers and otherwise similar control individuals should have the same pre-displacement occupation type and should be exposed to similar levels of RMBSC. Third, the displacement should not only be exogenous to the individual, but also exogenous to regional structural change. Otherwise, post-displacement outcomes may not be comparable between regions. For this requirement to hold, the probability of displacement should be independent of regional structural change. In addition, the composition of displaced workers should not differ systematically along the regional RMBSC distribution. The subsequent sections discuss how our empirical strategy takes account of these conditions.

### 1.3.1. Identification of Displacement Events

In order to construct a sample of displaced workers, we first need to identify establishments in which a displacement event occurs. For that purpose, we use data from the IAB Establishment History Panel (BHP) for the period of 1990 to 2010.[11] The BHP contains administrative employment data for the universe of all German establishments on June 30 of each year. To ensure that our results are comparable to other studies, we closely follow the definition of displacement events suggested by Hethey-Maier and Schmieder (2013). We only consider establishments with more than 10 employees in order to exclude small firms that may largely rely on the productivity of individual workers. In such cases, being laid off during a displacement event cannot be considered unrelated to individual productivity.

According to our definition, a displacement event occurs if either a plant closes permanently or a mass layoff takes place. A plant closure occurs when an establishment identifier disappears from the BHP between two consecutive years. For the definition of a mass layoff, we require that establishments had at least 100 employees in the year prior to the event. A mass layoff occurs when plant-level employment decreases by at least 30%, or at least 500 employees, between June 30 of two consecutive years (see e.g. Gathmann et al., 2020 for a comparable definition). We restrict the sample to event establishments with a stable pre-event workforce by excluding

[11] Dataset version BHP 7514 v1. For further information on the data and on data access see the website of the Research Data Center of the Institute for Employment Research: http://fdz.iab.de/.

establishments with employment fluctuations of more than 10% over the previous three years. We also exclude event establishments that fully recover within the following three years. Cases where a substantial share (>30%) of the work force moves to the same new establishment ID are also excluded to rule out misidentifying other events like ownership changes or outsourcing (Hethey-Maier and Schmieder, 2013).

### 1.3.2. Matching Displaced Workers and Control Individuals

**Sample of Displaced Workers and Potential Controls.** We identify workers who lost their jobs during a displacement event in the Integrated Employment Biographies (IEB).[12] This dataset contains spells of dependent employment, registered unemployment, job-search and benefit receipt for all dependent employees that contributed to the social security system at least once since 1975.[13] Since employment records also include the establishment ID of the employer, we can merge employer characteristics from the BHP such as the industry code, the size of the workforce, median wages as well as individual and establishment wage premia ('AKM' fixed effects).[14] Moreover, we can identify all workers who were employed in an establishment on June 30 of the year preceding the event and who leave the establishment in the subsequent year. We denote the year prior to the event the 'base year' $c$. By this definition, the displacement event takes place between June 30 of the base year $c$ and June 30 of the following year $c+1$. This results in a total sample of 87,934 displaced workers, with about 3,000 to 4,000 individuals per base year and up to 7,000 displaced individuals in some years.

Our sample is restricted to individuals who work full-time in the base year at a West German establishment, who are between 24 and 50 years old[15], have at least three years of establishment tenure and one year of county tenure in order to make sure that workers are leaving a stable job that most likely would have persisted in absence of displacement.[16]

The sample of non-displaced potential control individuals is a 15% random sample of individuals working in West German establishments with at least 10 employees and for whom the same age and employment restrictions apply as for the displaced workers on June 30 of a given base year $c$. Not-yet-displaced workers remain potential controls until they actually experience their first displacement event. For the subsequent analysis, we construct a yearly individual-level panel

[12] IAB Integrierte Erwerbsbiografien (IEB) V13.00.00, Nuremberg 2017. For a description of the IEB see Oberschachtsiek et al. (2009).

[13] It does not contain spells of self-employment, military or civil service or pension receipt.

[14] BHP and IEB do not contain a firm identifier that would allow linking affiliated establishments (see also Hethey-Maier and Schmieder, 2013). The individual and establishment wage premia are based on the method pioneered by Abowd et al. (1999) and provided by the IAB. We use AKM effects that were estimated on pre-displacement years, so they are not contaminated by the displacement events themselves. For a detailed description about the estimation of the AKM effects see Bellmann et al. (2020).

[15] Workers below 24 years of age may not have fully entered the labor force and workers older than 50 years might be generally less attached to the labor force, e.g. because of access to partial retirement programs.

[16] Specifically, we exclude interns, trainees, part-time workers and workers who are in part-time retirement schemes. We also exclude individuals who are employed in the sectors of mining, public administration, defense, activities of private households and extra-territorial organizations as well as those who have agricultural, mining or unspecified occupations.

dataset, which is centered around the base year $c$ and contains information on employment states and job characteristics observed on June 30 of the four preceding and six subsequent years.

**Matching Procedure.** We identify a control person for each displaced worker by adapting the two-stage matching procedure of Schmieder et al. (2020) to our setting. In a first step, we exactly match displaced workers and potential controls on the base year $c$, the worker's occupation type (1986 main-task: RM vs. other main task) and region type (R1/R2/R3: Strong/medium/weak local RM bias). The region types R1, R2 and R3 indicate the terciles of the weighted local RM occupation growth rate between 1990 an 2010 (see Figure 1.2 and Appendix A.1.2 for details). R1 refers to regions in the lowest tercile, i.e. with the strongest RM employment decline, R2 and R3 refer to the middle and upper tercile, respectively.[17] Exactly matching on these region types ensures that displaced and control workers start out in regions with a broadly comparable long-run structural change pattern. In the second step, we use nearest neighbor propensity score matching to select the most comparable control person from the set of potential control persons defined in step one.[18] We use a comprehensive set of pre-displacement worker, establishment and region characteristics as predictors of the propensity score. This set also contains the regional weighted growth rate of RM occupations over the last ten years preceding base year $c$ (see definition of $grRM_r^{c-10}$ in Appendix A.1.2) to ensure, that within region types R1 to R3, displaced and control workers originate from regions with similar medium-run structural change.

Table 1.1 compares the averages of these variables for displaced RM workers, a set of randomly chosen control individuals and the control individuals selected by our matching procedure. Columns (4) and (5) report the standardized differences $\Delta_X$ between displaced workers and either set of control workers as a scale-free measure of balancing.[19] Since there is no universally agreed criterion for how small the standardized difference must be to provide balance, we lean on two rules of thumb provided in the literature[20] and a similar notation as typically used for significance levels: We mark absolute values above 0.25 by $^{++}$, absolute values between 0.1 and 0.25 by $^{+}$ and absolute values below 0.1 are left blank to indicate close-to-perfect balancing for the respective variable.

Already the random controls are very similar to the displaced worker sample, as most standardized differences are insubstantial and only two exceed the threshold of 0.25. Most notably, displaced workers earn lower pre-treatment wages, are less common in large establishments and in the metal, machinery and automotive industry and have lower AKM establishment fixed effects.

[17] The average long-run growth rate of RM employment is −17.0% in R1, −9.7% in R2 and −0.7% in R3.

[18] We use matching with replacement such that the same non-displaced worker can be a control individual for several displaced workers, but this only concerns about 2.5% of the matches. 2% of the displaced workers serve as control persons before they experience their first displacement event.

[19] The standardized difference is defined as $\Delta_X = \left(\bar{X}_1 - \bar{X}_0\right) / \left((S_1^2 + S_0^2)/2\right)^{0.5}$, where $\bar{X}_w$ is the sample mean of displaced ($w = 1$) or control ($w = 0$) individuals and $S_w^2$ are the respective sample variances (Austin, 2011). The advantage of $\Delta_X$ over the usual $t$-statistic is that it does not mechanically increase with the sample size and therefore avoids exaggerating small imbalances that would still appear significant in a $t$-test.

[20] The criterion for balance of $\Delta_X < |0.25|$ is suggested by Imbens and Wooldridge (2009), the stricter criterion of $\Delta_X < |0.1|$ is suggested by Austin (2011).

**Table 1.1.:** Base Year Characteristics of Displaced Workers and Control Individuals

| | (1) | (2) | (3) | (4) | | (5) | |
|---|---|---|---|---|---|---|---|
| | | Controls | | Std. Diff. (Disp. - Contr.) | | | |
| | Displaced | Random | Matched | Random | | Matched | |
| **PS matching variables:** | | | | | | | |
| Worker: | | | | | | | |
| Log real wage in $c-1$ | 4.67 | 4.74 | 4.66 | -0.15 | + | 0.01 | |
| Log real wage in $c-2$ | 4.65 | 4.71 | 4.64 | -0.14 | + | 0.01 | |
| Female | 0.28 | 0.27 | 0.28 | 0.03 | | 0.00 | |
| Age | 37.79 | 37.86 | 37.89 | -0.01 | | -0.01 | |
| Low-skilled | 0.12 | 0.11 | 0.12 | 0.02 | | 0.00 | |
| Medium-skilled | 0.77 | 0.76 | 0.77 | 0.01 | | 0.00 | |
| High-skilled | 0.11 | 0.12 | 0.11 | -0.03 | | 0.00 | |
| Experience | 15.77 | 15.95 | 15.75 | -0.03 | | 0.00 | |
| Establishment tenure | 9.98 | 10.42 | 9.91 | -0.07 | | 0.01 | |
| Occupation: | | | | | | | |
| Production, crafts | 0.37 | 0.39 | 0.37 | -0.03 | | 0.00 | |
| Senior office occupations | 0.13 | 0.17 | 0.13 | -0.12 | + | 0.00 | |
| Sales occupations | 0.07 | 0.04 | 0.07 | 0.12 | + | -0.01 | |
| Office occupations | 0.28 | 0.26 | 0.28 | 0.05 | | 0.01 | |
| Service occupations | 0.15 | 0.14 | 0.15 | 0.01 | | -0.01 | |
| Establishment: | | | | | | | |
| 10-49 employees | 0.26 | 0.20 | 0.25 | 0.13 | + | 0.01 | |
| 50-99 employees | 0.18 | 0.11 | 0.19 | 0.20 | + | -0.02 | |
| 100-249 employees | 0.26 | 0.16 | 0.27 | 0.24 | + | -0.01 | |
| > 249 employees | 0.30 | 0.53 | 0.29 | -0.47 | ++ | 0.02 | |
| Establishment age | 39.17 | 38.94 | 39.32 | 0.06 | | -0.04 | |
| Median wage | 89.85 | 91.62 | 90.55 | -0.05 | | -0.02 | |
| Industry: | | | | | | | |
| Raw Materials and Goods | 0.07 | 0.10 | 0.09 | -0.09 | | -0.07 | |
| Metal, Machinery, Automotive | 0.18 | 0.31 | 0.15 | -0.29 | ++ | 0.10 | + |
| Consumption Goods | 0.12 | 0.11 | 0.12 | 0.02 | | 0.00 | |
| Construction | 0.09 | 0.06 | 0.09 | 0.12 | + | -0.01 | |
| Wholesale, Retail | 0.19 | 0.12 | 0.20 | 0.17 | + | -0.02 | |
| Business Services, Transport | 0.23 | 0.18 | 0.24 | 0.13 | + | -0.01 | |
| Priv. Services, Educ., Social Sector | 0.11 | 0.11 | 0.11 | -0.01 | | 0.00 | |
| Region: | | | | | | | |
| Active population [1k] † | 420.61 | 425.04 | 422.26 | -0.01 | | 0.00 | |
| Population density [pop/km$^2$] † | 562.90 | 550.78 | 561.57 | 0.02 | | 0.00 | |
| UE rate [pct] ‡ | 0.08 | 0.08 | 0.08 | 0.04 | | -0.01 | |
| Weight. Growth Rate RM occ. $(c, c-10)$ [pct] | -4.55 | -4.59 | -4.55 | 0.01 | | 0.00 | |
| **Not in PS matching:** | | | | | | | |
| AKM worker FE [log points] ¶ | 4.37 | 4.39 | 4.37 | -0.06 | | 0.00 | |
| AKM establishment FE [log points] § | 0.20 | 0.23 | 0.19 | -0.20 | + | 0.04 | |
| Observations | 87,934 | 87,934 | 87,934 | | | | |

*Notes:* PS = Propensity Score; UE = Unemployment; Weight. Growth Rate RM occ. $(c, c-10)$ = Regional weighted growth rate of RM occupations over decade preceding base year $c$; FE = Fixed Effect; RM occ. = Occupations with mainly routine manual tasks; Std. Diff. = standardized difference. The table compares the average base year $c$ characteristics of displaced workers to a set of random and matched non-displaced control individuals. For the displaced, $c$ is the year prior to job loss; control individuals are required to fulfill the sampling restrictions and to be not (yet) displaced in year $c$. Displaced and control individuals are exactly matched on the base year $c$, region type (R1/R2/R3: Strong/medium/weak local RM bias), and the main-task of their occupation (RM/Other as defined by GQCS wave 1986). Establishment characteristics are measured in $c-1$. AKM FE in the most recent time period available before year $c$. For a description of AKM fixed effects see Section 1.3.4 and Bellmann et al. (2020).
$^{+}$ marks standardized differences between $|0.1|$ and $|0.25|$, $^{++}$ marks standardized differences $> |0.25|$.
Varying observation numbers because of missing values: ¶ 84,197-84,647, § 86,170-87,244.
*Data:* BHP, IEB, GQCS, † The European Regional Database (EUI, 2021), ‡ Statistical Office of the Federal Employment Agency.

Even before matching, there are no substantial imbalances with respect to regional characteristics such as population density, unemployment rate or the growth rate of RM occupations over the past decade, supporting the notion that displacement is unrelated to regional conditions. After matching, any differences vanish – expect for a minor imbalance with respect to the metal, machinery and automotive industry share that hardly passes the lower threshold. Note that we deliberately do not include AKM person and establishment fixed effects in the propensity score estimation in order to be able to check the quality of the matching ex-post.[21] In fact, there are no notable differences in pre-displacement worker or firm wage premia after matching. Hence, our matching approach may also capture differences in unobserved wage determinants that were not directly accounted for. Overall, these results suggest that our matched control group represents a valid counterfactual for the sample of displaced workers.

### 1.3.3. Exogeneity of Displacements to Regional Structural Change

Our aim is to compare the estimated effects of displacement between workers who lost their jobs in regions with differential exposure to RMBSC. Therefore, the estimated displacement effects for different regions need to be comparable. This requirement could be threatened if plant closures and mass-layoffs were systematically more likely in regions that are strongly exposed to RMBSC. Reassuringly, this is not the case. If at all, the overall displacement rate is slightly positively correlated to RM job growth, but the relation's significance depends on a few outlier regions with exceptionally many displaced workers or strong positive RM occupation growth (see Figure A.3(a) in the Appendix). The same holds for the displacement rate for RM workers (see Figure A.3(b)). Hence, displacement events are not concentrated in specific regions. This can also be seen in Figure A.4 in the Appendix which shows maps with the spatial distribution of the overall displacement rate as well as the displacement rate for RM workers across West German local labor market regions. We conclude that the displacement risk is not higher in regions with strong RMBSC. Albeit this may be surprising at first sight, it is well in line with the finding that the decline in routine occupations is mainly driven by reduced inflow rates, rather than rising outflows into unemployment (Cortes et al., 2020). For the subsequent analysis, we thus assume that displacement events exogenously expose displaced workers to different degrees of RM-biased structural change.

Another threat to the comparability of post-displacement outcomes between regions would be differences in the composition of displaced workers. Indeed, Table A.2.2 in Appendix A.2.1 shows that there are some differences in the pre-displacement characteristics of displaced RM workers between region types. These differences are mostly small. Nonetheless, we will explicitly account for them in the matched DiD approach that we discuss in the next section.

[21] Instead we chose to include the individual pre-treatment wage in $c-1$ and $c-2$ as well as the median establishment wage, which are highly collinear to the AKM fixed effects.

### 1.3.4. Estimation Approach

In this section, we will introduce two different estimation approaches to identify the effect of routine-biased structural change on individual workers' careers after job displacement.

#### Event Study Design for the Evolution of Displacement Effects over Time

We first follow the general approach in the displacement literature and employ an event study design to study the effects of job loss within occupation-region type cells over time. This approach compares the change in displaced workers' outcomes at various points in time after the event to the corresponding changes in outcomes of similar workers who were not displaced. We estimate the following model:

$$y_{ict} = \sum_{k=-4}^{6} \delta_k \, D_i \times I(t=k) \, + \, \sum_{k=-4}^{6} \gamma_k \, I(t=k) \, + \, \pi_c \, + \, \epsilon_{ict} \ , \tag{1.1}$$

where $y_{ict}$ represents the employment status for an individual $i$ in year $t = \{-4, \ldots, +6\}$ before or after a displacement in base year $c$. $I(t = k)$ indicates the years around the base year, $D_i$ distinguishes displaced and control workers. $\pi_c$ are calendar year fixed effects that account for year-specific displacement effects unrelated to local structural change, like the current business cycle. $\epsilon_{ict}$ is the idiosyncratic error term. $\delta_k$ are the coefficients of interest, i.e. the effect of displacement in year $k$ before or after the event relative to non-displaced control workers.[22]

We split the sample by worker $i$'s pre-displacement occupation type (RM vs. other main task) and region type (R1/R2/R3: Strong/medium/weak local RM bias) and estimate equation (1.1) separately within occupation-region type cells. Standard errors are clustered at the individual level.

The event study estimates provide a first impression about how displacement effects differ for workers laid off in regions with broadly different long-run patterns of structural change. They may also be indicative of potentially problematic pre-trends and allow for an easy comparison of the post-displacement evolution of outcomes of RM and other workers within region types. Moreover, they are readily comparable to the existing displacement literature. However, further controlling for compositional differences between workers across regions would necessitate to introduce multiple interactions between region type, worker type, displacement indicator and event time, resulting in a computationally demanding specification. For this reason, we use a matched DiD approach which gives equivalent results[23], but is both easier to implement and interpret (see also Schmieder et al., 2020). The next section introduces the matched DiD method in more detail.

Moreover, the event study approach, uses an arguably arbitrary and time-constant aggregation

[22] Since our matching procedure yields treatment and control workers with very similar baseline characteristics (see Table 1.1), the inclusion of further control variables or individual and establishment fixed effects hardly affects the estimates.

[23] See Figure A.6 in the Appendix.

of regions (R1/R2/R3). As an alternative, we use a time-varying measure of local structural change that measures the regional growth in RM occupations in the ten years prior to the displacement event, $grRM_r^{c-10}$. This has the advantage of avoiding (1) the arbitrary classification of regions and (2) to model displacement effects based on structural change measured partly after the displacement event.

### Matched DiD Design for Identifying the Structural Change Effect

To study how structural change affects post-displacement outcomes, we exploit the heterogeneity along the RMBSC distribution and implement a matched DiD approach in the spirit of Schmieder et al. (2020). Since each displaced worker is matched to a statistical twin, we can compute an 'individual DiD' for each displaced worker $i$ at time $t$ as follows:

$$\Delta_{dd} y_{iocrt} = \Delta_d y_{iocrt} - \Delta_{nd} y_{iocrt} \ , \tag{1.2}$$

where $\Delta_{\{d,nd\}} y_{iocrt}$ measures individual $i$'s change in outcomes between the pre-displacement base year $c$ and the post-displacement year $t$ for each displaced worker ($\Delta_d y_{icrt}$) and her non-displaced matched control individual ($\Delta_{nd} y_{icrt}$). The indices $o$ and $r$ mark the pre-displacement occupation and region in base year $c$. In addition to employment, we also examine wages and mobility in terms of occupational or regional switches as outcomes.

**Effect of Exposure to RMBSC.** In order to explicitly study how the exposure to RMBSC affects the costs of job loss while controlling for differences in the worker composition, we use the 'individual DiD' as the dependent variable and the time-varying indicator $grRM_r^{c-10}$, the weighted growth rate of RM occupations in the worker's pre-displacement region $r$ over the decade preceding base year $c$, as the measure of RMBSC exposure:

$$\begin{aligned} \Delta_{dd} y_{iocrt} = {} & \omega \ grRM_r^{c-10} + \phi \ I\left(RM_o^c\right) \\ & + \beta \ grRM_r^{c-10} \times I\left(RM_o^c\right) \\ & + X_{ic}\theta + \pi_c + \alpha + \nu_{iocrt} \ , \end{aligned} \tag{1.3}$$

where $I(RM_o^c)$ is an indicator for the type of the worker's pre-displacement occupation $o$ (= 1 if RM, = 0 if other main task). The interaction term $grRM_r^{c-10} \times I\left(RM_o^c\right)$ thus allows the displacement effect to vary with RMBSC in a linear fashion.[24] Since $grRM_r^{c-10}$ ranges from $-16\%$ at the bottom to $+16\%$ at the top of the distribution, we will later present the average marginal effects of displacement for RM workers as well as for other worker types over this range. $X_{ic}$ contains individual pre-displacement characteristics (gender, skill level, age, tenure, experience, AKM worker fixed effects). $\pi_c$ are base year fixed effects, $\alpha$ is a constant and $\nu_{iocrt}$ is the idiosyncratic error term. The model is estimated separately for each post-displacement year $t$

[24]We will provide a robustness check using a more flexible specification and argue in favor of this functional form assumption.

but jointly across displaced workers in all regions *r*.

All in all, this approach provides a parsimonious and easily interpretable way of modeling how structural change affects outcomes after job loss for different workers types while controlling for compositional differences.

## 1.4. Employment and Wage Effects of Job Displacement

### 1.4.1. Event Study Estimates by Region and Occupation Type

Figure 1.4(a) displays the results of the event study models for the employment probability as the dependent variable – separately estimated for RM and other workers within region types R1 to R3. The plot provides no indication of an obvious violation of the parallel trends assumption, as the pre-treatment outcomes of all subgroups are close to zero and precisely estimated. After displacement, both RM and workers from other occupations face substantial drops in the employment probability.[25] One year after displacement, the re-employment probability of displaced workers from other occupations is between 10 to 12pp lower as compared to control persons, with little variation between region types. After partial recovery, displacement still leads to a 6 to 7pp lower probability of being employed six years after the event. Even in R3 regions with strong job growth in other occupations, displacement still comes with persistent negative employment effects.

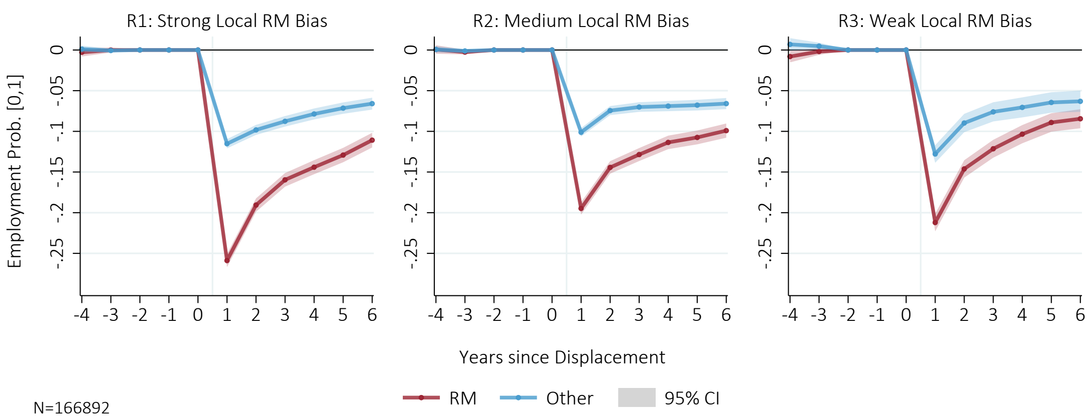
Compared to workers displaced from other occupations, RM workers generally experience stronger employment penalties in every region type. This is in line with findings of Blien et al. (2021) and Goos et al. (2020), who study how the costs of job displacement vary with routine intensity at the occupation level. Our results suggest that the regional context matters: While workers from other occupations have similar employment probabilities in all region types, the losses of RM workers are highest in regions with the strongest decline in RM occupations. In R1 regions, their employment probability drops by 26pp, as compared to 20pp in R2 and 21pp R3 regions. In addition, in R1 regions RM workers do not catch up as much with other workers: After six years, they are still about 5pp less likely employed than workers from other occupations in R1 regions. In region types R2 and R3 this gap narrows to about 3pp and turns insignificant in R3.

Our findings suggest that RM workers' employment prospects after job loss are more sensitive to structural change, resulting in a stronger and more persistent drop in their re-employment probabilities in more exposed regions. Therefore, this group has a higher risk of long-term unemployment and labor force exit in regions with strong RM biased structural change.

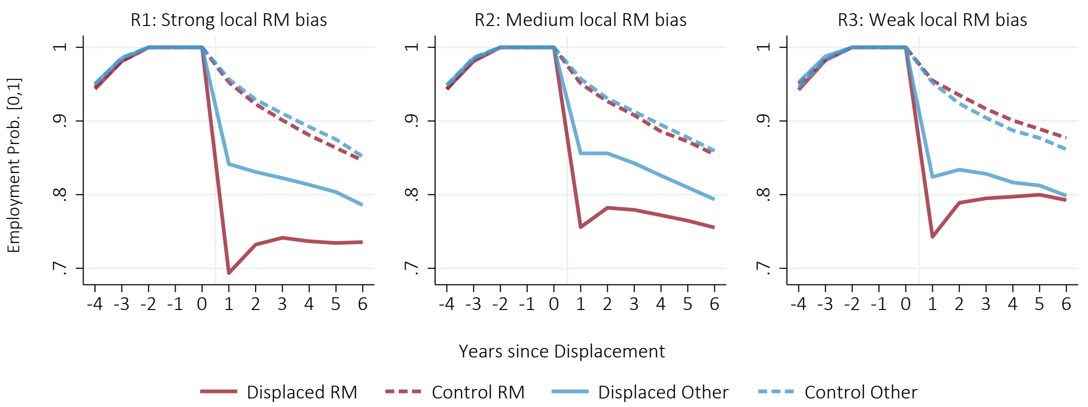
[25] On average, across both worker types and regions, displacement decreases employment by about -16pp after one year and -8pp after six years (see Figure A.5 in the Appendix). These results are in a comparable order of magnitude as in previous studies for Germany and other European countries (see e.g. Eliason and Storrie, 2006; Huttunen et al., 2011; Gulyas and Pytka, 2019; Schmieder et al., 2020; Blien et al., 2021; Goos et al., 2020 Fackler et al., 2021; Gathmann et al., 2020; Helm et al., 2022; Bertheau et al., 2022; ). Differences to these studies may result from different institutional settings, time frames and sample restrictions (we include women, study both small firm closures and large mass layoffs and we match on region and occupation types).

**Figure 1.4.:** Displacement Effects by Region Type and Main Task

**(a)** Employment Effects of Displacement (Event Study)




**(b)** Displacement vs. Counterfactual Employment (Means)




*Notes:* RM = Workers in occupations with mainly routine manual tasks, Other = Workers in occupations with other main tasks, CI = Confidence interval. Plot (a) shows coefficient estimates and confidence intervals from the event study model (see equation (1.1)), estimated separately by occupation type (RM/Other) and region type (R1/R2/R3: Strong/medium/weak local RM bias). Standard errors are clustered at the individual level. Plot (b) shows the unconditional means, i.e. the employment share of displaced and non-displaced RM/Other workers by region type R1/R2/R3. Region type refers to the terciles of the weighted regional growth in RM occupations between 1990-2010 (i.e. the 'red bars' in Figure 1.2, see also the formula for $grRM_r^{LR}$ in Appendix A.1.2); Average weighted growth within region types: R1=−17.0%, R2=−9.7%, R3=−0.7%.

*Data:* BHP, IEB, BeH, GQCS.

Figure 1.4(b) plots the unconditional employment share for displaced workers and their non-displaced controls. Since control individuals are matched on the initial region and occupation type and a comprehensive set of other characteristics, they provide a counterfactual for what would have happened to RM workers in absence of displacement. Strikingly, the employment trajectories of non-displaced control workers do not differ much by occupation or region type. Even in regions hit hardest by structural change, non-displaced RM and other workers experience very similar employment trajectories. Hence, RM workers seem to cope fairly well with structural change unless an unexpected lay-off forces them to look for a new job.

### 1.4.2. Matched Difference-in-Differences Estimates along the RMBSC Distribution

In this section we introduce the results of the matched DiD approach using the time-varying indicator of the RMBSC distribution.

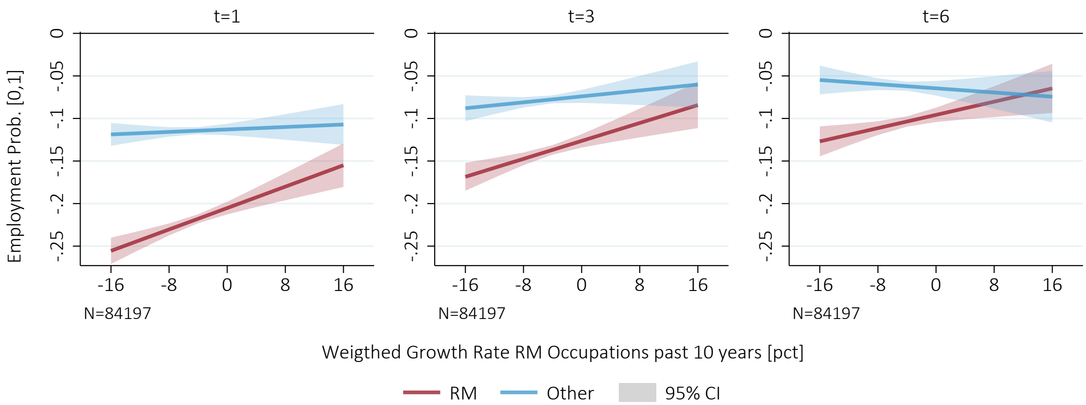
**Employment Effects along the RMBSC Distribution.** Figure 1.5 shows the results from the matched DiD model (equation (1.3)) that incorporates linear RM occupation growth as a continuous measure and its interaction with the RM occupation indicator. The plots show how the average marginal effect of displacement on employment for RM and other workers (vertical axis) varies with the regional growth rate of RM occupations in the past ten years (horizontal axis). The three panels provide the effects for one, three and six years after job loss. As regards workers from other occupations, the initial employment losses do not significantly differ with regional structural change. By contrast, for RM workers there is a strong positive gradient with RMBSC. At the bottom of the RMBSC distribution, where RM occupations strongly decline, displaced RM workers are about 25pp less likely employed after one year than their non-displaced controls. At the other end of the spectrum, where RM occupations grow, employment losses of RM workers are almost 10pp lower. Again, we observe some convergence between worker types over time and a flattening of the regional gradient for RM workers. However, at the bottom of the RMBSC distribution, RM workers are significantly less likely employed than other workers even after six years. At the upper end of the regional distribution, the difference between both worker types has vanished by then.

**Robustness.** We run several checks to test the robustness of these findings.

First, we document that the inclusion of individual control variables hardly affects the estimates, such that differences in worker composition between regions or occupation types are of minor importance (compare panel (a) and (b) in Figure A.7 in the Appendix). Estimates are also robust to the exclusion of outlier regions with unusually severe displacement events. Hence, the gradient is not driven by a few singular events in a certain part of the RMBSC distribution (see Figure A.7(c)).[26] We then relax the linearity assumption in model (1.3) by replacing the linear interaction

[26] Outliers are defined as labor market regions with average treatment effects below the 1%-ile or above the 99%-ile.

**Figure 1.5.:** Employment Effects along the Structural Change Distribution (matched DiD with ind. controls, t=1,3,6)



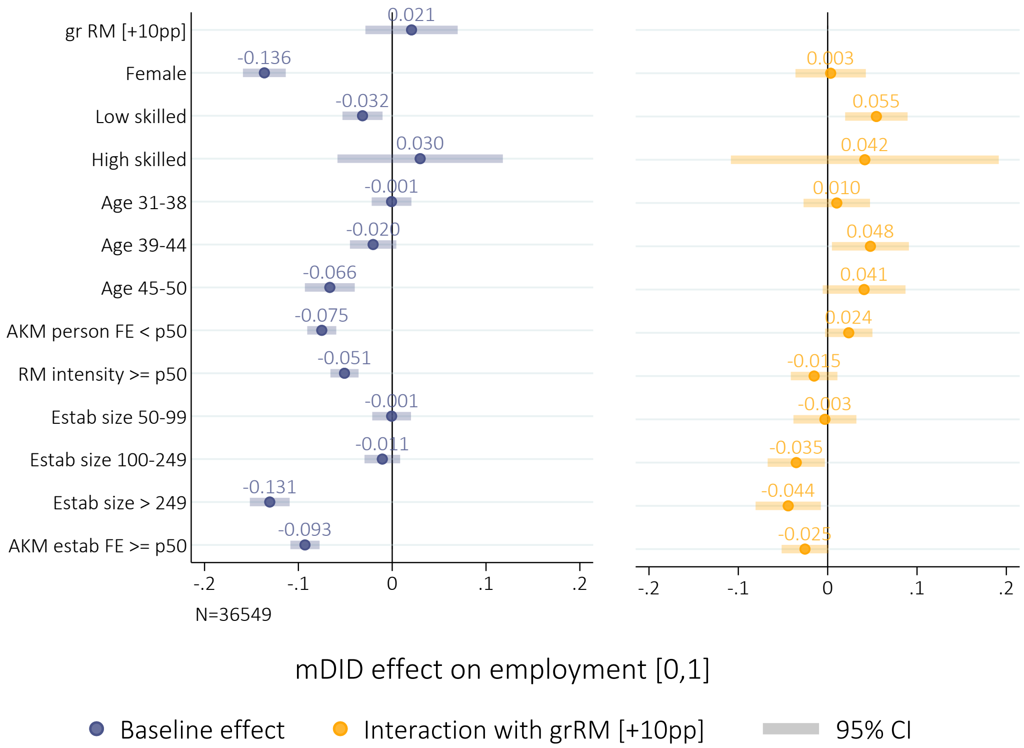

*Notes:* RM = Workers in occupations with mainly routine manual tasks, Other = Workers in occupations with other main tasks, CI = Confidence interval. Based on equation (1.3) in Section 1.3.4. The x-axis refers to the weighted regional growth in RM occupations over the decade preceding the base year $c$. Growth rates are weighted with the initial employment share in $c-10$ (see the formula for $grRM_r^{c-10}$ in Appendix A.1.2). Individual control variables include gender, skill level, age, experience, tenure and AKM worker fixed effects. Standard errors are clustered at the individual level.
*Data:* BHP, IEB, BeH, GQCS.

term with a separate interaction term for each quintile of the $grRM_r^{c-10}$ distribution. The results in Figure A.8 show that also in this more flexible specification, the RMBSC gradient is very close to a linear trend. In a further check, we exclude the *Ruhrgebiet*, an old industrialized rust belt type of region in the west of Germany that has seen a major economic decline since the 1980s. Again, excluding these regions yields almost identical estimates as our baseline specification (compare panel (a) and (b) in Figure A.9).

In another specification, we examine the employment probability of displaced routine cognitive rather than RM workers and compare their outcomes to those of all workers with a different non-routine main task (i.e. non-routine analytical, non-routine interactive or non-routine manual). The rationale is that much of the literature focuses on routine intensity per se, rather than comparing RM workers to all others (see e.g. Autor et al., 2008). The results suggest that routine cognitive workers are indeed more similar to other non-routine workers than to RM workers, as their employment probability lies in between both groups but much closer to all other non-routine workers (see Figure A.9(c)).

**Heterogeneity by Worker Characteristics.** Having established that RM workers' employment prospects are highly sensitive to regional conditions, we now analyze which sub groups of RM workers are more or less vulnerable to structural change. For this, we re-estimate equation (1.3) in Section 1.3.4 for the sample of RM workers only and interact individual characteristics with the regional growth rate of RM occupations in the past ten years. The left panel in Figure 1.6 provides the base coefficients for each $X$-variable, which reflect its effect on the re-employment probability independent of local structural change conditions. The right panel shows the coefficient of the interaction with regional RM growth ($grRM \times X$). The interaction effects are scaled to measure how the employment probability of a person with characteristic $X$ changes when $grRM$ increases by 10pp. A positive interaction effect means that a worker with characteristic $X$ has a lower employment probability in regions with a stronger decline of RM jobs (i.e. a 10pp lower value of $grRM$) and *vice versa*.

**Figure 1.6.:** Employment Effects along the Structural Change Distribution by Individual Characteristics
(RM workers, matched DiD with ind. controls, t=1)

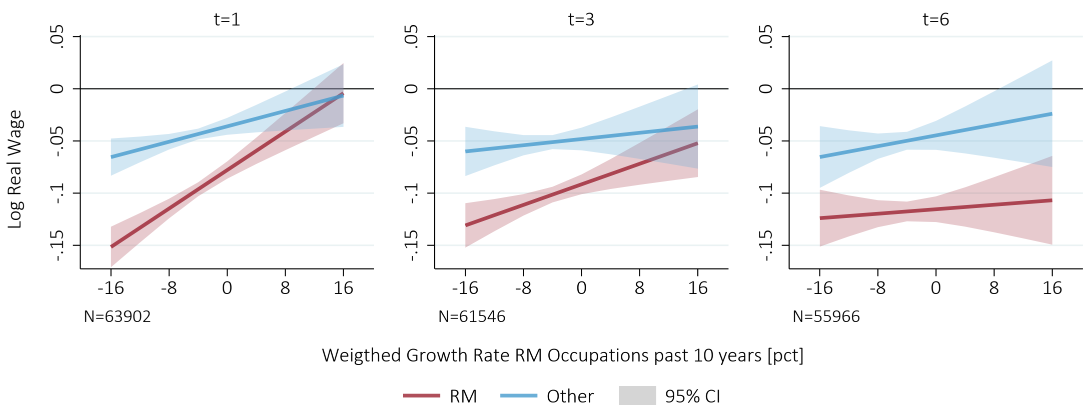



*Notes:* RM = Workers in occupations with mainly routine manual tasks, CI = Confidence interval. Based on equation (1.3) in Section 1.3.4, where $I(RM)$ is replaced by individual characteristics $X$ and their interaction with $grRM$. The sample is restricted to RM workers. The left panel shows the estimated baseline effect of $X$, the right panel shows the estimated effect of its interaction with $grRM$, the weighted regional growth in RM occupations over the decade preceding the base year $c$ (see the formula for $grRM_r^{c-10}$ in Appendix A.1.2). Standard errors are clustered at the individual level.
*Data:* BHP, IEB, BeH, GQCS.

First of all, the left hand side implies that women, workers between 45-50 years, low-skilled workers and workers with a pre-displacement RM task intensity above the median are generally less likely to be re-employed one year after displacement. The same holds for less productive workers, i.e. workers with an AKM person fixed effect below the median, as well as workers previously employed in large establishments or establishment with higher wage premiums, i.e. above median AKM establishment fixed effects. In line with much of the displacement literature, this suggests that older workers with less and more outdated skills are generally more at risk of poor post-displacement outcomes.

The interaction coefficients on the right-hand side of Figure 1.6 imply that older, low-skilled and low-productive workers are significantly more vulnerable to local RMBSC. For example, a low-skilled worker's re-employment probability would increase by 5.5pp if being displaced in a region with a 10pp higher RM occupation growth rate. Such an improvement in regional conditions would more than compensate the baseline penalty of -3.2pp for low-skilled workers.[27] By contrast, the employment probability of workers who were displaced from large well-paying firms is higher in strongly exposed regions where RM occupations decline more. Moreover, women's re-employment chances are generally lower, but do not significantly depend on local structural change conditions.

**Figure 1.7.:** Wage Effects along the Structural Change Distribution (matched DiD with ind. controls, cond. on re-employment, t=1,3,6)




*Notes:* RM = Workers in occupations with mainly routine manual tasks, Other = Workers in occupations with other main tasks, CI = Confidence interval. Based on equation (1.3) in Section 1.3.4. Estimated on the subsample of displaced workers who are re-employed. The x-axis refers to the weighted regional growth in RM occupations over the decade preceding the base year $c$. Growth rates are weighted with the initial employment share in $c-10$ (see the formula for $grRM_r^{c-10}$ in Appendix A.1.2). Individual control variables include gender, skill level, age, experience, tenure and AKM worker fixed effects. Standard errors are clustered at the individual level. *Data:* BHP, IEB, BeH, GQCS.

**Wage Effects.** Conditional on re-employment, displacement effects on wages differ substantially with exposure to structural change, as can be seen in Figure 1.7. For both worker types, wage losses are larger at the lower end of the local RMBSC distribution, but for RM workers they are roughly twice as large. In these regions, RM workers exhibit wage losses of about 14% in year one (-0.15 log points). In regions at the top of the distribution, where RM and total employment grow, the wage penalty from job loss is small and not significantly different from zero for both RM and other workers. This suggests that the average wage losses that are typically found for displaced workers in the literature differ markedly across space. The more exposed a region is to RMBSC, the higher are resulting wage losses, especially for RM workers. The regional gradient

[27] A 10pp difference corresponds to about one third of the range of $grRM$ observed in our data (-16 and +16%).

for RM workers flattens over time, but their wage losses are highly persistent. Even after six years, they still amount to about 12% in bottom regions.

## 1.5. Occupational and Regional Mobility

In order to examine whether regional and occupational mobility serve as an adjustment mechanism to regional structural change, we first analyze the probability of working in a different occupation type or a different labor market region one year after displacement – conditional on re-employment at that time. We then examine the potential costs of moving. Since re-employment and mobility after displacement are subject to individual self-selection, these results should be interpreted as descriptive rather than causal. We do, however, control for differences in observable pre-displacement characteristics in all specifications.

### 1.5.1. Switching Probabilities

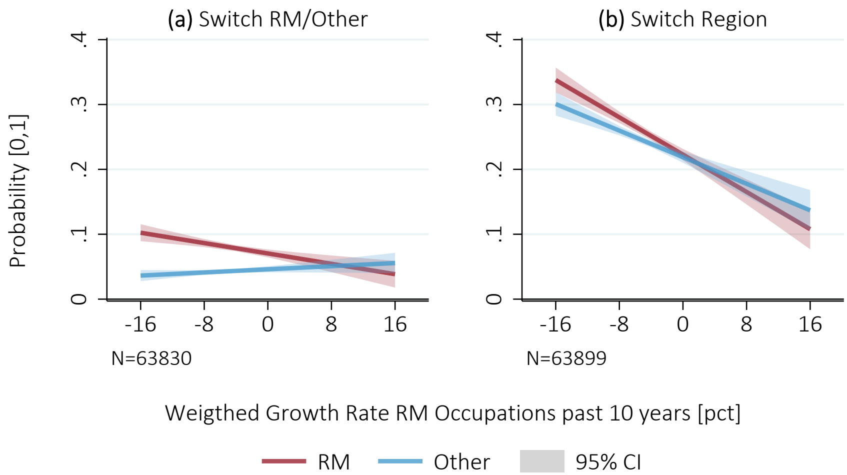
Figure 1.8 shows the switching probabilities for occupational mobility (panel (a)) and regional mobility (panel (b)) one year after displacement.[28] There are three main takeaways: First, occupational mobility is low compared to regional mobility. Conditional on re-employment, displaced workers are only 5 to 10pp more likely to have switched the occupation type after one year than their matched counterparts. By contrast, the displacement effect on the probability to work in a different labor market region ranges between 10 to 30pp. Note that this encompasses both re-location and commuting. When restricting regional mobility to residential mobility alone, effect sizes are much smaller (see e.g. Fackler and Rippe, 2017).

Second, there is a clear regional gradient for both worker types in regional mobility. This indicates that poorer job growth in the bottom part of the distribution incentivizes not only RM workers, but also other worker types to extend their search radius. For RM workers, regional mobility seems to be slightly more responsive to local structural change than for other worker types.

Third, occupational switching occurs mainly among displaced RM workers in regions hit hardest by structural change. The share of workers from other occupations who switch to an RM occupation is small and only slightly increasing along the RMBSC distribution. By contrast, RM workers are 10pp more likely to switch to an other occupation type in the bottom part of the distribution, but only 4pp more likely to be mobile in the upper part, which is similar to the effect of displaced workers from other occupations. Thus, occupational switching mainly occurs in regions where displaced RM workers compete for a declining number of RM jobs and is lower in regions with an abundant growth in other occupations. Put differently, occupational switching does not seem to be driven by opportunity, but rather by a lack of better alternatives.

[28] For these estimates, we replace the dependent variable in the matched DiD specification (1.3) by indicator variables for individuals that switch from RM to other occupations or *vice versa*, or who take up a job in a different labor market region at time $t$. Results for years three and six after displacement are provided in Figure A.10 in the Appendix. The general mobility patterns do not change much over the six post-displacement years.

**Figure 1.8.:** Effects on Occupational and Regional Mobility along the Structural Change Distribution
(matched DiD with ind. controls, cond. on re-employment, t=1)



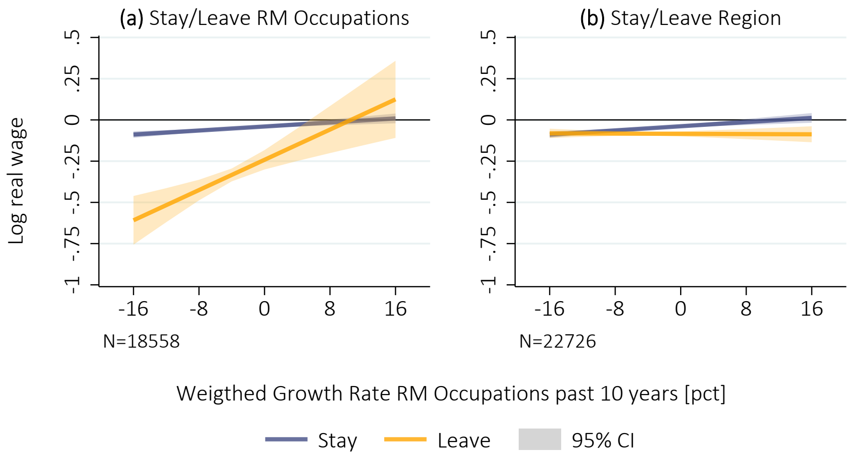

*Notes:* RM = Workers in occupation with mainly routine manual tasks, Other = Workers in occupations with other main tasks, CI = Confidence interval. Based on equation (1.3) in Section 1.3.4. Estimated on the subsample of displaced workers who are re-employed. The x-axis refers to the weighted regional growth in RM occupations over the decade preceding the base year $c$. Growth rates are weighted with the initial employment share in $c-10$ (see the formula for $grRM_r^{c-10}$ in Appendix A.1.2). Individual control variables include gender, skill level, age, experience, tenure and AKM worker fixed effects. Standard errors are clustered at the individual level. Panel (a) shows the probability of working in an occupation with a different main task as compared to the pre-displacement occupation (i.e. switching from RM to Other or *vice versa*). Panel (b) shows the probability of working in a local labor market other than the one in which displacement took place.

*Data:* BHP, IEB, BeH, GQCS.

### 1.5.2. Mobility Costs

If RM workers in bottom regions mainly switch occupations to avoid unemployment, we expect that they are also willing to accept lower wage offers than comparable workers in top regions. To shed light on this, we now focus on RM workers and examine how their post-displacement wages differ by their mobility status one year after displacement.[29] We focus on RM workers who either switch regions or the occupation type, but not both. By that, we avoid mixing the effects of regional and occupational mobility. Moreover, workers who switch along both dimensions are arguably a special selection of few highly flexible individuals.[30] The results are plotted in Figure 1.9 and we will discuss occupational (panel (a)) and regional mobility (panel (b)) one after another.

**Figure 1.9.:** Wage Effects along the Structural Change Distribution by Mobility Choices (RM workers, matched DiD with ind. controls, cond. on re-employment, t=1)




*Notes:* RM = Workers in occupation with mainly routine manual tasks, Other = Workers in occupations with other main tasks, CI = Confidence interval. Based on equation (1.3) in Section 1.3.4, where $I(RM)$ is replaced by an indicator variable for switchers. The sample is restricted to RM workers. Estimated on the subsample of displaced workers who are re-employed. The x-axis refers to the weighted regional growth in RM occupations over the decade preceding the base year $c$. Growth rates are weighted with the initial employment share in $c-10$ (see the formula for $grRM_r^{c-10}$ in Appendix A.1.2). Individual control variables include gender, skill level, age, experience, tenure and AKM worker fixed effects. Standard errors are clustered at the individual level. Panel (a) shows the wage losses of workers displaced from an RM occupation by whether they are re-employed in an RM or other occupation. Panel (b) shows the wage losses of workers displaced from an RM occupation by whether they are re-employed in the same labor market region or in a different region.
*Data:* BHP, IEB, BeH, GQCS.

**Occupational Mobility and Wages.** For RM workers in regions that are most exposed to RMBSC, occupational mobility is associated with substantial wage losses. One year after displacement, these workers earn almost half the wage (-0.6 log points) of their non-displaced peers. Even after six years, the wage penalty still amounts to around 18% (-0.2 log points, see

[29]For that purpose, we replace the indicator for RM-occupations in equation (1.3) by an indicator for occupation type or region switching and restrict the sample to RM workers.
[30]This pertains to 3.8% of all RM workers that are employed after one year.

Figure A.11(a) in the Appendix). In contrast, workers who enter a RM job again incur relatively small initial losses of 7% (-0.07 log points) that vanish over time. In the upper part of the RMBSC distribution, occupational switchers do not experience significant wage penalties – they even experience insignificant gains from switching occupation types after displacement.

Hence, the average wage losses of RM workers at the bottom of the RMBSC distribution that we document in Figure 1.7 are mainly driven by occupation type switchers. These higher switching costs in bottom regions are not explained by higher losses in task-specific human capital that could occur if switchers took up more dissimilar jobs. To the contrary, the task distance of RM to other task switchers in bottom regions is substantially smaller than in the upper part of the regional distribution (see Figure A.12(a) in the Appendix).[31] However, in bottom regions, RM to other task switchers incur substantially larger losses in establishment wage premia than RM stayers, while in the upper part of the regional distribution, the change in wage premia is insignificant for both RM to other task switchers and RM stayers (see Figure A.12(b)).

To sum up, in regions where RM occupations strongly decline, more RM workers switch occupation type despite of large and persistent wage losses – suggesting that these switches mainly occur because of a lack of better alternatives. These workers resort to the most similar jobs available to them, but still bear high costs – partly because of higher losses in establishment wage premia. This might reflect the link between RM-biased structural shifts and concurring shifts in the establishment structure that we discussed in Section 1.2.3: Employment losses in these regions were concentrated in initially large and highly productive manufacturing establishments. As a result, leaving an RM occupation in these regions, on average, coincides with switching to lower-paying firms and thus comes at high costs. Consistently, RM workers only incur low and temporary losses in overall wages and establishment premia if they return to an RM occupation in a bottom region.

In regions with relatively strong job growth in RM occupations and even higher job growth in other occupations, the story likely differs. Here, a rather small share of RM workers gain from taking advantage of job opportunities in other occupations and benefit from higher wages in the medium-run despite larger task distances.

**Regional Mobility and Wages.** Since the vast majority of individuals who take up a job in a different labor market region stick to RM jobs (86% of all regional movers), regional mobility seems to mainly serve as a strategy to keep an RM occupation that is locally no longer available. The task distances involved in these moves are small (see Figure A.13(a) in the Appendix).[32] However, RM workers who leave a bottom region experience wage losses of 10%. A substantial part of these wage losses, again, reflects losses in wage premia (see Figure A.13(b)). Similar to occupational switchers, regional switchers from regions in the bottom part of the distribution, tend

[31] In bottom (top) regions we estimate an average task distance of about 0.4 (0.6) for switchers. Given the 1986 task structure, this would for example correspond to switching from '06 Metal Production and Processing' to '42 Janitors' ('26 Technical Specialists'). See Appendix A.1.1 for details about our measure of task distance.

[32] The average estimated task distance both for region stayers and switchers is about 0.05, which would correspond to switching between '06 Metal Production and Processing' and '04 Chemistry and Plastics Production'.

to leave well-paying jobs in large establishments such that regional moves incur lower firm premia, on average. As a result, related wage losses are no less compared to those who are re-employed in a local RM job. Put differently, movers are not compensated for the monetary and non-monetary costs of moving. This could be one of the reasons why regional mobility for RM workers is only marginally more responsive to regional conditions than for other types of workers (see Figure 1.8).

Finally, workers who are displaced in one of the top regions and return to a local RM job do not experience any wage losses. These workers take up similar jobs as before, both in terms of tasks and wage premia (see Figure A.13 (a) and (b)). Leaving these top regions comes with small wage losses, but these are not explained by higher task distances or losses in establishment wage premia.

## 1.6. Conclusion

In this paper, we show that regional differences in the exposure to routine manual-biased structural change have important implications for the individual employment trajectories of displaced workers. By exploiting the regional heterogeneity in how local employment shifts are biased against routine manual (RM) occupations, we compare post-displacement outcomes across regions for workers specialized in RM or other tasks. In our empirical analysis, we focus on workers displaced during mass-layoffs or plant closures and apply a matched difference-in-differences (DiD) approach to identify causal effects that are comparable between regions. We thereby add a number of novel empirical insights.

First of all, we find that, even in the most exposed regions, workers in RM occupations are shielded from structural change as long as they remain on the job; it is only upon displacement that structural change starts to matter. We find that the disruptive consequences of displacement are amplified for workers in regions that underwent a stronger decline in task-related employment. One year after job loss, RM workers who got displaced in regions with the strongest long-run decline in RM jobs have a 10pp lower re-employment probability and 14pp greater wage losses than comparable workers in regions where RM occupations grow the most. This regional gap narrows over time but still persists after six years.

Secondly, related wage losses are closely linked to occupational switching. While RM workers who are re-employed after one year in an RM occupation suffer only small and temporary wage losses, those who switch occupations suffer wage losses of almost 50% after one year and 15% after 6 years. Moreover, a notable share of these wage losses comes from lower post-displacement firm wage premia, reflecting that regions hit hardest by structural change were initially dominated by large, highly productive manufacturing firms that experienced a subsequent decline. Hence, the costs of occupational mobility in these most exposed regions are particularly high. Regional mobility, on the other hand, provides a remedy only for workers with low moving costs because such moves do not yield a wage premium that would compensate workers for any related costs. As a result, low-skilled, low-productive, and older workers are put at the end of the local queue for a declining number of RM jobs, while neither regional nor occupational mobility is a feasible adjustment strategy for them. For example, for a low-skilled worker the risk of being unemployed after one year is 17.6pp higher in regions most exposed to structural change as compared to the least exposed regions.[33]

Therefore, our paper highlights the adverse effects of individual displacement shocks in the context of structural change. As different regions as well as more or less vulnerable groups of workers are unevenly affected, the interplay of individual shocks and structural change contributes to rising labor market inequalities both between as well as within regions.

From a policy perspective, our study thus calls for a place-sensitive approach to reduce risks that structural change may pose to individual workers. However, there is likely no easy way out as our results suggest severe barriers to occupational and regional mobility. Most importantly therefore, supportive measures should be directed to reducing related costs for the most vulnerable groups in declining regions. For this, a successful strategy likely necessitates a bundle of measures. While re-training measures should aim at facilitating occupational mobility, a temporary wage subsidy for occupational movers may reduce barriers related to the corresponding loss of firm wage premia. In addition, mobility subsidies that cover not only actual monetary moving costs, but also pay an additional compensation for non-monetary costs might help boosting regional mobility. Although these measures are costly, the costs of not addressing the disruptive and unequal character of displacement in declining regions may even be worse in a longer run, as this may be a source for the rise in discontent, anti-establishment sentiments, and populism that has been found particularly among low-skilled workers in lagging regions hit by local economic and industrial decline (Rodríguez-Pose, 2020, Dijkstra et al., 2020).

[33] These numbers are derived from Figure 1.6 by multiplying the regional gradient for low-skilled workers (that captures a 10% increase in RM employment growth) by a factor of 3.2 (reflecting the difference between a region with RM employment growth -16 as compared to +16%.

# 2. Changes in Occupational Tasks and the Costs of Job Loss

## 2.1. Introduction

*"The only constant is change."* This 2,500 year old quote by ancient philosopher Heraclitus certainly applies to the history of human labor. In fact, Heraclitus himself witnessed how papyrus and ink revolutionized the work of Greek scribes by replacing the wax coated tablet (Roemer, 2007). As a more recent example, the spread of ATMs redefined the job of bank tellers (Bessen, 2015). Looking forward, artificial intelligence will alter the tasks of many more occupations (Brynjolfsson et al., 2018).

This constant change challenges human adaptability: workers must trade off the costs of switching occupations or investing in new skills against potential productivity and wage losses. Learning-by-doing and job training likely play an important role for skill acquisition (Battisti et al., 2017). Unexpected career breaks, in turn, may preclude workers from gradually learning new skills on the job. This has important implications for worker welfare, but also for aggregate skill supply and the rate of technology diffusion and innovation (Acemoglu, 1998). And yet, there is little empirical evidence about how individuals adjust to changes in their occupation's tasks.

In this paper, I study the role of job loss during this process: Do workers experience greater earnings losses if they are laid off during a period of task restructuring? If such a task change penalty exists, is it permanent? And how is it related to occupational mobility?

To answer these questions, I use a newly available dataset that traces the task composition of occupations over three and a half decades. Taking this information to administrative social security records allows me to follow individual worker careers. I focus on layoffs during plant closures, because such separations are arguably unexpected, involuntary and unrelated to individual skills. I then compare the earnings losses of workers who were exposed to higher levels of task change before job displacement. However, these workers in different exposure groups also differ in characteristics other than task change itself. In order to neutralize this bias, I employ a Triple-Differences estimator that uses non-displaced workers with similar task changes as an additional control group.

My results provide three novel empirical facts about how changes in occupational tasks affect the costs of subsequent job loss: First, workers in the top-quartile of task change experience 4,400 Euros – or about 90% – higher annual earnings losses than workers in the bottom quartile. About half of this additional earnings penalty is explained by a lower re-employment rate of more exposed workers.

Second, workers who are exposed to larger task changes are almost twice as likely to switch occupations. Highly exposed workers who return to the same occupation experience a much lower task change penalty than switchers. However, the penalty for highly exposed switchers also varies a lot more: While some switchers' post-displacement earnings seem to respond strongly to changes in pre-displacement tasks, others do not incur additional earnings losses beyond workers whose tasks did not change.

Third, the earnings losses of older workers depend much more on pre-displacement changes in tasks. This is mainly driven by a lower re-employment rate. If older workers return to employment, they are more than twice as likely to switch occupations.

I conclude that changes in occupational tasks have a strong impact on the individual costs of job loss. Not all workers adapt equally fast when the task structure of their occupation changes. Especially older workers face higher learning costs and lower expected returns (Picchio, 2021). When workers with an outdated skill set are suddenly discharged, they may be forced to switch occupations and accept lower wages. If the arriving wage offers fall below the reservation wage, more workers will leave the labor force altogether. The earnings of workers who update their skill-set do not seem to be negatively affected by task changes prior to job loss. They either return to a job in the same occupation or find an equally good match in a different occupation.

Overall, my results emphasize the importance of job loss during the adjustment to changing task requirements. Unexpected separations preclude workers from gradual adjustment via on-the-job learning – or a smooth phasing out of older workers with higher adjustment costs. This highlights the importance of 'life-long' learning as an insurance against sudden career interruptions in a dynamically changing environment. In the future, digitization will rapidly alter the skill requirements of many jobs, while population ageing increases the adjustment costs. Hence, there may be a case for welfare-improving policy interventions. Especially for older workers, public training subsidies may foster skill investments and prevent early labor market exits after an unexpected career break, e.g., in case of job loss or when nursing family members.

In what follows, section 2.2 will relate my paper to the previous literature and discuss its contributions. Sections 2.3 and 2.4 introduce the data and empirical strategy. Section 2.5 presents the results and section 2.6 concludes.

## 2.2. Related Literature and Contribution

**Earnings Losses of Displaced Workers** The focus on job loss relates my paper to a large literature about the costs of job displacement during plant closures or mass layoffs. This literature consistently documents that displaced workers suffer large and persistent earnings losses (see e.g., Jacobson et al., 1993; Huttunen et al., 2011; Schmieder et al., 2022 or Bertheau et al., 2022).[1] A few recent papers have started exploring the role of technological change and changes in the demand for certain tasks. These papers fix the task structure of occupations in a base period.

[1] Carrington and Fallick (2017) provide a review of the empirical literature and theoretical explanations.

They generally conclude that workers who are displaced from initially more routine-intensive occupations experience larger earnings losses (Goos et al., 2020; Blien et al., 2021; Yakymovych, 2022). In chapter 1 of this dissertation, Melanie Arntz, Laura Pohlan an me show that this routine task penalty strongly depends on regional labor market conditions (see also Arntz et al., 2022).

As a novelty, Braxton and Taska (2021) explicitly study how *changes* in the computer-intensity of occupations during the 2010's affect the earnings losses of displaced workers in the US. They find that an increasing computer-intensity is associated to greater occupational mobility and larger earnings reductions after job loss. Braxton and Taska's main contribution is a theoretical model that rationalizes these stylized facts and informs the optimal policy response.

I complement their findings by providing causal evidence about how changes in occupational tasks and job loss interplay to shape worker outcomes. Using a novel German task dataset, I track changes in the overall task composition of occupations over 35 years, rather than focusing on a single skill in the past decade. Moreover, using administrative data allows me to follow individual workers over a decade around job displacement. To the best of my knowledge, this is the first paper to estimate the causal effect of within-occupation task change on the costs of job loss.

**Task Change within Occupations** My results therefore also complement an emerging literature that studies task restructuring within occupations. Atalay et al. (2020) use news paper job ads to show that most of the overall change the task structure of US employment between 1950 and 2000 occurred within narrowly defined job titles. Spitz-Oener (2006) suggests that computerization is an important driver of increasing complexity and skill intensity of occupations. Hershbein and Kahn (2018) provide evidence that the Great Recession accelerated this process. Cortes et al. (2021) show that also social skills have become more important and that this contributed to the self-selection of women into high-paying occupations. Ross (2017) and Bachmann et al. (2022) document that occupations with an increasing non-routine cognitive task intensity generate substantially higher wage growth over time. Fedorets (2018) shows that incumbents who stay in a changing occupation experience wage increases beyond a pure tenure effect.

I add to this literature, by providing causal evidence about the role of job loss during individual adjustment to changes in occupational tasks. For that, I rely on a novel survey dataset that traces occupational tasks since the early 1970's. In comparison to previous studies, this data is more frequent, more consistent and based on much larger samples, which enables me to study more disaggregated occupations. This is important, because even within groups of similar occupations, the magnitude and timing of task changes varies substantially. Moreover, I use plant closures as an exogenous shock to account for the endogeneity between task shifts, unobserved skills and job separation.

**Skill Demand or Supply Shocks** My paper is also related to a small literature that studies shocks on the demand or supply of particular skills or occupations. Horton et al. (2020) show that after the sudden abolition of Adobe Flash, many programmers left the market for 'Flash jobs' such that wages remained almost unchanged. Janssen and Mohrenweiser (2018) show that even young incumbents suffered long-lasting earnings losses when the IT skill requirements of a particular manufacturing occupation were suddenly raised. The specificity of these shocks makes the identification strategy of these papers very credible, but it also limits the generalizability of their results. Here, my paper adds a more general view on task change and worker adjustment: I study gradual changes in the overall task composition of occupations and use job loss as a relatively common individual-level shock during adjustment.

In this regard, my paper is closely related to Edin et al. (2019). Using Swedish data, they show that incumbent workers in declining occupations only experience small cumulative earnings losses. I provide similar evidence for Germany, but I also show that involuntary separations substantially increases the adjustment costs for workers.

## 2.3. Data

Following a common approach in the literature, I measure tasks at the occupation level and merge them to individual data via common occupation codes (see e.g., Autor et al., 2003; Gathmann and Schoenberg, 2010). In this section, I will briefly introduce the merits of my occupation-level task data and the administrative dataset I use to study individual employment biographies.

### 2.3.1. Occupational Tasks

**Description** The *Occupational Panel on Tasks and Education* (OPTE) is a dataset with yearly occupational-level information on work tasks and education investments between 1973 and 2011. The OPTE was created by Maier (2020) and is hosted by the GESIS Leibniz Institute for Social Sciences.[2] The data is derived from 16 waves of the German Microcensus, a representative cross-sectional survey of one percent of the German population. About every two or three years respondents were asked to choose their most important work place activity from a list of tasks. The OPTE aggregates this information to the level of 179 consistent occupations with at least 30 observations in every wave.[3] Each occupation $o$ is characterized by an 11-dimensional task vector $q_{ot} = (q_{1ot}, \dots, q_{jot}, \dots, q_{11ot})$, where the entries $q_{jot}$ measure the share of workers with main task $j = 1, \dots, 11$ in year $t$.

The focus on the main activity likely underestimates the complexity of occupations, especially when the majority of workers in an occupation carry out the same main task. For example, more than 90% of 'Educators and child care professionals' report 'Teaching/educating/publishing' as their main task in every year (see occupation 864 in Figure B.1 in Appendix B.1.2). Despite the apparent importance of teaching, most educators likely carry out some other tasks as well. But since these are not considered the main activity, their importance in a typical educator's job is likely understated. Hence, I only detect task changes that are 'severe' enough to substantially

[2] Occupational Panel on Tasks and Education (OPTE) for Western Germany from 1973 to 2011, Version 1.0.0, SowiDataNet|datorium of the GESIS Leibniz Institute for Social Sciences, DOI: https://doi.org/10.7802/2126. For a description in English also see Maier (2021).

[3] Smaller occupations were combined with others that feature a similar task focus (Maier, 2021).

shift the distribution of main tasks across workers. My estimates of the effect of task change on the costs of job loss are therefore a lower bound.

To ease exposition, I follow the literature and classify the 11 tasks into five groups (e.g., Autor et al., 2003, Spitz-Oener, 2006): routine manual tasks, non-routine manual tasks, cognitive tasks, and interactive tasks.[4] For some descriptive analyses, I group occupations into the broad categories 'Manufacturing', 'High-wage Services' and 'Low-/Mid-wage Services'. This grouping is based on KldB1988 1-digit codes and West German occupational mean wages in 1990 as kindly provided by Dauth (2014).

**Restrictions** The OPTE is restricted to persons living in Western Germany with at least one working hour per week. I drop agricultural and mining occupations (based on KldB1988 1-digit codes), because they are subject to very particular structural changes in Western Germany and only represent a small fraction of overall employment.[5] I use the task information from workers in social security employment to match the sample restrictions of the administrative data I use for individuals.

**Comparison to other Task Data Sets** There are only few data sets that allow following occupational tasks over a long time horizon. Many previous studies have used the *'German Qualifications and Career Surveys'* (GQCS) to describe differences in tasks *between* occupations at a given point in time (see e.g., Antonczyk et al., 2009; Gathmann and Schoenberg, 2010). However, some features of the GQCS complicate its use for studying changes *within* occupations over time: The inconsistency of the task definitions across the waves make harmonization a challenging and discretionary exercise and deriving a time-consistent set of occupations is restricted by the sample size (Rohrbach-Schmidt and Tiemann, 2013).

In comparison, the OPTE has several advantages: As the primary data source, the Microcensus features much more frequent and consistent task definitions, which facilitates a credible harmonization of tasks over time.[6] Moreover, its larger sample sizes (at least 179,000 as in 1973) allow for a much more disaggregated set of occupations. As I will show below, further aggregation would blur variation in the level and timing of task changes between similar occupations.

As an alternative, expert databases provide coherent and accurate task information for very disaggregated occupations. However, they are either only available for recent years (e.g., *Berufenet*

[4] Unlike earlier studies I do not distinguish routine and non-routine cognitive tasks. In practical terms, this means I do not classify 'typewriting/calculating' tasks as routine cognitive, because the actual routine intensity of these tasks may differ between occupations and change over time. Consistently, recent studies find no general reduction in returns to supposedly routine cognitive tasks in Western Germany (see e.g., Wang, 2020; Bachmann et al., 2022).

[5] Note that soldiers, people in community service or living in collective accommodation, as well as respondents with incomplete occupation or task information are also excluded (see Maier, 2021). I further exclude occupations that are usually carried out by public servants, such as judges, prison staff or firefighters (occupation codes 801, 802, 811, 813, 814 in the OPTE classification), because such jobs are not covered by my administrative data.

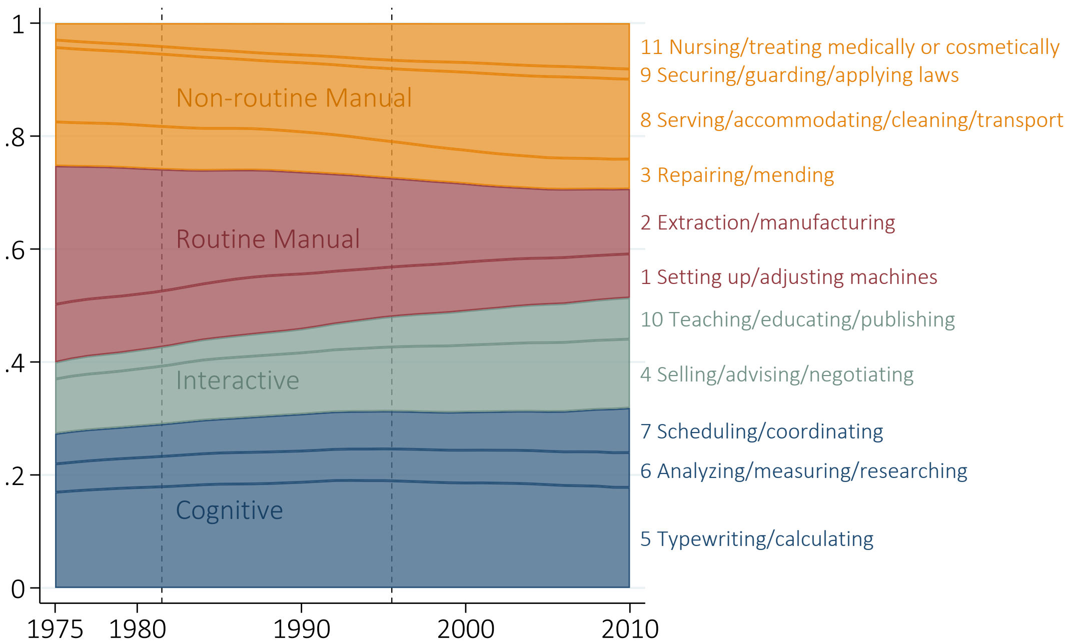
[6] The OPTE covers Microcensus waves 1973, 1976, 1978, 1980, 1982, 1985, 1987, 1989, 1991, 1993, 1995, 1996, 2000, 2004, 2007 and 2011. The gaps are filled by a +/- 3 years moving average. Over the entire period, the task items were modified twice (between 1980/82 and 1995/96). After harmonization across these intervals (see Maier, 2021 for details), there are no visible breaks in the task shares (see dashed vertical lines in Figure 2.1).

for Germany) or were not updated regularly in the past (e.g., *Dictionary of Occupational Titles* for the US).

Apart from the job ads data of Atalay et al. (2020), the OPTE provides the only dataset for outside the US that allows for credibly measuring changes in the task composition of occupations over a long time horizon.

**Descriptives** Figure 2.1 shows how the worker shares of the 11 main tasks develop between 1975 and 2010. The worker share in more easily automatable routine manual tasks decreases. In contrast, non-routine manual, interactive and cognitive tasks grow in importance. This is in line with previous research: automation replaces humans in easily codifiable and repetitive activities, while labor reallocates to non-routine tasks where humans are more productive (Autor et al., 2003, Acemoglu and Restrepo, 2018).

**Figure 2.1.:** Main Task Composition of Employment in OPTE Data, 1975-2010




*Notes:* The figure shows how the share of workers (in employment subject to social security contributions) with a given main task evolves in Western Germany. The plot is based on population-level estimates of occupational employment provided in the OPTE data. The dashed vertical lines mark changes in the task definitions of the Microcensus, which is the underlying microdata source of the OPTE. There are no visible breaks around these years. The classification of tasks follows a usual approach in the literature (see e.g., Autor et al., 2003, Spitz-Oener, 2006): *Routine-manual* tasks subsume production activities that are deemed repetitive and well codifiable and thus susceptible to automation. *Non-routine* tasks are carried out in dynamic environments or involve exchange with humans, which is not easily taken over by machines. *Cognitive* and *interactive* tasks are considered complementary to technology, because technology supports these activities and makes workers more productive.
*Data:* OPTE.

### 2.3.2. Measuring Changes in the Task Composition of Occupations

**Distance Measure** To measure how the task content of an occupation $o$ changes over time, I compute the Angular Separation between its task vectors $q$ at two points in time $t$ and $t'$:

$$\mathcal{D}\left(o,t,t'\right) = 1 - \frac{\sum_{j=1}^{11} q_{jot} \times q_{jot'}}{\sqrt{\sum_{j=1}^{11} q_{jot}^2 \times \sum_j q_{jot'}^2}} . \tag{2.1}$$

This scalar measure describes how far an occupation has 'moved' from its initial task composition: It is zero if the task vector remains unchanged and takes the value of one if it turns into the orthogonal direction.[7] I will refer to $\mathcal{D}\left(o,t,t'\right)$ as the 'within-distance'.

In previous studies, similar measures have been used to describe the task distance between occupations at a given point in time (see e.g., Poletaev and Robinson, 2008, Gathmann and Schoenberg, 2010 or Macaluso, 2019). To the best of my knowledge, Fedorets (2018) is the only other paper that uses a distance measure to study inter-temporal task changes.

**Descriptives** Figure 2.2 plots the within-task distance of all occupations in the OPTE as a measure of task change after 1975, i.e., $\mathcal{D}\left(o,1975,t'\right)$. The colored lines represent three example occupations and the mean within-distance across all occupations in a given year. Many occupations only change moderately over the observation period, resulting in a relatively low mean (red line).[8] To take up the example from above, the within-distance of '864 Educators and Child Care Professionals' (orange line) stays close to zero, because almost all workers name '10 teaching/educating/publishing' as their main task in every year.

On the other hand, there is a lot of variation both between occupations and over time. For example, occupation 631 ('Specialised biological-technical workers', blue line) changes substantially between 1975 the 1990, but remains relatively constant thereafter. For occupation 305 ('Musical-instrument makers and other precision mechanics', green line) the time pattern is just the other way around.

### 2.3.3. Individual Employment Biographies and Plant Closures

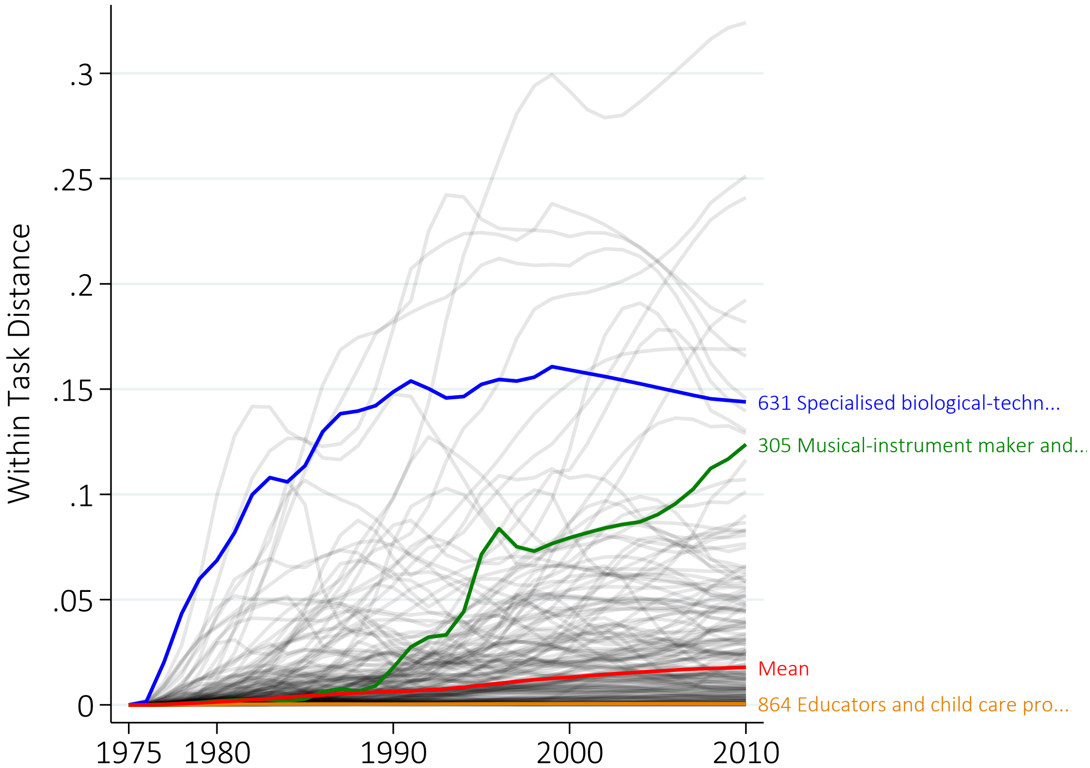
**Description** As my individual-level data source I use the *Sample of Integrated Employment Biographies* (SIAB), which is provided by the Institute of Employment Research (IAB).[9] The SIAB is a two percent random sample of all individuals who ever contributed to the German social security system at least once since 1975. It originates from administrative process data of the German social security system. These records track spells of employment subject to social

[7] Figure B.1 in Appendix B.1.2 illustrates how changes in the task composition translate into the distance measure for three example occupations.

[8] This general pattern also holds within manufacturing, high wage service occupation and low/mid wage service occupations (see Figure B.2 in Appendix B.1.2).

[9] Weakly anonymised Version of the Sample of Integrated Employment Biographies (SIAB) - Version 7519 v1. Research Data Center of the Federal Employment Agency (BA) at the Institute of Employment Research (IAB). DOI: 10.5164/IAB.SIAB7519.de.en.v1. For a description of the data see Frodermann et al. (2021).

**Figure 2.2.:** Changes in Occupational Task Compositions after 1975




*Notes:* The figure plots the within-distance of all occupations $o$ in the OPTE between 1975 and all consecutive years $\mathcal{D}^{o}_{[1975,t]}$ (see equation (2.1)). The colored lines mark example occupations 305 'Musical-instrument makers and precision mechanics' (green), 631 'Specialised biological-technical workers' (blue), 864 'Educators and child care professionals' (orange) and the employment-weighted mean across all occupations in a given year (red). The mean uses the OPTE's population-level estimates of occupational employment as weights.

*Data:* OPTE.

security contributions or benefit receipt with daily precision, while periods of self-employment, civil or military service and pension receipt are not covered. The data contains no information on working hours, but reports daily wages that are top-coded at the eligibility ceiling of the social security system. I closely follow the guidelines of Dauth and Eppelsheimer (2020) for preparing the data and imputing top-coded wages.

The weakly anonymised SIAB version also includes the ID of each worker's establishment. This allows me to merge employer characteristics such as industry code, workforce size, median wages and individual and employer wage premia ('AKM' fixed effects).[10]

**Identification of Plant Closures** Job loss is not a random shock. Especially in presence of task changes, job loss might be related to unobserved skills and productivity, which by themselves affect worker outcomes. I therefore focus on workers who lose their jobs during plant closures, because when the entire workforce of an establishment is laid off, job loss is reasonably independent of the relative productivity of workers.

Plant closures can only be inferred from plant IDs that disappear from the administrative records between June 30 of two consecutive years (see Dauth and Eppelsheimer, 2020). There may be other reasons why a plant ID disappears, like restructurings, mergers or ownership changes. In order to avoid falsely classifying such events as closures, I follow Hethey-Maier and Schmieder (2013) and exclude cases where more than 30 percent of the workforce jointly move to the same new establishment ID.

In what follows, I will call the last year before a plant disappears the baseyear $c$. For non-displaced workers, I mark all years in which they fulfill the same sample restrictions and pick a random year as the baseyear $c$.

**Restrictions and Construction of Panel** I apply a number of restrictions to displaced and non-displaced workers' baseyear characteristics. These restrictions are meant to assure that workers were employed in a stable job that would likely have persisted in absence of job loss. I only keep full-time workers in employment subject to social security contributions with at least two years of establishment and occupation tenure and restrict to workers between age 24 and 59 in the baseyear. Younger or older workers could either still be in education or close to be eligible to early retirement and therefore less attached to their jobs. For the same reason, I exclude workers who were not observed in the administrative data at least once over the past four years, which could be due to inactivity. Moreover, I exclude workers in agricultural and mining occupations, because they represent a very small share of the labor force that is concentrated in declining industries.[11] Individuals who were employed in Eastern Germany during the four years preceding

[10] The individual and establishment premia are based on the method pioneered by Abowd et al. (1999). They are estimated and directly provided by the IAB for linkage to the SIAB (for details see Bellmann et al., 2020). Note that I always use AKM effects that were estimated on a time window preceding the plant closures, so they are not contaminated by the displacement events themselves.

[11] Like in the OPTE, I also exclude occupations that would usually be employed as public servants and should not be covered by the SIAB under normal circumstances (see footnote 5).

the baseyear are dropped to match the OPTE's restriction to Western Germany. In addition, I also drop workers from establishments with more than 500 employees, because these are very rare in the plant closure sample.[12]

I then construct an individual level panel dataset that covers $t = -4$ to $t = +6$ years around the individual baseyear $c$. This panel measures individual outcomes like the employment status or occupation changes on June 30 of each year. It also includes annual aggregates like days employed and labor earnings, which cover the entire calendar year around June 30.

The resulting sample consists of 634,002 workers with about 15,000 to 40,000 individuals per baseyear. It includes 14,527 displaced workers with roughly 200 to 700 individuals per baseyear.

## 2.4. Empirical Strategy

### 2.4.1. Research Design and Estimation Specification

**Exposure Groups** My goal is to estimate whether exposure to changes in tasks affects the costs of job loss. For that purpose, I compute the within-distance $\mathcal{D}_i(o, e, c)$ for all workers $i$ in my sample as a measures of how much an individual's occupation $o$ has changed between the year of occupation entry $e$ and the displacement baseyear $c$.

I then split the distribution of observed changes into quartiles and define three exposure groups: The first quartile represents the 'zero' exposure group ($E_0$), for whom occupational tasks hardly changed. The second and third quartile are combined into the 'low' exposure group ($E_1$). The fourth quartile is exposed to much larger levels of task change and is therefore classified as the 'high' exposure group ($E_2$).[13] Figure B.3 in Appendix B.1.2 illustrates how the within-distance varies between and within the exposure groups and for displaced and non-displaced workers.

My empirical approach estimates the effect of task change on the costs of job loss by contrasting the change in outcomes of the low or high exposure group against the zero exposure group.

**Non-parallel Trends between Exposure Groups** The exposure groups do not only differ in task change, but also in other characteristics that are related to earnings. Table 2.1 shows that a higher exposure is related to a lower share of females and a higher share of workers without a professional degree. Before job loss, high exposure workers are more likely employed in manufacturing occupations or industries and in larger establishment with higher median wages.

More exposed workers are also older, have more labor market experience, job and occupation tenure and higher AKM fixed effects, daily wages and annual earnings. Workers in an early career stage have spent less time in their occupation and are less likely to have already experienced task changes. At the same time, they typically experience steeper earnings growth as they build up specific human capital and obtain better job matches. This, however, also implies that workers in different exposure groups would likely have experienced different post-displacement earnings

[12] As a robustness check, I keep these observations in the sample and explicitly match displaced and non-displaced workers in the same establishment size class (for details see section 2.5.5.)

[13] The results are qualitatively very similar when using a quartile or decile grouping.

losses also in absence of any changes in occupational tasks.

A simple comparison of the earnings losses of more or less exposed workers against the zero exposure control group would therefore be contaminated by non-parallel trends bias (Callaway et al., 2021).

**The Triple-Differences Estimator** To account for this bias, I use non-displaced workers as an additional control group in a Triple-Differences (DDD) design. In a given baseyear, these workers remain in a stable job while being exposed to the same task shifts. Therefore, also the deviation between the earnings trends of the exposure groups should be similar to the displaced worker sample. If this so-called Bias Stability assumption is fulfilled, taking the third difference between displaced and non-displaced workers will cancel out the non-parallel trends bias between the exposure groups (Olden and Møen, 2022).

**Empirical Specification** I estimate the following triple-differences specification to obtain the effect of a higher task change exposure on the costs of job loss:

$$\begin{aligned} Y_{ioect} = & \sum_{k=1}^{2} \beta_{1k} E_k \cdot Post_t \cdot Disp_{ic} \\ & + \sum_{k=1}^{2} \beta_{2k} E_k \cdot Post_t + \sum_{k=1}^{2} \beta_{3k} Disp_i \cdot E_k + \beta_4 Disp_{ic} \cdot Post_t \\ & + \sum_{k=1}^{2} \beta_{5k} E_k + \beta_6 Post_t + \beta_7 Disp_{ic} \\ & + X_{ic}\phi + \gamma(s,b,o,c) + \alpha + \epsilon_{ioect} , \end{aligned} \tag{2.2}$$

where $Y_{ioect}$ is the outcome of worker $i$, who entered occupation $o$ in year $e$ and with displacement baseyear $c$. The main outcome of interest is labor earnings per year, but I will also consider days employed per year and the probability of re-employment and switching out of the baseyear occupation. $Disp_i$ marks displaced workers and $Post_t$ marks the post-displacement periods $t > -1$.

$E_k$ is an indicator for workers in the low ($k = 1$) and high ($k = 2$) exposure groups. The parameters of interest are the $\beta_{1k}$ coefficients of the three-way interactions. They identify the additional penalty on the effect of job loss for workers in exposure group $E_k$ as compared to the omitted zero exposure group $E_0$. The coefficients of the two-way interactions and level terms account for differences in the outcomes levels and time-trends between the exposure groups and displaced and non-displaced workers.

$X_{ic}$ is a set of baseyear control variables to account for observable differences between the groups, including person characteristics like education, experience, job and occupation tenure, as well as two lags of individual wages, the baseyear occupations' aggregate long-term employment growth rate as well as industry and establishment size fixed effects (see table B.1.1 in Appendix B.1.1 for a list and description of all variables). As a robustness check, I also add AKM

**Table 2.1.:** Baseyear Characteristics of Displaced Workers By Exposure Group

| | (1)<br>Zero Exposure ($E_0$) | (2)<br>Low Exposure ($E_1$) | (3)<br>High Exposure ($E_2$) |
|---|---|---|---|
| Person: | | | |
| Female | .607 | .368 | .281 |
| Age | 39.262 | 41.781 | 43.678 |
| German | .928 | .908 | .896 |
| No professional training | .107 | .144 | .193 |
| Vocational training | .852 | .812 | .741 |
| Academic degree | .04 | .045 | .066 |
| Experience | 10.084 | 12.197 | 15.145 |
| Job tenure | 5.258 | 7.318 | 9.368 |
| No of benefit receipts | 1.321 | 1.279 | 1.174 |
| No of n-spells | 1.279 | .993 | .923 |
| AKM person FE | 4.181 | 4.284 | 4.347 |
| Occupation: | | | |
| Within-distance since entry | 0 | .001 | .013 |
| Occupation tenure | 7.226 | 10.462 | 13.816 |
| Agriculture | . | . | . |
| Mining | . | . | . |
| Manufacturing | .112 | .44 | .649 |
| Mid/High Wage Services (>=p25) | / | / | / |
| Low Wage Services (<p25) | .885 | .512 | .244 |
| wGR baseyear occupation (1980-2010) | .006 | .002 | -.002 |
| Industry: | | | |
| Agriculture/Fishing/Mining | / | / | / |
| Manufacturing/Energy/Construction | .292 | .536 | .664 |
| Trade/Hospitality/Traffic/Communication | .551 | .321 | .21 |
| Credit/Real estate/Public Sector | .092 | .089 | .083 |
| Education/Health/Other services | .06 | .046 | .036 |
| Establishment: | | | |
| Establishment size | 43.567 | 53.511 | 64.064 |
| <10 | .284 | .233 | .19 |
| 10-50 | .486 | .467 | .445 |
| 51-100 | .122 | .147 | .169 |
| 101-250 | .079 | .116 | .146 |
| 251-500 | .029 | .037 | .051 |
| >500 | . | . | . |
| Median Daily Wage | 62.781 | 65.647 | 72.609 |
| AKM establishment FE | .064 | .114 | .145 |
| Outcomes: | | | |
| Labor earnings per Year | 28417.624 | 34124.762 | 37530.723 |
| Employed | 1 | 1 | 1 |
| Days employed per year | 362.015 | 360.193 | 360.906 |
| Switch occupation | 0 | 0 | 0 |
| Log real daily wage | 4.235 | 4.436 | 4.532 |
| min(N) | 1618 | 3702 | 2801 |
| max(N) | 2868 | 7033 | 4192 |

*Notes:* Columns (1) to (4) show the mean baseyear characteristics of displaced workers in the low ($E_1$, first quartile), medium ($E_1$, second and third quartile) and high ($E_4$, fourth quartile) exposure group. The sample size varies because of missing values in some characteristics. The AKM fixed effects are only available for about half of the sample. '.' marks cells that are empty by restriction, '/' mark cells that contain less than 20 observations and must be censored in accordance with data protection regulations of the IAB.
*Data:* SIAB, OPTE.

worker and establishment fixed effect.[14] $\gamma(s, b, o, c)$ is a set of fixed effects for baseyear industry $s$, establishment size class $b$, occupation $o$ and the calendar baseyear $c$. $\alpha$ is the intercept and $\epsilon_{ioect}$ is the idiosyncratic error term.

**Additional Specifications** I also estimate a fully interacted event study model to explore how the task change penalty evolves over time. For this purpose, I replace the $Post$ indicator in equation (2.2) by a set of time-period $t$ dummies, excluding $t = -1$ as the reference period.[15] To study effect heterogeneity across groups, I fully interact the model in (2.2) with an indicator for different subgroups of workers, e.g., women, older persons or occupation switchers. All models are estimated with OLS.

**Differential Timing** A stream of recent papers has shown that OLS estimates of Difference-in-Differences models can be biased if the treatment timing varies across units (see Roth et al., 2022 for a review). Goodman-Bacon (2021) shows that this bias arises, because the OLS estimator involves comparisons of earlier and later treated units that may even flip the sign of the estimated average treatment effect. To avoid these 'sinful' comparisons, I transform the data into a balanced panel where time is defined relative to the displacement baseyear. This 'stacked' regression approach assures that the OLS estimator is a weighted average of the baseyear-specific average treatment effects with strictly positive weights (Gardner, 2022).[16]

### 2.4.2. Discussion of Assumptions

**No Anticipation and No Spillovers** Workers should not anticipate the plant closing and adjust their behavior in a way that affects their outcomes in advance. Moreover, non-displaced workers' outcomes should not be affected in any way, e.g., by spillovers or market adjustments.

Following a common approach in the displacement literature, I only consider job loss during plant closures and focus on stable job matches. When all workers are discharged simultaneously, then job loss is likely unrelated to relative individual productivity. I exclude workers who leave the plant within two years before closure, because early leavers may be positively selected and bias the estimated costs of job loss downwards. This and the other stable match restrictions imply that workers are reasonably attached to their jobs and would not have quit in absence of plant closure. From the individuals' point of view, displacement can thus be considered an unexpected

[14] I do not include the AKM effects in the main specification, because this reduces the sample size by about half while not changing the results much.

[15] Note that the annual earnings and days employed in the baseyear ($t = 0$) may already be affected by the plant closure, because some plants close down between June 30 and December 31 of the baseyear.

[16] Other estimators for the differential timing setting are available (e.g., Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). But they typically require a fully balanced panel, where all units are observed for the same time interval during which treatment occurs in different periods. In my setting, I would have to observe all workers over the entire observation period between 1975 and 2010. Clearly, many workers retire or drop-out of the labor force over a period this long. So, to apply these estimators, I would have to impose strong restrictions on the data. The stacking approach taken here also requires setting a time window around the baseyear, but – in my view – this is more transparent in my application. Moreover, this approach has been commonly used in the previous displacement literature.

and exogenous shock. Consistently, there are no signs of anticipation before the baseyear for annual earnings, the employment probability, days worked per year or occupational mobility (see panel a in Figures B.6 to B.9 in Appendix B.1.2).[17]

The focus on complete plant closures also precludes any spillovers on workers remaining in the establishment. The relatively small average size of closing plants (see Table B.1.2) suggests that local spillovers or general equilibrium effects are not a concern (see e.g., Gathmann et al., 2020).

**Bias Stability** The Triple-Differences framework requires that the deviation from parallel trends between the exposure groups is the same for displaced and non-displaced workers. One way to assess this assumption is to compare the event studies of the low and high exposure group versus the zero exposure group in the displaced and non-displaced worker sample.

Figure 2.3 provides the unconditional event study plot for the annual labor earnings of the low exposure group in the left panel. The right panel shows the same plot for the high exposure group.[18] The reference time period is $t = -1$. The solid line represents displaced workers, the dashed line non-displaced workers. Both for the low and high exposure group, the pre-trends deviate from the zero line, which represents the pre-trend of the zero exposure group. This implies a violation of parallel trends between the low and high exposure group relative to the zero exposure comparison group. However, for the pre-displacement period the deviation is almost identical for displaced and non-displaced , which delivers some confidence that the DDD estimator would also cancel out the non-parallel trends in the post-displacement years.

Nevertheless, there are some noteworthy differences between displaced and non-displaced workers. Table B.1.2 in Appendix B.1.1 shows that non-displaced workers have lower labor market experience and lower job and occupation tenure and are less likely to work in manufacturing occupations or industries. Importantly, they are employed in larger plants and obtain higher average wages and earnings.[19] Firm size is an important predictor of technology adoption, firm survival (Acemoglu et al., 2020; Koch et al., 2021) and on-the-job training (Oi and Idson, 1999). Workers in 'non-displacing' plants may therefore develop new skills more quickly and have better outside options in case of job loss. Such a bias would result in an overestimation of the effect of task change exposure on worker outcomes.

I will therefore control for baseyear occupation tenure and person characteristics as well as two lags of individual wages and other observed differences. Moreover, I successively add establishment size, industry, occupation and baseyear fixed effects to account for unobserved confounders that are constant within subgroups.

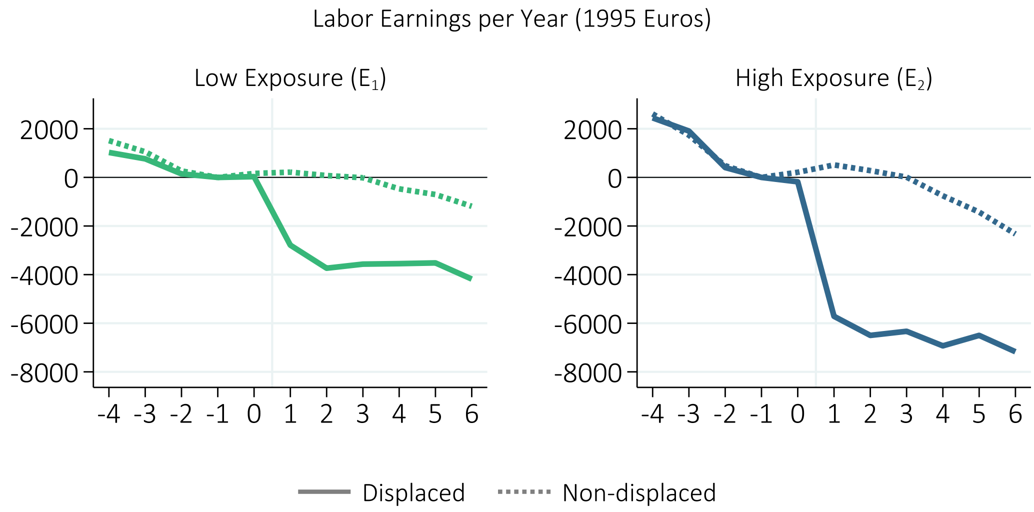
I do not observe individual skills or plant-level technology. Instead, I include AKM worker and plant fixed effects in a robustness check. These fixed effects serve as proxies for unobserved wage

[17] Panel b in in Figures B.6 to B.9 in in Appendix B.1.2) show the mean outcomes of non-displaced workers. The gradual reduction in non-displaced workers' earnings after the baseyear is a consequence of lifting the stable match restriction, which allows workers in the sample to leave employment or switch occupations.

[18] The corresponding plots for the employment probability, days worked per year and occupational mobility are given in Figures B.10 to B.12 in Appendix B.1.2.

[19] These differences between displaced and non-displaced workers also hold within exposure groups (see Table B.1.1 in Appendix B.1.1).

**Figure 2.3.:** Deviation from Parallel Trends in Earnings between Exposure Groups for Displaced and Non-Displaced Workers




*Notes:* The figure plots the unconditional event studies for the earnings of lowly and highly exposed workers in comparison to the zero exposure group, separately for displaced and non-displaced workers. Time trends are relative to the reference period $t = -1$. The plots support the validity of the Bias Stability assumption for the pre-displacement period, i.e., that the non-parallel trends bias for the low/high exposure group is almost identical in the displaced and non-displaced worker sample.
*Data:* SIAB, OPTE.

components like worker ability, plant productivity or rent sharing. In a further robustness check, I explicitly match displaced and non-displaced workers to eliminate all imbalances in observables ex-ante and re-estimate the results.

**No Selection into Exposure** The previous two assumptions identify the causal effect of a low or high exposure to task change as compared to the zero exposure group. However, the difference between these estimates only reveals the true effect of a greater exposure under an additional assumption: Individuals should not select into exposure groups based on expected 'gains' (Callaway et al., 2021).

In my application, workers should not strategically enter or leave occupations based on expected changes in tasks *and* the resulting additional costs in case of an unexpected layoff. To warrant this assumption, I only include workers who have stayed in their occupation for at least two years before job loss. This excludes workers who recently entered or left the occupation in response to task changes or to obviate displacement. In my sample, the average occupation tenure of displaced workers with a non-zero exposure ranges between 10.4 and 13.8 years (see Table B.1.1 in Appendix B.1.1). Arguably, for many occupations it is difficult to predict how tasks will change over more than a decade. Moreover, the risk and timing of job loss are uncertain. It therefore seems unlikely that workers mainly chose their baseyear occupation, because they anticipated how future task changes would alter the costs of job loss.

Nevertheless, I do control for observed differences between the exposure groups that may be related to outside options and unobserved determinants of occupational mobility. This includes baseyear characteristics, as well as pre-displacement wages and industry, establishment size class, occupation and baseyear fixed effects. In a robustness check, I add AKM person and establishment fixed effects to account for unobserved wage components that may correlate with occupation choice.

## 2.5. Results

### 2.5.1. Descriptive Results

Columns 1 and 2 of Table 2.2 provide the mean values of the outcomes of displaced workers in different exposure groups before and after job loss. Column 3 reports the before/after change of each exposure group. Columns 4 and 5 contrast the change in outcomes of the low and high exposure group to the change of the zero exposure group.

**Table 2.2.:** Mean Outcomes of Displaced Workers Before and After Job Loss by Exposure to Pre-Displacement Task Change

| | | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|---|
| | | Mean | | Change | Diff to Zero Exposure | |
| | Exposure | Pre | Post | Absolute | Absolute | In % |
| Labor Earnings per Year (1995 Euros) | Zero | 26,957.43 | 22,173.78 | -4,783.65 | . | . |
| | Low | 33,568.40 | 25,361.53 | -8,206.87 | -3,423.23 | -72% |
| | High | 38,319.63 | 26,630.38 | -11,689.26 | -6,905.61 | -144% |
| Employed | Zero | 0.97 | 0.75 | -0.23 | . | . |
| | Low | 0.98 | 0.72 | -0.26 | -0.03 | -14% |
| | High | 0.99 | 0.70 | -0.29 | -0.06 | -27% |
| Days Employed per Year | Zero | 349.46 | 263.50 | -85.96 | . | . |
| | Low | 354.41 | 254.98 | -99.43 | -13.46 | -16% |
| | High | 358.54 | 247.98 | -110.56 | -24.60 | -29% |
| Switch Occupation (rel. to baseyear) | Zero | 0.10 | 0.30 | +0.20 | . | . |
| | Low | 0.05 | 0.33 | +0.28 | +0.09 | +44% |
| | High | 0.02 | 0.37 | +0.35 | +0.16 | +82% |

*Notes:* The table shows the mean outcomes of displaced workers in different exposure groups over the pre- and post-displacement period (columns 1 and 2). The exposure groups represent different intensities of task changes between individual occupation entry and displacement (see 2.4). Column 3 reports the change in mean outcomes after job loss for each exposure group. Columns 4 and 5 contrast the change in outcomes of the low/high exposure group with the change of the zero exposure group.
*Data:* SIAB, OPTE.

More exposed workers generally have higher pre-displacement earnings, more stable employment relationships and they switch occupations less often. As mentioned earlier, this is because more exposed workers tend to be more advanced in their careers and therefore better matched. Column 3 shows that after job loss, a higher exposure to task change is associated with substantially larger earnings and employment losses and a greater likelihood to switch occupations. As compared to the zero exposure group, highly exposed workers experience 144% higher earnings losses and an additional penalty of almost 30% on the employment probability and days worked per year. Conditional on re-employment, they are 82% more likely to switch occupations than the zero exposure group.

However, given the association between career progress and task change these values cannot be interpreted as a pure consequence of exposure itself. The next sections will therefore discuss the results of the Triple-Differences estimator, which purges the values in column 4 of Table 2.2 of the non-parallel trends bias.

### 2.5.2. The Task Change Penalty on Post-Displacement Earnings

**Triple-Differences Event Study** Figure 2.4 plots the Triple-Differences equivalent of an event study plot for annual earnings.[20] The estimates originate from a model that fully interacts equation (2.2) with a set of time dummies. The specification controls for baseyear characteristics as well as baseyear industry, establishment size class, occupation and calendar baseyear fixed effects.

First, the pre-trends are close to zero for both exposure groups, which is in line with the above assessment of the unconditional 'Double-Difference' event studies in Figure B.6. For the low exposure group there is a slight deviation from zero, but it is insignificant and small. After displacement, the earnings losses strongly increase with prior exposure to task change. Even though there is some recovery, these additional losses are highly persistent.

**Average Post-Displacement Effect** Table 2.3 provides the average earnings penalty of the two exposure groups over the entire post-displacement period of six years. Low exposed workers experience about 2,100 Euros greater annual earnings losses than workers in the zero exposure group. Highly exposed workers have about 4,400 Euros higher earnings losses than zero exposure workers. Relative to the losses in the zero exposure group, this corresponds to an additional penalty of about 44% for lowly exposed workers and 91% for highly exposed workers.

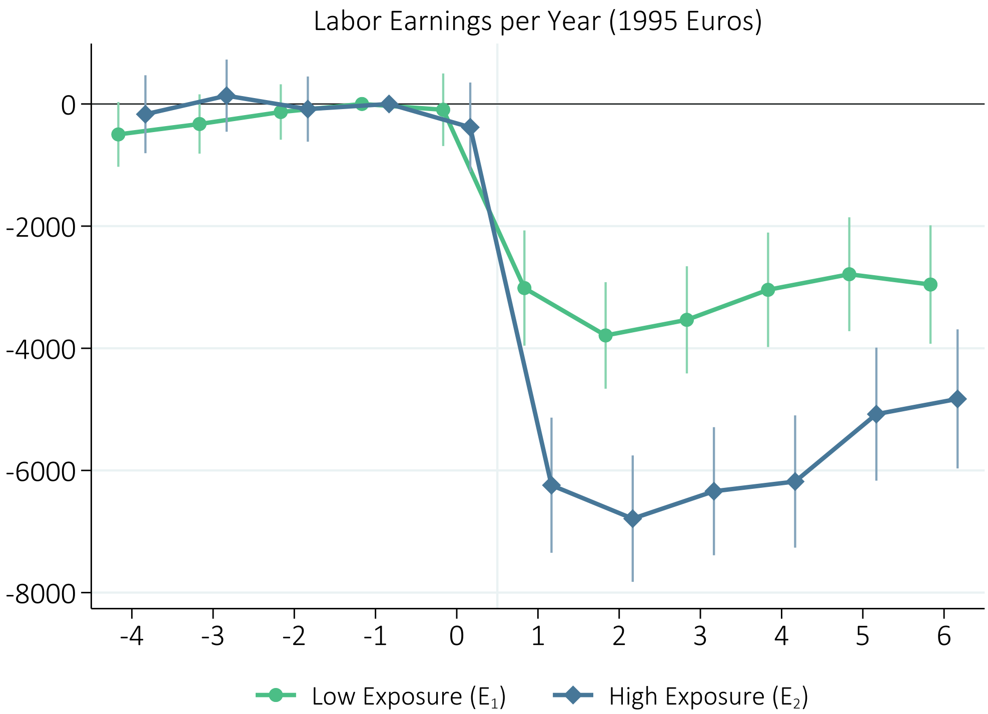
These estimates are considerably lower than the unconditional penalty in column 4 of Table 2.2, which highlights the importance of controlling for differences in pre-trends between the exposure groups. However, the earnings losses are still strongly increasing with exposure to task change and this gradient is robust to adding control variables or different sets of fixed effects. This implies that the effect of exposure is not explained by observed differences between displaced and non-displaced workers or exposure groups. Adding AKM fixed effects as proxies for unobserved wage determinants raises the estimates and reduces the gradient, but the earnings losses are still clearly increasing in exposure.

### 2.5.3. Re-Employment Prospects and Occupational Mobility

**Re-Employment Prospects** Table 2.4 provides the DDD estimates for the probability of being employed (column 1), days worked per year (column 2) and the annual labor earnings

[20] The corresponding plots for the employment probability, days employed per year and occupational mobility are provided in Figures B.13, B.14 and B.15 in Appendix B.1.2.

**Figure 2.4.:** Triple-Differences Event Study Estimates of Penalty for Exposure to Task Change on Labor Earnings per Year




*Notes:* The plot shows the estimates for labor earnings per year from a fully interacted version of the Triple-Differences specification in equation (2.2), where the *Post* indicator has been replaced by a set of indicators for each relative time period $t = -4, \ldots, +6$, with $t = -1$ as the omitted reference period. The specification controls for baseyear characteristics (see Table B.1.1 in Appendix B.1.1), occupation fixed effects and calendar baseyear fixed effects. The coefficients represent the average additional penalty over six post-displacement years for displaced workers in exposure groups $E_1$ (low) and $E_2$ (high) relative to the zero exposure group $E_0$. The vertical line illustrates that the plant closure occurs between $t = 0$ and $t = 1$.

*Data:* SIAB, OPTE.

**Table 2.3.:** Effect of Exposure to Task Change on Labor Earnings per Year (1995 Euros)

| **Labor Earnings per Year** | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Low Exposure ($E_1$) | -2124.03*** | -2083.125*** | -2083.125*** | -3007.264*** |
| | (321.753) | (323.811) | (323.817) | (478.33) |
| High Exposure ($E_2$) | -4456.263*** | -4441.491*** | -4441.491*** | -4927.887*** |
| | (376.843) | (379.204) | (379.212) | (524.156) |
| Baseyear Control Variables | | ✓ | ✓ | ✓ |
| Industry FE | | ✓ | ✓ | ✓ |
| Estab. Size Category FE | | ✓ | ✓ | ✓ |
| Occupation Tenur (+sq) | | ✓ | ✓ | ✓ |
| Occupation FE | | | ✓ | ✓ |
| Baseyear FE | | | ✓ | ✓ |
| AKM Estab. & Person FE | | | | ✓ |
| N | 5151036 | 5068316 | 5068316 | 2404699 |
| Adj. $R^2$ | .03 | .44 | .46 | .47 |

*Notes:* The table provides the Triple-Differences coefficient estimates for the three-way interactions of the exposure groups in equation (2.2). The columns show the estimates from specifications with a growing set of baseyear control variables and fixed effects (see Table B.1.1 in Appendix B.1.1 for a list and description). ***/**/* mark statistical significance at the 1/5/10% level.
*Data:* SIAB, OPTE.

**Table 2.4.:** Effect of Exposure to Task Change on Employment and Contribution to Losses in Labor Earnings per Year (1995 Euros)

| | (1) **Employment Prob.** | (2) **Days Employed per Year** | (3) **Labor Earnings per Year** |
|---|---|---|---|
| | All | All | Re-employed |
| Low Exposure ($E_1$) | -.028*** | -12.843*** | -657.260*** |
| | (.007) | (2.497) | (252.517) |
| High Exposure ($E_2$) | -.070*** | -27.117*** | -1,859.134*** |
| | (.007) | (2.756) | (291.887) |
| Baseyear Control Variables | ✓ | ✓ | ✓ |
| Industry FE | ✓ | ✓ | ✓ |
| Estab. Size Category FE | ✓ | ✓ | ✓ |
| Years since Occ. Entry (+sq) | ✓ | ✓ | ✓ |
| Occupation FE | ✓ | ✓ | ✓ |
| Baseyear FE | ✓ | ✓ | ✓ |
| N | 5068316 | 5068316 | 4355059 |
| Adj. $R^2$ | 0.09 | 0.11 | 0.61 |

*Notes:* The table provides the Triple-Differences coefficient estimates for the three-way interactions of the exposure groups in equation (2.2). Columns (1) show the estimates show the estimates for the probability of being employed on June 30 of a given panel year, column (2) shows the results for the number of days employed per year. Column (3) shows the estimates for annual labor earnings conditional on being employed. All specifications controls for baseyear characteristics (see Table B.1.1 in Appendix B.1.1 for a list and description) and industry, occupation and calendar baseyear fixed effects. ***/**/* mark statistical significance at the 1/5/10% level.
*Data:* SIAB, OPTE.

of re-employed individuals (column 3). Each model controls for baseyear characteristics and industry, establishment size class, occupation and calendar baseyear fixed effects.

The re-employment probability is decreasing with exposure to pre-displacement task change. After job displacement, low exposure workers are about 3 percentage points less likely employed and work 13 days less per year than zero exposure workers. This is equivalent to an additional penalty of 12% on the employment probability and 15% on days worked as compared to the average losses of the zero exposure group. For highly exposed workers, the penalty decreases to -7 percentage points on the employment probability and -27 days employed per year, which corresponds to a relative penalty of 30% and 32% on top of the average reduction in the zero exposure group.

As column 3 shows, the earnings losses are substantially lower when conditioning on re-employment. About half of the overall earnings penalty of more exposed workers in Table 2.3 is explained by a lower re-employment probability. This is consistent with the idea that changes in occupational tasks depreciate worker skills, resulting in worse outside options and lower re-employment prospects after job loss.

**Occupational Mobility** Table 2.5 shows the estimated effect of task change on the probability of switching to a different occupation after job loss and the associated earnings penalties. Columns 2 and 3 are obtained from fully interacting equation (2.2) with an indicator for occupation switching. This decomposes the earnings penalty of all re-employed workers (column 3 of Table 2.4) into a separate effect for occupation stayers and switchers. Each model controls for baseyear characteristics, industry fixed effects, establishment size class, occupation and calendar baseyear fixed effects. Note that occupations are only observed for employed workers, such that the estimates are subject to self-selection into re-employment and should be interpreted as descriptive rather than causal.

The probability of switching occupations increases substantially with exposure to task change (column 1). Low exposed workers are about 4.7 percentage points – or 24% – more likely to switch occupations after job loss than zero exposure workers. Highly exposed workers are about 9 percentage points more likely to switch occupations than the zero exposure group, which corresponds to a 46% higher switching probability.

Among both low and high exposure workers, occupation switchers experience a substantially higher earnings penalty than workers who return to the same occupation (compare columns 2 and 3 of Table 2.5). The estimates for switchers are very close to the average earnings penalty for all re-employed workers (see column 3 in Table 2.4). This implies that the overall earnings losses of re-employed workers are mainly driven by occupation switchers. At the same time, the point estimates of switchers also varies a lot. The top panel of Figure 2.5 shows that for highly exposed occupation switchers, the 95% confidence interval for earnings ranges from below -4,000 up to about +750 Euros per year. This means, that some switchers experience no larger earnings reductions after job loss than workers in the zero exposure group.

Also the earnings reductions of occupation stayers increase with the previous exposure to task

**Table 2.5.:** Effect of Exposure to Task Change on Occupational Mobility and Contribution to Earnings Losses (1995 Euros)

| | (1) **Switch Occupation** | (2) **Labor Earnings per Year** | (3) |
|---|---|---|---|
| | Re-employed | Occupation Stayers | Occupation Switchers |
| Low Exposure ($E_1$) | .047*** | -437.501* | -981.805 |
| | (.009) | (251.597) | (962.631) |
| High Exposure ($E_2$) | .092*** | -840.209*** | -1,746.741 |
| | (.009) | (292.807) | (1,273.357) |
| Baseyear Control Variables | ✓ | ✓ | ✓ |
| Industry FE | ✓ | ✓ | ✓ |
| Estab. Size Category FE | ✓ | ✓ | ✓ |
| Occupation Tenure (+sq) | ✓ | ✓ | ✓ |
| Occupation FE | ✓ | ✓ | ✓ |
| Baseyear FE | ✓ | ✓ | ✓ |
| N | 4305163 | 4305163 | |
| Adj. $R^2$ | .09 | 0.61 | |

The table provides the Triple-Differences coefficient estimates for the three-way interactions of the exposure groups in equation (2.2). Columns (1) shows the estimates for the probability switching occupations conditional on re-employment. Columns (2) and (3) shows the estimates for annual labor earnings conditional on switching occupations or returning to the same occupation. All specifications controls for baseyear characteristics (see Table B.1.1 in Appendix B.1.1 for a list and description) and industry, occupation and calendar baseyear fixed effects. ***/**/* mark statistical significance at the 1/5/10% level.
*Data:* SIAB, OPTE.

change, but for them the penalty is much lower than for switchers (see column 2 of Table 2.5). Highly exposed occupation stayers yield a penalty of 840 Euros pear year as compared to zero exposure workers. For occupation switchers, the penalty increases to -1,700 Euros. This indicates that workers who manage to return to the same occupation have at least partially updated their skills to the new requirements. Hence, they can transfer more specific human capital and suffer lower earnings losses.

Among highly exposed workers, both occupation switchers and stayers have slightly less days employed than the zero exposure group (see top panel of Figure 2.5). However, with -3 and -7 days per year on average over a six year period the penalty is small in absolute terms.

### 2.5.4. Effect Heterogeneity

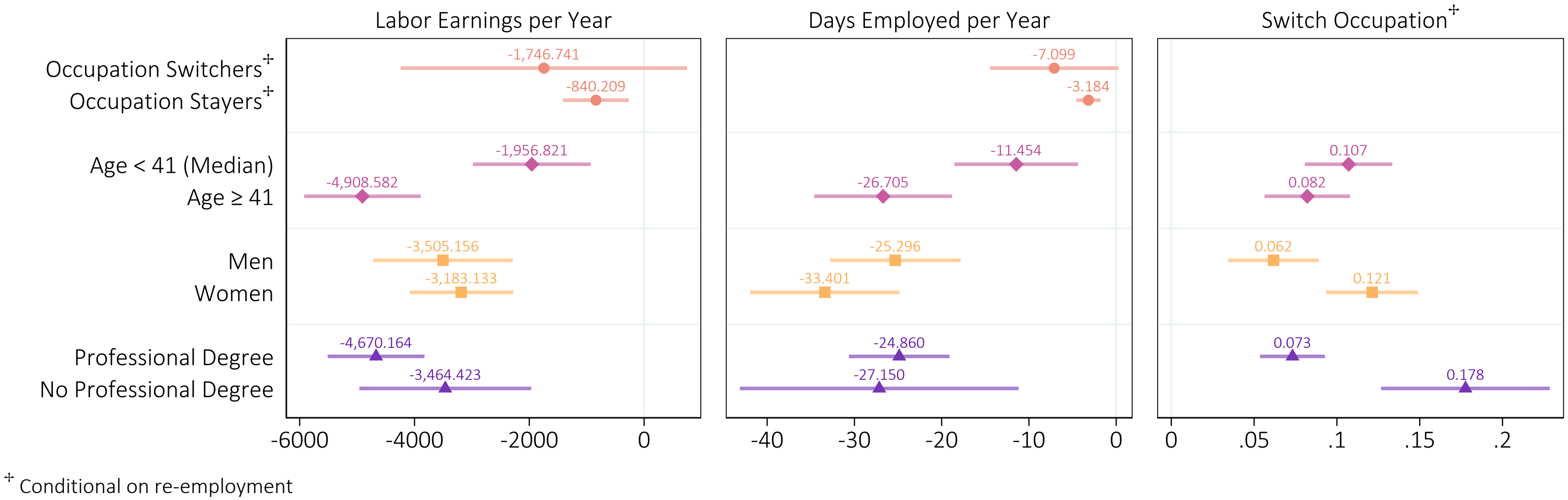
Figure 2.5 provides the estimates of the four-way interaction model that further decomposes the exposure penalty into different subgroups. For ease of display, the plots only show the decomposition for the high exposure group's penalty.[21] Again, the model controls for baseyear characteristics, industry, establishment size class, occupation and baseyear fixed effects.

**Age** Highly exposed workers above the median age of 41 experience much larger earnings losses than younger workers. In fact, their losses almost coincide with the average penalty of the entire high exposure group (see Table 2.3). The same holds for the effect on the employment probability and days employed (see Table 2.4). In contrast, conditional on re-employment older workers are less likely to switch occupations. These results suggest that changes in occupational tasks devalue the skills of older workers more, such that they qualify less for a new job in their previous occupations. At the same time, it is more costly for older individuals to start all over again in a new occupation, such that more workers remain unemployed.

**Gender** Recent studies have shown that women often suffer substantially larger earnings losses after job displacement than men, because mothers with young children accept part-time jobs with lower wages more often (Frodermann and Müller, 2019; Illing et al., 2021). I find that for women a high exposure to task change has a greater effect on the probability to stay unemployed and to switch occupations. The resulting earnings penalty, however, is only slightly more negative than for men. This suggest that for a given exposure to task change, women make more beneficial matches when switching occupations. It would be an interesting avenue for further research to explore how changes in tasks affect the self-selection of women who return to work and the role of motherhood.

[21] Note that the decomposition results for the high exposure group do not necessarily carry over to the low exposure group. For the sake of brevity, I spared the heterogeneity results for the employment probability, as they are very similar to days employed. They can be found in Figure B.16 in Appendix B.1.2.

**Figure 2.5.:** Heterogeneity in the Earnings Penalty of the High Exposure Group




*Notes:* The Figure shows a decomposition of the Triple-Differences effect on labor earnings per year for the high exposure group ($E_2$) into separate estimates for different sub groups of workers. The three panels in the columns represent different outcomes, the rows represent the different subgroups. These estimates are derived from a four-way-interaction model, i.e., equation (2.2) is fully interacted with indicator variables for the sub groups. All models control for baseyear characteristics (see Table B.1.1 in Appendix B.1.1), occupation fixed effects and calendar baseyear fixed effects.

*Data:* SIAB, OPTE.

**Professional Degree** The high-exposure earnings penalty is lower for workers without a professional degree, but it also varies more as the confidence interval reveals. The same applies to the effect on days employed. Conditional on re-employment, workers without a degree are much more likely to switch occupations. One explanation could be that individuals without a degree tend to work in occupations that require more general skills and pay lower average wages. As a consequence, these workers may find it easier to transfer their skills to a different occupation that pays a similar wage.

### 2.5.5. Robustness Checks

**Matched Sample** As I have discussed above, displaced and non-displaced workers differ in some characteristics that may be related to the relative earnings trajectories of the exposure groups. If this was related to different non-parallel trends biases in the displaced versus the non-displaced sample, then the Bias Stability assumption of the Triple-Differences design could be violated.

Even though Bias Stability seems to be satisfied without any control variables (see Figure 2.3), especially the difference in occupation tenure and establishment size may be a matter of concern. I therefore control for these and other observed differences in baseyear characteristics in most specifications. Such a control variables approach requires sufficient overlap between the groups – otherwise, explicitly matching each displaced worker with a similar non-displaced control may be more effective. For the two most apparent confounders, occupation tenure and establishment size, the boxplots in Figures B.4 and B.5 suggest that there is sufficient common support for a control variables approach.

Nevertheless, I also construct a matched sample as a robustness check. I first use the set of control variables to predict the individual propensity of displacement for each worker in the sample. Then I exactly match displaced and non-displaced workers with the same baseyear, exposure group and establishment size class. Within these cells, I pick each displaced worker's nearest neighbor in terms of the propensity score as the control unit. The resulting matched sample is clean of any significant imbalances (see Table B.1.4 in Appendix B.1.1).

I use the matched sample to re-estimate the triple-differences model in equation (2.2). Again, I add control variables to account for differences between the exposure groups. Table B.1.5 reports the results of this exercise. Reassuringly, they are very similar to the ones obtained from the unmatched sample.

**Task Change over a Fixed Ten Year Time Window** For my main analyses, I measure within-occupation task changes between each worker's individual year of occupation entry and job loss (or the baseyear for non-displaced workers). This creates an additional source of identifying variation: Even for workers with the same occupation who are displaced in the same year, the exposure to task change differs by entry years. However, this approach also contributes to the systematic relationship between exposure and occupation tenure, because entering an occupation earlier and staying longer mechanically increases the chance of experiencing task changes. But then, changes that happened many years ago may not really affect outcomes today. Moreover, individuals who endure task changes over many years might be selected in terms of unobserved ability or other relevant characteristics. In this case, the estimated exposure penalty could be confounded by unobservables that are jointly related to task change, occupation tenure and earnings.

To address this concern, I fix the time window for changes in tasks to ten years before the baseyear and computing the within distance according to equation (2.1), i.e. $\mathcal{D}(o,c-10,c)$. I then define exposure groups in the same way as before: the low quartile is classified as the zero exposure group, the second and third quartile are combined into the low exposure group and the fourth quartile is the high exposure group. The groups are then used to re-estimate the model in (2.2).

Table B.1.6 in Appendix B.1.1 shows the results. For earnings, the results are qualitatively similar to the main analyses: a greater exposure leads to higher earnings losses as compared to the zero exposure group, but the penalty is lower in absolute terms. Also the probability to switch occupations is still increasing with exposure, even to a slightly higher degree than in the main analyses. Only for employment, the exposure gradient basically vanishes; the low exposure group now has a significantly higher employment probability (+1.5 percentage points) and more days worked (about +4 days per year) than the zero exposure group – but the effects are very small in absolute terms. For the high exposure group, the outcomes do not differ to the zero group.

Note that the entry-year specific distance measure of the main-analyses and the fixed ten-year distance measure are not perfectly correlated ($\rho = 0.63$) such that the assignment of displaced workers to exposure groups somewhat differs. Therefore, the results may not be perfectly comparable to the main analyses.

## 2.6. Discussion and Conclusion

This paper shows that changes in occupational tasks before job loss are an important source of post-displacement earnings losses. To establish this result, I focus on layoffs during plant closures, where job loss can be considered independent of individual skills or productivity. I split the sample of displaced workers into a group with a low and a high exposure to pre-displacement task shifts. I then compare their post-displacement outcomes to a zero exposure group for which tasks remained largely constant. These groups do not only differ in terms of exposure to task change, but also in characteristics that may determine earnings and employment prospects. The eliminate the resulting bias, I use non-displaced workers with similar task changes as an additional control group in a Triple-Differences design.

On average, displaced workers with a high exposure to task change experience about 90% higher annual earnings losses than workers with a zero exposure. In absolute terms, the average annual earnings earnings of highly exposed workers fall by an additional -4,400 Euros as compared to workers with zero task change. This task change penalty on the costs of job losses persists over six years and about half of it is explained by a lower re-employment probability. If highly exposed workers return to employment, they are almost 50% more likely to switch occupations than workers whose previous occupation remained unchanged. Such switches often involve substantial reductions in earnings. However, the earnings of highly exposed switchers vary strongly. Some switchers do not incur greater earnings losses or even obtain lower earnings losses than the average worker in the zero exposure group.

My result suggest that not all workers are equally quick to adjust their skill set when the task content of their occupation shifts. Especially older workers suffer large reductions in the re-employment probability and earnings when being displaced during a period of task restructuring. Job loss seems to interrupt on-the-job adjustment, e.g., via learning-by-doing or job training, leading to worse outside options, more involuntary occupation switches and higher earnings losses. However, there are also workers who switch occupations without additional earnings loss. For these workers, job displacement may provide an unexpected opportunity to find a better match after having successfully acquired new skills.

I contribute to a growing literature that links the costs of job loss to technological change and task restructuring. Most of these papers fix occupational tasks in a baseyear and then compare the outcomes of workers in initially more or less routine intensive occupations. However, recent theoretical and empirical advances highlight the importance of task restructuring *within* occupations (see e.g., Acemoglu and Restrepo, 2018; Atalay et al., 2020). This paper is the first to provide causal evidence about how such within-changes affect the costs of job loss. An interesting avenue for future research would be to study self-selection into 'involuntary' and 'voluntary' occupational mobility in response to task changes and the role of specific tasks in more detail. Another natural follow-up questions regards the role of firms, training and knowledge spillovers between colleagues for individual adjustment to changes in workplace tasks.

Overall, my results highlight the importance of continuous training and 'lifelong learning'. Digitization will profoundly change the skill requirements of many jobs, while population ageing increases the adjustment costs. Targeted policy interventions like training subsidies could foster the skill acquisition of groups with higher learning costs and lower returns. This could be especially relevant to avoid early labor market exits after sudden career breaks, e.g., in case of job loss or domestic caring obligations. Such policy interventions could be welfare-improving, if the joint risk of future task changes and career interruptions is hard to predict when workers make long-lasting occupation choices early in their career (Cunha and Heckman, 2016).

# 3. Do Job Creation Schemes Improve Social Integration and Well-Being of the Long-Term Unemployed?

*With Friedhelm Pfeiffer and Laura Pohlan*[1]

## 3.1. Introduction

Permanent unemployment is a serious risk factor for social exclusion and reduced well-being in many Western societies (see e.g. Clark and Oswald, 1994; Paul and Moser, 2009; Pohlan, 2019). Beyond the economic strain it puts on individuals, unemployment may incur substantial psychological and social costs that can even persist across generations (see e.g. Clark and Lepinteur, 2019). Compared to working individuals, especially the long-term unemployed[2] often suffer from a greater risk of depression, suicide, alcohol abuse and stigmatization that stresses self-esteem and personal relationships (see e.g. Frey and Stutzer, 2002). Even a generous social welfare system may not suffice to compensate for the negative consequences of job loss, because employment in itself has essential psychosocial functions including social purpose, status and identity (Jahoda, 1981). This raises the question, whether labor market policies can act to improve the quality of life of the long-term unemployed.

In this paper, we use a recent German job creation scheme (JCS) to analyze whether subsidized employment improves the social integration and well-being of unemployed individuals with severe employment impediments. In contrast to previous JCSs, the federal pilot project 'Social Integration within the Labor Market' (SILM, *Soziale Teilhabe am Arbeitsmarkt*) explicitly aimed at promoting social integration rather than re-employment prospects.

Instead of providing subsidized employment opportunities, policy makers could also foster re-integration into the labor market, for example by job search incentives or training programs. These may be effective for relatively healthy and skilled individuals, but may fail for the long-term unemployed. One reason is that employers often interpret long unemployment durations as a negative signal (see e.g. Kroft et al., 2013; Bhuller et al., 2017). Alternatively, the barriers to early retirement or disability pension could be lowered, as many long-term unemployed are of advanced

[1]This chapter is a slightly modified version of the published article in Ivanov et al. (2020). An earlier version has been circulated as a discussion paper (Ivanov et al., 2019).

[2]We use the terms 'long-term unemployed' and 'welfare recipients' interchangeably, because in our application all long-term unemployed individuals are also welfare recipients.

age or face health problems. Retirement can have a greater identity value than unemployment (Hetschko et al., 2014), but incentivizing work-capable individuals to leave the active labor force may come at great fiscal cost (see Autor and Duggan, 2006; Autor et al., 2016). Hence, the government may directly provide subsidized job opportunities to long-term unemployed individuals.

By now, a comprehensive program evaluation literature has established that JCSs, in general, have either insignificant or negative employment effects for participants (see e.g. the meta-analyses of Kluve, 2010 and Card et al., 2018 and the review for German employment subsidies by Wolff and Stephan, 2013). Primarily, this is due to so-called 'lock-in' effects which reflect reduced job search efforts during participation (see e.g. van Ours, 2004; Hujer and Thomsen, 2010; Huber et al., 2011b; Bergemann et al., 2017). However, there is also some evidence that especially long-term unemployed and hard-to-place individuals do benefit from participation (see e.g. Caliendo et al., 2008; Hohmeyer and Wolff, 2012). Lock-in effects might be less important for these individuals, because they are less likely to find a job in the counterfactual of no program participation. At the same time, they are also particularly exposed to the above mentioned psychosocial costs of unemployment (Paul and Moser, 2009).

Yet, few studies have explicitly investigated whether subsidized employment may improve measures of well-being and social integration; and the evidence is mixed. Crost (2016) shows that, between 1992 and 2004, German JCSs improved the life satisfaction of participants. In a similar vein, Knabe et al. (2017) use cross-sectional data to show that participation in a more recent German job creation program, 'One-Euro-Jobs', is associated with increased life satisfaction. Using fixed effects regressions, Wulfgramm (2011) provides evidence that One-Euro-Jobs can partially counteract the negative effects of unemployment on life satisfaction. Gundert and Hohendanner (2015) find that this program does not generally improve social belonging. Huber et al. (2011a) estimate slightly negative effects of welfare-to-work programs on mental health in Germany.[3]

We contribute to this emerging strand of the program evaluation literature in several ways. First, we focus on long-term unemployed individuals with employment impediments; a group that is expected to be most responsive to both the harms of unemployment and the potential benefits of subsidized employment. Although the target group of SILM amounts to about 670 thousand individuals in Germany,[4] sample sizes in large scale surveys like the SOEP are typically too small to conduct quantitative analyses. Therefore, we use a novel and unusually rich dataset, which links social security data with a panel survey of program participants and control individuals (see Brussig et al., 2019).

This dataset allows us to study the interplay of both employment outcomes and subjective

[3]There are some studies that analyze the effects of other ALMPs on subjective outcomes; see e.g. Andersen (2008) for a UK training program, Vuori and Silvonen (2005) for a job search program in the US, Fairlie et al. (2015) for an entrepreneurship training program in the US and Caliendo and Tübbicke (2019) for a German start-up subsidy program. Sage (2015) analyses ALMPs in the UK without differentiating between different types.

[4]The estimate of the number of potential participants was kindly provided by the Institute for Employment Research (IAB). According to this approximation, SILM covered about 3% of the target population.

measures of well-being and social integration. While the effect of program participation might be negative on employment, it may still improve well-being and social integration. It is possible, however, that the participants would have achieved similar subjective outcomes without participating in SILM, e.g. through finding non-subsidized employment. We therefore closely examine how much of the average effect on well-being and social integration can be attributed to changes in the employment share within the treatment and control group.

To grasp the multidimensional nature of individual well-being and social integration, we use four different subjective indicators and study precisely which of these aspects are most affected. Life satisfaction can be viewed as a proxy for individual utility or a summary measure for quality of life (Frey and Stutzer, 2002). Mental health problems might be a typical side effect of unemployment (Paul and Moser, 2009) and may be reduced by participation in SILM. Our measures of social integration – social belonging and social status – reflect the subjective perception of an individual's role and position in society. Subsidized employment might positively affect perceived status and identity compared to the state of unemployment (Stutzer and Lalive, 2004). However, knowing the program is only temporary and targets disadvantaged individuals could dampen this effect.

We apply nearest-neighbor propensity score matching to estimate the average treatment effect on the treated (ATT) for three different survey waves that roughly correspond to a period of 7, 18 and 29 months since individual program entry. Our findings suggest that participation significantly improves well-being and social integration, but to varying degrees for the four outcome measures. While life satisfaction strongly increases, social status improves only moderately. Furthermore, the ATTs substantially decrease over the course of the program. Using the administrative data, we link these results to the estimated employment effects. First, we document considerable lock-in effects during participation. We then explore how compositional changes in the treatment and control group influence the average effects on well-being and social integration: over the course of the program, an increasing share of control individuals enter non-subsidized employment, while a growing share of participants leave SILM. These shifts can completely explain the observed decrease in the average program effects on well-being and social integration. To estimate the short-term post-program effects, we are limited to individuals with an early planned end date. For them, the effects on both employment and subjective outcomes vanish upon leaving the program. Finally, heterogeneity analyses reveal that individuals with an above-average duration of welfare dependence and those with health impairments benefit more from participation.

The rest of the paper is structured as follows: Section 3.2 gives an overview of the program's implementation and its design. Section 3.3 describes the data sources. Section 3.4 explains how we link administrative and survey data and construct the final estimation sample. Section 3.5 describes the identification strategy and potential threats. Section 3.6 presents the results of the main analysis, along with robustness checks and a heterogeneity analysis. Section 3.7 concludes.

## 3.2. The Federal Program 'Social Integration within the Labor Market'

The federal job creation program 'Social Integration within the Labor Market' was launched as a pilot project to combat long-term unemployment and its consequences. It offered subsidized employment for up to 20,000 participants between the fourth quarter of 2015 and the end of 2018.

In order to participate in the program, job centers had to apply for funding from the Federal Ministry of Labor and Social Affairs. Out of the 408 German job centers, 265 filed an application. The ministry decided on these applications in two rounds based on criteria reflecting the local labor market situation and the job centers' implementation plans. In 2015, funding for 10,000 places was granted to 105 job centers. In 2017, 90 additional job centers were given access to the program. In this second round, 10,000 additional program places were allocated to both new and previously participating job centers. Participating job centers were concentrated in areas with weak economic conditions and a high share of welfare recipients. Within job centers, case workers matched individuals with employers. Individual participation was voluntary.[5]

The target group of SILM were individuals who were at least 35 years of age, had been welfare claimants for at least four years and either had health impairments, minor children or both.[6] Table 3.1 provides descriptive statistics for participants and characteristics of the program design, based on administrative data and the participant survey. Participants were, on average, 49 years old, and had been dependent on welfare for 7.5 years. Around half of them were recorded as having some kind of health issue as an impediment to regular employment. About one quarter of the participants lived in households with minor children. According to our data, at least one quarter had neither minor children nor a health impairment and thus would not fulfill the eligibility criteria. We expect the true share of program entrants with health problems to be higher, because unless individuals had an officially recognized severe disability, it was up to the responsible case worker's discretion whether or not a health impairment was formally recorded as a justification for participation.

A defining characteristic of SILM was its focus on improving social integration and well-being, which is reflected in the specific design. While earlier JCS, such as One-Euro-Jobs, mostly offered short-term and part-time employment, SILM provided up to 36 months of subsidized employment, resulting in an average planned duration of 27 months. Participants received a regular work contract including pension claims, holiday entitlements etc. The program fully subsidized the national minimum wage (8.50 EUR per hour in 2015 and 2016, 8.84 EUR from 2017 onwards) and the employer share of social security contributions.

The subsidy covered up to 30 working hours a week. To meet the needs of both participants and employers, SILM also allowed for different part-time agreements (15, 20, 25 hours a week)

[5] We will discuss potential selection issues related to this design in Section 3.5.

[6] The program aimed at welfare recipients under Book II of the Social Code (*Sozialgesetzbuch II*, SGB II), i.e. individuals who are considered capable of working and part of the labor force. Exceptions in the admission rules applied to individuals without a vocational degree and to former participants of the public employment program 'Citizen Work' (*Bürgerarbeit*).

**Table 3.1.:** Program Design and Participants' Assessment

| | |
|---|---|
| **Eligibility criteria** | |
| Age | 48.85 |
| Years of welfare receipt | 7.49 |
| Health impairment | 0.49 |
| Children in household | 0.27 |
| **Program characteristics** | |
| Planned program duration [months] | 27.20 |
| Welfare receipt | 0.66 |
| Program drop-out | 0.21 |
| *Employment-accompanying activities:*§ | |
| Personal counseling | 0.31 |
| Training/qualification measure | 0.30 |
| Support by case worker/coach | 0.30 |
| Activities with other participants | 0.14 |
| Healthy lifestyle counseling | 0.14 |
| Any employment-accompanying activity | 0.61 |
| **Job characteristics** | |
| Average working hours per week | 27.44 |
| Full-time employment [≥30h/week] | 0.67 |
| *Tasks:*§ | |
| Social work | 0.31 |
| Gardening/crafts/janitor | 0.30 |
| Administration/archive/library | 0.10 |
| Kitchen/food distribution | 0.08 |
| Cleaning/housekeeping | 0.06 |
| Sales/social department stores | 0.13 |
| Others | 0.18 |
| **Participant assessments** | |
| Job satisfaction [0-10] | 8.33 |
| Good relations with colleagues | 0.84 |
| Work is meaningful | 0.92 |
| Observations | 3,579 - 3,797 |

*Notes:* The table shows the means of selected characteristics for participants who entered the survey in the first wave (see Section 3.3 and 3.4). Eligibility criteria are based on administrative data. Information on program drop-out and participation in employment-accompanying activities refers to the whole program duration. For details, see Tables C.1.1 and C.1.2 in the Appendix. The number of observations differs due to missing values. § Multiple answers possible.
*Data:* SILM Evaluation Dataset, Brussig et al. (2019).

as well as a gradual increase in the number of hours worked. On average, participants worked about 27 hours per week and about 67% worked 30 hours or more. For a 30 hour contract, the maximum wage subsidy amounted to 1,320 euros per month or 15,840 euros per year. According to the German social security laws, most of the net earnings from working in the program were deducted from participants' welfare claims (see Article 11b(2) SGB II). As a result, the additional average disposable income from working in the program amounted to around 3,350 Euros per year (280 Euros per month).[7] Hence, the additional wage costs for the government, as compared to a state of pure welfare dependence, were limited. Despite working in SILM, 66% of participants still received supplementary welfare payments to ensure that household income covered the subsistence level. In total, 21% of the participants dropped out of the program earlier than initially planned.

The tasks carried out in the program were required to be of public interest, competition neutral and additional in nature, i.e. they should not crowd out existing jobs or tasks. Most of the program places were assigned to public employers or charity organizations. Thus, the majority of participants performed tasks in social work (31%) or gardening, crafts or janitorial work (30%). Other tasks included administrative, kitchen, cleaning and sales activities.

SILM also provided different accompanying activities that were meant to stabilize the employment relationship or to foster social integration beyond employment itself. Table 3.1 presents the fraction of individuals participating in such activities at least once. They included counseling on strengths and weaknesses or personal goals (31%), training and qualification measures like computer courses or acquisition of a forklift license (30%), support from case workers or coaches, e.g. if workplace conflicts occurred (30%), as well as recreational activities with other participants (14%) or healthy lifestyle counseling (14%). 61% of all participants took part in at least one such accompanying activity.

The participants themselves rated the program very positively. Their average job satisfaction amounted to 8.33 on a 0 to 10 scale. This may be due to the quality of social interactions at work, and a perception that the tasks carried out are appreciated by others. For example, 84% enjoyed good relations with their colleagues and superiors and 92% perceived their work as meaningful.

## 3.3. The SILM Evaluation Dataset

Our analysis is based on a unique dataset that combines administrative data with a telephone survey of the program's target group. The administrative data source, the *Integrated Employment Biographies* (IEB) of the German Federal Employment Agency, consist of employer notifications to the social security system. They are available for all individuals with at least one entry in their social security records after 1975 for West Germany and 1992 for East Germany and up to the end of our observation period in December 2018. The IEB contain detailed information on spells of dependent employment, registered unemployment, job-search and benefit or pension receipt with daily precision. Periods of self-employment, civil service or military service are not included.

[7] See Section 7 in Ivanov et al. (2019) for a detailed description of the calculation of this figure.

Our matching approach is based on sociodemographic characteristics (gender, age, indicator for health impairment, number of minor children, education level), as well as the job (tenure, wage, occupation) and firm characteristics (industry code, firm size) of the individuals' last job. Moreover, we use detailed information on different employment states including, for instance, employment with social security contributions, marginal employment, previous participation in ALMP measures and welfare receipt. We construct precise measures of the number of spells and durations of different employment states throughout the individual's employment history. Regional aggregates provided by the Statistical Office of the Federal Employment Agency are added as additional control variables reflecting the local labor market situation. We also use the IEB to estimate the lock-in effects over the course of the program.

The administrative data is supplemented by a longitudinal *telephone survey* of program participants and non-participants, that provides different measures of well-being and social integration.[8] Our outcome variables include two measures of subjective well-being: life satisfaction and mental health. Life satisfaction is based on a question which is standard in large-scale surveys like the SOEP or the European Value Study. Individuals were asked to assess their current satisfaction with life in general on a 0 to 10 scale, where 0 means "completely dissatisfied" and 10 "completely satisfied". To measure mental health, respondents assessed how often they had been struggling with problems like fear, dejection, irritability or insomnia in the past four weeks. Here, the scale ranges from 1 "all the time" to 5 "never".

Social integration is measured in two dimensions: the subjective perceptions of social belonging and social status. Social belonging refers to the feeling of being part of society; measured on a 1 to 10 scale where 1 means "I feel excluded" and 10 means "I feel included". To measure social status, individuals were asked to rank themselves on a scale from 1 "the bottom" of society to 10 "the very top" of society.[9]

We chose the outcome measures to match the variables in the PASS dataset, so the results are comparable to other studies.[10] In addition to the outcome variables, the survey data contain detailed information on the particular implementation of the program at the individual level (e.g. tasks performed, hours worked, participation in activities accompanying employment and subjective evaluations).

## 3.4. Sample Construction

The final estimation sample was constructed in five steps. In steps 1 to 3, participants and control individuals were identified and matched in the administrative data. In step 4, the resulting treatment and control groups entered the telephone survey, and in step 5 the final estimation

[8] The linked survey and administrative dataset is named 'SILM Evaluation Dataset'.

[9] Figure C.1 in the Appendix plots the distribution of the outcome variables and their means and standard deviations in the final sample.

[10] The PASS covers 21,000 individuals from low-income households in Germany (see Trappmann et al., 2010). The same measure of social belonging is used by Gundert and Hohendanner (2014; 2015). Pohlan (2019) analyzes the effects of unemployment on the same four outcomes.

sample was obtained. Table C.1.1 in the Appendix gives an overview of this process and shows how the observation numbers evolve over these five steps.

**Step 1: Identification of participants.** As a first step, three cohorts of participants were identified in the administrative data. Cohort 1 includes all participants who entered the program up to June 2016, cohort 2 entered the program during the period June-December 2016 and cohort 3 includes a random draw of 3,500 participants who entered the program in the period January-June 2017. This results in a sample of 12,412 individuals.

**Step 2: Identification of potential control individuals.** For each participant, 20 non-participants were pre-matched along the eligibility criteria of SILM (age, duration of benefit receipt, incidence of health impairments, minor children) and gender. Moreover, potential control individuals were required to be welfare recipients and not participating in SILM at the cut-off dates 12/11/2015 (cohort 1), 13/06/2016 (cohort 2) and 17/01/2017 (cohort 3), respectively.

**Step 3: Matching.** Then nearest-neighbor propensity score matching with replacement was applied to narrow this sample down to the four most comparable controls for each participant. The propensity of program participation was estimated by probit models using a large set of pre-treatment variables (see Table C.1.1 in the Appendix for a detailed description). These individuals form the gross sample for the first wave of the telephone survey.

**Step 4: Telephone survey.** For *wave 1* of the survey, all program participants were contacted as soon as possible to obtain a measurement of the outcomes at an early phase of program participation.[11] This leads to a sample of 3,821 participants, which corresponds to a response rate of 30.8%, which is similar to other surveys in comparable populations like the PASS (Berg et al., 2016). The mean program duration in wave 1 amounts to 7 months. If a participant responded to wave 1, the survey institute tried to reach at least one of the four nearest neighbors in the control group.[12] This results in a sample of 3,427 surveyed controls in wave 1. On average, control individuals were surveyed around 4 months after their matched participant.

Each successfully surveyed match entered the pool of the follow-up survey. *Wave 2* was obtained by a similar procedure: once a program participant from wave 1 responded to wave 2, his or her matched control was contacted again. The response rate of participants in wave 2 was 71% and the average elapsed time since program entry was 18 months. In the control group, the response rate was slightly lower (64%). Again, they were surveyed around 4 months after the participants.

All program participants who answered the survey in wave 2 were approached again for *wave 3*. For participants, who entered the program after the expansion in January 2017, only two waves were conducted. The response rate in wave 3 was 71% for participants and 69% for non-participants

[11] It should be noted that participants could only be identified in the administrative data once they had entered the program, such that it was not possible to conduct a pre-treatment survey of the outcome variables.

[12] If none of them responded, the scope was widened to the 10 nearest neighbors. This does not affect the quality of the matches since the estimated propensity scores of participants and their first 10 matches are very close.

who had responded in wave 2. At wave 3, the average time elapsed since the start of the program amounted to 29 months for both groups.

**Step 5: Final estimation sample of matched pairs.** In a last step, the estimation sample was restricted to all matched pairs, for which both sides were successfully surveyed and all outcome and control variables are non-missing. We exclude matches for which the control individual later entered the program. We also drop matches if the control individual took part in the 'ESF federal program', which constitutes a very similar treatment.[13] This last step results in a sample of 2,531 matched pairs in wave 1, 1,191 in wave 2 and 450 in wave 3. We use this unbalanced panel to estimate the program effects in each wave. The waves are referred to as $t_1$, $t_2$ and $t_3$ in the following analyses.

## 3.5. Empirical Strategy

### 3.5.1. Empirical Model

We estimate the ATTs of SILM by comparing the group means of program participants and matched non-participants in three different waves, representing different average program durations, while controlling for differences in pre-treatment characteristics. This can be summarized with the following estimation equation:

$$y_{it} = \sum_{s=1}^{3} (\beta_s \, Treat_i \times t_{is}) + X_i \, \gamma + \epsilon_{it}, \tag{3.1}$$

where $Treat_i$ is an indicator of program participation for individual $i = 1,...,N$ and $t_{is}$ is an indicator for waves $s = 1,2,3$. $y_{it}$ represents the outcome variables for individual $i$ in wave $t = 1,2,3$. To make the estimated effect sizes comparable across outcomes, we standardize them to mean zero and unit standard deviation. $X_i$ is a set of pre-treatment control variables (for a description see Table C.1.1 in the Appendix) and $\epsilon_{i,t}$ is the idiosyncratic error term. The parameters of interest are $\beta_s$, which identify the ATT of SILM in waves 1 to 3 relative to never participating.

It should be noted that our multi-step-sampling design does not allow us to use standard approaches to adjust the variance of our matching estimator for additional error from the first-stage estimation of the propensity score (see Abadie and Imbens, 2016). We thus report the unadjusted standard errors and confidence intervals throughout the paper. Although they are most likely too small, the bias would have to be large to change our main conclusions.[14]

[13] The 'ESF federal program' was launched in parallel to SILM. It also offered subsidized employment to a similar target group but its main emphasis was on improving re-employment prospects (see Boockmann et al., 2017).

[14] The standard error for the largest point estimate (life satisfaction in wave 1) would have to increase by 970% to turn the effect insignificant at the 5% level. For the lowest estimate (social status in wave 3), the standard error would need to increase by 18% to turn the effect insignificant.

Threats to identification of the ATTs may arise at four different levels, which will be discussed in the following subsection.

### 3.5.2. Threats to Identification

**(i) Job Center Selection into the Program and Spillover Effects** Not all job centers applied for funding under SILM. Hence, participating and non-participating job centers might differ with respect to characteristics that may also affect outcomes. Participating job centers are, in general, more concentrated in regions with higher unemployment rates and unfavourable employment prospects for welfare recipients. Worse economic conditions are likely related to well-being and social integration, but the direction of a potential bias is not clear.[15] To account for differences at the job center level, we use a classification of job center types developed by Dauth et al. (2013) as a predictor of the estimated propensity score. This classification reflects disparities in the local economic and social structure that affect the strategies of job centers.

Another aspect refers to spillover effects within participating job centers. The federal program did not provide any funding for additional administrative costs, therefore job centers had to bear them. As a consequence, case workers might have spent more time and effort on placing and counselling SILM participants and less on serving non-participants. If this negatively affected the outcomes of non-participants appearing in our control group, our estimates might be biased upwards. However, the program covered only 3% of the target population, such that spillover effects should be of minor concern. We still address these issues by comparing the estimates of matches with control individuals from participating and non-participating job centers in a robustness check.

**(ii) Individual Selection into the Program** In addition, there could be individual level selection into SILM within participating job centers: program entry was voluntary and expert interviews at job centers suggest that case workers tried to ensure a good fit to a particular job or employer when considering potential participants (Brussig et al., 2019). When deciding individual program entry, they might have taken into account characteristics like soft-skills and competencies, or motivation and ability to work. Such traits are likely correlated with pre-treatment outcome levels, but they are not directly observed in the data. Again, the direction of the potential bias is ambiguous.[16]

Individual selection into the program is mainly addressed in step 3 of the sample construction

[15] Since a higher general unemployment rate is associated with lower levels of individual well-being, participating job centers might be negatively selected with respect to overall well-being (Di Tella et al., 2001). This would bias our estimated ATTs upwards. On the other hand, individual well-being is positively associated with reference group unemployment (Clark, 2003). Being one of many welfare recipients in a participating job center might have less of a negative reputation effect, such that welfare recipients in participating job centers might be positively selected and our estimated ATTs would be biased downwards.

[16] On the one hand, individuals with greater well-being might be more motivated to enter the program, causing an upward bias in our estimated ATTs. On the other hand, program participants might ascribe greater value to employment and have suffered more from unemployment prior to entering the program, which would result in a downward bias.

process, where we apply nearest-neighbor propensity score matching to choose the most comparable control individuals for each participant. The key assumption for identification is that the estimate of the propensity score reflects all characteristics that jointly determine program entry and the outcomes (Rosenbaum and Rubin, 1984).

To warrant this arguably strong assumption, we use the detailed employment and ALMP records in the administrative data to construct different proxy measures for unobservable time-invariant confounders.[17] In particular, information on previous participation in ALMP programs might be informative about selection into the current program. Indeed, prior participation in One-Euro-Jobs is a strong predictor of participation in SILM. Both are likely related to time-invariant unobservables like willingness to work, attitudes towards the case worker and the job center's support opportunities. We also include the share of previously unfinished ALMP programs as a proxy measure for motivation, endurance and capability. As a further robustness check, we include control variables reflecting an individual's willingness to work from our survey.

To check the quality of the matching procedure, we assess common support and whether observable characteristics are balanced between participants and controls in our final estimation sample of wave 1. Figure C.2 in the Appendix shows that the propensity scores of treated and controls overlap almost perfectly. Table 3.2 compares the sample means of selected pre-treatment characteristics between both groups. Participants and controls do not significantly differ with respect to most of these, except for years of welfare dependence and the share with health impairments. Considering an average of around 7 years of welfare receipt, a difference of 2.6 months seems to be of negligible importance for the analysis. Similarly, the difference is small for health impairments. We nevertheless include pre-treatment control variables in our baseline regressions to account for any remaining imbalances in observables.[18]

**(iii) Dynamic Assignment** As welfare recipients do not enter the program after a fixed welfare duration, we face a dynamic assignment problem (see Fredriksson and Johansson, 2008). Selection arises because program entry is only observed for individuals who remain unemployed long enough to be offered participation. If this is not accounted for, the treatment group might be negatively selected in terms of the employment probability and thus also well-being and social integration. This would bias the estimated ATTs downwards.

Our sampling design does not perfectly account for the dynamic selection problem, as we only required control individuals to be unemployed on a certain cut-off date before the start of the program (see step 2 in Section 3.4). We address this issue in a robustness check by excluding matches if the control individual had already found a job before the matched participant entered the program.

Moreover, excluding matched pairs of future participants in the control group introduces a potential selectivity problem. Alternatively, we right-censor these observations or keep them in

[17] Caliendo et al. (2017) show that including explicit measures for personality traits, expectations, social networks etc. does not change the estimated impact of ALMPs on employment once labor market histories are controlled for.

[18] Not controlling for pre-treatment variables in our regressions does not affect our estimates of the ATTs, but slightly reduces precision. The results are available on request.

**Table 3.2.:** Comparison of Participants and Non-participants in the Final Estimation Sample

| | (1) | (2) | (3) | |
|---|---|---|---|---|
| | Means at $t_1$ | | | |
| | Participants | Non-participants | Difference | |
| **Sociodemographics** | | | | |
| Female | 0.47 | 0.46 | 0.01 | |
| Age | 48.97 | 48.68 | 0.29 | |
| Health impairment | 0.50 | 0.48 | 0.02 | * |
| Children in household | 0.26 | 0.28 | -0.02 | |
| German | 0.93 | 0.93 | -0.01 | |
| Married | 0.25 | 0.25 | 0.00 | |
| No professional degree | 0.17 | 0.18 | 0.00 | |
| Vocational degree | 0.78 | 0.79 | 0.00 | |
| Academic degree | 0.05 | 0.04 | 0.01 | |
| Region with weak employment prospects† | 0.79 | 0.77 | 0.02 | |
| **Employment history** | | | | |
| Years of welfare receipt | 7.43 | 7.21 | 0.22 | ** |
| Cum. years of ssc. employment | 6.54 | 6.68 | -0.14 | |
| Cum. years of marg. employment | 1.25 | 1.37 | -0.11 | |
| Cum. no of ALMP measures | 6.67 | 6.81 | -0.14 | |
| Prior participation in "One-Euro-Job" | 0.77 | 0.75 | 0.01 | |
| Share of unfinished ALMP measures‡ | 0.12 | 0.11 | 0.01 | |
| Observations | 2,531 | 2,531 | | |

*Notes:* The table shows the means of selected pre-treatment characteristics for participants (column (1)) and matched non-participants (column (2)) at $t_1$ based on the final estimation sample (see Section 3.4). Column (3) shows the differences in the means and their significance levels from two-sample t-tests. Pre-treatment characteristics are based on administrative data, for details see Table C.1.1 in the Appendix. † Defined as job center regions of SGB II-Type 3 according to the classification of the Federal Employment Agency (Dauth et al., 2013). ‡ An ALMP spell is defined as unfinished if it ends before the planned end date without a transition into employment. ***/**/* marks significance at the 1/5/10% level.
*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

the sample.

**(iv) Selection into the Survey** Selection into the telephone survey and non-random panel attrition might affect our estimates if they are related to pre-treatment outcomes.[19] To address selection into the initial survey, we first check whether the composition of the participant sample changes throughout the steps of the sample construction process. Table C.2.2 in the Appendix shows the mean values of pre-treatment characteristics and outcome variables of participants in the full sample of the administrative data, in the survey sample of wave 1 and in the final sample of matches of wave 1. Although observation numbers decline substantially, there is no evidence of a change in composition. Turning to the control individuals, they have very similar levels of well-being and social integration as individuals in the PASS dataset who would also fulfill the eligibility criteria of SILM (compare columns (2) and (4) in Table C.2.1 in the Appendix). As the PASS is not stratified on the eligibility criteria of SILM, we take this as evidence that non-response did not result in a systematic selection of the control individuals.[20]

To address selection into the follow-up surveys, Table C.2.3 in the Appendix shows observable characteristics of participants and their controls in the final sample of matches for waves 1 to 3. The descriptive statistics suggest that panel mortality does not lead to a different composition of surveyed individuals over time. We still conduct several robustness checks in order to deal with challenges regarding the telephone survey. In a first robustness check, the estimation equation is re-weighted by the estimated individual probability of program participants to respond to the first and follow-up waves of the survey. Secondly, we estimate the ATTs using the balanced panel sample only. Thirdly, we use cell-means from the survey sample to impute the outcome variables for non-respondents and re-estimate the ATTs in the full administrative sample. Finally, we account for the fact that matched control individuals are surveyed, on average, four months after the participant.

## 3.6. Results

### 3.6.1. Well-being and Social Integration during Program Participation

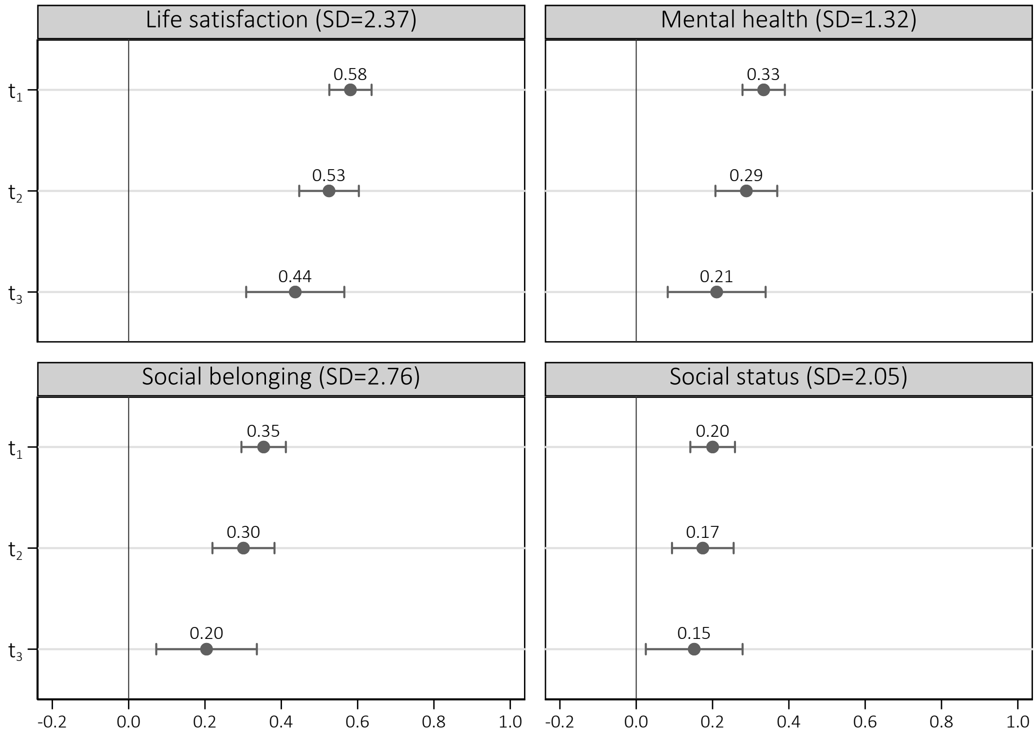
Figure 3.1 depicts the estimated ATTs on our four outcome variables for the three survey waves. In wave 1, the largest program effect is on life satisfaction which increases by 0.58 standard deviations (SDs). The impact on mental health amounts to 0.33 SDs. The program effects are 0.35 SDs on social belonging and 0.20 SDs on social status.

To put these estimates into perspective, we compare the mean levels of the outcome variables of participants to the same variables in different subsamples of the PASS survey (see Table C.2.1

[19] Suppose individuals with lower pre-treatment outcomes benefit more from the program, but are less likely to answer the telephone survey. This would lead to an underestimation of the ATTs. Conversely, the estimates would be biased upwards if participants feel obliged to participate in the survey, or report higher outcome values, in return for being supported by the program or their case workers.

[20] Please note that the PASS is a voluntary survey and the group of eligible individuals for SILM may not be representative.

**Figure 3.1.:** ATTs on Standardized Outcomes over the Course of the Program




*Notes:* SD: standard deviation. The figure shows the estimated ATTs at different program durations (mean duration of 7 months at $t_1$, 18 months at $t_2$, 29 months at $t_3$) from pooled OLS regressions (see equation 3.1) based on the final estimation sample (see Section 3.4). Control variables consist of sociodemographics and the individual employment history (see Table C.1.1 in the Appendix). The total number of observations amounts to 8,344 (5,062 at $t_1$, 2,382 at $t_2$, 900 at $t_3$). Standard errors are clustered at the individual level. Whiskers represent 95% confidence intervals.
*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

in the Appendix). This comparison shows that SILM increases participants' life satisfaction to the level of employed individuals in PASS, whereas social belonging and, in particular, social status are still lower. Life satisfaction is likely to summarize economic, psychological, social and other benefits of program participation, which might explain the large effect size. Being employed in the program increases social status, but not up to the average level of regularly employed individuals. Participants may recognize that the program aims at disadvantaged individuals and that work contracts are only temporary.

In absolute terms, our estimated program effects are similar in magnitude to the estimated effects of job loss of Pohlan (2019), who studies the causal short-term effects of job loss on the same well-being and social integration measures in the PASS. She finds effect sizes of −0.55 SDs on life satisfaction, −0.31 SDs on mental health, −0.34 SDs on social belonging and −0.25 SDs on social status. While our results confirm that JCSs partially counteract the negative effects of unemployment on life satisfaction (Knabe et al., 2017) and social belonging (Gundert and Hohendanner, 2015), our estimated effect sizes in wave 1 are larger. Moreover, our positive estimate for mental health contrasts the study of Huber et al. (2011a), who find that participation in JCS may even moderately increase negative mental symptoms and sleeplessness among welfare recipients.

The larger effects of SILM may be due to its specific target group and explicit focus on improving social integration (see also Subsection 3.6.5). Among the population of welfare recipients, SILM targets those with the highest entry barriers to the regular labor market and very long unemployment durations. As these individuals face a particularly high risk of social exclusion, they may also have a greater scope for improvement than the average unemployed person. Unlike previous programs, SILM offered a relatively long-term work contract with social security contributions and working conditions were meant to be tailored to the needs of individual participants. Qualitative evidence suggests that case workers made considerable efforts to find jobs that fit the requirements of both the participants and the employers (Brussig et al., 2019). In line with this, surveyed participants consistently reported that case workers were supportive and that their jobs fit their experience and allowed them to develop their skills. Only very few perceived the work as too (un-)demanding or stressful.[21]

However, Figure 3.1 also shows that the average program effects substantially decrease over time. In wave 3, the ATT on life satisfaction declines to 0.44 SDs, on mental health to 0.21 SDs and on social belonging to 0.20 SDs. With 0.15 SDs in wave 3, the ATT on social status remains more stable.

Over time the positive effects of working in SILM may vanish, because participants get used to it or they may anticipate of returning to unemployment as the program end comes closer. However, in the next subsection we will show that such psychological mechanisms are unlikely to drive the decline in the average program effect. Instead, we will show that it can be completely explained by changes in the composition of participants and control individuals – while the effect of active participation remains stable over time.

### 3.6.2. Lock-in Effects, Employment Status and the Effect of Active Participation

In this subsection, we analyze the relationship between program participation, employment and the subjective outcomes. As a first step, we use the administrative data to estimate how SILM affects an individual's probability to be in regular employment at the individual survey date of waves $t_1$, $t_2$ and $t_3$.[22] For that purpose, we apply the specification in equation 3.1 in a probit model and plot the estimated ATTs in Figure C.3 in the Appendix.

Throughout the program, participants are 18 to 21 percentage points less likely to be regularly employed than their matched controls. The size of this lock-in is similar to earlier JCSs (see e.g. Caliendo et al., 2008; Hujer and Thomsen, 2010) and larger than for more contemporaneous

[21]These results are available on request.

[22]Employment refers to any kind of non-subsidized work, i.e. both marginal and with social security contributions, because in terms of social integration and well-being any kind of employment is expected to be important – especially for long-term unemployed individuals.

One-Euro-Jobs (see e.g. Hohmeyer and Wolff, 2012).[23]

This results implies that over time the share of regularly employed control individuals increases, while participants are occupied with SILM. At the same time, more workers drop out of the program and return to unemployment. These systematic changes in the share of employed workers are likely to affect the average levels of well-being and social integration in the treated and control group and thus the estimated ATT – even if the effect of working in the program itself does not wear off over time.

To gain a deeper insight into this relationship, Figure 3.2 plots the mean values of well-being and social integration for four different groups: active and inactive program participants (in blue and solid line) as well as employed and unemployed non-participants (in orange and dashed line). The size of the circles reflects the share of participants and control individuals in the respective state in each wave. These shares are the same in all four sub-figures, so we only report the percentage values in the top left panel.

The levels of well-being and social integration within the sub groups and the differences between them are quite constant over time. Inactive participants and unemployed controls exhibit considerably lower values than active participants and employed controls. What changes is the relative size of these sub groups. In the treatment group the share of active participants drops from 92% in wave 1 to 74% in wave 3, while the share of employed non-participants in the control group increases from 20% to 26%.

This suggests that the observed drop in the ATT is due to compositional changes rather than a decreasing effect of participation itself: As more participants become inactive, the treatment group becomes less 'happy' on average; as more non-participants find a job, the control group becomes more 'happy' on average. As a consequence, the estimated average program effect – which results from a comparison of group means across all participants and non-participants – decreases.

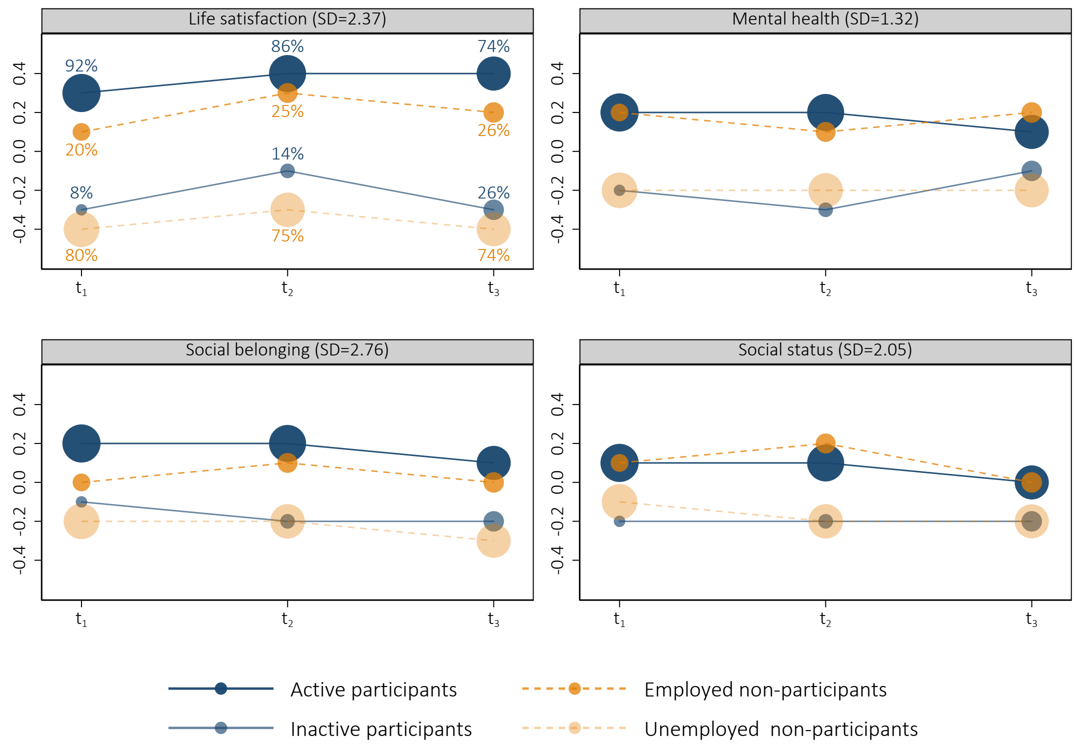
To account for this change in composition, we divide the estimated ATTs by the difference in the share of active participants in the treated group and the share of employed non-participants in the control group. Figure 3.3 shows that after applying this normalization, the estimated ATTs remain stable and even increase slightly for life satisfaction. Indeed, program participation itself continuously increases individual well-being and social integration, while the decline in its average effect can be entirely explained by compositional changes.

### 3.6.3. Drop out and the Short-term Effects after the End of Participation

This section investigates the short-term effects for participants after they have left the program. The third survey wave was scheduled to take place shortly before the program ended for most participants, such that we only observe the post-program outcomes for 311 individuals. The majority of these individuals are early drop-outs, of which 84% returned to unemployment. This

[23] Please note that, in contrast to other studies, our definition of employment includes marginal employment. Moreover, the business cycle and particularities of the target group and the program design may complicate the comparison of effect sizes with earlier programs.

**Figure 3.2.:** Standardized Outcomes of active and inactive Participants compared to employed and unemployed Non-participants



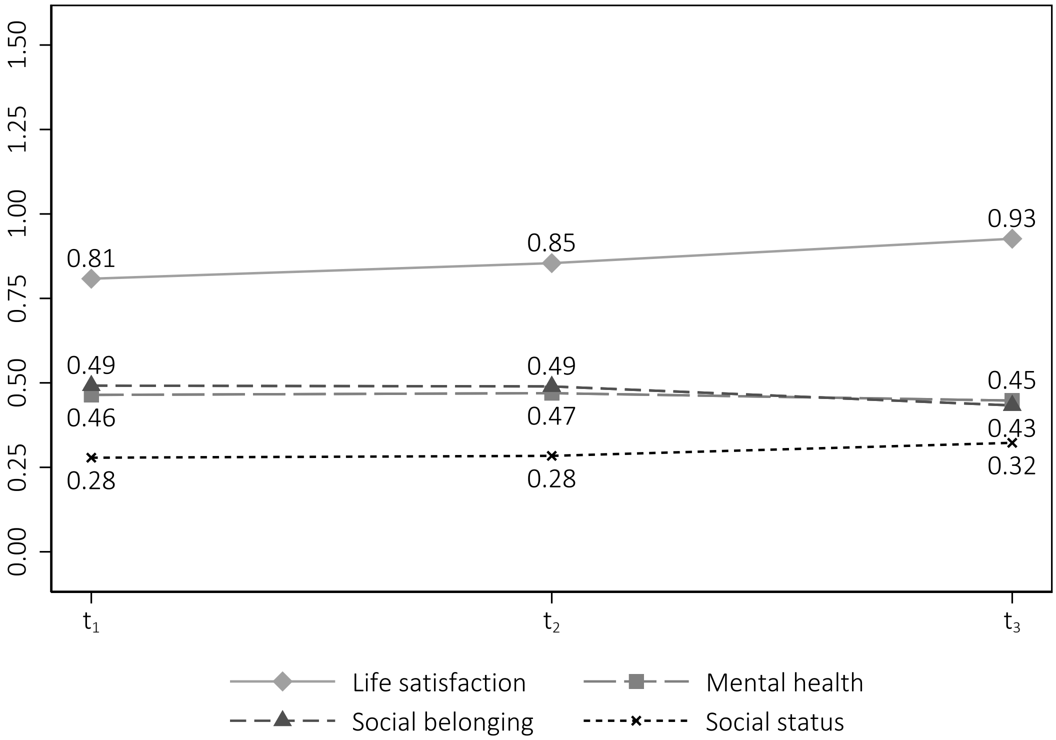

*Notes:* The figure shows the means of the outcome variables for active and inactive participants in comparison to employed and unemployed non-participants at different program durations (mean duration of 7 months at $t_1$, 18 months at $t_2$, 29 months at $t_3$) based on the final estimation sample (see Section 3.4). The size of the markers (and the percentage labels in the top left sub-figure) represents the share of active and inactive participants among all participants (in blue and solid line) or the share of employed and unemployed non-participants among all non-participants (in orange and dashed line). The size of the markers is identical in each sub-figure. The total number of observations amounts to 8,344 (5,062 at $t_1$, 2,382 at $t_2$, 900 at $t_3$).
*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

comes with considerably lower levels of well-being and social integration than a transition into non-subsidized employment (see Table C.2.4 in the Appendix). As the most prominent reason for leaving the program early, survey respondents named health issues and mental or physical overload, followed by a mismatch between the expected and actual tasks or conflicts at the workplace. The outcomes of unemployed drop-outs are comparable to non-participants, while employed drop-outs reach comparable values as employed individuals in PASS (compare Tables C.2.1 and C.2.4 in the Appendix).

To study the post-program outcomes of participants who regularly leave the program after the initially planned duration, we have to rely on 99 individuals with a relatively early end date. Table C.2.4 in the Appendix shows that the differences in the levels of well-being and social integration between those who were employed and those who were unemployed were less pronounced after the program ended.

Figure 3.4 shows the ATTs in the first survey wave after the planned end date, i.e. about 5

**Figure 3.3.:** Normalized ATTs on Standardized Outcomes



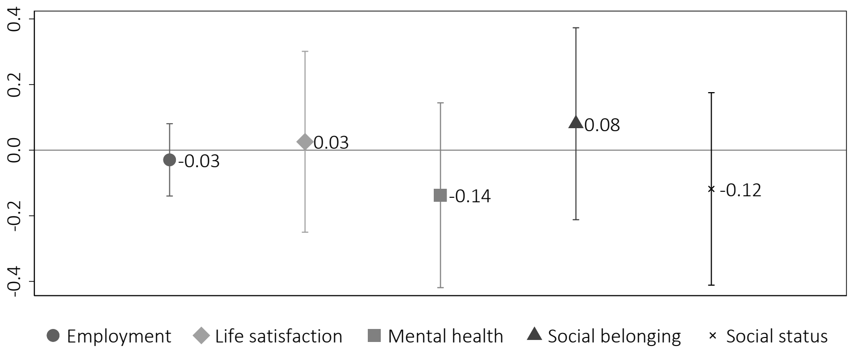

*Notes:* The figure shows the estimated ATTs at different program durations (mean duration of 7 months at $t_1$, 18 months at $t_2$, 29 months at $t_3$) from pooled OLS regressions (see equation 1) based on the final estimation sample (see Section 4). Control variables consist of sociodemographics and the individual employment history (see Table C.1.1 in the Appendix). The estimates are normalized by the difference between the share of active participants in the treatment group and the share individuals in non-subsidized employment in the control group. This accounts for the fact that over time the share of program drop-outs and control individuals in employment increases. Since employment inside and outside of the program comes with similar well-being and social integration levels, accounting for these composition changes shows that the effect of active program participation does not decrease over time. The total number of observations amounts to 8,344 (5,062 at $t_1$, 2,382 at $t_2$, 900 at $t_3$).
*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

months later.[24] Given the small sample size and that participants with shorter program duration might be selected, these estimates should be interpreted with caution. The lock-in effects on employment decrease to zero, but we also find no evidence that the positive effects on well-being and social integration outlast the end of the participation.

We also use the full administrative sample to estimate the employment effects before and after the planned end date.[25] Figure C.6 in the Appendix shows that the lock-in effect amounts to about −16 percentage points before the end, but drops to −7 percentage points one month after the program end. Six months after the end, the lock-in effect vanishes and stays at zero thereafter. This reduction is faster than in previous studies.

[24]Figure C.4 in the Appendix shows the distribution of time spans between the planned end date and the date of the first post-program survey for this sample.
[25]For most of the participants the planned end date corresponds to 31/12/2018, which is the end of the observation period in the administrative data (see Figure C.5 in the Appendix).

**Figure 3.4.:** ATTs on the Employment Probability and Standardized Outcomes after the planned Program End




*Notes:* The figure shows the estimated ATTs on the probability of finding non-subsidized employment and the standardized outcomes in the first wave after the participants' individual planned end date (as recorded in the administrative data) from probit (for employment) and OLS (for well-being and social integration) regressions, respectively. The total number of observations amounts to 198. Standard errors are clustered at the individual level. Whiskers represent 95% confidence intervals.

*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

### 3.6.4. Robustness checks

In this subsection, we provide robustness checks of the ATTs on well-being and social integration measures that relate to the threats to identification introduced in Section 3.5, and address potential misspecification.

**(i) Job Center Selection, Spillover Effects and individual Selection** In a first robustness check, we exploit the fact that not all job centers implemented the program. We split the sample of matches into two subsets depending on whether or not the control individual is assigned to a participating job center. Individual selection and spillover effects should be less of an issue for control individuals in non-participating job centers, given it was not possible for them to participate in the program. On the other hand, job center selection might play a more important role as participating and non-participating job centers are likely to differ with respect to regional conditions and their strategies.

Table C.2.5 in the Appendix shows the estimated ATTs in wave 1 for both subsamples. For control individuals at participating job centers the effects on life satisfaction and the social integration measures are still significantly positive, though smaller in magnitude. The ATT on mental health is slightly higher. However, none of these differences are significant at the 1% level. As compared to the baseline results, the estimates indicate that selection on the job center and individual level may play a role to some extent, but they provide no evidence that would contradict our main conclusions on the impacts of the program.

In a further robustness check, we analyze whether selection into the program might be related to the availability of program places. Participants at job centers that offer fewer jobs per eligible individual might be positively selected, as more potential participants compete for the same program place. Availability is measured by the ratio of program places to welfare recipients on the job center level. We split the sample into matches with participants at job centers with above and below average availability of program places (see Table C.2.6 in the Appendix).[26] The estimated ATTs in wave 1 are similar in both subsamples, providing no evidence for selection into the program. We draw the same conclusion when adding indicators of willingness to work as control variables (see Figure C.7 in the Appendix).[27]

**(ii) Dynamic Assignment.** To address the dynamic assignment problem discussed in Section 3.5.2, we exclude those matched pairs from our estimation sample for which the control person had already found a job when the assigned treated individual entered the program. As expected, the estimated coefficients are slightly larger but very close to our baseline results (see Figure C.10 in the Appendix).

We intend to estimate the ATTs relative to never participating and implement this by excluding matches of control individuals who eventually enter SILM or the ESF federal program. In a robustness check, we either keep them in the sample or right-censor matches from the moment that controls enter one of these programs. This does not affect our results (see Figure C.10 in the Appendix).

**(iii) Selection into the Survey.** Besides selection into the program, the results might be affected by non-random selection into the survey. Figure C.11 in the Appendix presents the results from a weighted specification using the participants' individual probability of responding to the first and follow-up waves. The probability of entering the survey is predicted by pre-treatment characteristics from the administrative records, as well as an indicator for program drop-out. For the follow-up waves, additional survey information are used to account for lags of the outcome variables and the need for support in different life domains (see Table C.1.2 in the Appendix).

In a further check for non-random panel attrition, we re-estimate the ATTs using only observations from the balanced panel. The results are shown in Figure C.12 in the Appendix. As the sample size in each wave is smaller, the estimates from the balanced panel are less precise. Both sensitivity checks point to similar results in wave 1 and slightly smaller effects in the follow-up surveys. This suggests that the baseline estimates might be slightly upward biased by selective non-response to the survey. This bias, however, is too small to change the main conclusions.

Both our subjective outcomes and the response to the survey might be related to the employment status. We use the full administrative sample of all participants and one nearest neighbor in the control group to check whether the employment effects differ between respondents and non-respondents. The lock-in effects are only slightly smaller than in the final estimation sample (see Figure C.13 in the Appendix).[28] Respondents and non-respondents are comparable with respect to

[26] The average job center offered about 2 places per 100 welfare benefit recipients.

[27] See Table C.1.2 for a description of the indicators of willingness to work and Figure C.8 in the Appendix for plots of the distributions.

[28] We obtain almost identical results when using four or ten nearest neighbors for every participant. These results are available on request.

their employment states (see Table C.2.7 in the Appendix). We impute the subjective outcomes for non-respondents with cell means by treatment-cohort-wave-employment status from the survey. The results are very similar to our baseline estimates (see Figure C.14 in the Appendix).

To take into account that the outcomes of non-participants are measured with a certain delay compared to participants, we control for the time span between the first possible program entry date and the survey date. The estimated effects remain unaltered (see Figure C.15 in the Appendix).

**(iii) Empirical Specification.** By using a linear regression model, we follow a common practice in the literature. In doing so, the ordinal outcome variables are implicitly treated as if they were measured on a cardinal scale (see e.g. Ferrer-i Carbonell and Frijters, 2004). Chen et al. (2019), however, argue that treating ordinal variables as cardinal might render comparisons of group means invalid. Instead, regression models for ordered data should be applied and the coefficients can be interpreted as comparisons in group medians. The results from ordered probit regressions are very similar to our baseline results (see Table C.2.8 in the Appendix).

In a further robustness check, we use a fixed effects model as an alternative to the pooled specification in equation 3.1 to identify the change in the ATTs between the waves.[29] The estimates from the differenced model are slightly more negative than from the pooled model but still comparable (see Table C.2.9 in the Appendix). This suggests that the negative time trends in the ATTs are not driven by unobserved individual fixed effects.

### 3.6.5. Heterogeneous Effects and the Role of Program Design

To assess whether the effectiveness of the program could be increased, we study effect heterogeneity for subgroups who are expected to have a particularly low probability of finding non-subsidized employment: individuals with health impairments and individuals with an above-average unemployment duration.[30]

Figure 3.5a plots the estimates for individuals with and without health impairments. Individuals with health impairments benefit more from program participation. The estimated ATTs in wave 1 are larger for each outcome variable. With respect to life satisfaction, the effects are also more stable over time than for individuals without health impairments. Figure 3.5b shows that the program is more beneficial for individuals with pre-treatment welfare durations above the sample mean of seven years. Effect heterogeneity is more pronounced for the social integration measures than for the well-being measures. While the effects for life satisfaction are decreasing over time for both groups, the estimated ATTs on mental health, social belonging and social status are more stable for participants with a lengthier duration of welfare dependence.

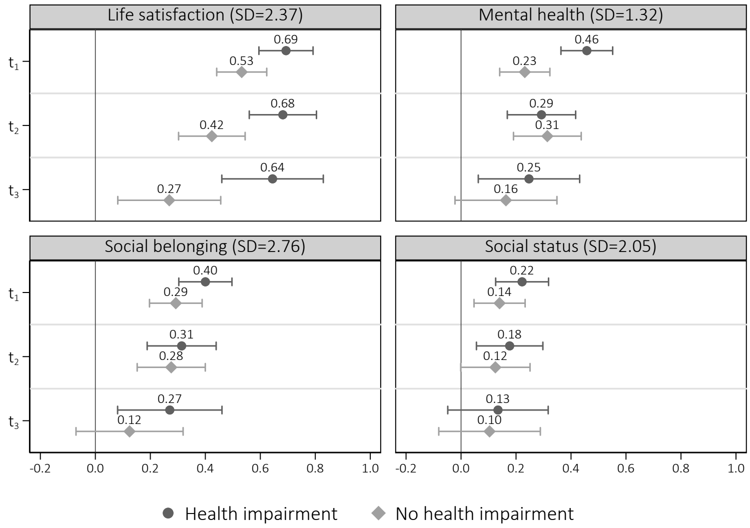
Again, we normalize the ATTs to analyze the importance of compositional changes in the share of active participants and employed non-participants (see Section 3.6.2). For both subgroups, the normalized ATTs are still larger than for all participants (compare Figures B.17 and B.3 in the

[29] Note that this approach can only identify the change in the ATTs over the course of the program, but not its level, as we do not have a pre-treatment measurement of the outcomes.

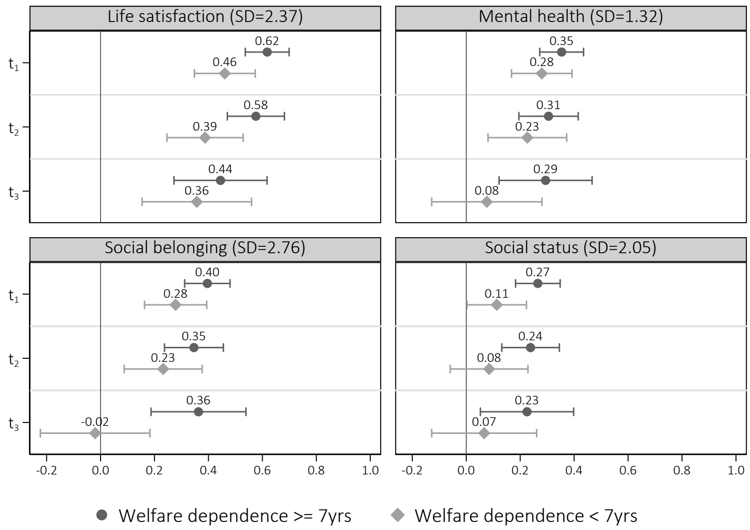
[30] We find lower lock-in effects of the program for these subgroups (see Figures C.16a and b in the Appendix).

**Figure 3.5.:** ATTs on Standardized Outcomes for Participants with low Employment Prospects

**(a)** Participants with and without Health Impairments




**(b)** Participants with < and ≥ 7 years of prior Welfare Dependence




*Notes:* SD: standard deviation. The figures show the estimated ATTs at different program durations (mean duration of 7 months at $t_1$, 18 months at $t_2$, 29 months at $t_3$) from pooled OLS regressions (see equation 3.1) for different subsamples of participants and their matches in the final estimation sample (see Section 3.4). Control variables consist of sociodemographics and the individual employment history (see Table C.1.1 in the Appendix). Sub-figure (a) shows the ATTs separately for participants with and without health impairments and their matched control individuals. The number of observations for individuals with health impairments amounts to 4,306 (2,552 at $t_1$, 1,276 at $t_2$, 478 at $t_3$). The number of observations for individuals without health impairments amounts to 4,038 (2,510 at $t_1$, 1,106 at $t_2$, 422 at $t_3$). Sub-figure (b) shows the ATTs separately for participants with a prior welfare dependence duration below and above the sample mean of seven years and their matched control individuals. The number of observations for individuals with above average welfare dependence amounts to 5,142 (3,110 at $t_1$, 1,498 at $t_2$, 534 at $t_3$). The number of observations for individuals with below average welfare dependence amounts to 3,202 (1,952 at $t_1$, 884 at $t_2$, 366 at $t_3$). Standard errors are clustered at the individual level. Whiskers represent 95% confidence intervals.

*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

Appendix). For these individuals the average program effects are not only larger because of a lower counterfactual employment probability, but they seem to benefit more from participation itself.[31]

Next, we turn to the role of different design aspects of SILM. The estimates presented in Table 3.3 are derived from fixed effects regressions using the panel survey sample of participants (see step 4 in Section 3.4). The results show that entering an accompanying training measure alongside the program is associated with 0.147 SDs higher social belonging and 0.107 SDs higher social status. Supportive interventions by case workers or external coaches – e.g. to resolve conflicts at the workplace or to give advice to employers – are positively related to social status (+ 0.074 SDs). There is no significant association between engaging in activities with other participants and well-being or social integration. Furthermore, Table 3.3 suggests that life satisfaction decreases by 0.227 SDs when participants drop-out of the program earlier than initially planned.

**Table 3.3.:** Program Design and Standardized Outcomes, Fixed Effects Estimates

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Well-being | | Social integration | |
| | Life satisfaction | Mental health | Social belonging | Social status |
| Training | 0.047<br>(0.032) | 0.061*<br>(0.032) | 0.147***<br>(0.036) | 0.107***<br>(0.039) |
| Activities with other participants | 0.047<br>(0.038) | 0.048<br>(0.042) | -0.032<br>(0.054) | -0.006<br>(0.053) |
| Support by case workers/coaches | 0.023<br>(0.030) | 0.021<br>(0.031) | 0.027<br>(0.033) | 0.074**<br>(0.034) |
| Drop-out | -0.227***<br>(0.077) | -0.042<br>(0.074) | -0.092<br>(0.076) | 0.001<br>(0.072) |
| Observations | 7,308 - 7,799 | 7,374 - 7,872 | 7,299 - 7,787 | 7,270 - 7,760 |

*Notes:* The table shows coefficient estimates of different program characteristics on standardized outcomes from fixed effects models based on survey data of participants (see Section 3.4). For each program characteristic, a separate model is estimated. The number of observations differs due to missing values. Standard errors are clustered at the individual level. ***/**/* mark significance at the 1/5/10 % level.
*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

## 3.7. Conclusion

Unemployment not only puts individuals under economic strain, it also reduces well-being and social integration. The consequences might be especially pronounced for long-term unemployed individuals with limited access to the regular labor market. For this group in particular, publicly subsidized employment might be an effective policy instrument to improve well-being and social integration.

[31] We also analyze effect heterogeneity with respect to other sociodemographic characteristics, such as gender, age and parental status. We do not find significant differences in the ATTs for these groups.

Previous studies have established that JCSs usually have strong lock-in effects, because individuals reduce their job search efforts while engaged in the program. In this paper, we extend the scope of the evaluation literature by analysing how a recent German JCS affected subjective well-being and social integration. Although we confirm the expected lock-in effect on finding regular employment, we also show that well-being and social integration substantially improve. The effect sizes are largest for life satisfaction and smallest for social status.

The estimated average program effects decline over time. This, however, is entirely explained by status changes in the group of participants and non-participating controls: Over the program duration of up to three years, an increasing share of control individuals find regular jobs and catch up to similar levels of well-being and social integration as program participants. At the same time, more participants drop-out and return to unemployment. As a result, the estimated average program effect decreases over time, even though active participants consistently benefit throughout the entire program duration. We only observe the post-program outcomes for a small sample of workers with an early end date. For these individuals we find no evidence that the effects outlast the end of participation.

Our results show that a well-targeted JCS is an effective policy instrument to improve the well-being and social integration of long-term unemployed individuals. Deducting the earnings from individual welfare claims limits the additional costs for taxpayers without affecting the beneficial effects on well-being. This highlights the importance of the psychosocial functions of work – like providing a regular day structure, social interaction and meaningful activity. However, it also implies that subsidized employment would have to be provided permanently to maintain its benefits. In this case, policy makers face a trade off with respect to the duration of subsidized jobs: On the one hand, long-term unemployed individuals with arguably low job prospects substantially benefit as long as they are in the program. On the other hand, even in this group lock-in effects are non-negligible, such that a longer duration may obstruct otherwise possible job matches that would be equally beneficial.

This has important implications for the design of future programs. Access should be restricted to workers with the lowest job market prospects, because they experience the largest gains in well-being and social integration and exhibit the lowest lock-in effects. Over the course of the program, participants' outside options should be regularly assessed.

Whether the psychosocial benefits of JCSs may directly improve the employability of participants remains a fascinating question for future research. We also make no attempt to quantify the value of the public goods produced by participants or other indirect effects like lower health care spending or positive spillovers on children in the household. If such programs were to be extended in the future, a comprehensive cost-benefit analysis would have to take account of these aspects as well. On the other hand, also potentially negative general equilibrium effects like crowding out of regular jobs would need to be considered carefully when increasing the size and duration of subsidized employment programs.

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# Appendix

# List of Appendix Tables

# List of Appendix Figures

# A. Appendix: Regional Structural Change and the Effects of Job Loss

## A.1. Data Appendix

### A.1.1. Occupational Tasks

#### German Qualifications and Career Surveys (GQCS)

In order to characterize the task content of occupations, we use the 1979, 1985/86, 1991/92, 1998/99, 2006 and 2012 waves of the GQCS (see Rohrbach-Schmidt and Tiemann, 2013 for a detailed description of the dataset). The GQCS are repeated cross-sectional surveys of about 20,000 to 30,000 individuals per wave. We restrict the sample to regularly employed workers between 15 and 65 years of age and exclude agriculture and mining occupations as well as trainees, interns, individuals still in education and retirees. All waves classify occupations by KldB-1988 3 digit codes, which we aggregate to the 52 occupation fields used in our main analysis (see Tiemann et al., 2008 for the mapping between classifications). This step assures sufficient observation numbers for each occupation to compute mean task intensities and allows us to merge them to our other data sets.

#### Occupational Task Intensities

Among many other things, the GQCS contain information about the tasks individuals carry out at work and the tools they use. One of the great merits of the data is the long time span it covers, that allows us to study long-term shifts in the task structure of occupations. The downside is that the task definitions, the item scales and the survey populations differ across waves, such that using the data requires careful harmonization and, in some cases, imputation in order to avoid mechanical trends or breaks. Following Rohrbach-Schmidt and Tiemann (2013), we condense a set of 22 binary task indicators that are consistently available in most of the waves. We then impute missing tasks at the individual level by using skill requirements or work tools that are available across several waves to predict whether a person likely carries at a certain task.[1]

We then follow the common practice in the task literature and categorize each task as either non-routine abstract, non-routine interactive, routine cognitive, routine manual or non-routine

[1] For example, in the 1986 and 1992 wave the missing task 'measuring' is set to 1 if individuals use measuring devices as a main work tool. We validate this approach by checking for sufficient correlations in waves where both variables are present. For the previous example the correlation is 0.8 in 1999.

manual (see Table 4, column 3 in Rohrbach-Schmidt and Tiemann, 2013). The data only tell us whether or not an individual carries out a given task, but not the time spent on doing so. To proxy the share of working time spent on each task, we follow the approach of Antonczyk et al. (2009) and compute task intensities. For example, if an individual carries out 4 tasks, then each task is assumed to take up 1/4 of the total working time. The same holds for the intensity of task categories: if 3 of these 4 tasks are routine manual (RM), the RM task intensity would be 3/4. We define the main task to be the task category with the highest intensity. In the previous example, the main task would thus be RM.

We then average the task intensities over all individuals in each occupation and wave and close remaining gaps for some tasks and waves by linear extrapolation.[2] This provides us with a vector of 22 average task intensities, and alternatively, a vector of the 5 broad task category-intensities, for each of the 52 occupations and most of the GQCS waves.

In order to arrive at an occupation-year panel, we expand the data and linearly interpolate the average occupational task intensities between the survey waves. This implicitly assumes that changes in tasks occur gradually between survey waves. The final dataset allows us to merge the task content of occupations to our regional and individual level data via occupation-year cells.

### Bilateral Task Distances between Occupations

We use the detailed vectors of 22 occupational task intensities to compute bilateral task distances $d_t^{o,o'}$ between all occupation pairs $(o, o')$ in every year $t$. Following Gathmann and Schoenberg (2010), we measure distance in terms of the angular separation, which describes the angle between two vectors, i.e. the difference in their orientation in the task space:

$$AngSep_t^{o,o'} = \frac{\sum_j q_{jo} \times q_{jo'}}{[(\sum_j q_{jo}^2) \times (\sum_j q_{jo'}^2)]^{0.5}} ,$$

where $q_{jo}$ and $q_{jo'}$ is the average task-'j'-intensity of any two occupations $o$ and $o'$, i.e. the 22 elements of each occupation's task vector as described above.

If two task vectors point in the exact same direction, their angular separation is 1; if they are orthogonal it is 0. [3] We therefore use $d_t^{o,o'} = 1 - AngSep_t^{o,o'}$ as our task distance measure, which has been shown to be a strong predictor of worker transitions between occupations and wage growth (Gathmann and Schoenberg, 2010).

Since the task distances $d_t^{o,o'}$ are year-specific, they change over time as occupations shift their task contents. For example, in 1986 the occupation pair with the minimum task distance of 0.01 is '28 Wholesale/Retail Dealers' and '30 Other Mercantile Occupations (excl. Retail/Wholesale/Banking)', the pair with the maximum distance of 0.96 are '20 Laborers' and

[2] We account for differences in the total number of tasks surveyed in each wave such that the imputed task intensities still sum to one.

[3] In contrast to the Euclidean distance, the angular separation disregards the task vectors' distance to the origin. In our application this is not relevant, because the task intensities always sum to 1 by definition, such that each occupation's task vector has unit length.

‘49 Social Occupations’. Until 2012, the minimum and maximum task distances decline to 0.03 (‘04 Chemistry and Plastics Production’ and ‘05 Paper Production and Processing, Printing’) and 0.82 (‘37 Finance, Accounting, Bookkeeping’ and ‘14 Bakers, Confectioners, Candy Production’), respectively.

### A.1.2. Indicators of Local Structural Change

#### Long-run Changes in Local Occupation Structures

In order to calculate long-run changes in local occupation structures, we use regional and occupational employment data based on the BeH at three points in time, i.e. 1990, 2000 and 2010. This data was aggregated from register data of the German social security system at the level of local labor market regions and KldB1988-3-digit occupations and provided by Dauth (2014). [4]

Our first use of this data is to characterize the RM-bias of structural change in each West German local labor market region. In Section 1.2.2 we justified classifying occupations by their initial main task in 1986, i.e RM and Other, because specialization with respect to these tasks is strongly related to either occupational decline or growth at the West German aggregate. To characterize regional differences in the exposure to long-run RM-biased structural change, we compute the weighted employment growth rates of RM occupation types in each local labor market region between 1990 and 2010 (this is the observation period for which we observe displacement events):

$$grRM_r^{LR} = \frac{E_{r,1990}^{RM}}{E_{r,1990}} \cdot \frac{E_{r,2010}^{RM} - E_{r,1990}^{RM}}{E_{r,1990}^{RM}} .$$

where $E_{r,t}^{RM}$ is the sum of employment in all occupations $o$ of type RM in region $r$ and $E_{r,t}$ is total employment in region $r$ at time $t = \{1990, 2010\}$. The first term on the right-hand side is the occupation type’s initial employment share. This weighting factor avoids overstating the impact of initially small occupations on long-term growth. The weighted growth rates can be interpreted as the contribution of RM occupations to overall local employment growth between 1990 and 2010. Using the same formula, we also compute the weighted long-term growth rate for occupations with a main task other than RM. By definition, $grRM_r^{LR}$ and $grOther_r^{LR}$ sum up to the local growth rate of total employment between 1990 and 2010.

We plot these growth rates in Figure 1.2 to illustrate regional heterogeneity with respect to structural change. We then classify regions into types R={R1, R2, R3} that indicate a region’s tercile in the distribution of $grRM_r^{LR}$. These region types enter our matching procedure, i.e. we directly match displaced workers and control individuals from the same tercile of long-run local RM-Biased Structural Change (RMBSC) distribution and use these region types to study effect heterogeneity in our event study models (see Section 1.4.1).

[4] For further information about underlying micro data see Section 1.2.1. A detailed description of the sample restrictions and the aggregation procedure is given in the Appendix of Dauth (2014). The regional level of aggregation are local labor market regions, which basically reflect commuting zones (BBSR, 2021). We further aggregate the data from KldB1988-3-digit occupations to 52 occupation fields (as defined by Tiemann et al., 2008).

### RM-Biased Structural Change Preceding Displacement Base Years

In addition to long-run structural change at the local level, we compute a time-varying measure of RMBSC for each labor market region and potential base year $c$ which covers the time span 1990 to 2010. In order to merge region and individual level data, we expand the regional employment data to a region-occupation-year panel and fill the gaps between decades by linear interpolation.

Next, we compute the weighted growth rate of RM occupations for each local labor market and a ten year window preceding each potential base year $c$ between 1990 and 2010:

$$grRM_r^{c-10} = \frac{E_{r,c-10}^{RM}}{E_{r,c-10}} \cdot \frac{E_{r,c}^{RM} - E_{r,c-10}^{RM}}{E_{r,c-10}^{RM}} .$$

In our propensity score estimation, this measure accounts for differences with respect to structural change within region types R1 to R3 in the decade before the displacement event. Moreover, we explicitly use this base year $c$-specific measure in our matched DiD analyses to analyze how the effects of displacement vary along the distribution of regional RMBSC (see variable $grRM_r^{c-10}$ in equation (1.3) in Section 1.3.4).

## A.2. Supplementary Results

### A.2.1. Supplementary Tables

**Table A.2.1.:** Characteristics of Declining and Growing Occupations

| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|---|
| Rank | Occupation | Category | Wage | Share | wGR | Task Intensity 1986 / 2012 | | | | |
| | | | 1990 | 1990 [pct] | 1990-2010 [pct] | NRA [pct] | NRI [pct] | RC [pct] | RM [pct] | NRM [pct] |
| **Growing Occupations:** | | | | | | | | | | |
| 52 | 48 Health Occ.s without Approbation | Serv. | Mid | 4.17 | 1.73 | 14.0 / 27.3 | 28.2 / 25.6 | 16.2 / 13.2 | 4.9 / 6.1 | 33.4 / 20.5 |
| 51 | 39 Commercial Office Occ.s | Serv. | Mid | 11.90 | 1.53 | 26.7 / 42.9 | 23.9 / 27.9 | 42.5 / 14.6 | 4.8 / 3.7 | 1.7 / 2.2 |
| 50 | 20 Laborers | Manuf. | Low | 1.04 | 1.45 | 2.1 / 21.5 | 2.3 / 17.3 | 4.4 / 12.2 | 74.0 / 10.6 | 14.2 / 30.0 |
| 49 | 38 IT Core Occ.s | Techn. | High | 1.15 | 1.27 | 55.3 / 45.5 | 17.5 / 25.0 | 12.3 / 13.0 | 11.9 / 6.4 | 2.3 / 5.3 |
| 48 | 21 Engineers | Techn. | High | 2.39 | 0.73 | 41.9 / 37.3 | 23.5 / 33.4 | 19.7 / 15.3 | 5.9 / 3.8 | 3.9 / 2.9 |
| 47 | 35 Management, Auditing and Business Consulting | Serv. | High | 2.20 | 0.50 | 31.9 / 34.4 | 42.8 / 41.9 | 17.9 / 9.0 | 4.0 / 2.9 | 2.5 / 2.0 |
| 46 | 47 Health Occ.s with Approbation | Serv. | High | 0.63 | 0.37 | 23.4 / 30.9 | 26.0 / 34.2 | 13.7 / 7.3 | 4.5 / 4.5 | 27.5 / 13.3 |
| 45 | 50 Teachers | Serv. | High | 0.69 | 0.32 | 19.0 / 39.1 | 63.9 / 37.2 | 12.3 / 9.4 | 1.6 / 1.8 | 1.9 / 5.9 |
| 44 | 49 Social Occ.s | Serv. | Low | 0.50 | 0.31 | 11.1 / 26.9 | 65.9 / 34.2 | 7.6 / 12.8 | 0.9 / 0.9 | 14.0 / 17.2 |
| 43 | 28 Wholesale/Retail Dealers | Serv. | Mid | 1.93 | 0.25 | 13.7 / 32.8 | 48.9 / 47.0 | 26.0 / 3.4 | 8.0 / 5.5 | 2.7 / 3.9 |
| 42 | 31 Advertising Specialists | Serv. | High | 0.18 | 0.25 | 29.9 / 35.5 | 45.7 / 39.8 | 17.1 / 16.0 | 4.0 / 2.4 | 1.8 / 1.3 |
| 41 | 53 Hotel, Restaurant and Housekeeping Occ.s | Serv. | Low | 1.41 | 0.20 | 5.7 / 17.7 | 32.0 / 36.7 | 10.4 / 10.0 | 7.0 / 7.2 | 44.4 / 20.7 |
| 40 | 16 Cooks | Serv. | Low | 1.19 | 0.17 | 4.6 / 12.4 | 14.1 / 22.4 | 7.0 / 12.3 | 36.8 / 19.1 | 35.1 / 27.2 |
| 39 | 36 Public Administration Occ.s | Serv. | High | 0.16 | 0.16 | 42.3 / 41.1 | 31.2 / 32.9 | 22.1 / 11.8 | 2.3 / 0.8 | 1.5 / 1.2 |
| 38 | 22 Chemists, Phyisicists, Natural Scientists | Techn. | High | 0.38 | 0.14 | 48.1 / 35.3 | 22.0 / 27.0 | 13.7 / 23.8 | 8.0 / 4.7 | 2.9 / 2.2 |
| 37 | 41 Personal Protection and Guarding | Serv. | Low | 0.46 | 0.13 | 15.7 / 23.0 | 21.2 / 19.6 | 4.0 / 23.6 | 6.1 / 7.2 | 52.3 / 18.3 |
| 36 | 44 Legal Occ.s | Serv. | High | 0.06 | 0.11 | 50.4 / 38.1 | 30.0 / 41.7 | 15.1 / 4.7 | 1.2 / 1.2 | 3.2 / 1.4 |
| 35 | 51 Publication, Library, Translation and related Scientific Occ.s | Serv. | High | 0.38 | 0.10 | 29.7 / 39.5 | 48.9 / 31.2 | 18.5 / 19.8 | 2.0 / 2.8 | 0.9 / 1.3 |
| 34 | 45 Artists and Musicians | Serv. | Mid | 0.21 | 0.05 | 9.1 / 32.7 | 55.0 / 38.8 | 9.8 / 5.3 | 16.1 / 6.6 | 6.6 / 8.8 |
| 33 | 32 Traffic Occ.s | Serv. | Low | 3.52 | 0.04 | 4.1 / 15.7 | 10.2 / 14.5 | 6.3 / 8.3 | 62.9 / 34.4 | 15.5 / 20.1 |
| 32 | 42 Janitors | Serv. | Mid | 0.40 | 0.03 | 3.3 / 22.5 | 8.8 / 15.7 | 18.0 / 10.0 | 16.6 / 16.1 | 44.1 / 32.1 |
| 31 | 43 Security | Serv. | High | 0.06 | 0.01 | 42.1 / 34.3 | 18.4 / 27.8 | 16.0 / 9.4 | 5.3 / 4.3 | 16.7 / 11.8 |
| 30 | 25 Surveyors | Techn. | Mid | 0.07 | 0.00 | 48.3 / 23.1 | 11.0 / 23.4 | 24.8 / 42.2 | 6.6 / 3.5 | 2.9 / 0.6 |
| Sum/Average† | | - | - | 35.07 | 9.85 | 21.4 / 33.0 | 26.5 / 28.6 | 23.7 / 12.5 | 14.8 / 7.6 | 12.1 / 10.5 |
| **Declining Occupations:** | | | | | | | | | | |
| 29 | 46 Designers, Fotographers, Promoters | Serv. | Mid | 0.30 | 0.00 | 31.4 / 38.6 | 29.7 / 38.4 | 16.0 / 11.5 | 12.0 / 6.5 | 6.6 / 1.5 |
| 28 | 33 Aviation and Seafaring Occ.s | Serv. | High | 0.20 | -0.04 | 11.9 / 18.2 | 17.3 / 18.6 | 12.8 / 36.3 | 30.8 / 13.6 | 21.8 / 5.1 |
| 27 | 40 Office assistants, telephonists | Serv. | Mid | 0.94 | -0.06 | 28.1 / 50.0 | 12.9 / 17.6 | 35.4 / 11.5 | 18.8 / 10.7 | 4.1 / 2.8 |
| 26 | 10 Precision Mechanics | Manuf. | Mid | 0.70 | -0.07 | 10.4 / 19.9 | 11.7 / 22.0 | 19.0 / 20.5 | 27.1 / 19.6 | 19.7 / 14.4 |
| 25 | 29 Banking/Insurance Professionals | Serv. | High | 3.52 | -0.09 | 18.4 / 28.6 | 46.3 / 36.8 | 31.5 / 26.3 | 3.2 / 1.1 | 0.6 / 1.2 |
| 24 | 34 Packager, Warehouse and Transport Workers | Serv. | Low | 4.13 | -0.09 | 5.9 / 22.2 | 11.1 / 17.5 | 12.3 / 9.3 | 60.6 / 32.4 | 8.2 / 10.5 |
| 23 | 52 Personal and Body Care Occ.s | Serv. | Low | 0.63 | -0.11 | 6.1 / 21.9 | 42.0 / 40.6 | 7.2 / 3.8 | 7.0 / 5.3 | 37.0 / 18.8 |
| 22 | 26 Technical Specialists | Techn. | High | 0.62 | -0.11 | 35.4 / 43.9 | 9.3 / 8.8 | 25.6 / 20.2 | 12.8 / 14.7 | 3.9 / 10.2 |
| 21 | 14 Bakers, Confectioners, Candy Prod. | Manuf. | Low | 0.59 | -0.12 | 6.4 / 8.0 | 19.4 / 19.3 | 7.9 / 14.8 | 60.1 / 27.9 | 3.5 / 24.6 |
| 20 | 30 Other Mercantile Occ.s (excl. Retail/Wholesale/Banking) | Serv. | Mid | 2.01 | -0.12 | 14.2 / 27.2 | 50.6 / 40.4 | 22.1 / 16.1 | 9.2 / 6.4 | 2.8 / 4.2 |
| 19 | 17 Beverage, Luxury Foods and Other Food Prod. | Manuf. | Low | 0.45 | -0.15 | 10.9 / 14.5 | 13.3 / 14.1 | 12.8 / 10.9 | 46.9 / 28.5 | 11.5 / 23.8 |
| 18 | 24 Technical Drawers | Techn. | Mid | 0.68 | -0.16 | 61.1 / 60.1 | 5.3 / 13.2 | 24.2 / 19.1 | 3.4 / 3.2 | 0.7 / 1.5 |
| 17 | 15 Butchers | Manuf. | Low | 0.54 | -0.16 | 8.0 / 15.1 | 21.4 / 18.9 | 9.2 / 26.3 | 52.3 / 24.5 | 5.3 / 10.4 |
| 16 | 54 Cleaning and Disposal | Serv. | Low | 2.56 | -0.21 | 4.5 / 14.4 | 6.6 / 17.2 | 3.8 / 12.8 | 19.1 / 11.4 | 64.2 / 36.6 |
| 15 | 03 Stone, Constr. Material, Ceramics/Glas Prod. and Processing | Manuf. | Low | 0.46 | -0.26 | 5.4 / 11.3 | 5.8 / 11.1 | 13.8 / 14.7 | 48.8 / 42.5 | 16.7 / 15.3 |
| 14 | 27 Salespersons (Retail) | Serv. | Low | 5.88 | -0.29 | 4.1 / 19.5 | 65.2 / 44.5 | 12.3 / 7.9 | 14.4 / 8.8 | 3.1 / 11.9 |
| 13 | 37 Finance, Accounting, Bookkeeping | Serv. | High | 1.13 | -0.31 | 31.5 / 41.0 | 14.5 / 22.1 | 51.8 / 25.6 | 2.1 / 0.9 | 0.1 / 1.6 |
| 12 | 23 Technicians | Techn. | High | 4.06 | -0.32 | 28.6 / 26.9 | 18.5 / 21.9 | 20.8 / 30.7 | 13.6 / 7.9 | 9.7 / 6.9 |
| 11 | 09 Vehicle and Aircraft Constr. and Maintenance | Manuf. | Low | 1.89 | -0.36 | 4.4 / 21.9 | 10.8 / 16.8 | 19.3 / 14.7 | 20.3 / 17.0 | 32.4 / 24.9 |
| 10 | 12 Spinners, Textile Prod. and Refinement | Manuf. | Low | 0.54 | -0.40 | 2.0 / 25.2 | 7.4 / 8.7 | 4.4 / 19.4 | 68.1 / 23.5 | 15.1 / 18.1 |
| 9 | 11 Electrics Occ.s | Manuf. | Mid | 3.20 | -0.65 | 7.0 / 24.1 | 7.8 / 16.4 | 20.8 / 21.2 | 25.2 / 12.2 | 24.9 / 21.0 |
| 8 | 05 Paper Prod. and Processing, Printing | Manuf. | Mid | 1.63 | -0.74 | 12.6 / 25.4 | 10.8 / 17.2 | 14.6 / 12.5 | 48.6 / 25.5 | 8.6 / 12.6 |
| 7 | 19 Goods inspection, Preparation for Shipment | Manuf. | Mid | 2.26 | -0.74 | 12.0 / 35.0 | 4.8 / 14.9 | 14.1 / 23.9 | 57.0 / 12.9 | 3.8 / 7.0 |
| 6 | 08 Industrial Mechanics and Tool Makers | Manuf. | Mid | 4.09 | -0.94 | 5.4 / 18.3 | 5.5 / 12.7 | 19.4 / 15.2 | 36.2 / 27.5 | 19.3 / 19.9 |
| 5 | 04 Chemistry and Plastics Prod. | Manuf. | Mid | 2.88 | -0.96 | 10.8 / 22.8 | 4.7 / 11.6 | 10.8 / 15.8 | 55.4 / 29.8 | 11.6 / 14.2 |
| 4 | 13 Textile Processing and Leather Prod. | Manuf. | Low | 1.22 | -0.97 | 4.7 / 12.8 | 9.7 / 22.0 | 3.2 / 12.7 | 58.3 / 34.0 | 23.3 / 13.2 |
| 3 | 07 Metal, Plant and Sheet Metal Constr., Installation and Assembly | Manuf. | Mid | 6.44 | -1.74 | 3.7 / 13.1 | 5.7 / 15.8 | 17.5 / 19.4 | 34.0 / 24.5 | 25.9 / 21.5 |
| 2 | 06 Metal Prod. and Processing | Manuf. | Mid | 4.08 | -1.82 | 3.3 / 18.5 | 3.3 / 10.0 | 15.0 / 18.3 | 51.0 / 32.6 | 15.3 / 15.4 |
| 1 | 18 Constr. and Wood/Plastics Processing | Constr. | Low | 7.31 | -2.87 | 3.8 / 13.3 | 7.9 / 23.0 | 15.8 / 15.1 | 37.0 / 19.2 | 24.1 / 21.9 |
| Sum/Average† | | - | - | 64.93 | -14.86 | 9.6 / 22.2 | 17.1 / 22.8 | 16.9 / 17.0 | 32.5 / 17.1 | 16.4 / 14.6 |

*Notes:* Share = employment share, wGR = employment growth rate weighted by 1990 employment share, Manuf. = Manufacturing, Constr. = Construction, Techn. = Technical, Serv. = Service. NRA = Non-routine analytical, NRI = non-routine interactive, RC = routine cognitive, RM = routine manual, NRM = non-routine manual. Underlined figures mark the occupations' main task in 1986/2012, i.e. the task category with the largest intensity. Mean task intensities are weighted by occupational employment. Occupation categories are based on KldB1988 1-digit codes (*Berufsbereiche*). The categorization of wages is based on the terciles of the West German distribution of occupational mean wages in 1990, as provided by Dauth (2014). † This line provides the column sum for the 1990 employment share and weighted employment growth rate of 1990-2010, as well as the column average for the task intensities in the GQCS waves of 1986/2012.

*Source:* BeH, GQCS.

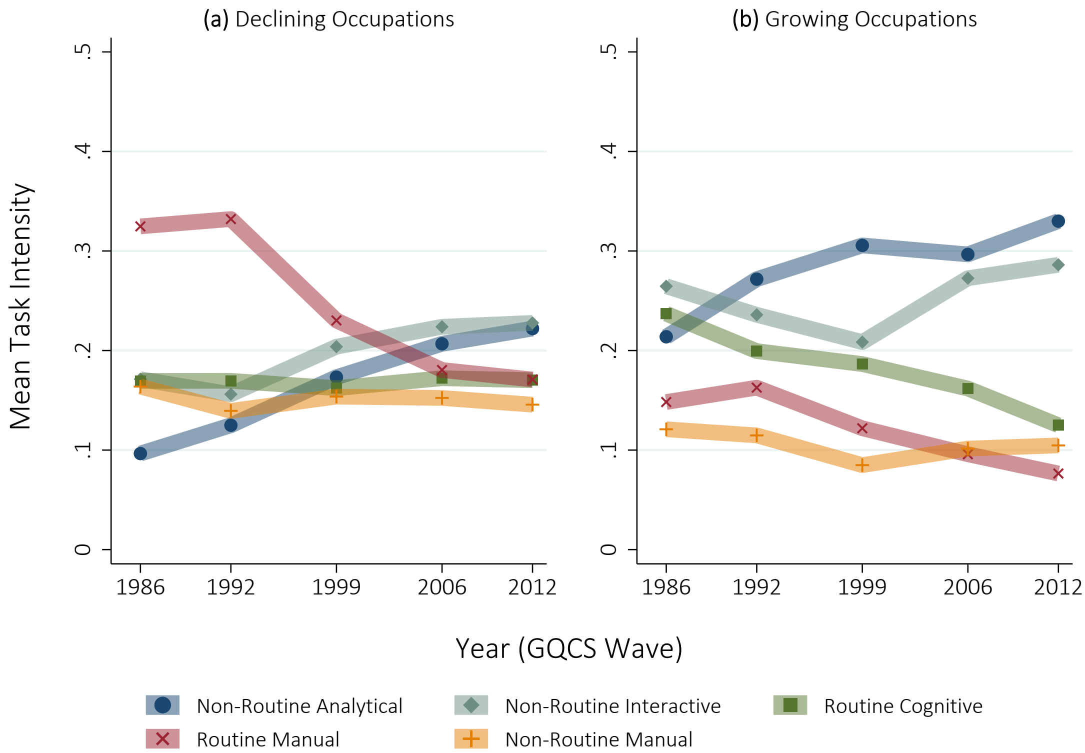
**Table A.2.2.:** Base Year Characteristics of Displaced RM Workers by Region Type

| | (1) | (2) | (3) | (4) | | (5) | |
|---|---|---|---|---|---|---|---|
| | | Region Type | | Difference | | | |
| | R1 | R2 | R3 | R1 - R2 | | R1 - R3 | |
| **PS matching variables:** | | | | | | | |
| Worker: | | | | | | | |
| Log real wage in $c-1$ | 4.51 | 4.51 | 4.46 | -0.01 | | 0.05 | ** |
| Log real wage in $c-2$ | 4.52 | 4.52 | 4.48 | -0.01 | | 0.04 | |
| Female | 0.14 | 0.13 | 0.15 | 0.01 | | -0.01 | |
| Age | 38.06 | 37.68 | 37.81 | 0.38 | ** | 0.25 | |
| Low-skilled | 0.24 | 0.16 | 0.14 | 0.07 | *** | 0.09 | *** |
| Medium-skilled | 0.75 | 0.82 | 0.84 | -0.07 | *** | -0.10 | *** |
| High-skilled | 0.01 | 0.01 | 0.01 | 0.00 | | 0.00 | |
| Experience | 15.89 | 15.72 | 16.50 | 0.17 | | -0.61 | ** |
| Establishment tenure | 10.41 | 10.38 | 10.38 | 0.04 | | 0.04 | |
| Displacement year | 1998.89 | 1998.45 | 2000.70 | 0.44 | | -1.81 | ** |
| Occupation: | | | | | | | |
| Production, crafts | 0.83 | 0.77 | 0.83 | 0.06 | *** | 0.00 | |
| Service occupations | 0.17 | 0.23 | 0.17 | -0.06 | *** | 0.00 | |
| Establishment: | | | | | | | |
| 10-49 employees | 0.29 | 0.27 | 0.30 | 0.02 | | -0.01 | |
| 50-99 employees | 0.19 | 0.17 | 0.23 | 0.02 | | -0.04 | |
| 100-249 employees | 0.26 | 0.24 | 0.25 | 0.02 | | 0.01 | |
| > 249 employees | 0.26 | 0.32 | 0.22 | -0.06 | | 0.04 | |
| Establishment age | 40.27 | 39.31 | 38.93 | 0.97 | *** | 1.34 | *** |
| Median wage | 78.93 | 78.37 | 77.64 | 0.55 | | 1.29 | |
| Industry: | | | | | | | |
| Raw Materials and Goods | 0.10 | 0.11 | 0.09 | -0.01 | | 0.01 | |
| Metal, Machinery, Automotive | 0.28 | 0.23 | 0.24 | 0.05 | | 0.03 | |
| Consumption Goods | 0.19 | 0.16 | 0.26 | 0.04 | | -0.07 | * |
| Construction | 0.18 | 0.17 | 0.15 | 0.01 | | 0.03 | |
| Wholesale, Retail | 0.11 | 0.11 | 0.10 | 0.00 | | 0.01 | |
| Business Services, Transport | 0.09 | 0.16 | 0.11 | -0.07 | *** | -0.02 | |
| Priv. Services, Educ., Social Sector | 0.05 | 0.07 | 0.04 | -0.02 | | 0.01 | |
| Region: | | | | | | | |
| Active population (1k) † | 441.51 | 375.76 | 130.57 | 65.75 | ** | 310.94 | *** |
| Population density (pop/km$^2$) † | 834.56 | 425.45 | 169.68 | 409.11 | *** | 664.88 | *** |
| UE rate ‡ | 0.08 | 0.09 | 0.07 | -0.01 | *** | 0.02 | *** |
| Weight. Growth Rate RM occ. $(c, c-10)$ [pct] | -6.89 | -4.17 | 0.58 | -2.72 | *** | -7.47 | *** |
| **Not in PS matching:** | | | | | | | |
| AKM worker FE ¶ | 4.26 | 4.27 | 4.29 | -0.01 | | -0.03 | * |
| AKM establishment FE § | 0.20 | 0.19 | 0.16 | 0.01 | | 0.04 | ** |
| Observations | 15,036 | 15,248 | 7,586 | | | | |

*Notes:* PS = Propensity Score; UE = Unemployment; Weight. Growth Rate RM occ. $(c, c-10)$ = Regional weighted growth rate of RM occupations over decade preceding base year $c$; FE = Fixed Effect; RM occ. = Occupations with mainly routine manual tasks. The table compares the average base year $c$ characteristics of displaced workers in different region types (R1/R2/R3: Strong/medium/weak local RM bias), see Section 1.2.3). Establishment characteristics are measured in $c-1$. AKM FE in the most recent time period available before year $c$. For a description of AKM fixed effects see Section 1.3.4 and Bellmann et al. (2020). ***/**/* mark significant differences at the 1/5/10% significance level. # The weighted growth rate of RM occupations differs between region types by definition. ¶ Lower observation numbers because of missing values: 15,180 in R1; 14,248 in R2; 8,052 in R3. § Lower observation numbers because of missing values: R1: 15,504; R2: 14,539; R3: 8,262.
*Data:* BHP, IEB, BeH, GQCS, † The European Regional Database (EUI, 2021), ‡ Statistical Office of the Federal Employment Agency.

### A.2.2. Supplementary Figures

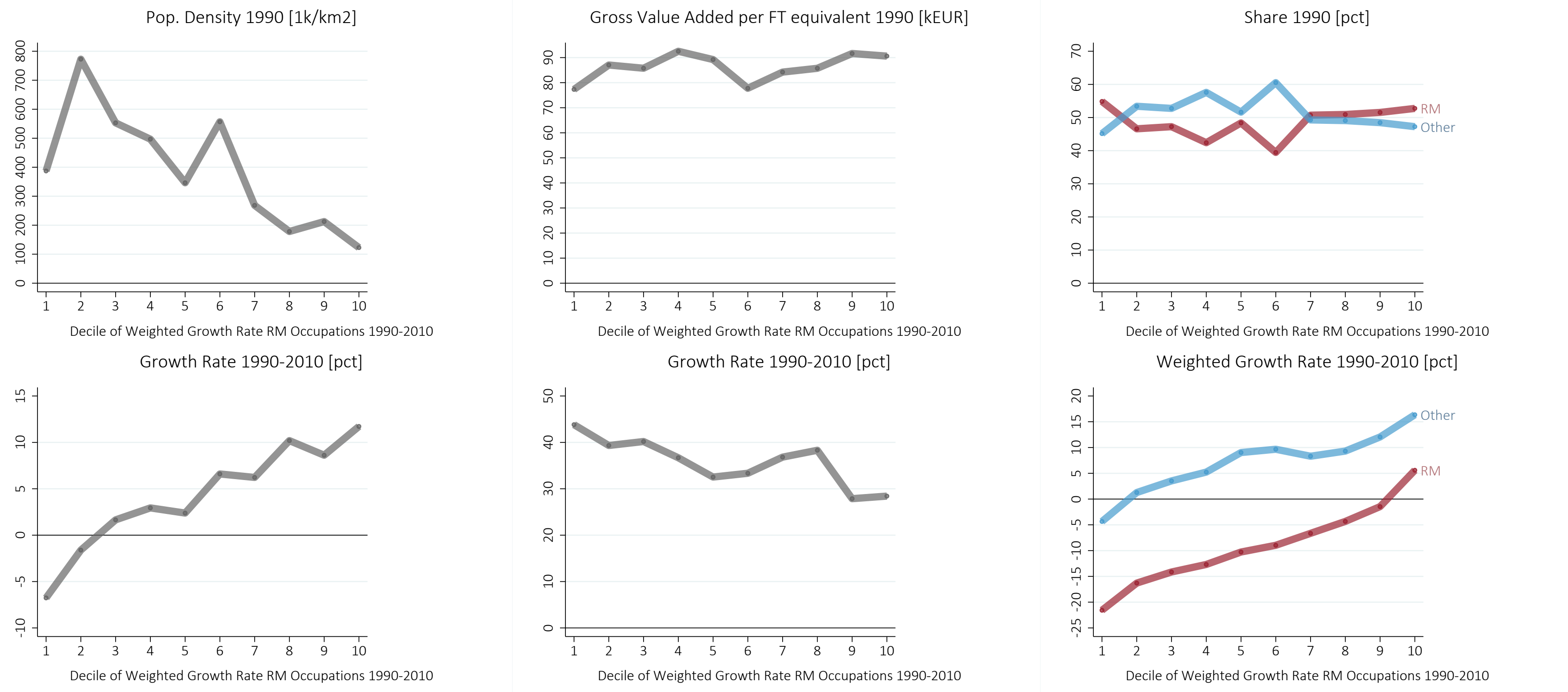
**Figure A.1.:** Task Content of Declining and Growing Occupations 1986-2012




*Notes:* The figure plots shifts in the average task intensity of declining and growing occupations (below/above rank 30 in Figure 1.1). Averages are weighted by occupational employment in the year of the respective GQCS wave.
*Data:* GQCS, BeH.

**Figure A.2.:** Initial Region Characteristics and Change over Time

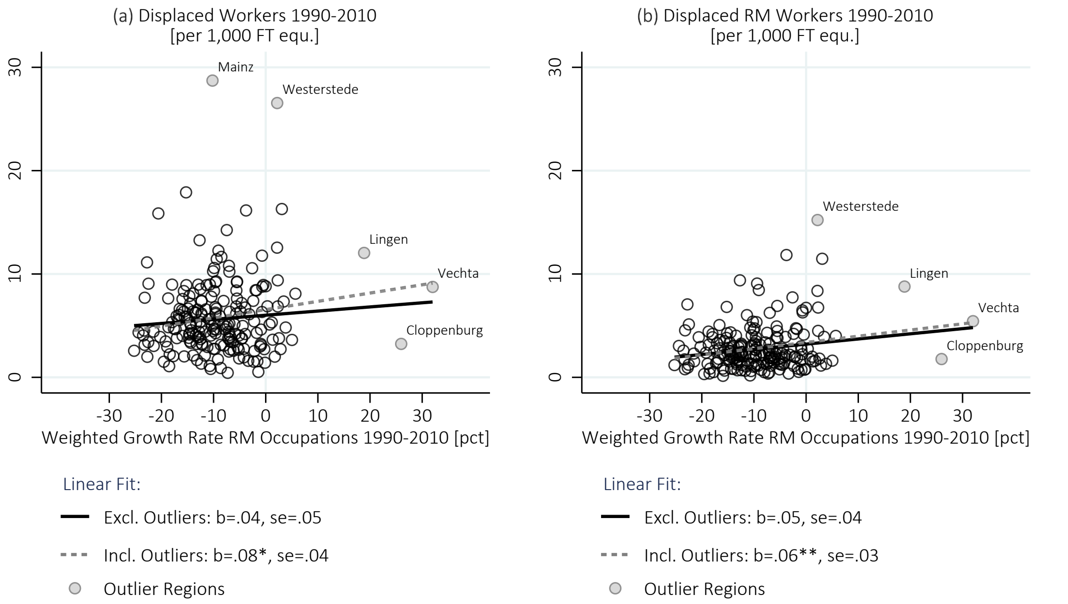
**(a)** Population Density **(b)** Labor Productivity **(c)** RM/Other Occupations




*Notes:* FT = Full-time.
Residual category "Other industries" omitted from the graph for ease of display. The x-axis refers to the deciles of the distribution of weighted regional growth in RM occupations between 1990 and 2010 (i.e. the 'red bars' in Figure 1.2, see also the formula for $\Delta_{LR} E_r^{RM}$ in Appendix A.1.2). Growth rates are weighted with the initial employment share in 1990. The growth rates of RM/Other occupations within these deciles plotted in the lower panel are computed in the same way. For population density and labor productivity growth rates are unweighted. Region definitions (and thus a region's area) are time-invariant, such that increases in population density imply absolute population growth.

*Data:* European Regional Database (EUI, 2021), BeH, GQCS.

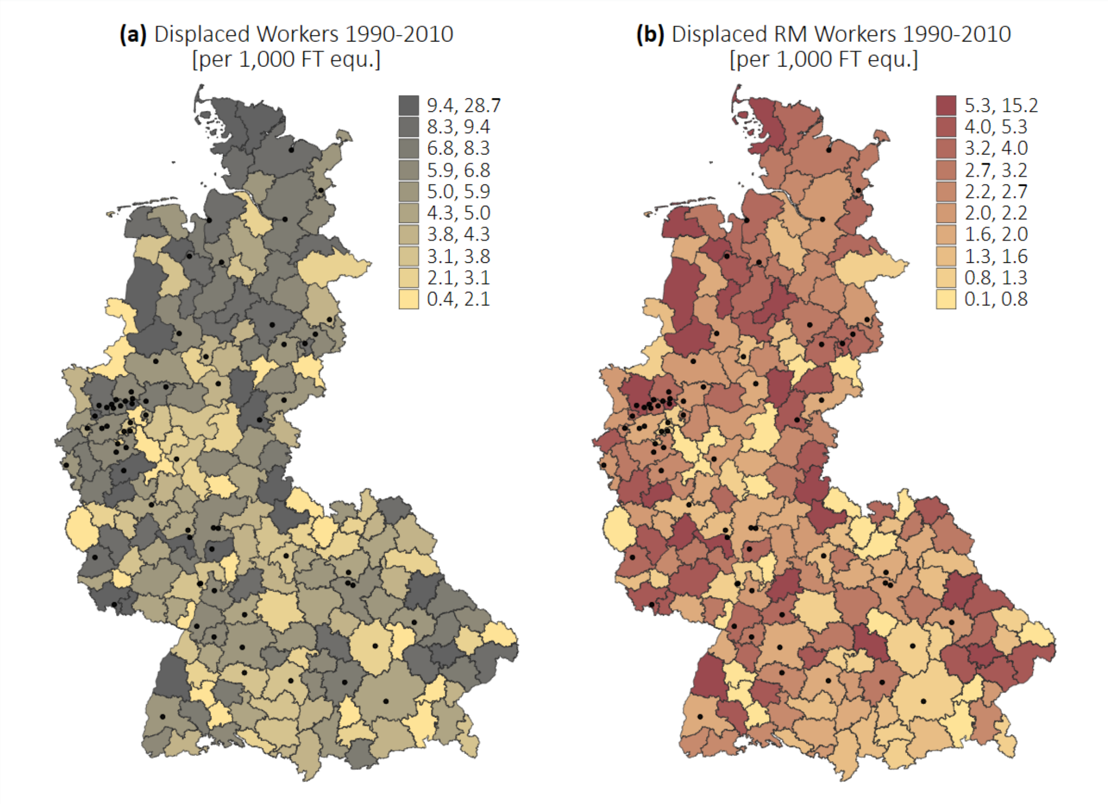
**Figure A.3.:** Local Incidence of RM-biased Structural Change and Displacement, 1990-2010




*Notes:* Displ. = Displaced, RM = Workers in occupations with mainly routine manual tasks, FT equ. = FT equivalents. The vertical axis represents the number of displaced workers as defined in Section 1.3.1 over the number of full-time equivalent employment in 1990. The horizontal axis refers to the weighted regional growth in RM occupations between 1990 and 2010. Growth rates are weighted with the initial employment share in 1990 (see the formula for $\Delta_{LR} E_r^{RM}$ in Appendix A.1.2). The labelled dots represent outliers with an exceptionally high number of displaced workers or exceptionally strong growth of RM occupations. The dashed (solid) regression line includes (excludes) these outliers. The fitted lines are derived from linear regressions that control for initial regional characteristics in 1990 (population density, gross value added, gross value added per full-time equivalent employment, industry and establishment size structure of employment). The legend provides the coefficient estimate *b* and its standard error *se* from the linear model including/excluding the outlier regions. ***/**/* mark significant differences at the 1/5/10% significance level.

*Data:* BHP, IEB, BeH, GQCS.

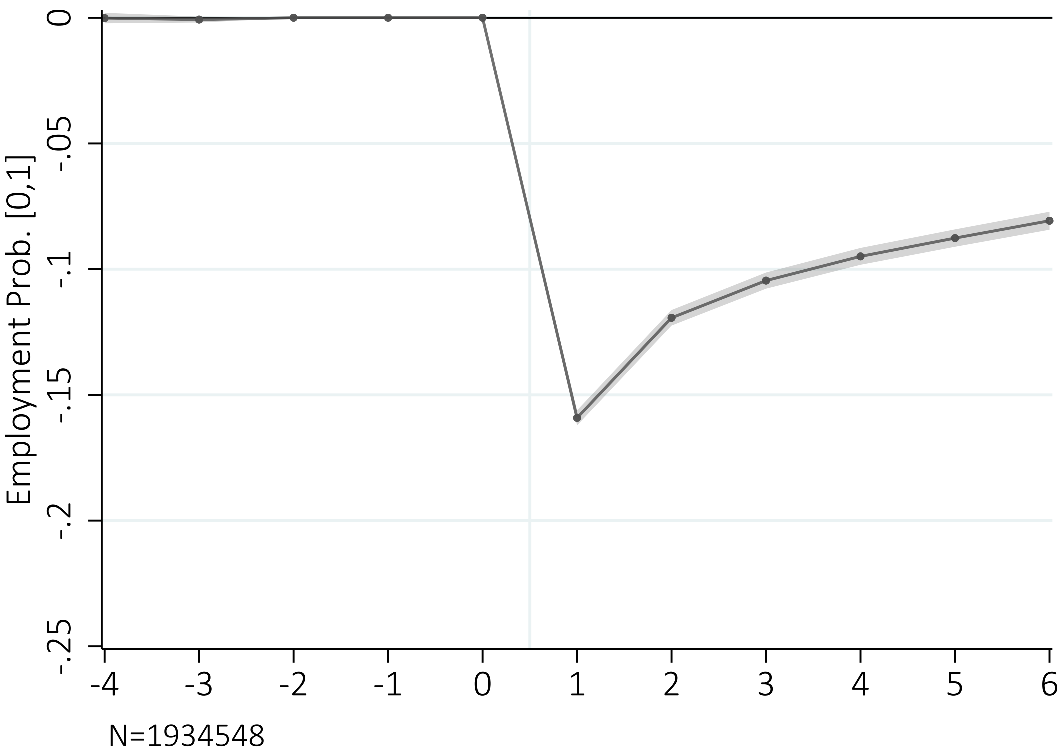
**Figure A.4.:** Spatial Distribution of RM-Biased Structural Change and Displacement across West German Local Labor Market Regions, 1990-2010




*Notes:* Disp. = Displaced, RM = Workers in occupations with mainly routine manual tasks, FT equ. = FT equivalents. Map (a) plots the total number of displaced workers (between 1990 and 2010) per 1,000 FT equivalents (as of 1990), map (b) plots the number of workers displaced from RM occupations (between 1990 and 2010) per 1,000 FT equivalents (as of 1990). Boundaries mark West German local labor market regions as defined by BBSR (2021). Black dots mark cities with 100,000 inhabitants or more.

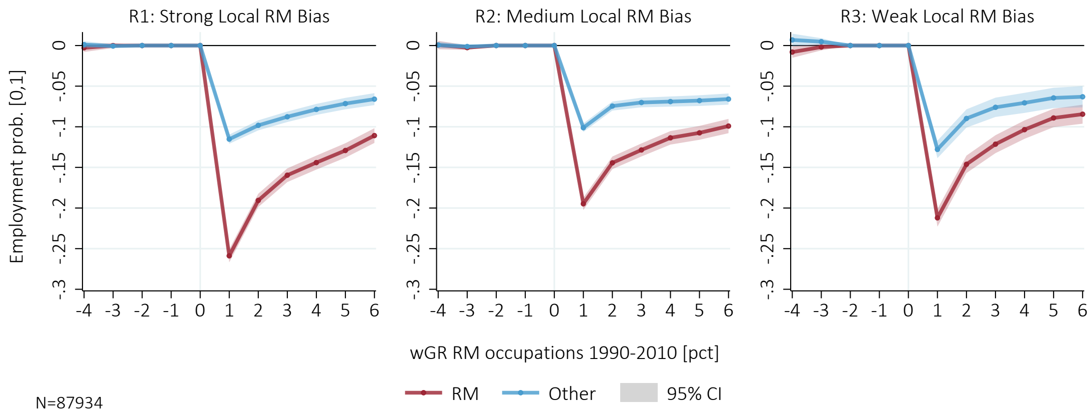
*Data:* IEB, BHP, BeH, GQCS.

**Figure A.5.:** Average Employment Effects of Displacement
(All Workers, Event Study)



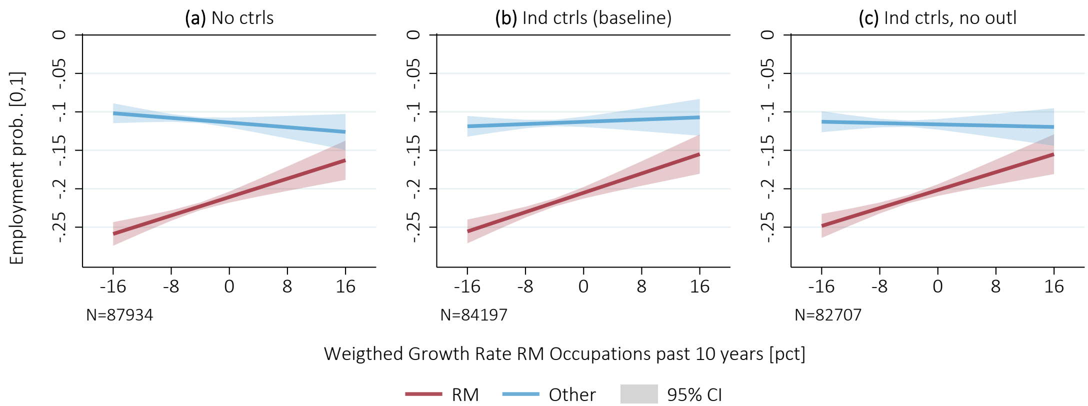

*Notes:* The plot shows coefficient estimates (dots) and 99% confidence intervals (shaded area) from the event study model in equation (1.1) with additionally controlling for base year occupation (RM/Other) and region type (R1/R2/R3) based on the full sample of treated and control individuals. Standard errors are clustered at the individual level.
*Data:* BHP, IEB, BeH.

**Figure A.6.:** Reproducing the Event Study Estimates by Region Type and Main-Task with Matched DiD



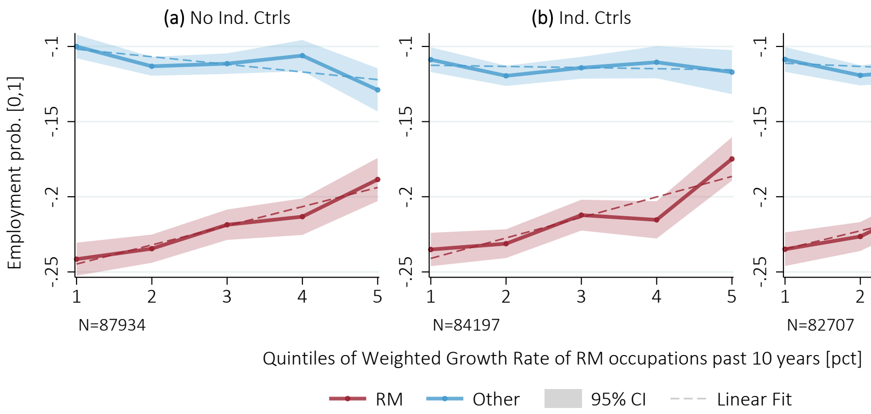

*Notes:* RM = Workers in occupations with mainly routine manual tasks, Other = Workers in occupations with other main tasks, CI = Confidence interval. Based on equation (1.3) in Section 1.3.4, where $grRM^{c-10}$ is replaced by dummies for region types R1/R2/R3 (Strong/medium/weak local RM bias) and no control variables are included in order to reproduce the event study model in equation 1.1. Standard errors are clustered at the individual level. Average weighted growth rate within region types: R1=−17.0%, R2=−9.7%, R3=−0.7%.
*Data:* BHP, IEB, BeH, GQCS.

**Figure A.7.:** Employment Effects along the Structural Change Distribution, Robustness Checks I
(matched DiD, various specifications, t=1)



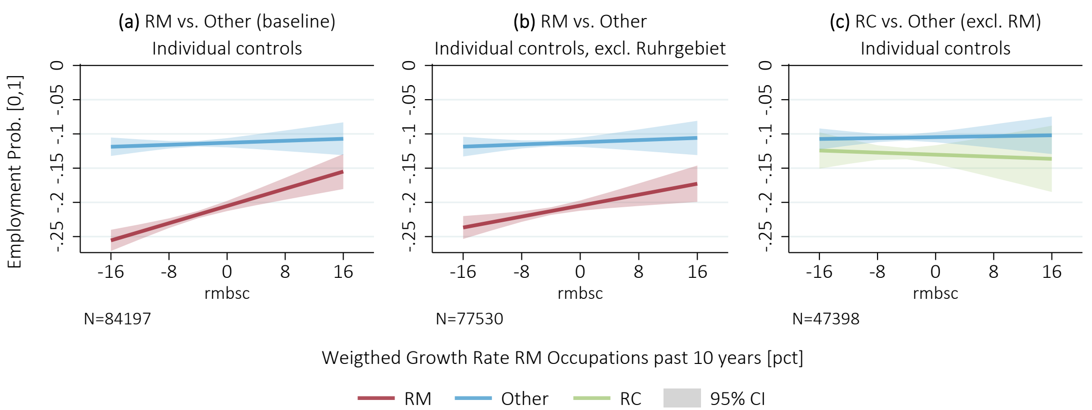

*Notes:* RM = Workers in occupations with mainly routine manual tasks, Other = Workers in occupations with other main tasks, Ind ctrls = individual control variables, Outl = outliers, CI = Confidence interval. Based on equation (1.3) in Section 1.3.4. The x-axis refers to the weighted regional growth in RM occupations over the decade preceding the base year $c$. Growth rates are weighted with the initial employment share in $c-10$ (see the formula for $grRM_r^{c-10}$ in Appendix A.1.2). Individual control variables include gender, skill level, age, experience, tenure and AKM worker fixed effects. Standard errors are clustered at the individual level. Outliers are defined as labor market regions with average treatment effects below the 1%-ile or above the 99%-ile.
*Data:* BHP, IEB, BeH, GQCS.

**Figure A.8.:** Employment Effects by Quintiles of the Structural Change Distribution, Robustness Checks II
(matched DiD, various specifications, t=1)




*Notes:* RM = Workers in occupations with mainly routine manual tasks, Other = Workers in occupations with other main tasks, Ind ctrls = individual control variables, Outl = outliers, CI = Confidence interval. Based on equation (1.3) in Section 1.3.4, where continuous $grRM$ is replaced with indicator variables for the quintiles of the $grRM$ distribution and their interaction with $I(RM)$. Quintiles are computed from the distribution of the weighted regional growth in RM occupations over the decade preceding the base year $c$. Growth rates are weighted with the initial employment share in $c-10$ (see Appendix A.1.2). Individual control variables include gender, skill level, age, experience, tenure and AKM worker fixed effects. Standard errors are clustered at the individual level. Outliers are defined as labor market regions with average treatment effects below the 1%-ile or above the 99%-ile.
*Data:* BHP, IEB, BeH, GQCS.

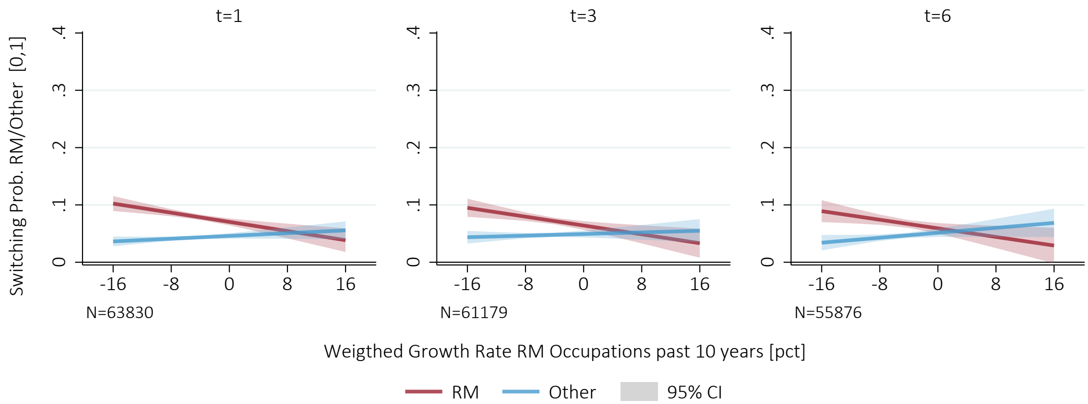
**Figure A.9.:** Employment Effects along the Structural Change Distribution, Robustness Checks III
(matched DiD with ind. controls, various specifications, t=1)



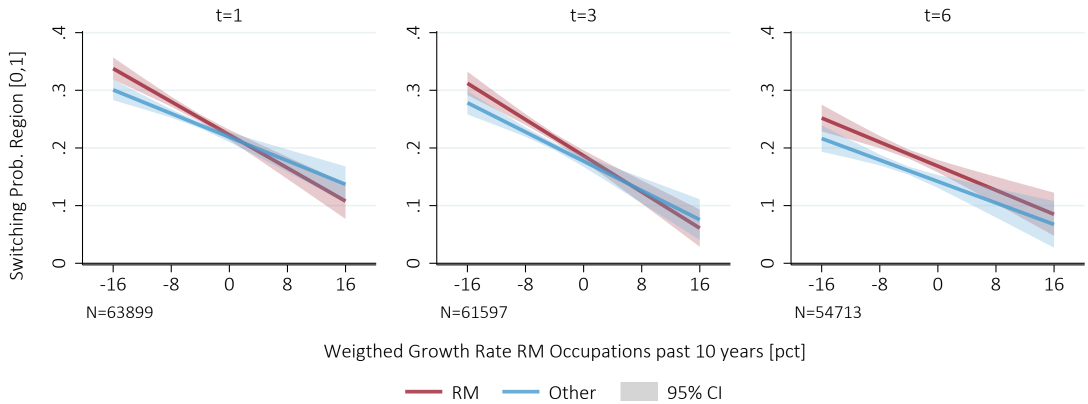

*Notes:* RM = Workers in occupations with mainly routine manual tasks, Other = Workers in occupations with other main tasks, CI = Confidence interval. Based on equation (1.3) in Section 1.3.4. The x-axis refers to the weighted regional growth in RM occupations over the decade preceding the base year $c$. Growth rates are weighted with the initial employment share in $c-10$ (see Appendix A.1.2). Individual controls include gender, skill level, age, experience, tenure, AKM worker fixed effects. Standard errors are clustered at the individual level. Panel (b) shows effects based on a sample that excludes labor market regions in the *Ruhrgebiet*, which is a densely populated area that underwent specific structural changes due to a gradual decline of the coal mining industry. Panel (c) shows the effects for routine cognitive (RC) occupations (as defined by their 1986 main task) and compares them to occupations with a task focus other than that (i.e. non-routine abstract, non-routine interactive or non-routine manual).
*Data:* BHP, IEB, BeH, GQCS.

**Figure A.10.:** Effects on Occupational and Regional Mobility along the Structural Change Distribution
(matched DiD with ind. controls, cond. on re-employment, t=1,3,6)

**(a)** Switch RM/Other




**(b)** Switch Region

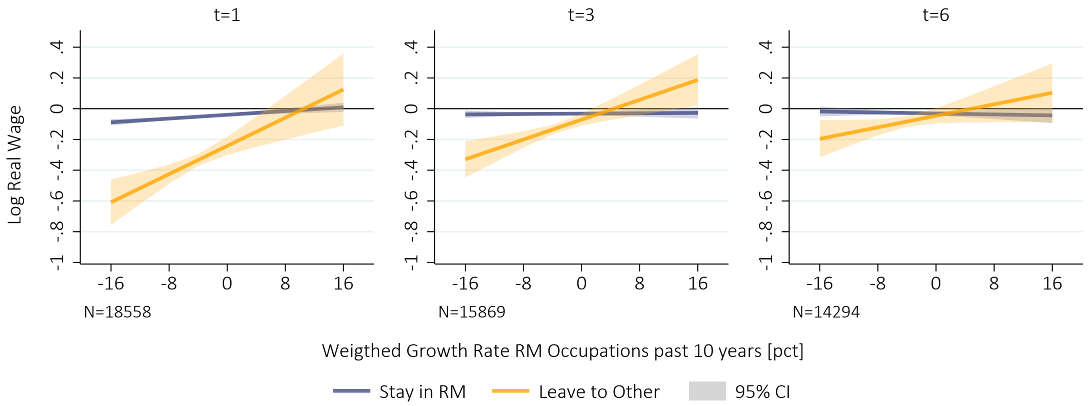



*Notes:* RM = Workers in occupations with mainly routine manual tasks, Other = Workers in occupations with other main tasks, CI = Confidence interval. Based on equation (1.3) in Section 1.3.4. Estimated on the subsample of displaced workers who are re-employed. The x-axis refers to the weighted regional growth in RM occupations over the decade preceding the base year $c$. Growth rates are weighted with the initial employment share in $c-10$ (see the formula for $grRM_r^{c-10}$ in Appendix A.1.2). Individual control variables include gender, skill level, age, experience, tenure and AKM worker fixed effects. Standard errors are clustered at the individual level. Panel (a) shows the probability of working in an occupation with a different main task as compared to the pre-displacement occupation (i.e. switching from RM to Other or *vice versa*). Panel (b) shows the probability of working in a local labor market other than the one in which displacement took place.

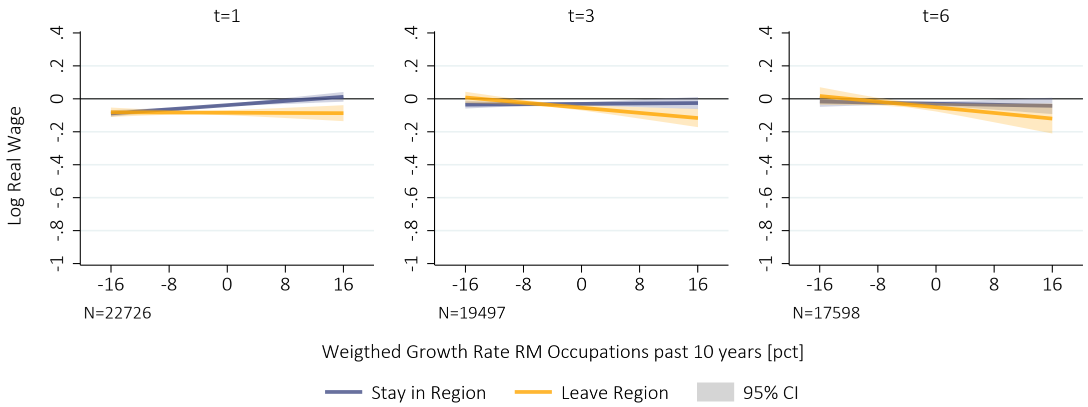
*Data:* BHP, IEB, BeH, GQCS.

**Figure A.11.:** Wage Effects along the Structural Change Distribution by Mobility Choices (RM workers, matched DiD with ind. controls, cond. on re-employment, t=1,3,6)

**(a)** RM Occupation Stayers vs. Switchers




**(b)** Region Stayers vs. Switchers

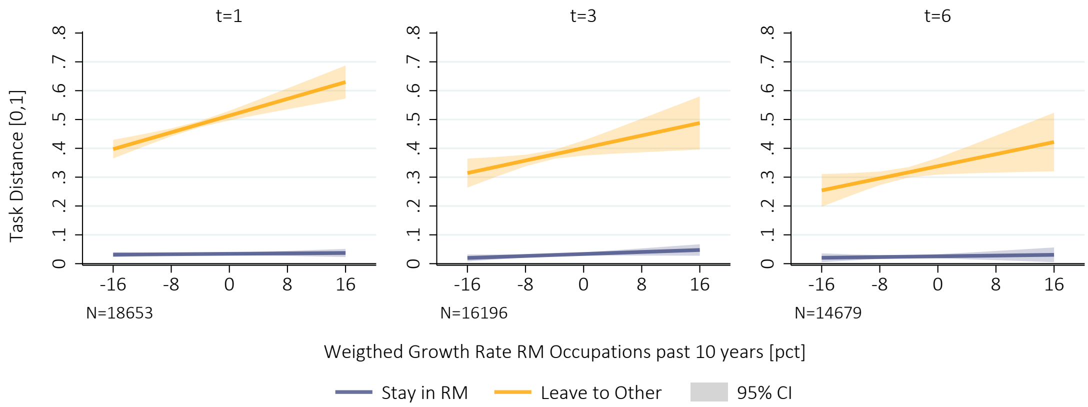



*Notes:* RM = Workers in occupations with mainly routine manual tasks, Other = Workers in occupations with other main tasks, CI = Confidence interval. Based on equation (1.3) in Section 1.3.4, where $I(RM)$ is replaced by an indicator variable for switchers. The sample is restricted to RM workers. Estimated on the subsample of displaced workers who are re-employed. The x-axis refers to the weighted regional growth in RM occupations over the decade preceding the base year $c$. Growth rates are weighted with the initial employment share in $c-10$ (see the formula for $grRM_r^{c-10}$ in Appendix A.1.2). Individual control variables include gender, skill level, age, experience, tenure and AKM worker fixed effects. Standard errors are clustered at the individual level. Panel (a) shows the wage losses of workers displaced from an RM occupation by whether they are re-employed in an RM or other occupation. Panel (b) shows the wage losses of workers displaced from an RM occupation by whether they are re-employed in the same labor market region or a different region.

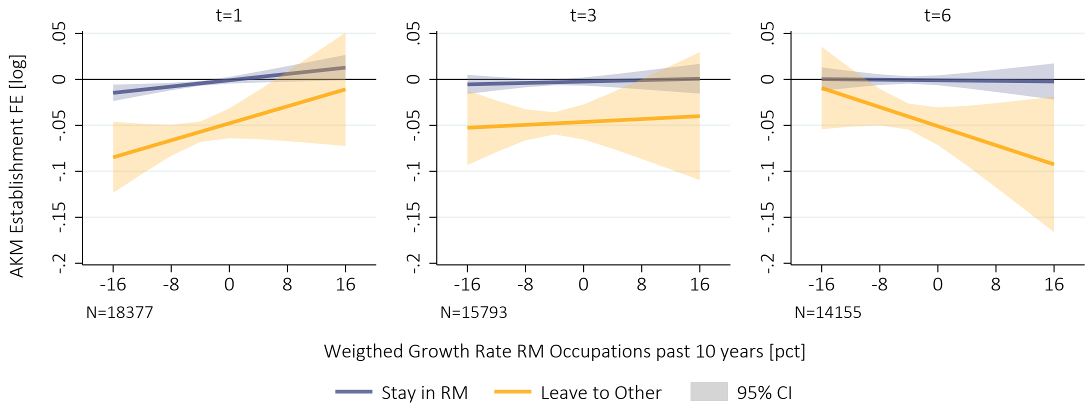
*Data:* BHP, IEB, BeH, GQCS.

**Figure A.12.:** Effects on Task Distance and AKM Establishment Fixed Effects along the Structural Change Distribution, by occupational mobility
(RM workers, matched DiD with ind. controls, cond. on re-employment, t=1,3,6)

**(a)** Task Distance of RM Stayers vs. Leavers




**(b)** AKM Establishment FE of RM Stayers vs. Leavers

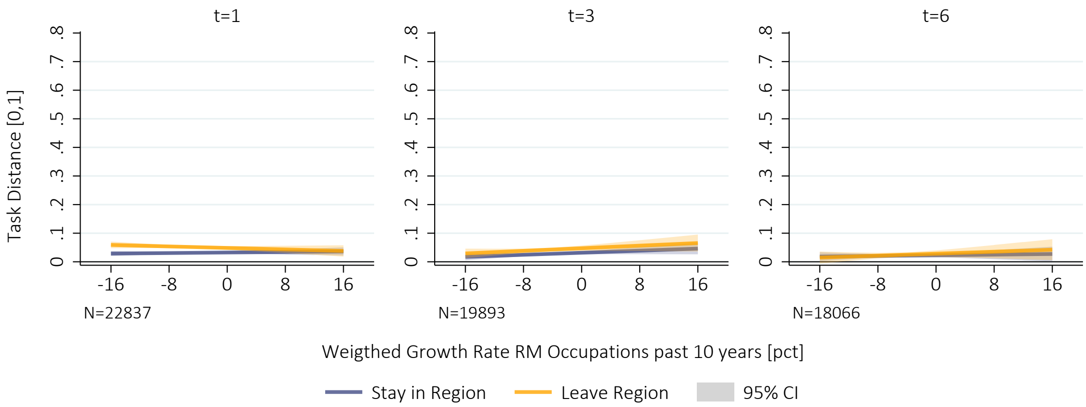



*Notes:* RM = Workers in occupations with mainly routine manual tasks, Other = Workers in occupations with other main tasks, CI = Confidence interval. Based on equation (1.3) in Section 1.3.4, where $I(RM)$ is replaced by an indicator variable for switchers. The sample is restricted to RM workers. Estimated on the subsample of displaced workers who are re-employed. The x-axis refers to the weighted regional growth in RM occupations over the decade preceding the base year $c$. Growth rates are weighted with the initial employment share in $c-10$ (see the formula for $grRM_r^{c-10}$ in Appendix A.1.2). Individual control variables include gender, skill level, age, experience, tenure and AKM worker fixed effects. Standard errors are clustered at the individual level. Panel (a) shows the estimated task distance as a measure of occupational dissimilarity (see Section A.1.1 in the Appendix). Panel (b) shows the estimated losses of AKM establishment fixed effects as a measure of establishment wage premia (see Section 1.3.2).

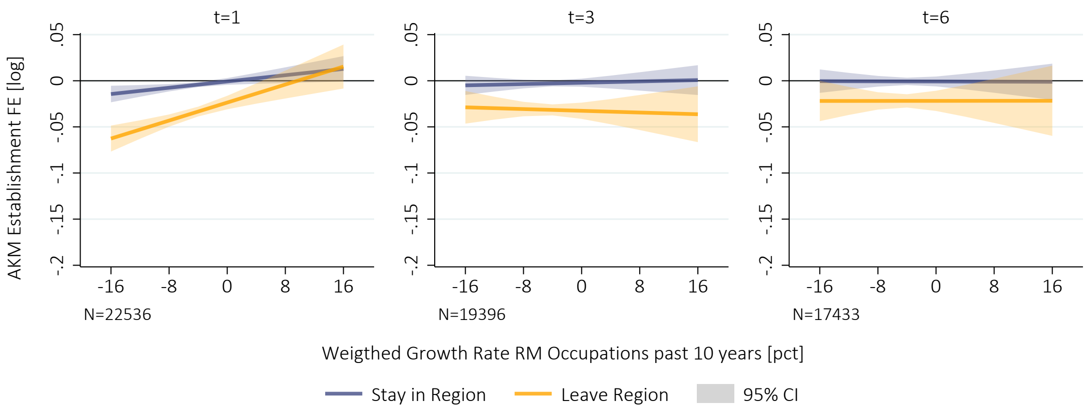
*Data:* BHP, IEB, BeH, GQCS.

**Figure A.13.:** Effects on Task Distance and AKM Establishment Fixed Effects along the Structural Change Distribution, by regional mobility
(RM workers, matched DiD with ind. controls, cond. on re-employment, t=1,3,6)

**(a)** Task Distance of Region Stayers vs. Leavers




**(b)** AKM Establishment FE of Region Stayers vs. Leavers




*Notes:* RM = Workers in occupations with mainly routine manual tasks, Other = Workers in occupations with other main tasks, CI = Confidence interval. Based on equation (1.3) in Section 1.3.4, where $I(RM)$ is replaced by an indicator variable for switchers. The sample is restricted to RM workers. Estimated on the subsample of displaced workers who are re-employed. The x-axis refers to the weighted regional growth in RM occupations over the decade preceding the base year $c$. Growth rates are weighted with the initial employment share in $c-10$ (see the formula for $grRM_r^{c-10}$ in Appendix A.1.2). Individual control variables include gender, skill level, age, experience, tenure and AKM worker fixed effects. Standard errors are clustered at the individual level. Panel (a) shows the estimated task distance as a measure of occupational dissimilarity (see Section A.1.1 in the Appendix). Panel (b) shows the estimated losses of AKM establishment fixed effects as a measure of establishment wage premia (see Section 1.3.2).

*Data:* BHP, IEB, BeH, GQCS.

# B. Appendix: Changes in Occupational Tasks and the Costs of Job Loss

## B.1. Supplementary Results

### B.1.1. Supplementary Tables

**Table B.1.1.:** List of Variables

| Variable Group | Description |
|---|---|
| **Outcomes:** | |
| | Employed |
| | Labor earnings per year (1995 Euros) |
| | Days employed per year |
| | Switching out of baseyear occupation |
| | Female (0/1) |
| **Baseyear Control Variables Main Specification:** | |
| Person | Age (years) |
| | German (0/1) |
| | No professional degree (1/0, omitted reference category) |
| | Vocational training (0/1) |
| | Academic degree (0/1) |
| | No of benefit receipts |
| | No of n-spells |
| | Labor market experience (days) |
| | Labor market experience squared (days) |
| | Job tenure (days) |
| | Job tenure squared (days) |
| | Occupation tenure (years) |
| | Occupation tenure squared (years) |
| | Weighted growth rate of baseyear occupation (Western Germany, 1980-2010) |
| | Log real daily wage in $c-1$ (1995 Euros) |
| | Log real daily wage in $c-2$ (1995 Euros) |
| Industry (0/1) | Agriculture/forestry (omitted reference category) |
| | Pisciculture/fishery |
| | Mining/mineral extraction |
| | Manufacturing |
| | Energy/water supply |
| | Construction |
| | Retail, maintenance and repair of cars and durables |
| | Hospitality |
| | Transportation/communication |
| | Credit/insurance |
| | Real estate/renting of movable goods/business-services |
| | Public administration/defense/social insurance |
| | Education |
| | Health/veterinary/social Care |
| | Other services |
| | Private households |
| Establishment Size (0/1) | <10 (omitted reference category) |
| | 10-50 |
| | 51-100 |
| | 101-250 |
| | 251-500 |
| | >500 |
| **Additional Baseyear Control Variables:** | |
| AKM Fixed Effects (logs) | Person |
| | Establishment |

*Data:* SIAB, † Dauth (2014).

**Table B.1.2.:** Baseyear Characteristics of Displaced and Non-displaced Workers

| | (1) Displaced | (2) Non-Displaced | (3) Diff. (1)-(2) | |
|---|---|---|---|---|
| **Person:** | | | | |
| Female | .393 | .48 | -.087 | + |
| Age | 41.834 | 40.018 | 1.816 | + |
| German | .908 | .928 | -.02 | |
| No professional training | .152 | .141 | .011 | |
| Vocational training | .798 | .771 | .028 | |
| Academic degree | .05 | .088 | -.038 | + |
| Experience | 12.63 | 9.882 | 2.748 | ++ |
| Job tenure | 7.501 | 5.927 | 1.574 | ++ |
| No of benefit receipts | 1.259 | 1.139 | .12 | |
| No of n-spells | 1.031 | 1.383 | -.352 | + |
| AKM person FE | 4.284 | 4.308 | -.024 | |
| **Occupation:** | | | | |
| Within-distance since entry | .004 | .003 | .001 | |
| Occupation tenure | 10.786 | 8.6 | 2.186 | ++ |
| Agriculture | . | . | . | |
| Mining | . | . | . | |
| Manufacturing | .434 | .287 | .147 | ++ |
| Mid/High Wage Services (>=p25) | .056 | .058 | -.002 | |
| Low Wage Services (<p25) | .51 | .655 | -.145 | ++ |
| wGR baseyear occupation (1980-2010) | .002 | .004 | -.002 | + |
| **Industry:** | | | | |
| Agriculture/Fishing/Mining | .004 | .006 | -.002 | |
| Manufacturing/Energy/Construction | .523 | .346 | .177 | ++ |
| Trade/Hospitality/Traffic/Communication | .336 | .277 | .059 | + |
| Credit/Real estate/Public Sector | .088 | .194 | -.107 | ++ |
| Education/Health/Other services | .047 | .172 | -.125 | ++ |
| **Establishment:** | | | | |
| Establishment size | 54.584 | 108.208 | -53.624 | ++ |
| <10 | .23 | .198 | .032 | |
| 10-50 | .465 | .295 | .17 | ++ |
| 51-100 | .148 | .147 | .001 | |
| 101-250 | .117 | .206 | -.089 | + |
| 251-500 | .039 | .153 | -.114 | ++ |
| >500 | . | . | . | |
| Median Daily Wage | 67.082 | 72.891 | -5.809 | + |
| AKM establishment FE | .113 | .112 | .002 | |
| **Outcomes:** | | | | |
| Labor earnings per Year | 33923.408 | 34618.6 | -695.192 | |
| Employed | 1 | 1 | 0 | |
| Days employed per year | 360.727 | 361.512 | -.786 | |
| Switch occupation | 0 | 0 | 0 | |
| Log real daily wage | 4.422 | 4.408 | .014 | |
| min(N) | 8149 | 224403 | | |
| max(N) | 14141 | 457693 | | |

*Notes:* Columns (1) and (2) show the mean baseyear characteristics of displaced and non-displaced workers. Column (3) provides the difference between both groups and its significance in terms of the absolute value of the standardized difference: $^{+}$ marks 'marginal' differences between 0.1 and 0.25; $^{++}$ marks 'significant' differences above 0.25 (Imbens and Wooldridge, 2009; Austin, 2011). In contrast to the usual $t$-statistic, this measure does not mechanically increase in large samples. The sample size varies because of missing values. The AKM fixed effects are only available for about half of the sample. '.' marks cells that are empty by restriction.
*Data:* SIAB.

**Table B.1.3.:** Baseyear Characteristics of Displaced and Non-Displaced Workers By Exposure Group

| | (1) | (2) | (3) | | (4) | (5) | (6) | | (7) | (8) | (9) | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | $E_0$ | | | | $E_1$ | | | | $E_2$ | | |
| | Disp. | Non-Disp. | Diff | | Disp. | Non-Disp. | Diff | | Disp. | Non-Disp. | Diff | |
| Person: | | | | | | | | | | | | |
| Female | .607 | .67 | -.062 | + | .368 | .446 | -.078 | + | .281 | .31 | -.029 | |
| Age | 39.262 | 37.474 | 1.788 | + | 41.781 | 40.471 | 1.31 | + | 43.678 | 41.931 | 1.746 | + |
| German | .928 | .935 | -.007 | | .908 | .925 | -.018 | | .896 | .926 | -.031 | + |
| No professional training | .107 | .116 | -.009 | | .144 | .149 | -.005 | | .193 | .143 | .05 | + |
| Vocational training | .852 | .811 | .041 | + | .812 | .763 | .049 | + | .741 | .747 | -.006 | |
| Academic degree | .04 | .073 | -.032 | + | .045 | .088 | -.043 | + | .066 | .11 | -.044 | + |
| Experience | 10.084 | 7.654 | 2.43 | ++ | 12.197 | 9.874 | 2.323 | ++ | 15.145 | 12.608 | 2.537 | ++ |
| Job tenure | 5.258 | 4.2 | 1.058 | ++ | 7.318 | 5.987 | 1.331 | ++ | 9.368 | 7.877 | 1.491 | + |
| No of benefit receipts | 1.321 | 1.209 | .112 | | 1.279 | 1.136 | .143 | | 1.174 | 1.056 | .118 | |
| No of n-spells | 1.279 | 1.408 | -.129 | | .993 | 1.369 | -.376 | + | .923 | 1.364 | -.441 | + |
| AKM person FE | 4.181 | 4.224 | -.043 | + | 4.284 | 4.308 | -.024 | | 4.347 | 4.4 | -.053 | + |
| Occupation: | | | | | | | | | | | | |
| Within-distance since entry | 0 | 0 | 0 | + | .001 | .001 | 0 | | .013 | .013 | 0 | |
| Occupation tenure | 7.226 | 5.676 | 1.55 | ++ | 10.462 | 8.761 | 1.702 | + | 13.816 | 11.813 | 2.003 | ++ |
| Agriculture | . | . | . | | . | . | . | | . | . | . | |
| Mining | . | . | . | | . | . | . | | . | . | . | |
| Manufacturing | .112 | .073 | .039 | + | .44 | .331 | .109 | + | .649 | .457 | .193 | ++ |
| Mid/High Wage Services (>=p25) | / | / | / | | / | / | / | | / | / | / | |
| Low Wage Services (<p25) | .885 | .921 | -.036 | + | .512 | .612 | -.1 | + | .244 | .418 | -.174 | ++ |
| wGR baseyear occupation (1980-2010) | .006 | .009 | -.003 | + | .002 | .004 | -.001 | + | -.002 | -.001 | -.002 | ++ |
| Industry: | | | | | | | | | | | | |
| Agriculture/Fishing/Mining | / | / | / | | / | / | / | | / | / | / | |
| Manufacturing/Energy/Construction | .292 | .205 | .087 | + | .536 | .368 | .168 | ++ | .664 | .477 | .187 | ++ |
| Trade/Hospitality/Traffic/Communication | .551 | .384 | .167 | ++ | .321 | .255 | .066 | + | .21 | .198 | .013 | |
| Credit/Real estate/Public Sector | .092 | .191 | -.099 | ++ | .089 | .194 | -.105 | ++ | .083 | .204 | -.121 | ++ |
| Education/Health/Other services | .06 | .212 | -.151 | ++ | .046 | .172 | -.127 | ++ | .036 | .112 | -.076 | ++ |
| Establishment: | | | | | | | | | | | | |
| Establishment size | 43.567 | 97.954 | -54.388 | ++ | 53.511 | 108.247 | -54.736 | ++ | 64.064 | 121.15 | -57.086 | ++ |
| <10 | .284 | .221 | .063 | + | .233 | .196 | .037 | | .19 | .173 | .017 | |
| 10-50 | .486 | .311 | .175 | ++ | .467 | .295 | .172 | ++ | .445 | .278 | .167 | ++ |
| 51-100 | .122 | .145 | -.023 | | .147 | .149 | -.002 | | .169 | .143 | .026 | |
| 101-250 | .079 | .191 | -.112 | ++ | .116 | .207 | -.091 | ++ | .146 | .224 | -.078 | + |

*(continued on next page)*

| | (1) | (2) | (3) | | (4) | (5) | (6) | | (7) | (8) | (9) | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | $E_0$ | | | | $E_1$ | | | | $E_2$ | | |
| | Disp. | Non-Disp. | Diff | | Disp. | Non-Disp. | Diff | | Disp. | Non-Disp. | Diff | |
| *(continued)* | | | | | | | | | | | | |
| 251-500 | .029 | .132 | -.103 | ++ | .037 | .152 | -.116 | ++ | .051 | .182 | -.131 | ++ |
| >500 | . | . | . | | . | . | . | | . | . | . | |
| Median Daily Wage | 62.781 | 70.502 | -7.721 | ++ | 65.647 | 71.231 | -5.585 | + | 72.609 | 79.865 | -7.255 | + |
| AKM establishment FE | .064 | .083 | -.018 | | .114 | .111 | .003 | | .145 | .15 | -.005 | |
| Outcomes: | | | | | | | | | | | | |
| Labor earnings per Year | 28417.624 | 29291.881 | -874.257 | | 34124.762 | 34934.058 | -809.296 | | 37530.723 | 40862.859 | -3332.136 | + |
| Employed | 1 | 1 | 0 | | 1 | 1 | 0 | | 1 | 1 | 0 | |
| Days employed per year | 362.015 | 361.667 | .348 | | 360.193 | 361.285 | -1.092 | | 360.906 | 361.885 | -.979 | |
| Switch occupation | 0 | 0 | 0 | | 0 | 0 | 0 | | 0 | 0 | 0 | |
| Log real daily wage | 4.235 | 4.248 | -.013 | | 4.436 | 4.425 | .011 | | 4.532 | 4.581 | -.049 | + |
| min(N) | 1618 | 59756 | | | 3702 | 103807 | | | 2801 | 59706 | | |
| max(N) | 2868 | 124233 | | | 7033 | 226271 | | | 4192 | 103679 | | |

*Notes:* The table shows the mean baseyear characteristics of displaced (Disp.) and non-displaced (Non-Disp.) workers in dosage groups zero ($E_0$), low ($E_1$) and the exposure high ($E_1$) group (for the classification of exposure groups see 2.4). The sample size varies because off missing values. The AKM fixed effects are only available for about half of the sample. '.' marks cells that are empty by restriction, '/' mark cells that contain less than 20 observations and must be censored in accordance with data protection regulations of the IAB.

*Data:* SIAB, OPTE.

**Table B.1.4.:** Balancing in the Matched Sample

| | (1) Displaced | (2) Non-Displaced | (3) Diff. (1)-(2) |
|---|---|---|---|
| **Person:** | | | |
| Female | .395 | .403 | -.008 |
| Age | 41.751 | 41.666 | .085 |
| German | .915 | .924 | -.009 |
| No professional training | .142 | .138 | .004 |
| Vocational training | .81 | .814 | -.004 |
| Academic degree | .048 | .048 | 0 |
| Experience | 12.653 | 12.116 | .538 |
| Job tenure | 7.457 | 7.295 | .162 |
| No of benefit receipts | 1.3 | 1.23 | .069 |
| No of n-spells | 1.059 | 1.027 | .032 |
| AKM person FE | 4.283 | 4.285 | -.002 |
| **Occupation:** | | | |
| Within-distance since entry | .004 | .004 | 0 |
| Years since occupation entry | 10.878 | 10.582 | .296 |
| Agriculture | . | . | . |
| Mining | . | . | . |
| Manufacturing | .429 | .413 | .016 |
| Mid/High Wage Services (>=p25) | .054 | .05 | .004 |
| Low Wage Services (<p25) | .518 | .537 | -.02 |
| wGR baseyear occupation (1980-2010) | .002 | .002 | 0 |
| **Industry:** | | | |
| Agriculture/Fishing/Mining | .004 | .01 | -.006 |
| Manufacturing/Energy/Construction | .509 | .484 | .025 |
| Trade/Hospitality/Traffic/Communication | .351 | .345 | .006 |
| Credit/Real estate/Public Sector | .086 | .099 | -.014 |
| Education/Health/Other services | .049 | .059 | -.011 |
| **Establishment:** | | | |
| Establishment size | 36.758 | 38.608 | -1.85 |
| <10 | .289 | .289 | 0 |
| 10-50 | .484 | .484 | 0 |
| 51-100 | .137 | .137 | 0 |
| 101-250 | .09 | .09 | 0 |
| 251-500 | . | . | . |
| >500 | . | . | . |
| Median Daily Wage | 66.281 | 67.546 | -1.265 |
| AKM establishment FE | .107 | .094 | .013 |
| **Outcomes:** | | | |
| Employed | 1 | 1 | 0 |
| Labor earnings per Year | 33540.376 | 33487.135 | 53.242 |
| Days employed per year | 360.689 | 360.671 | .018 |
| Switch occupation | 0 | 0 | 0 |
| Log real daily wage | 4.412 | 4.387 | .025 |
| min(N) | 8409 | 8099 | |
| max(N) | 13699 | 13699 | |

*Notes:* Columns (1) and (2) show the mean baseyear characteristics of displaced and non-displaced workers in the matched sample. Column (3) provides the difference between both groups and its significance in terms of the absolute value of the standardized difference: $^{+}$ marks 'marginal' differences between 0.1 and 0.25 by; $^{++}$ marks 'significant' differences above 0.25 (Imbens and Wooldridge, 2009; Austin, 2011). In contrast to the usual $t$-statistic, this measure does not mechanically increase in large samples. The sample size varies because of missing values. The AKM fixed effects are only available for about half of the sample. '.' marks cells that are empty by restriction.
*Data:* SIAB.

**Table B.1.5.:** Triple-Differences Estimate for Average Penalty for Exposure to Task Change from Matched Sample

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **Labor Earnings per Year** | | | | | |
| Low Exposure ($E_1$) | -2186.54*** | -2186.54*** | -2186.54*** | -3320.762*** | -2160.532*** |
| | (458.757) | (458.787) | (458.95) | (689.374) | (458.913) |
| High Exposure ($E_2$) | -4375.654*** | -4375.654*** | -4375.654*** | -5302.03*** | -4344.571*** |
| | (532.957) | (532.992) | (533.18) | (748.219) | (532.634) |
| N | 274406 | 274406 | 274406 | 147191 | 274197 |
| Adj. $R^2$ | .04 | .38 | .39 | .39 | .39 |
| **Employment Probability** | | | | | |
| Low Exposure ($E_1$) | -.039*** | -.039*** | -.039*** | -.049*** | -.039*** |
| | (.01) | (.01) | (.01) | (.013) | (.01) |
| High Exposure ($E_2$) | -.082*** | -.082*** | -.082*** | -.077*** | -.081*** |
| | (.011) | (.011) | (.011) | (.014) | (.011) |
| N | 274406 | 274406 | 274406 | 147191 | 274197 |
| Adj. $R^2$ | .08 | .1 | .11 | .11 | .11 |
| **Days Employed per Year** | | | | | |
| Low Exposure ($E_1$) | -15.95*** | -15.95*** | -15.95*** | -19.475*** | -15.817*** |
| | (3.563) | (3.563) | (3.564) | (4.862) | (3.565) |
| High Exposure ($E_2$) | -30.451*** | -30.451*** | -30.451*** | -29.273*** | -30.344*** |
| | (3.92) | (3.921) | (3.922) | (5.14) | (3.923) |
| N | 274406 | 274406 | 274406 | 147191 | 274197 |
| Adj. $R^2$ | .09 | .12 | .13 | .13 | .13 |
| **Switch Occupation** | | | | | |
| Low Exposure ($E_1$) | .049*** | .047*** | .047*** | .041** | .047*** |
| | (.011) | (.011) | (.011) | (.016) | (.011) |
| High Exposure ($E_2$) | .098*** | .097*** | .096*** | .096*** | .096*** |
| | (.012) | (.012) | (.012) | (.017) | (.012) |
| N | 222727 | 222727 | 222727 | 121532 | 222585 |
| Adj. $R^2$ | .11 | .14 | .16 | .16 | .16 |
| Baseyear Control Variables | | ✓ | ✓ | ✓ | ✓ |
| Industry FE | | ✓ | ✓ | ✓ | ✓ |
| Estab. Size Category FE | | ✓ | ✓ | ✓ | ✓ |
| Occupation Tenur (+sq) | | ✓ | ✓ | ✓ | ✓ |
| Occupation FE | | | ✓ | ✓ | ✓ |
| Baseyear FE | | | ✓ | ✓ | ✓ |
| AKM Estab. & Person FE | | | | ✓ | |
| Excl. Large Estab. ($\geq$ 500) | | | | | ✓ |

*Notes:* The table provides the Triple-Differences coefficient estimates for the three-way interactions of the exposure groups in equation (2.2) using the matched sample (see Section 2.5.5 for details about the matching procedure). The columns show the estimates from specifications with a growing set of baseyear control variables and fixed effects (see Table B.1.1 in Appendix B.1.1 for a list and description). The horizontal panels show the estimates for labor earnings per year, the probability of being employed, days employed per year and the probability of switching out of the baseyear occupation as outcomes. ***/**/* mark statistical significance at the 1/5/10% level.
*Data:* SIAB, OPTE.

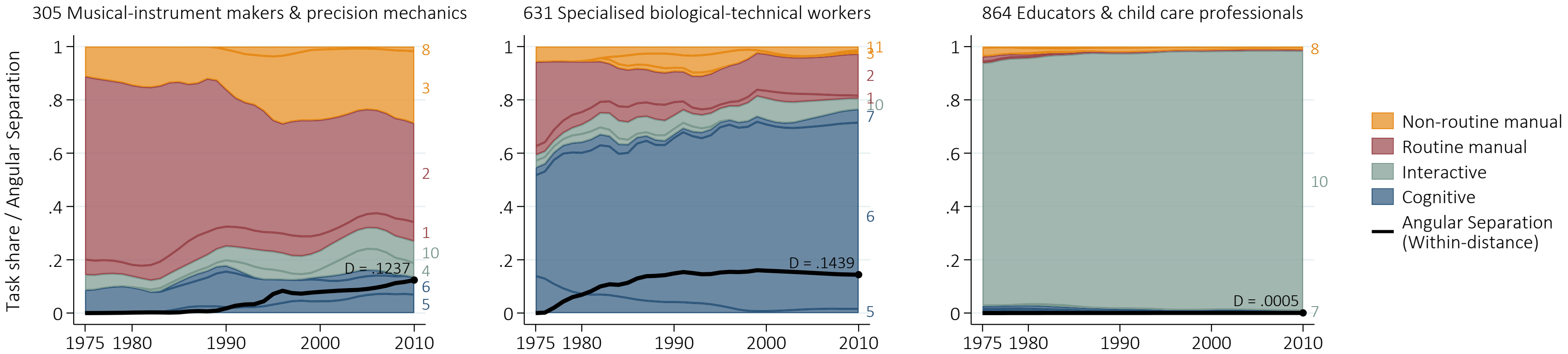
**Table B.1.6.:** Triple-Differences Estimate for Average Penalty for Exposure to Task Change with Fixed Ten-Year Time Window for Task Change

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **Labor Earnings per Year** | | | | | |
| Low Exposure ($E_1$) | -915.89*** | -932.822*** | -932.822*** | -2090.337*** | -1850.375*** |
| | (325.626) | (327.418) | (327.426) | (438.208) | (549.325) |
| High Exposure ($E_2$) | -1434.189*** | -1445.005*** | -1445.005*** | -2132.143*** | -2354.116*** |
| | (416.389) | (418.896) | (418.905) | (553.435) | (724.348) |
| N | 4454219 | 4391563 | 4391563 | 2404699 | 1071510 |
| Adj. $R^2$ | .02 | .45 | .47 | .47 | .5 |
| **Employment Probability** | | | | | |
| Low Exposure ($E_1$) | .015** | .015** | .015** | -.007 | .005 |
| | (.007) | (.007) | (.007) | (.008) | (.011) |
| High Exposure ($E_2$) | .005 | .005 | .005 | -.009 | -.01 |
| | (.008) | (.008) | (.008) | (.01) | (.013) |
| N | 4454219 | 4391563 | 4391563 | 2404699 | 1071510 |
| Adj. $R^2$ | .04 | .07 | .08 | .07 | .08 |
| **Days Employed per Year** | | | | | |
| Low Exposure ($E_1$) | 4.79* | 4.55* | 4.55* | -3.604 | 2.138 |
| | (2.467) | (2.471) | (2.471) | (3.129) | (3.993) |
| High Exposure ($E_2$) | .89 | 1.024 | 1.024 | -4.617 | -3.981 |
| | (2.92) | (2.923) | (2.923) | (3.641) | (4.852) |
| N | 4454219 | 4391563 | 4391563 | 2404699 | 1071510 |
| Adj. $R^2$ | .05 | .08 | .1 | .09 | .1 |
| **Switch Occupation** | | | | | |
| Low Exposure ($E_1$) | .062*** | .063*** | .063*** | .063*** | .066*** |
| | (.008) | (.008) | (.008) | (.01) | (.013) |
| High Exposure ($E_2$) | .139*** | .139*** | .139*** | .134*** | .138*** |
| | (.009) | (.01) | (.01) | (.012) | (.016) |
| N | 3825145 | 3776445 | 3776445 | 2107211 | 962066 |
| Adj. $R^2$ | .02 | .07 | .09 | .09 | .09 |
| Baseyear Control Variables | | ✓ | ✓ | ✓ | ✓ |
| Industry FE | | ✓ | ✓ | ✓ | ✓ |
| Estab. Size Category FE | | ✓ | ✓ | ✓ | ✓ |
| Occupation tenure (+sq) | | ✓ | ✓ | ✓ | ✓ |
| Occupation FE | | | ✓ | ✓ | ✓ |
| Baseyear FE | | | ✓ | ✓ | ✓ |
| AKM Estab. & Person FE | | | | ✓ | |
| Occupation Tenure $\geq$ 10 yrs | | | | | ✓ |

*Notes:* The table provides the Triple-Differences coefficient estimates for the three-way interactions of the exposure groups in equation (2.2) using the matched sample (see Section 2.5.5 for details about the matching procedure). The classification of exposure groups is based on a fixed ten-year time window of occupational task change before the baseyear $c$ for all workers, i.e., $\mathcal{D}(o, c-10, c)$ (see equation (2.1) section 2.4). The columns show the estimates from specifications with a growing set of baseyear control variables and fixed effects (see Table B.1.1 in Appendix B.1.1 for a list and description). The horizontal panels show the estimates for labor earnings per year, the probability of being employed, days employed per year and the probability of switching out of the baseyear occupation as outcomes. ***/**/* mark statistical significance at the 1/5/10% level.
*Data:* SIAB, OPTE.

### B.1.2. Supplementary Figures

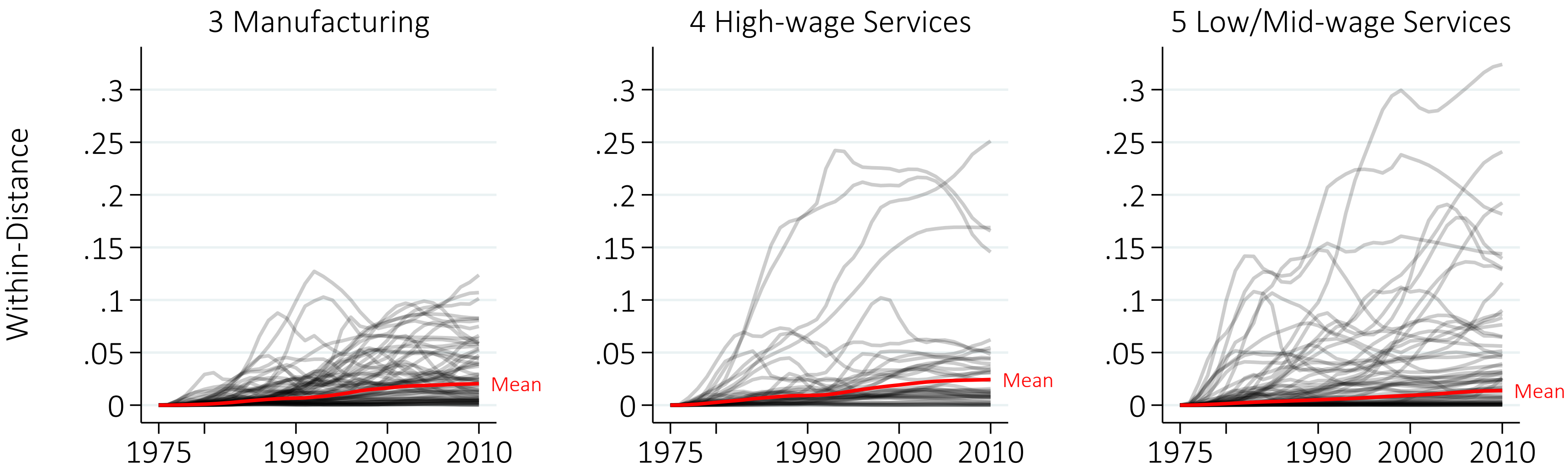
**Figure B.1.:** Occupational Task Compositions and Within-Distance as a Measure of Change, Example Occupations




*Notes:* The figure plots the share of workers with a given main task (colored areas) in three example occupations and how they translate into the Angular-Separation $\mathcal{D}(o,1975,t)$ ('within-distance', see equation (2.1)) as a scalar measure of changes in composition relative to 1975 (thick black line). The labels on the right margin of the subplots mark the main tasks: 1 Setting up/adjusting machines, 2 Extraction/manufacturing, 3 Repairing/mending, 4 Selling/advising/negotiating, 5 Typewriting/calculating, 6 Analyzing/measuring/researching, 7 Scheduling/coordinating, 8 Serving/accommodating/cleaning/transport, 9 Securing/guarding/applying laws, 10 Teaching/educating/publishing, 11 Nursing/treating medically or cosmetically. The labels of tasks with very small or zero shares have been omitted.

*Data:* OPTE.

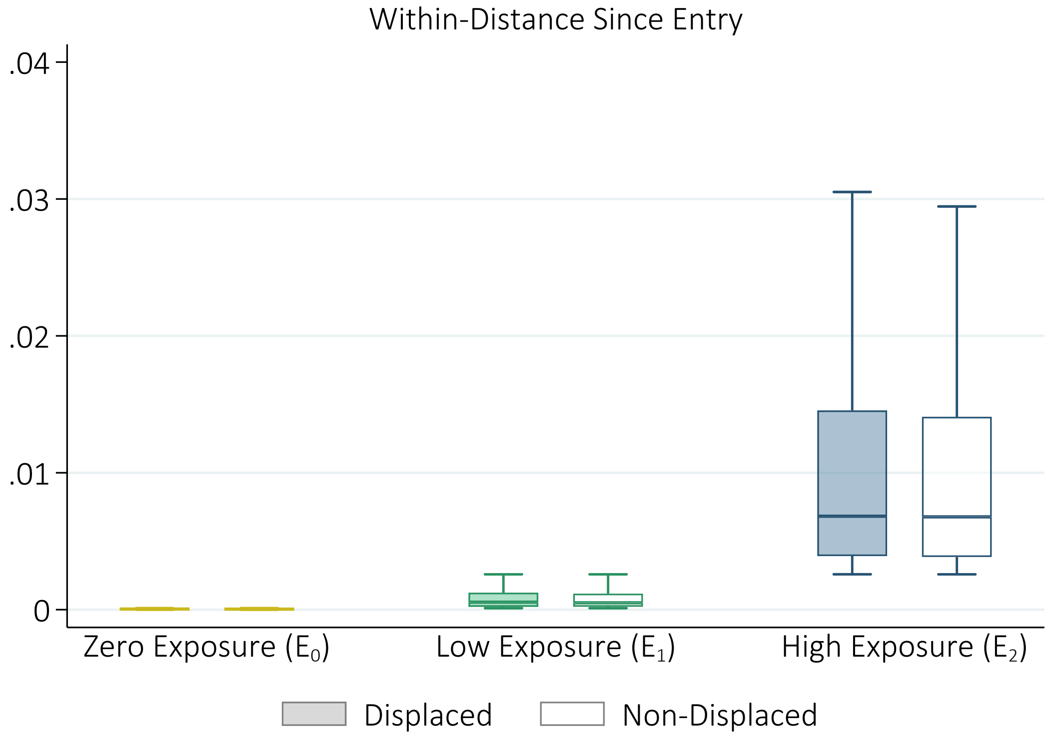
**Figure B.2.:** Within-Distance as a Measure of Changes in Occupational Tasks, by Occupation Type




*Notes:* The figure plots the within-distance of occupations between 1975 and all consecutive years $\mathcal{D}(o,1975,t)$ (see equation (2.1)) for three broad occupation groups. The classification of occupations is based on KldB1988 1-digit codes and occupational mean wages in Western Germany as kindly provided by Dauth (2014). The thick red line provides the employment-weighted mean across all occupations in a given grouping group and given year. The mean uses the OPTE's population-level estimates of occupational employment as weights.

*Data:* OPTE, Dauth (2014).

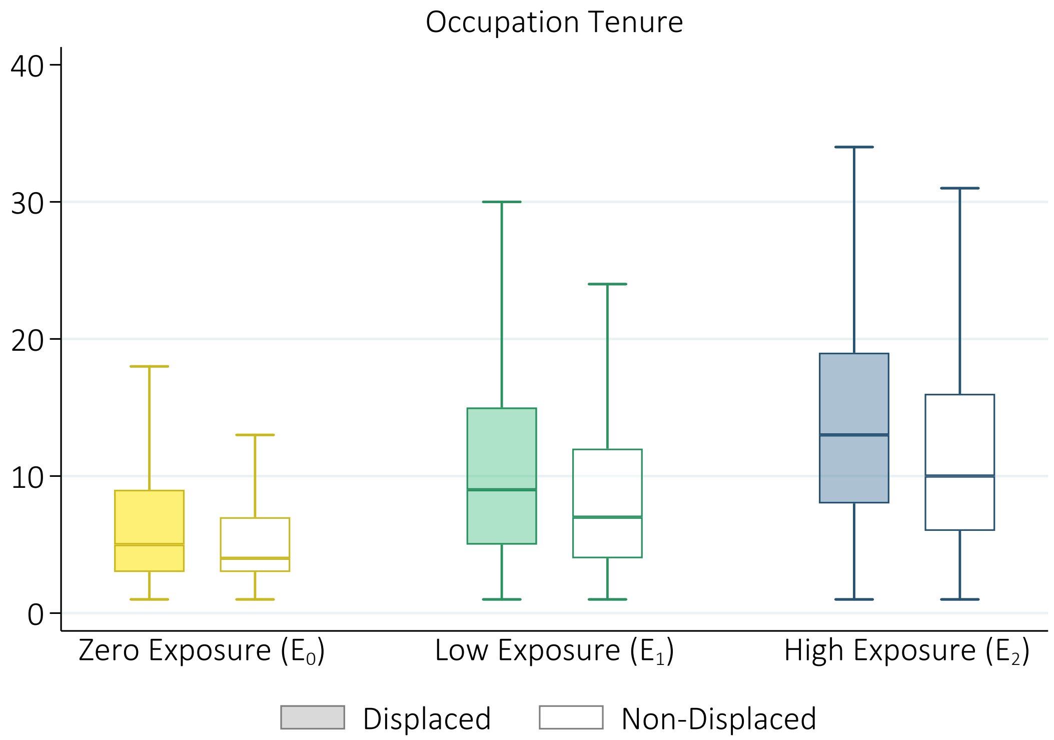
**Figure B.3.:** Variation in Task-Change since Individual Occupation Entry across Groups




*Notes:* The boxplot illustrates how the within-distance varies by exposure groups and between the displaced and non-displaced worker sample. The within distance measures the composition change in occupational tasks between individual entry into occupation $o$ in year $e$ and the displacement baseyear $c$, i.e., $\mathcal{D}(o,e,c)$ (see equation 2.1). The line in the middle of the box is the median, the top and bottom margin of the box mark the bottom and top quartiles. The whiskers mark the interquartile range.

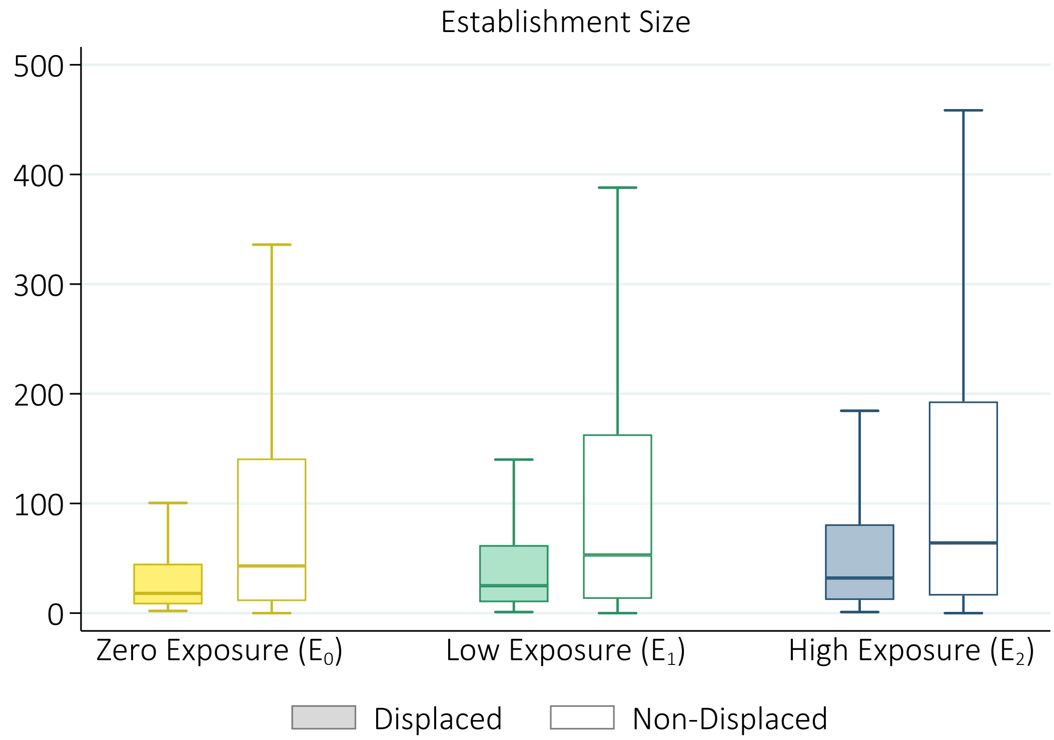
*Data:* SIAB, OPTE.

**Figure B.4.:** Variation in Occupation Tenure across Groups




*Notes:* The boxplot illustrates how the baseyear occupation tenure varies within exposure groups and between the displaced and non-displaced worker sample. The line in the middle of the box is the median, the top and bottom margin of the box mark the bottom and top quartiles. The whiskers mark the interquartile range.
*Data:* SIAB, OPTE.

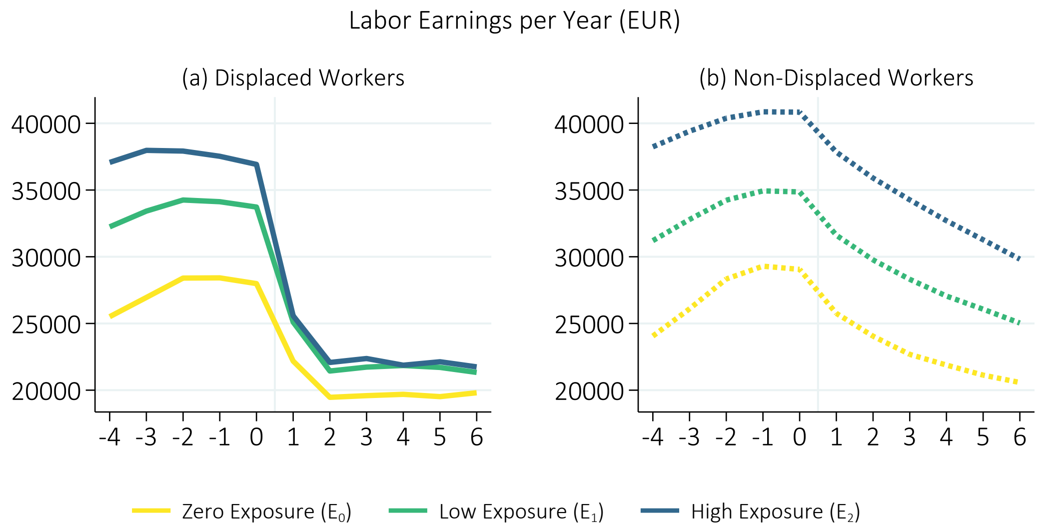
**Figure B.5.:** Variation in Baseyear Establishment Size across Groups




*Notes:* The boxplot illustrates how the baseyear establishment size varies within exposure groups and between the displaced and non-displaced worker sample. The line in the middle of the box is the median, the top and bottom margin of the box mark the bottom and top quartiles. The whiskers mark the interquartile range.

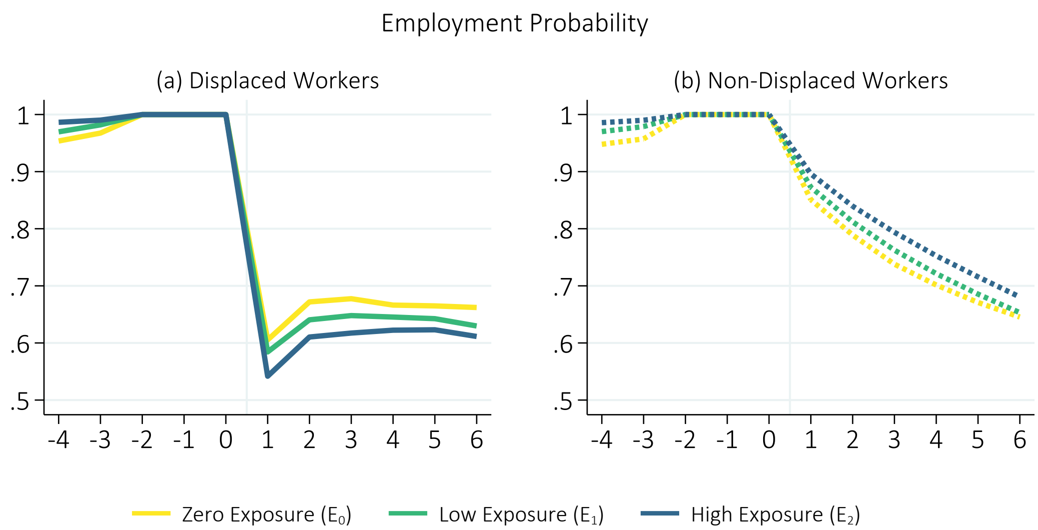
*Data:* SIAB, OPTE.

**Figure B.6.:** Mean Labor Earnings per Year by Displacement Status and Exposure Group



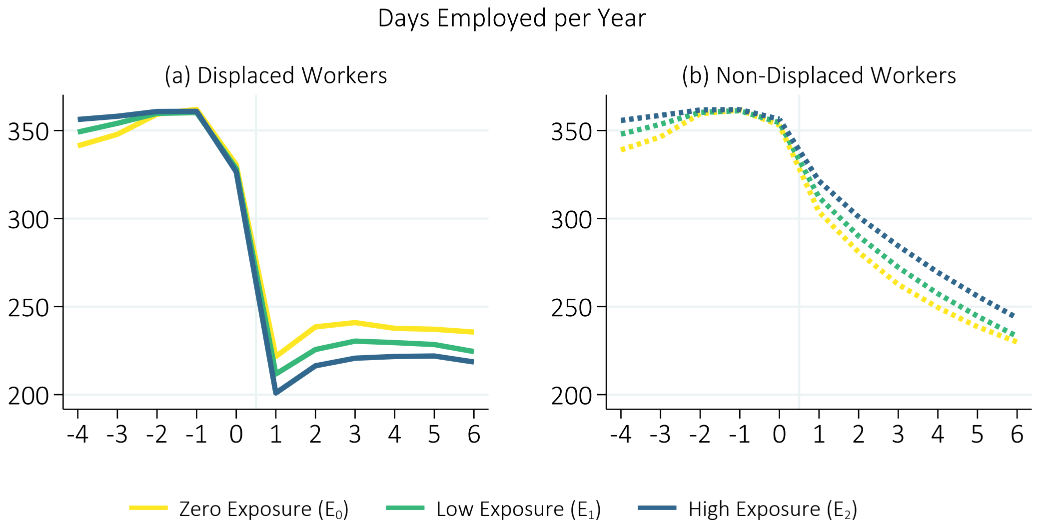

*Notes:* The figure plots the unconditional mean labor earnings per year for the zero ($E_0$), low ($E_1$) and high exposure ($E_2$3, see 2.4.1 for how exposure groups are classified). Panel (a) shows the mean for displaced workers, panel (b) for non-displaced workers.
*Data:* SIAB, OPTE.

**Figure B.7.:** Share of Employed Workers by Displacement Status and Exposure Group



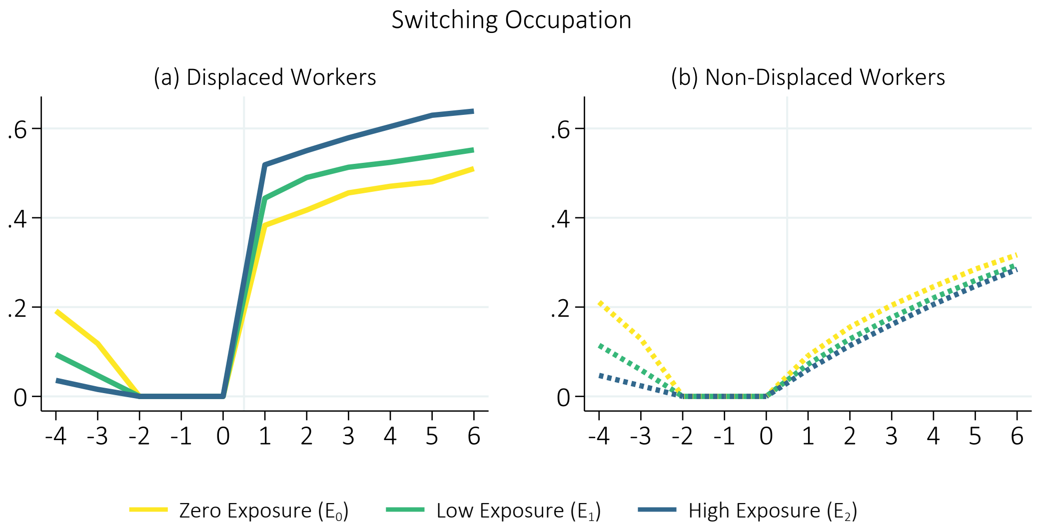

*Notes:* The figure plots the share of employed workers for the zero ($E_0$), low ($E_1$) and high exposure ($E_2$3, see 2.4.1 for how exposure groups are classified). Panel (a) shows the mean for displaced workers, panel (b) for non-displaced workers.
*Data:* SIAB, OPTE.

**Figure B.8.:** Mean Days Employed per Year by Displacement Status and Exposure Group



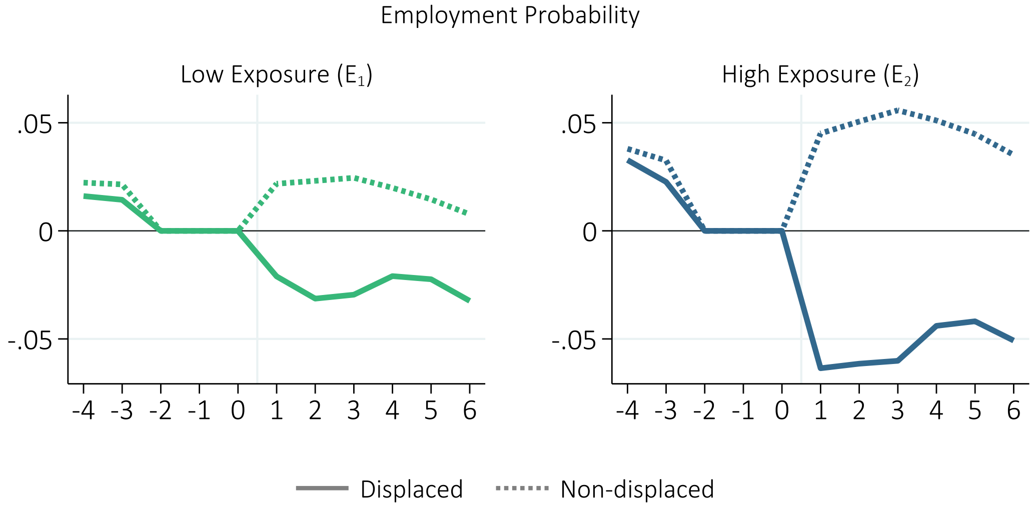

*Notes:* The figure plots the unconditional mean days employed per year for the zero ($E_0$), low ($E_1$) and high exposure ($E_2$3, see 2.4.1 for how exposure groups are classified). Panel (a) shows the mean for displaced workers, panel (b) for non-displaced workers.
*Data:* SIAB, OPTE.

**Figure B.9.:** Share of Employed Workers in an Occupation other than in the Baseyear by Displacement Status and Exposure Group



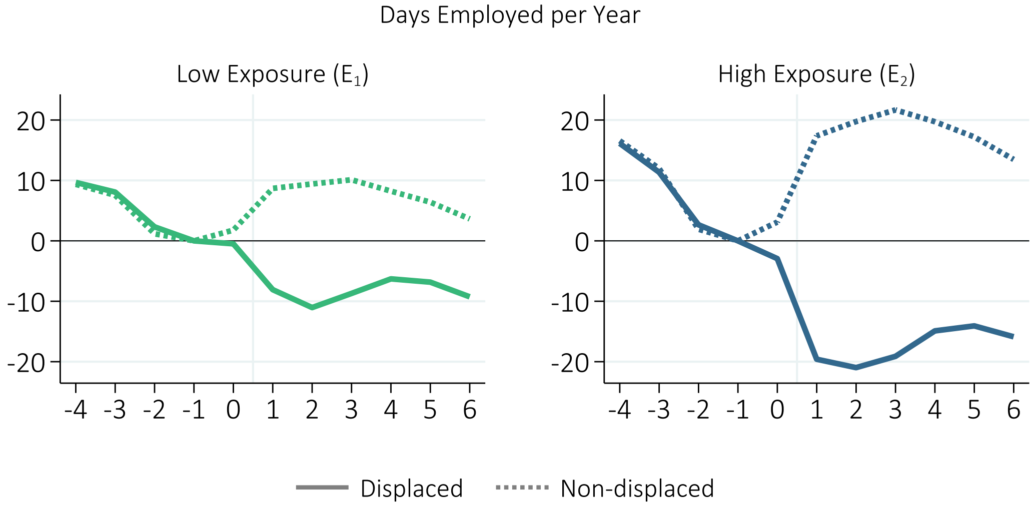

*Notes:* The figure plots the share of workers in an occupation other than in the baseyear for the zero ($E_0$), low ($E_1$) and high exposure ($E_2$3, see 2.4.1 for how exposure groups are classified). Panel (a) shows the mean for displaced workers, panel (b) for non-displaced workers.
*Data:* SIAB.

**Figure B.10.:** Deviation from Parallel Trends in the Employment Probability between Exposure Groups for Displaced and Non-Displaced Workers



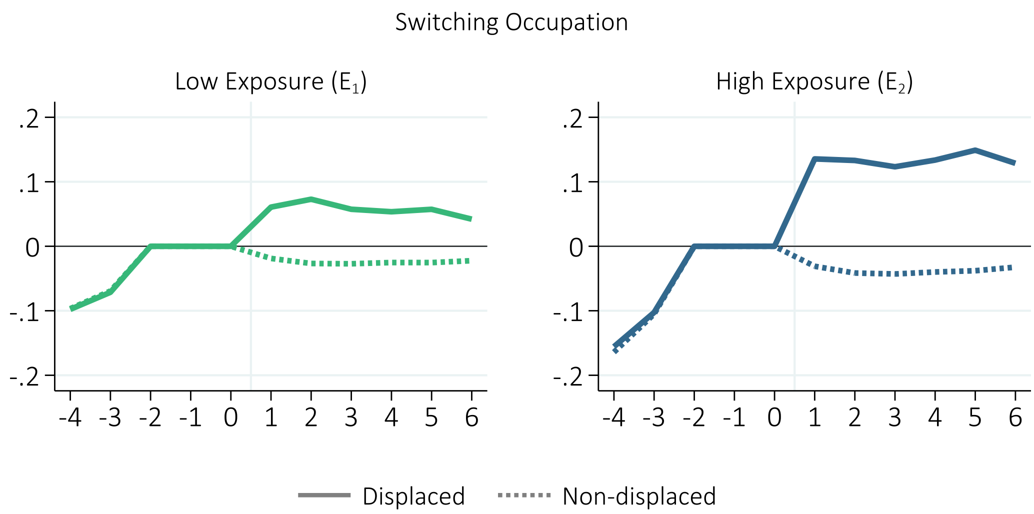

*Notes:* The figure plots the unconditional event studies for the employment probability of lowly and highly exposed workers in comparison to the zero exposure group, separately for displaced and non-displaced workers. Time trends are relative to the reference period $t = -1$. The plots support the validity of the Bias Stability assumption for the pre-displacement period, i.e., that the non-parallel trends bias for the low/high exposure group is almost identical in the displaced and non-displaced worker sample.
*Data:* SIAB, OPTE.

**Figure B.11.:** Deviation from Parallel Trends in Days Employed per Year between Exposure Groups for Displaced and Non-Displaced Workers




*Notes:* The figure plots the unconditional event studies for days employed per year of lowly and highly exposed workers in comparison to the zero exposure group, separately for displaced and non-displaced workers. Time trends are relative to the reference period $t = -1$. The plots support the validity of the Bias Stability assumption for the pre-displacement period, i.e., that the non-parallel trends bias for the low/high exposure group is almost identical in the displaced and non-displaced worker sample.
*Data:* SIAB, OPTE.

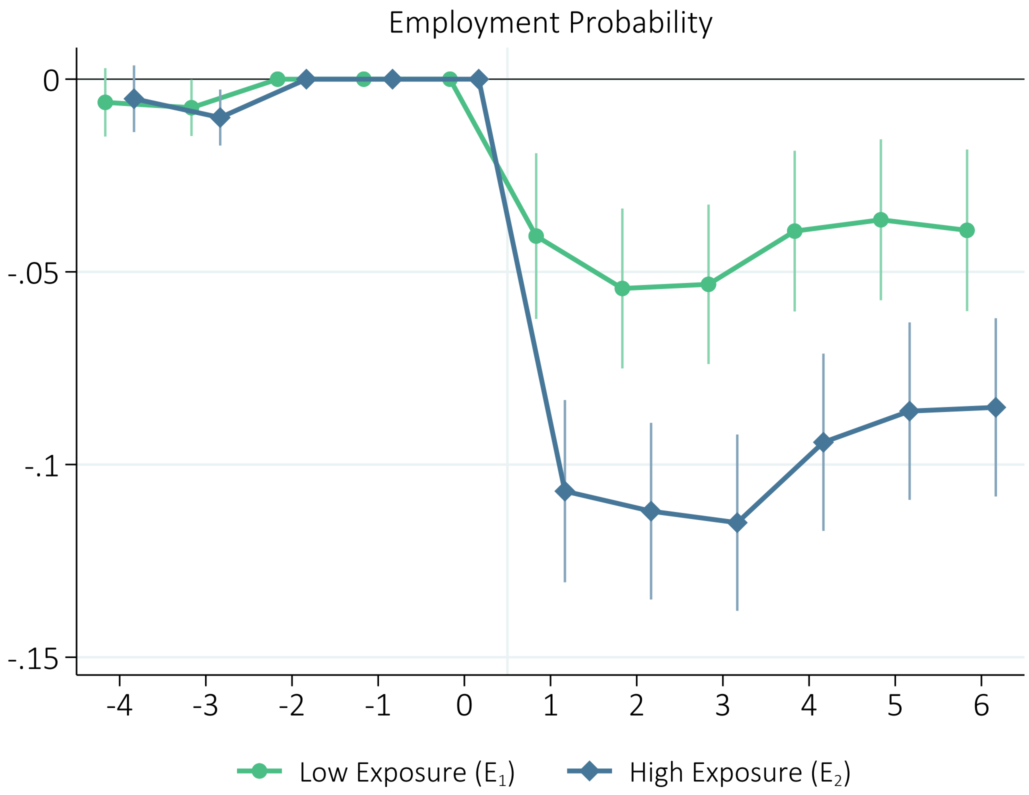
**Figure B.12.:** Deviation from Parallel Trends in Occupational Mobility between Exposure Groups for Displaced and Non-Displaced Workers




*Notes:* The figure plots the unconditional event studies for probability of working in an occupation other than in the baseyear of lowly and highly exposed workers in comparison to the zero exposure group, separately for displaced and non-displaced workers. Time trends are relative to the reference period $t = -1$. The plots support the validity of the Bias Stability assumption for the pre-displacement period, i.e., that the non-parallel trends bias for the low/high exposure group is almost identical in the displaced and non-displaced worker sample.

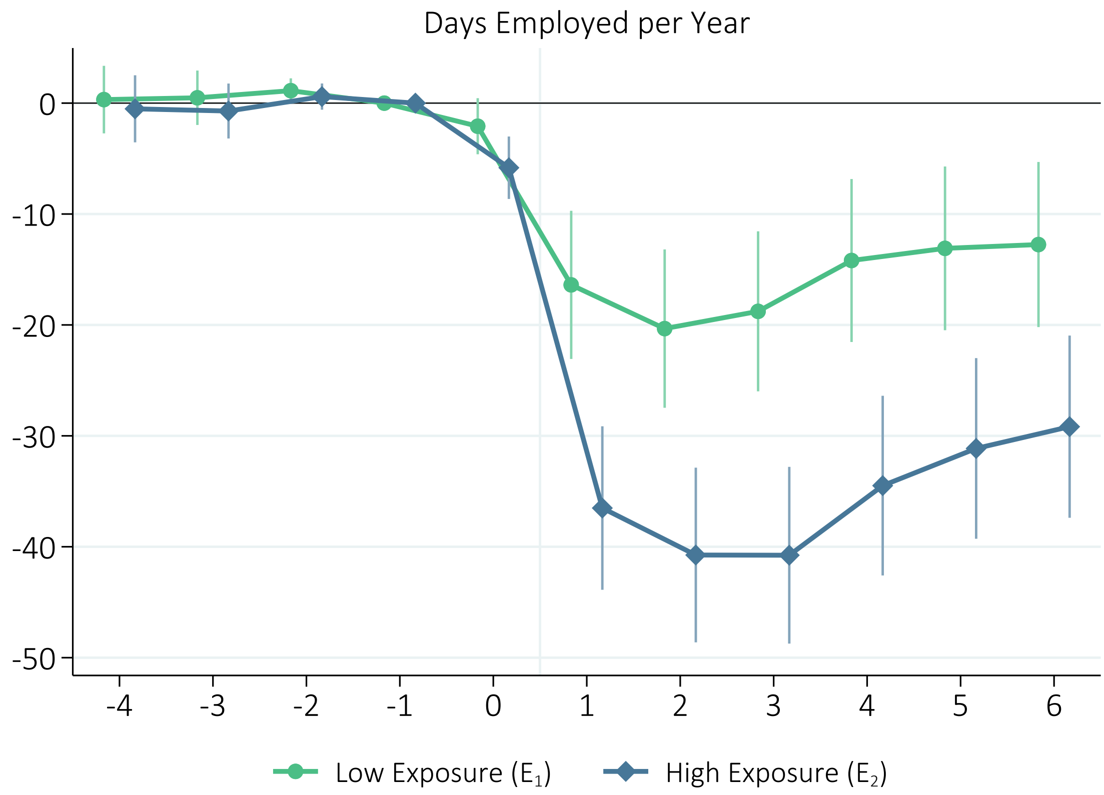
*Data:* SIAB, OPTE.

**Figure B.13.:** Triple-Differences Event Study Estimates of Penalty for Exposure to Task Change on the Employment Probability



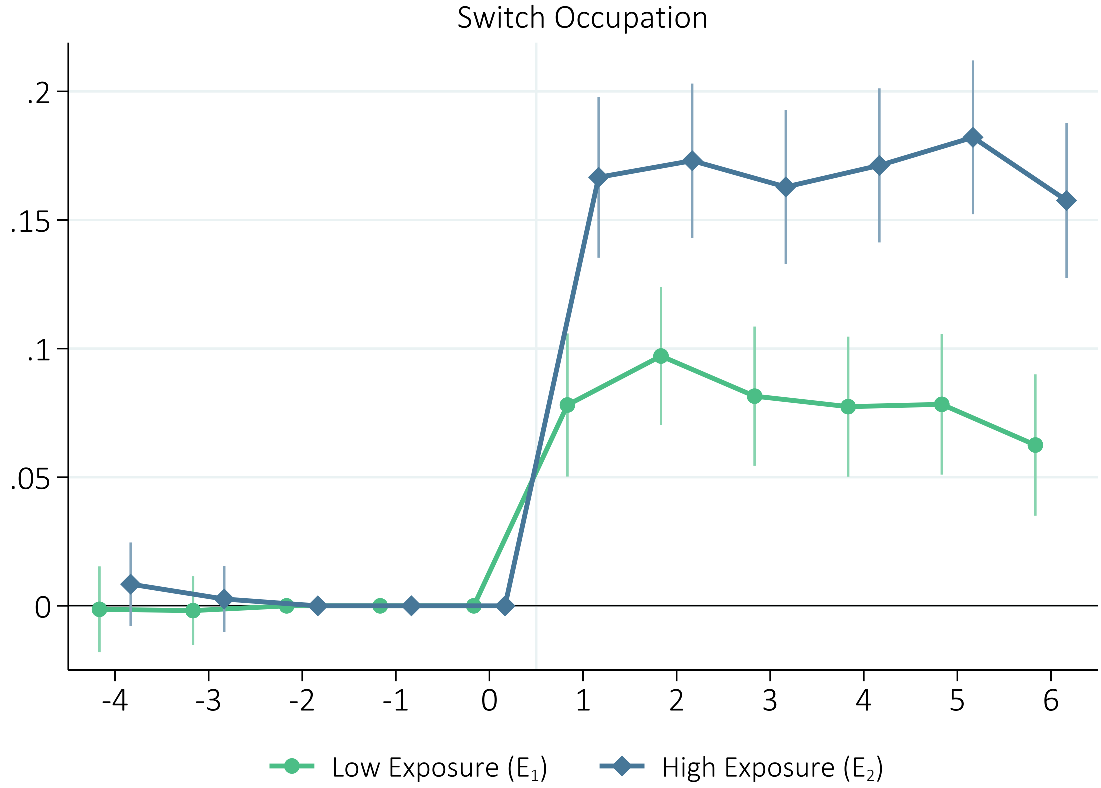

*Notes:* The plot shows the estimates for the employment probability per year from a fully interacted version of the Triple-Differences specification in equation (2.2), where the *Post* indicator has been replaced by a set of indicators for each relative time period $t = -4, \ldots, +6$, with $t = -1$ as the omitted reference period. The specification controls for baseyear characteristics (see Table B.1.1 in Appendix B.1.1), occupation fixed effects and calendar baseyear fixed effects. The coefficients represent the average additional penalty over six post-displacement years for displaced workers in exposure groups $E_1$ (low) and $E_2$ (high) relative to the zero exposure group $E_0$. The vertical line illustrates that the plant closure occurs between $t = 0$ and $t = 1$.

**Figure B.14.:** Triple-Differences Event Study Estimates of Penalty for Exposure to Task Change on Days Employed per Year



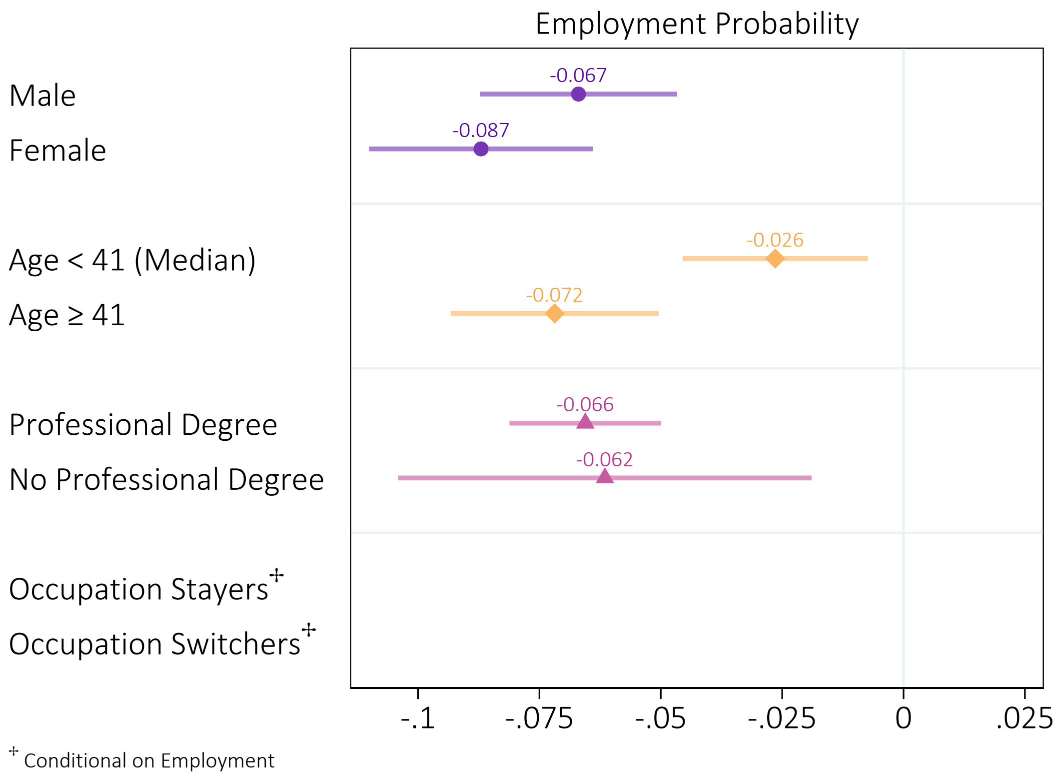

*Notes:* The plot shows the estimates for days worked per year from a fully interacted version of the Triple-Differences specification in equation (2.2), where the *Post* indicator has been replaced by a set of indicators for each relative time period $t = -4, \ldots, +6$, with $t = -1$ as the omitted reference period. The specification controls for baseyear characteristics (see Table B.1.1 in Appendix B.1.1), occupation fixed effects and calendar baseyear fixed effects. The coefficients represent the average additional penalty over six post-displacement years for displaced workers in exposure groups $E_1$ (low) and $E_2$ (high) relative to the zero exposure group $E_0$. The vertical line illustrates that the plant closure occurs between $t = 0$ and $t = 1$.

**Figure B.15.:** Triple-Differences Event Study Estimates of Penalty for Exposure to Task Change on the Probability working in an Occupation other than in the Baseyear




*Notes:* The plot shows the estimates for the probability of working in an occupation other than in the baseyear from a fully interacted version of the Triple-Differences specification in equation (2.2), where the *Post* indicator has been replaced by a set of indicators for each relative time period $t = -4, \ldots, +6$, with $t = -1$ as the omitted reference period. The specification controls for baseyear characteristics (see Table B.1.1 in Appendix B.1.1), occupation fixed effects and calendar baseyear fixed effects. The coefficients represent the average additional penalty over six post-displacement years for displaced workers in exposure groups $E_1$ (low) and $E_2$ (high) relative to the zero exposure group $E_0$. The vertical line illustrates that the plant closure occurs between $t = 0$ and $t = 1$.
*Data:* SIAB, OPTE.

**Figure B.16.:** Heterogeneity in the Earnings Penalty of the High Exposure Group on the Employment Probability




*Notes:* The Figure shows a decomposition of the Triple-Differences effect on the employment probability for the high exposure group ($E_2$) into separate estimates for different sub groups of workers. These estimates are derived from a four-way-interaction model, i.e., equation (2.2) is fully interacted with indicator variables for the sub groups. All models control for baseyear characteristics (see Table B.1.1 in Appendix B.1.1), occupation fixed effects and calendar baseyear fixed effects. There is no estimate for occupation switchers/stayers, because occupational mobility is conditional on re-employment and thus collinear to being employed as an outcome.
*Data:* SIAB, OPTE.

# C. Appendix: Do JCS Improve Social Integration and Well-being?

## C.1. Data Appendix

**Table C.1.1.:** Construction of the SILM Evaluation Dataset

| Step | Description | Observations[†] |
|---|---|---|
| Step 1 | Identification of program participants in administrative data. | 12,412 participants |
| Step 2 | Pre-selection of 20 control individuals for each program participant along key characteristics in administrative data. | 248,240 non-participants |
| Step 3 | Nearest-neighbor propensity score matching with replacement to select 4 control individuals for each participant. | 12,412 participants, 49,648 non-participants |
| Step 4 | *Telephone survey* of all program participants and matched control individuals. | |
| Step 4.1 | *Wave 1:* Survey of all program participants as soon as possible (average time since entry: 7 months) and of one nearest neighbor. | 3,821 participants, 3,427 non-participants |
| Step 4.2 | *Wave 2*: Survey of all participants (average time since entry: 18 months) and controls from wave 1. | 2,711 participants, 2,178 non-participants |
| Step 4.3 | *Wave 3:* Survey of all participants (average time since entry: 29 months) and controls from wave 2. No third wave for late entrants (after 01/2017). | 1,415 participants, 1,126 non-participants |
| Step 5 | *Final sample of matched pairs:* Restriction to successfully surveyed matched pairs with non-missing information on key variables. Exclude treated who never enter the program and control individuals in SILM or the "ESF federal program". | |
| | *Wave 1* ($t_1$): | 2,531 participants, 2,531 non-participants |
| | *Wave 2* ($t_2$): | 1,191 participants, 1,191 non-participants |
| | *Wave 3* ($t_3$): | 450 participants, 450 non-participants |

*Notes:* [†] Due to matching with replacement (see step 3), some non-participants are observed multiple times.

**Table C.1.2.:** Description of Variables in Integrated Employment Biographies

| Variable | Description |
|---|---|
| **Control variables measured at the date 31/12/2014 (cohort 1) and 31/12/2015 (cohorts 2 and 3)** | |
| **Sociodemographics:** | |
| Female | Dummy for being female |
| Age | Dummies for age groups: 35 - 44 years, 45 - 54 years, > 54 years, reference category is < 35 years |
| Health impairment | Dummy for having serious health restrictions. Variable is based on a combination of information on the disability status and a subjective assessment of the caseworker |
| Children in household | Dummy for having children aged ≤ 18 in the household |
| Number of children in household | Number of children aged ≤ 15 in the household |
| German | Dummy for being German |
| Family status | Dummy for family status: married - separated, married, widowed and divorced, reference category is single |
| School qualification | Dummies for highest school qualification: *Sonderschulabschluss/Hauptschulabschluss*, *Mittlere Reife*, *Fachhochschulreife* and *Abitur*, reference category is no school degree |
| Professional qualification | Dummies for highest professional qualification: vocational training, *Abitur* only, *Abitur* and vocational training, academic degree of *Fachhochschule* and university degree, reference category is no vocational training |
| **Previous job characteristics (for marginal employment and employment with ssc):** | |
| Employment full-time | Dummy for being employed full-time |
| Job classifications | Dummies for 6 job classifications: 1 Farmer, 2 Production/Craftspeople/Technician, 3 White-collar employee, 4 Salesperson, 5 Clerical workers, 6 Service workers, reference category is 1 |
| Tenure | Dummies for employment duration: categories are spitted according to percentiles of distribution: 25 - 50, 50 - 75, > 75, reference category is 0 - 25 |
| Daily wage | Dummies for daily wage in Euros (2010 prices): categories are spitted according to percentiles of distribution: 25 - 50, 50 - 75, > 75, reference category is 0 - 25 |
| **Previous firm characteristics (for marginal employment and employment with ssc):** | |
| Firm size | Dummies for number of employees: 10 - 49, 50 - 249, 250 - 499, > 500, reference category is < 10 |
| Median wage of firm | Dummies for median wage in Euros (2010 prices): categories are spitted according to percentiles of distribution: 25 - 50, 50 - 75, > 75, reference category is 0 - 25 |
| Sector of firm | Dummies for 17 sectors: 1 Agriculture, 2 Goods production, 3 Metal , 4 Vehicles, 5 Consumption, 6 Food, 7 Construction I, 8 Construction II, 9 Wholesale, 10 Retail, 11 Transportation, 12 Services I, 13 Services II, 14 Education/Health, 15 Associations, 16 Public, 17 other, reference category is 1 |
| **Employment history:** | |

*(continued on next page)*

*(continued)*

| Variable | Description |
|---|---|
| Previous employment status | Dummies for previous employment status (multiple answers are possible): employed with social security contributions (ssc), marginally employed, unemployed, welfare claimant (UB II), unemployed with sick note, unemployed comment "difficult to place", non-employed (no data entry) |
| Previous participation in ALMP measures | Dummies for type of previous labor market policy measure: job creation scheme, employment subsidies, training, 'Citizen work' (*Bürgerarbeit*), 'One-Euro-Job' (*Arbeitsgelegenheiten*), 'Program at an employer' and 'Program at an institution' (*MAG and MAT*) and other. |
| Number of periods | Number of periods in respective labor market states |
| Duration of periods | Dummies for duration in respective labor market states: categories are spitted according to percentiles of distribution: 25 - 50, 50 - 75, > 75, reference category is 0 - 25 |
| SGB II-Types | Dummies indicating SGB II-Typ of job center: 1 - 15 (see Dauth et al., 2013). Job centers that face different regional conditions (e.g. density of jobs, share of welfare claimants and share of foreigners) are grouped into 15 distinct types with an increasing share of SGB II benefit claimants and decreasing placement prospects for the long-term unemployed. |
| **Program and job characteristics:** | |
| Planned program duration | Program duration in months defined as difference between start and planned end |
| Program drop-out | Dummy for leaving the program before the planned end |
| Welfare receipt | Dummy for being welfare claimant (UB II) during program duration |

*Notes:* ssc = social security contributions.

**Table C.1.3.:** Description of Variables in Survey Data

| Variable | Description |
|---|---|
| **Outcomes:** | |
| Life satisfaction | Categorical variable measuring life satisfaction ranging from 0 (completely dissatisfied) to 10 (completely satisfied) |
| Mental health | Categorical variable for assessment of mental health over the last 4 weeks ranging from from 1 (extreme problems) to 5 (no problems) |
| Social belonging | Categorical variable measuring perceived social affiliation ranging from 1 (feeling excluded) to 10 (feeling affiliated) |
| Social status | Categorical variable measuring assessment of position in society ranging from 1 (belonging to bottom) to 10 (belonging to the top) |
| **Need for support:** | |
| Care of minor children | Dummy for need for support in the care of minor children |
| Psychological problems or addiction | Dummy for need for support with psychological problems or addiction |
| Indebtedness | Dummy for need for support with debts |
| Other life domains | Dummy for need for support in other life domains |
| **Willingness to work:** | |
| Accept commute >1h | Dummy for willingness to accept (rather or absolutely) a commute of one hour of more to get a job (1 to 4 scale) |
| Accept unfavorable working hours | Dummy for willingness to accept (rather or absolutely) unfavorable working hours to get a job (1 to 4 scale) |
| Accept work below qualification level | Dummy for willingness to accept (rather or absolutely) tasks below the own qualification level to get a job (1 to 4 scale) |
| Accept unfavorable conditions | Dummy for willingness to accept (rather or absolutely) unfavorable conditions like noise or dirt to get a job (1 to 4 scale) |
| Accept employer with bad image | Dummy for willingness to accept (rather or absolutely) an employer with a bad image to get a job (1 to 4 scale) |
| Importance of earning own money | Dummy for agree (rather of fully) with statement "Earning my own money is important to me" (1 to 5 scale) |
| **Program and job characteristics:** | |
| Employment-accompanying activities | Dummies for employment-accompanying activities: personal counseling, training/qualification measure, Support by case worker/coach, activities with other participants, healthy lifestyle counseling |
| Average working hours per week | Average working hours per week |
| Program tasks | Dummies for program tasks: social work, gardening/crafts/janitor, administration/archive/library, kitchen/food distribution, cleaning/housekeeping, sales/social department stores, others |
| **Participants assessments of the program** | |
| Job satisfaction | Categorical variable measuring job satisfaction ranging from 0 (completely dissatisfied) to 10 (completely satisfied) |
| Good relations with colleagues | Dummy taking the value of one if the average level of approval to the following four items is greater or equal to 3 (1 to 4 scale): "I receive help and support from my colleagues if needed", "Overall, I am treated fairly at the workplace", "I am acknowledged by my superior(s)", "At work I am more or less on my own" (scale flipped such that higher value marks less approval). |

*(continued on next page)*

*(continued)*

| Variable | Description |
|---|---|
| Work is meaningful | Dummy for agree (rather or fully) with statement "I perceive my work as meaningful" (1 to 4 scale) |

## C.2. Supplementary Results

### C.2.1. Supplementary Tables

**Table C.2.1.:** Comparison of Outcomes in SILM Evaluation Dataset and PASS

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Final sample of matches | | PASS | |
| | Participants | Non-participants | Employed | Eligible |
| **Well-being** | | | | |
| Life satisfaction [0-10] | 7.13 | 5.77 | 7.30 | 5.35 |
| Mental health [1-5] | 3.64 | 3.22 | 4.00 | 3.36 |
| **Social integration** | | | | |
| Social belonging [1-10] | 6.87 | 5.90 | 7.80 | 5.60 |
| Social status [1-10] | 5.00 | 4.60 | 6.10 | 4.47 |
| Observations | 2,531 | 2,531 | 16,600 | 4,600 |

*Notes:* The table shows the means of the outcome variables for participants (column (1)) and matched non-participants (column (2)) at $t_1$ based on the final estimation sample (see Section 4). Column (3) shows the mean outcomes for a sample of employed individuals and column (4) shows mean outcomes for individuals who fulfill the eligibility criteria of SILM based on PASS.
*Source:* SILM Evaluation Dataset, see Brussig et al. (2019); PASS, see Trappmann et al. (2010).

**Table C.2.2.:** Comparison of Participants throughout the Sample Construction Process

| | (1) Step 1 (Administrative data) | (2) Step 4.1 (Survey sample, $t_1$) | (3) Step 5 (Final sample, $t_1$) |
|---|---|---|---|
| **Sociodemographics** | | | |
| Female | 0.43 | 0.47 | 0.47 |
| Age | 48.39 | 48.87 | 48.97 |
| Health impairment | 0.47 | 0.49 | 0.50 |
| Children in household | 0.26 | 0.28 | 0.26 |
| German | 0.91 | 0.92 | 0.93 |
| Married | 0.28 | 0.25 | 0.25 |
| No professional degree | 0.20 | 0.17 | 0.16 |
| Vocational degree | 0.76 | 0.73 | 0.73 |
| Academic degree | 0.04 | 0.04 | 0.04 |
| Region with weak employment prospects$^{\dagger}$ | 0.75 | 0.78 | 0.79 |
| **Employment history** | | | |
| Years of welfare receipt | 7.50 | 7.48 | 7.43 |
| Cum. years of ssc. employment | 6.09 | 6.47 | 6.54 |
| Cum. years of marg. employment | 1.06 | 1.30 | 1.25 |
| Cum. no of ALMP measures | 6.20 | 6.51 | 6.67 |
| Prior participation in "One-Euro-Job" | 0.71 | 0.74 | 0.77 |
| Share of unfinished ALMP measures$^{\ddagger}$ | 0.12 | 0.12 | 0.12 |
| **Outcomes** | | | |
| Life satisfaction [0-10] | - | 7.17 | 7.13 |
| Mental health [1-5] | - | 3.66 | 3.64 |
| Social belonging [1-10] | - | 6.90 | 6.87 |
| Social status [1-10] | - | 5.03 | 5.00 |
| Observations | 12,412 | 3,554 | 2,531 |

*Notes:* The table shows the means of selected participant characteristics in different stages of the sample construction process (see Section 4). Column (1) shows the means from the initial sample of participants drawn from the administrative records. Column (2) shows the means from the sample of participants who entered the survey in wave 1. The sample is restricted to participants with non-missing values for all variables. Column (3) shows the mean values in wave 1 of the final estimation sample of matched pairs. Sociodemographics and the employment history are based on administrative data, the outcomes are obtained from the survey (see Tables C.1.1 and C.1.1 in the Appendix). $^{\dagger}$ Defined as job center regions of SGB II-Type 3 according to the classification of the Federal Employment Agency (Dauth et al., 2013). $^{\ddagger}$ An ALMP spell is defined as unfinished if it ends before the planned end date without a transition into employment.
*Source:* SILM Evaluation Dataset, see Brussig et al. (2019).

**Table C.2.3.:** Comparison of Participants and Non-participants in Final Estimation Sample across waves

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | | Mean at $t_1$ | | | Mean at $t_2$ | | | Mean at $t_3$ | |
| | Participants | Non-participants | Diff. | Participants | Non-participants | Diff. | Participants | Non-participants | Diff. |
| **Sociodemographics** | | | | | | | | | |
| Female | 0.47 | 0.46 | 0.01 | 0.47 | 0.47 | 0.00 | 0.47 | 0.47 | 0.00 |
| Age | 48.97 | 48.68 | 0.29 | 49.84 | 49.23 | 0.61 * | 50.03 | 49.16 | 0.87 * |
| Health impairment | 0.50 | 0.48 | 0.02 * | 0.54 | 0.48 | 0.05 ** | 0.53 | 0.50 | 0.04 |
| Children in household | 0.26 | 0.28 | -0.02 | 0.25 | 0.27 | -0.02 | 0.23 | 0.25 | -0.02 |
| German | 0.93 | 0.93 | -0.01 | 0.94 | 0.93 | 0.01 | 0.97 | 0.93 | 0.04 ** |
| Married | 0.25 | 0.25 | 0.00 | 0.26 | 0.26 | 0.01 | 0.25 | 0.23 | 0.02 |
| No professional degree | 0.16 | 0.16 | 0.00 | 0.15 | 0.15 | 0.00 | 0.16 | 0.16 | 0.00 |
| Vocational degree | 0.73 | 0.74 | 0.00 | 0.75 | 0.74 | 0.01 | 0.76 | 0.73 | 0.02 |
| Academic degree | 0.04 | 0.04 | 0.01 | 0.05 | 0.05 | 0.00 | 0.06 | 0.06 | 0.00 |
| Region with weak employment prospects† | 0.79 | 0.77 | 0.02 | 0.78 | 0.77 | 0.02 | 0.77 | 0.77 | 0.00 |
| **Employment history** | | | | | | | | | |
| Years of welfare receipt | 7.43 | 7.21 | 0.22 ** | 7.50 | 7.25 | 0.24 * | 7.12 | 6.92 | 0.20 |
| Cum. years of ssc. employment | 6.54 | 6.68 | -0.14 | 6.90 | 6.82 | 0.08 | 7.17 | 7.13 | 0.04 |
| Cum. years of marg. employment | 1.25 | 1.37 | -0.11 | 1.31 | 1.53 | -0.22 * | 1.35 | 1.41 | -0.06 |
| Cum. no of ALMP measures | 6.67 | 6.81 | -0.14 | 6.66 | 6.82 | -0.16 | 6.91 | 7.14 | -0.23 |
| Prior participation in "One-Euro-Job" | 0.77 | 0.75 | 0.01 | 0.79 | 0.76 | 0.03 | 0.79 | 0.78 | 0.00 |
| Share of unfinished ALMP measures‡ | 0.12 | 0.11 | 0.01 | 0.12 | 0.11 | 0.01 | 0.11 | 0.11 | 0.01 |
| **Outcomes** | | | | | | | | | |
| Life satisfaction [0-10] | 7.13 | 5.77 | 1.36 *** | 7.30 | 6.10 | 1.19 *** | 6.99 | 6.02 | 0.97 *** |
| Mental health [1-5] | 3.64 | 3.22 | 0.41 *** | 3.57 | 3.23 | 0.34 *** | 3.51 | 3.24 | 0.27 *** |
| Social belonging [1-10] | 6.87 | 5.90 | 0.97 *** | 6.68 | 5.87 | 0.80 *** | 6.28 | 5.72 | 0.57 *** |
| Social status [1-10] | 5.00 | 4.60 | 0.40 *** | 4.91 | 4.59 | 0.32 *** | 4.72 | 4.45 | 0.27 ** |
| Observations | 2,531 | 2,531 | | 1,191 | 1,191 | | 450 | 450 | |

*Notes:* The table shows the means of selected pre-treatment characteristics and post-treatment outcomes for participants and matched non-participants at $t_1$, $t_2$ and $t_3$ based on the final estimation sample (see Section 4). Columns (3), (6) and (9) show the differences in means and their significance levels from two-sample t-tests. Sociodemographics and the employment history are based on administrative data, the outcomes are obtained from the survey (for details see Tables C.1.1 and C.1.2 in the Appendix). † Defined as job center regions of SGB II-Type 3 according to the classification of the Federal Employment Agency (Dauth et al., 2013). ‡ An ALMP spell is defined as unfinished if it ends before the planned end date without a transition into employment. ***/**/* marks significance at the 1/5/10% level.
*Source:* SILM Evaluation Dataset, see Brussig et al. (2019).

**Table C.2.4.:** Outcomes of Participants after Drop-out or the planned Program End by Employment State

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Drop-out | | Planned program end | |
| | Employed | Unemployed | Employed | Unemployed |
| **Well-being** | | | | |
| Life satisfaction [0-10] | 8.09 | 5.53 | 6.67 | 6.15 |
| Mental health [1-5] | 4.12 | 3.00 | 3.50 | 3.10 |
| **Social integration** | | | | |
| Social belonging [1-10] | 7.74 | 5.72 | 6.61 | 5.81 |
| Social status [1-10] | 5.94 | 4.21 | 5.33 | 4.36 |
| Observations (Share) | 34 (16.0%) | 178 (84.0%) | 18 (18.2%) | 81 (81.8%) |

*Notes:* The table shows the means of the outcome variables for participants that enter non-subsidized employment or unemployment after leaving the program – either after an early drop-out (columns (1) and (2)) or after the planned end date (columns (3) and (4)). The bottom line provides the number of individuals in the respective group and the share of drop-outs or individuals with regular end in the respective employment state.
*Source:* SILM Evaluation Dataset, see Brussig et al. (2019).

**Table C.2.5.:** ATT on Standardized Outcomes in Wave 1 by Job Center Type of Control Individual

| | (1) | (2) | (3) |
|---|---|---|---|
| | JC type of matched control | | |
| | participating | non-participating | Difference |
| Life satisfaction | 0.54*** | 0.63*** | -0.09 |
| | (0.03) | (0.05) | |
| Mental health | 0.36*** | 0.29*** | 0.07 |
| | (0.03) | (0.05) | |
| Social belonging | 0.32*** | 0.42*** | -0.10 |
| | (0.04) | (0.05) | |
| Social status | 0.17*** | 0.23*** | -0.06 |
| | (0.04) | (0.05) | |
| Observations | 3,284 | 1,778 | |

*Notes:* The table shows the estimated ATT at $t_1$ (mean duration of 7 months) from pooled OLS regressions (see equation 1) based on the final estimation sample (see Section 4). Control variables consist of sociodemographics and the individual employment history (see Table C.1.1 in the Appendix). Estimates are based on the subsample of matches with control individuals from job centers participating in the program (column (1)) and non-participating job centers (column (2)). Column (3) shows the difference between columns (1) and (2) and its significance level from two sample t-tests. Standard errors are clustered at the individual level. ***/**/* marks significance at the 1/5/10% level.
*Source:* SILM Evaluation Dataset, see Brussig et al. (2019).

**Table C.2.6.:** ATT on Standardized Outcomes in Wave 1 by Availability of Program Places in the Job Centers of Participants

| | (1) | (2) | (3) |
|---|---|---|---|
| | Availability of program places in participant JC | | |
| | Above average | Below average | Difference |
| Life satisfaction | 0.56*** | 0.58*** | -0.02 |
| | (0.05) | (0.03) | |
| Mental health | 0.31*** | 0.34*** | -0.03 |
| | (0.05) | (0.04) | |
| Social belonging | 0.40*** | 0.34*** | 0.06 |
| | (0.05) | (0.04) | |
| Social status | 0.25*** | 0.20*** | 0.05 |
| | (0.05) | (0.04) | |
| Observations | 1,948 | 3,114 | |

*Notes:* The table shows the estimated ATT at $t_1$ (mean duration of 7 months) from pooled OLS regressions (see equation 1) based on the final estimation sample (see Section 4). Control variables consist of sociodemographics and the individual employment history (see Table C.1.1 in the Appendix). Estimates are based on the subsample of matches with treated individuals from job centers with above (column (1)) and below average availability of program places (column (2)). Availability is approximated by the number of program places divided by the number of welfare recipients per job center. The average availability amounts to 0.022, i.e. 2.2 program places per 100 welfare recipients. Column (3) shows the difference between column (1) and (2) and its significance from two sample t-tests. Standard errors are clustered at the individual level. ***/**/* marks significance at the 1/5/10% level.
*Source:* SILM Evaluation Dataset, see Brussig et al. (2019).

**Table C.2.7.:** Comparison of Employment States of Respondents and all Individuals

| | | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|---|
| | | Participants | | | Non-participants | | |
| | | Respondents | All | Diff. | Respondents | All | Diff. |
| $t_1$ | SSC employment | 0.008 | 0.007 | -0.001 | 0.113 | 0.091 | -0.022 |
| | Marginal employment | 0.004 | 0.005 | 0.001 | 0.089 | 0.067 | -0.022 |
| | ALMP | 0.928 | 0.902 | -0.026 | 0.162 | 0.170 | 0.007 |
| | Not employed or ALMP | 0.060 | 0.086 | 0.026 | 0.635 | 0.672 | 0.037 |
| | Observations | 2,531 | 12,305 | | 2,531 | 12,305 | |
| $t_2$ | SSC employment | 0.021 | 0.024 | 0.003 | 0.166 | 0.118 | -0.048 |
| | Marginal employment | 0.008 | 0.008 | 0.000 | 0.099 | 0.073 | -0.026 |
| | ALMP | 0.883 | 0.833 | -0.051 | 0.123 | 0.155 | 0.032 |
| | Not employed or ALMP | 0.088 | 0.136 | 0.048 | 0.612 | 0.654 | 0.042 |
| | Observations | 1,191 | 11,931 | | 1,191 | 11,931 | |
| $t_3$ | SSC employment | 0.053 | 0.042 | -0.011 | 0.173 | 0.145 | -0.028 |
| | Marginal employment | 0.011 | 0.012 | 0.001 | 0.100 | 0.068 | -0.032 |
| | ALMP | 0.767 | 0.762 | -0.005 | 0.131 | 0.129 | -0.003 |
| | Not employed or ALMP | 0.169 | 0.185 | 0.016 | 0.596 | 0.658 | 0.063 |
| | Observations | 450 | 8,102 | | 450 | 8,102 | |

*Notes:* The table shows the shares of participants and their matched non-participants in different employment states based on the final estimation sample (columns (1) and (4)) and the full administrative sample (columns (2) and (5)). Columns (3) and (6) show the differences in the respective shares.

*Source:* SILM Evaluation Dataset, see Brussig et al. (2019).

**Table C.2.8.:** ATTs on Standardized Outcomes, Ordered Probit Models

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Well-being | | Social integration | | |
| | Life satisfaction | Mental health | Social belonging | Social status | Observations |
| $t_1$ | 0.58*** | 0.33*** | 0.34*** | 0.20*** | 5,062 |
| | (0.03) | (0.03) | (0.03) | (0.03) | |
| $t_2$ | 0.52*** | 0.29*** | 0.30*** | 0.18** | 2,382 |
| | (0.04) | (0.04) | (0.04) | (0.04) | |
| $t_3$ | 0.38*** | 0.21*** | 0.19*** | 0.13*** | 900 |
| | (0.06) | (0.07) | (0.07) | (0.06) | |

*Notes:* The table shows the estimated ATTs (average marginal effects) at different program durations (mean duration of 7 months at $t_1$, 18 months at $t_2$, 29 months at $t_3$) from ordered probit models based on the final estimation sample (see Section 4). Control variables consist of sociodemographics and the individual employment history (see Table C.1.1 in the Appendix). For each wave, a separate model was estimated in the cross section of matched participants and controls. Standard errors are clustered at the individual level. ***/**/* marks significance at the 1/5/10% level.
*Source:* SILM Evaluation Dataset, see Brussig et al. (2019).

**Table C.2.9.:** ATTs on Standardized Outcomes, Pooled and Differenced model

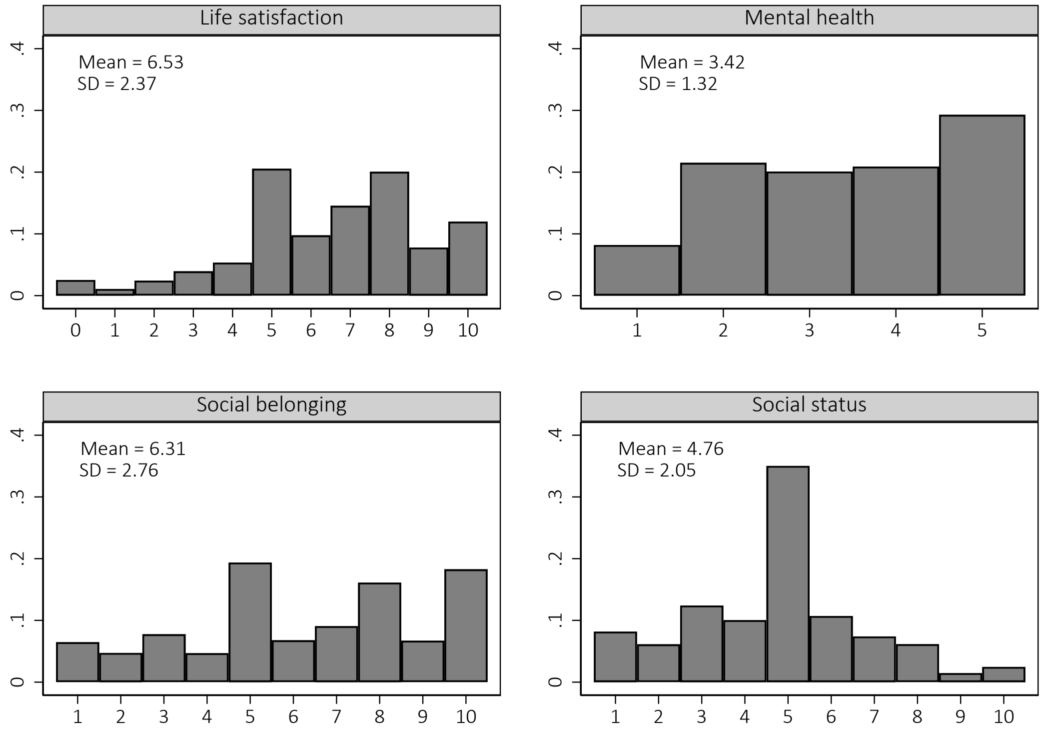
| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|---|
| | | Life Satisfaction | | Mental health | | Social belonging | | Social status | |
| | | Pooled | Differenced | Pooled | Differenced | Pooled | Differenced | Pooled | Differenced |
| $Treat$ | | 0.58*** | - | 0.33*** | - | 0.35*** | - | 0.20*** | - |
| | | (0.03) | | (0.03) | | (0.03) | | (0.03) | |
| $Treat \times t_2$ | | -0.06 | -0.09** | -0.05 | -0.08** | -0.05 | -0.07 | -0.03 | -0.07 |
| | | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) | (0.05) | (0.05) | (0.05) |
| $Treat \times t_3$ | | -0.14** | -0.20*** | -0.12* | -0.18*** | -0.15** | -0.15** | -0.05 | -0.11 |
| | | (0.07) | (0.07) | (0.07) | (0.07) | (0.07) | (0.07) | (0.07) | (0.07) |
| Observations | $t_1$ | 5,062 | - | 5,062 | - | 5,062 | - | 5,062 | - |
| | $t_2$ | 2,382 | 2,382 | 2,382 | 2,382 | 2,382 | 2,382 | 2,382 | 2,382 |
| | $t_3$ | 900 | 900 | 900 | 900 | 900 | 900 | 900 | 900 |

*Notes:* The table shows the estimated ATTs at $t_1$ and the changes at $t_2$ and $t_3$ based on the final estimation sample (see Section 4). The pooled models (columns (1), (3), (5) and (7)) use the following specification to predict the levels of the outcomes variables:
$y_{it} = \alpha_0 + \beta_1 Treat_i + \beta_2 Treat_i \times t_{2,it} + \beta_3 Treat_i \times t_{3,it} + \alpha_1 t_{2,it} + \alpha_2 t_{3,it} + X_{it}\gamma + c_i + \epsilon_{it}$.
The coefficients of the interactions terms are the change of the treatment effect as compared to $t_1$. $c_i$ is a time-invariant and unobserved individual-specific error term. The estimates from this model can be compared to those from a "differenced" model (columns (2), (4), (6) and (8)) that use the change in outcomes w.r.t. $t_1$ as the dependent variable:
$\Delta y_{it} = y_{it} - y_{i1} = \delta_0 + \beta_2 Treat_i \times t_{2,it} + \beta_3 Treat_i \times t_{3,it} + \delta_1 t_{2,it} + \delta_2 t_{3,it} + X_{it}\theta + \eta_{it}$.
Control variables consist of sociodemographics and the individual employment history (see Table C.1.1 in the Appendix). Standard errors are clustered at the individual level. ***/**/* marks significance at the 1/5/10% level.

*Notes:* SILM Evaluation Dataset, see Brussig et al. (2019).

### C.2.2. Supplementary Figures

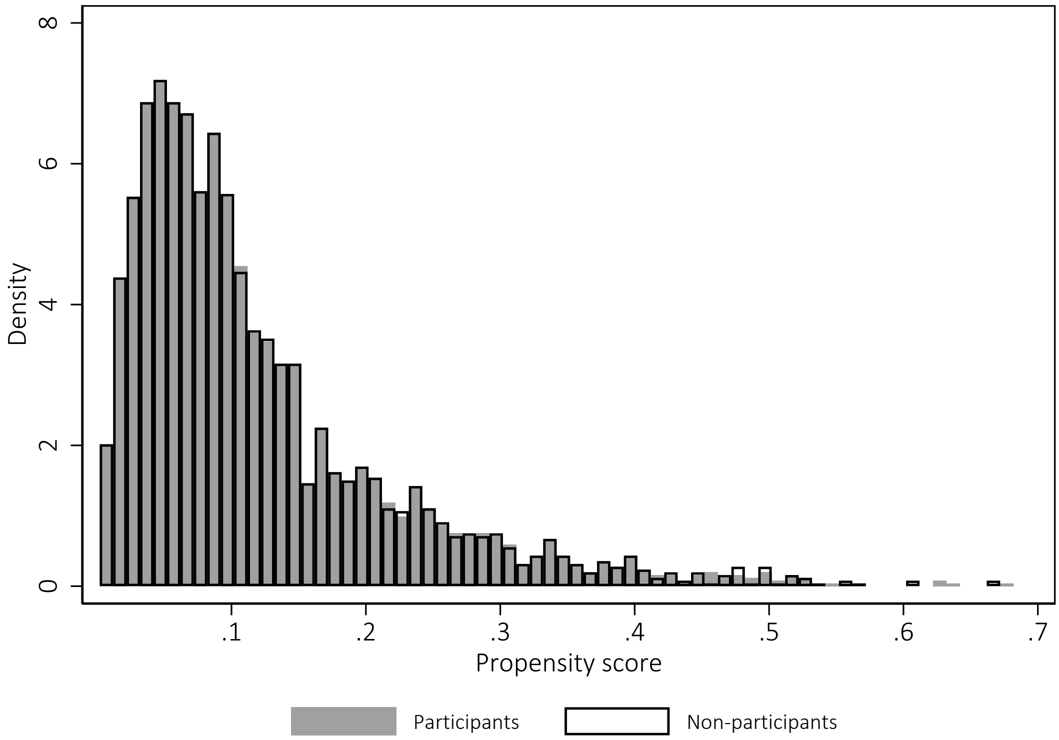
**Figure C.1.:** Distribution of Outcome Variables




*Notes:* SD: standard deviation. The figure shows the densities of the outcome variables in their original scale and provides the means and standard deviations in the final estimation sample pooled over waves 1 to 3 (see Section 4).

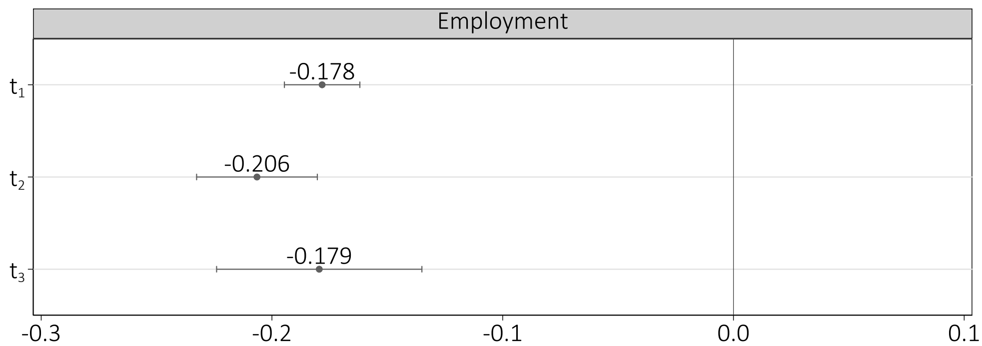
*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

**Figure C.2.:** Common Support of the Propensity Score



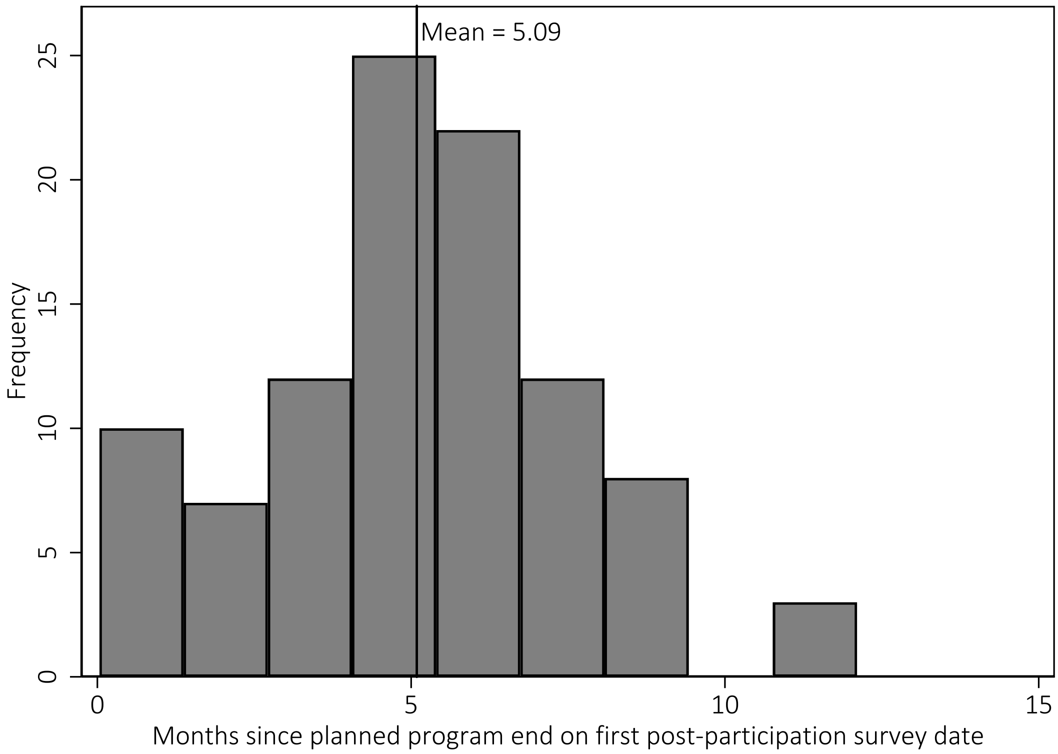

*Notes:* The figure shows the estimated propensity of participating in the program for participants and non-participants in the final estimation sample (see Section 4).
*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

**Figure C.3.:** ATTs on the Employment Probability



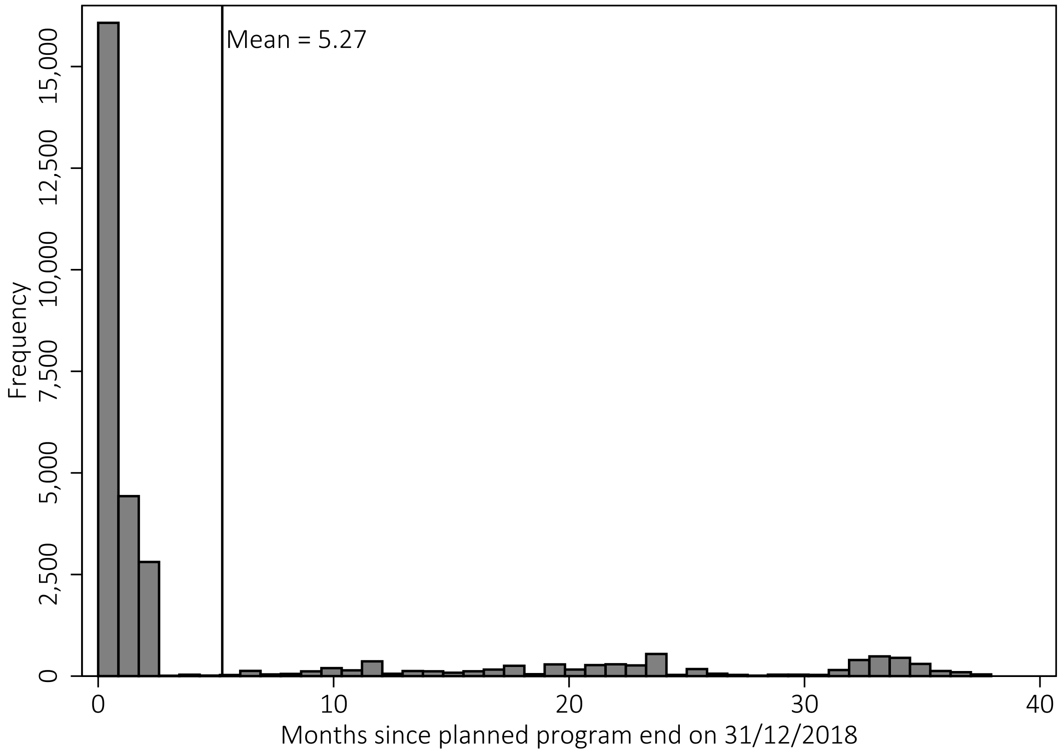

*Notes:* The figure shows the estimated ATTs on the probability of finding non-subsidized employment at different program durations (mean duration of 7 months at $t_1$, 18 months at $t_2$, 29 months at $t_3$) from pooled probit regressions (see equation 3.1) based on the final estimation sample (see Section 3.4). Control variables consist of sociodemographics and the individual employment history (see Table Table C.1.1 in the Appendix). The total number of observations amounts to 8,344 (5,062 at $t_1$, 2,382 at $t_2$, 900 at $t_3$). Standard errors are clustered at the individual level. Whiskers represent 95% confidence intervals.
*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

**Figure C.4.:** Distribution of Time Span between the planned End of Participation and the first Survey thereafter, Final Estimation Sample



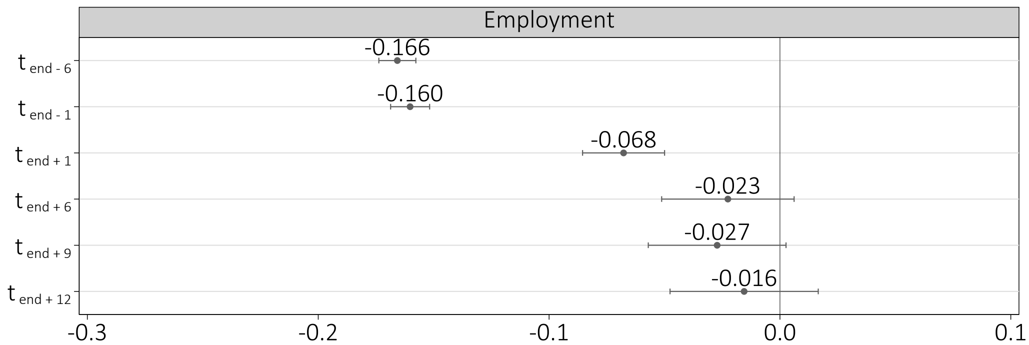

*Notes:* The figure shows the frequency of observations with respect to the timespan between individual planned end of participation and the first survey thereafter in the final estimation sample. The vertical line marks the mean. The total number of observations amounts to 99.
*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

**Figure C.5.:** Distribution of Time Span between planned End of Participation and the End of the Observation Period (31/12/2018), Full Administrative Sample




*Notes:* The figure shows the frequency of observations with respect to the timespan between individual planned end of participation and the end of the observation period (31/12/2018) in the full administrative sample. The vertical line marks the mean. The total number of observations amounts to 59,364.
*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

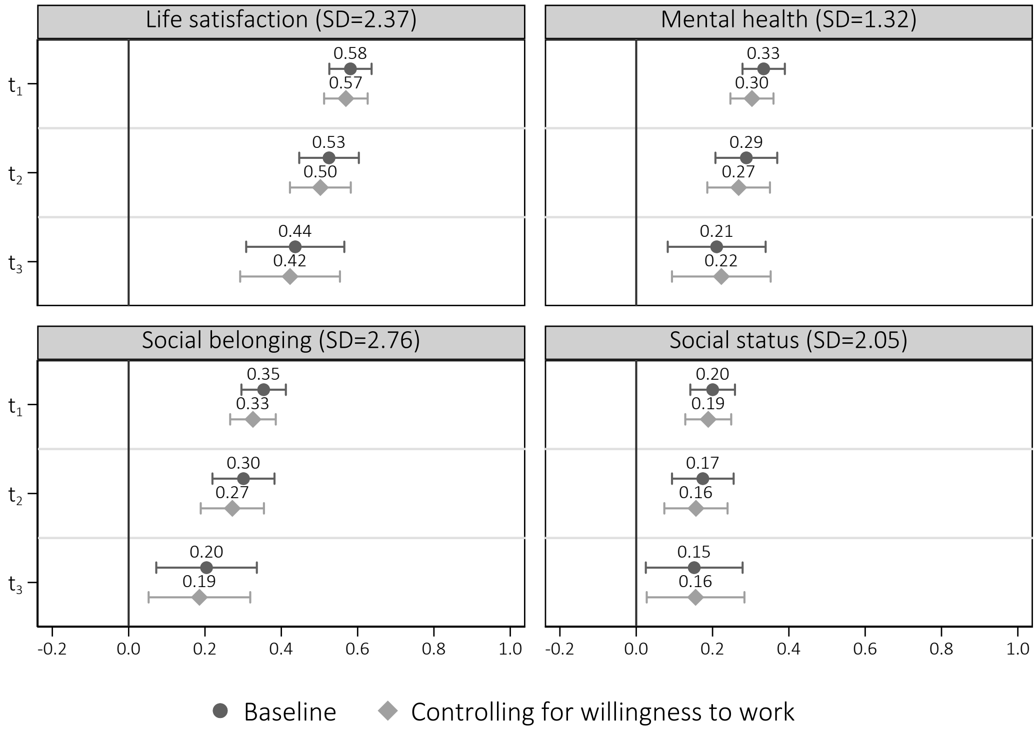
**Figure C.6.:** ATTs on the Employment Probability before and after the planned Program End, Full Administrative Sample




*Notes:* The figure shows the estimated ATTs on the probability of finding non-subsidized employment six months before ($t_{end-6}$), one month before ($t_{end-1}$), one month after ($t_{end+1}$), six months after ($t_{end+6}$), nine months after ($t_{end+9}$) and twelve months after ($t_{end+12}$) participants' individual planned end date as recorded in the administrative data from pooled probit regressions. Control variables consist of sociodemographics and the individual employment history (see Table C.1.1 in the Appendix). The total number of observations amounts to 53,352 (23,280 at $t_{end-6}$, 23,278 at $t_{end-1}$, 6,530 at $t_{end+1}$, 2,250 at $t_{end+6}$, 2,120 at $t_{end+9}$, 1,912 at $t_{end+12}$). Standard errors are clustered at the individual level. Whiskers represent 95% confidence intervals.

*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

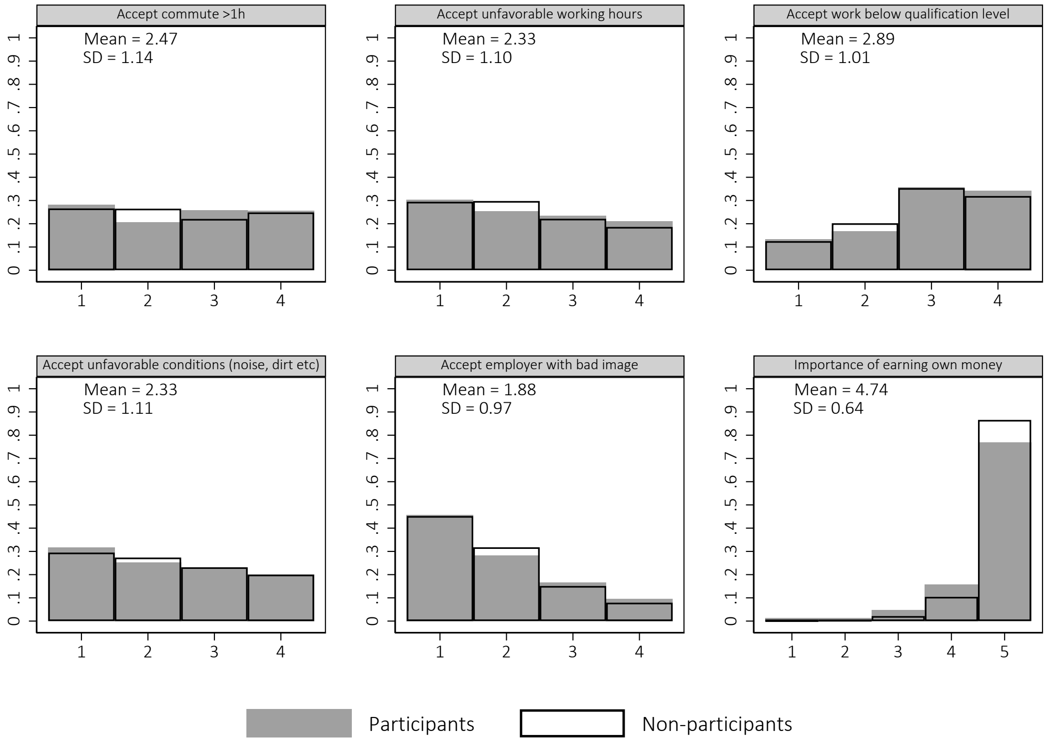
**Figure C.7.:** ATTs on Standardized Outcomes, with and without controlling for Willingness to Work




*Notes:* The figure shows the estimated ATTs at different program durations (mean duration of 7 months at $t_1$, 18 months at $t_2$, 29 months at $t_3$) from pooled OLS regressions (see equation 1) with and without controlling for willingness to work based on the final estimation sample (see Section 4). Control variables consist of sociodemographics and the individual employment history (see Table C.1.1 in the Appendix). The variables reflecting willingness to work are derived from survey data and contain indicators of individual willingness to accept different adverse working conditions like noise, dirt or longer commutes in order to get a job and the importance of earning own money (see Table C.1.2 in the Appendix). For the baseline model, the total number of observations amounts to 8,344 (5,062 at $t_1$, 2,382 at $t_2$, 900 at $t_3$). Due to missing values the total number of observations for the model with controlling for willingness to work amounts to 7,020 (4,366 at $t_1$, 1,934 at $t_2$, 720 at $t_3$). Standard errors are clustered at the individual level. Whiskers represent 95% confidence intervals.

*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

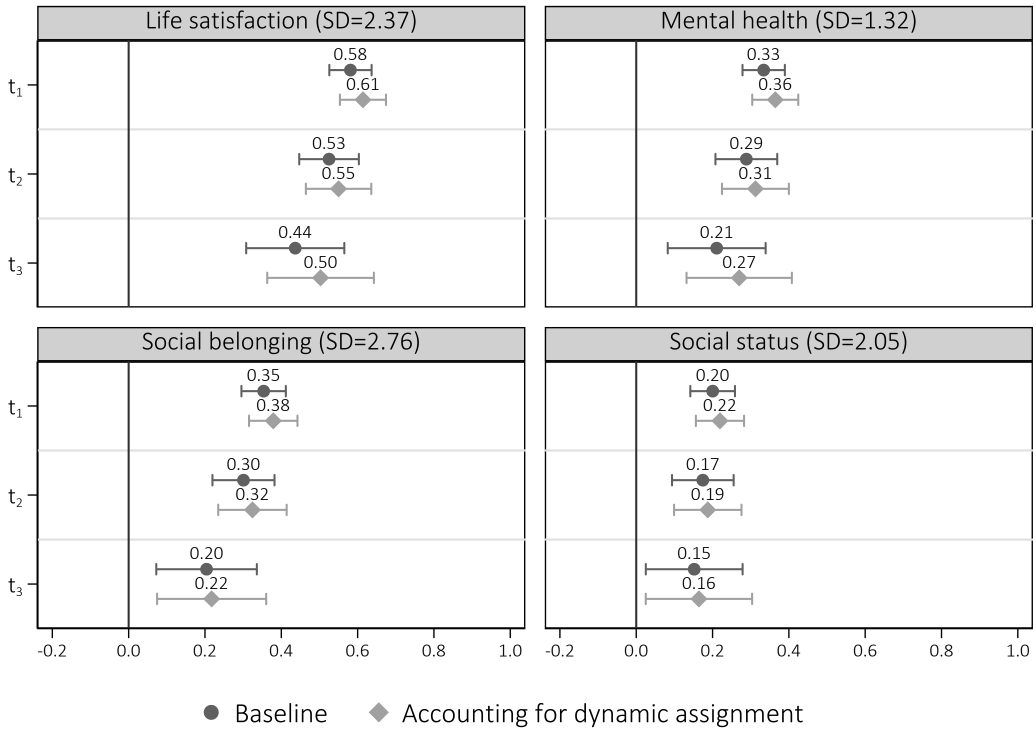
**Figure C.8.:** Distribution of Indicators of Willingness to Work




*Notes:* SD: standard deviation. The figure shows the densities of the control variables for 'willingness to work' (see Table A.2 in the Appendix of the paper) separately for participants and non-participants in the final estimation sample pooled over wave 1 to 3 (see Section 4). Means and standard deviations are computed for the full sample (participants and non-participants).

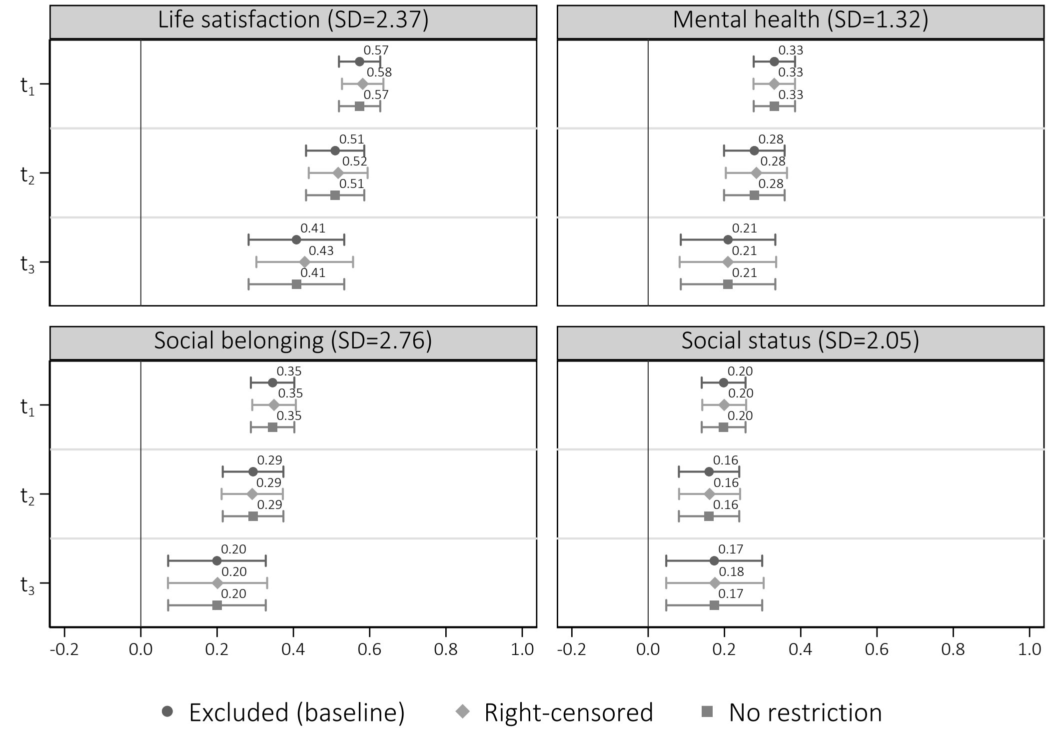
*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

**Figure C.9.:** ATTs on Standardized Outcomes, accounting for Dynamic Assignment



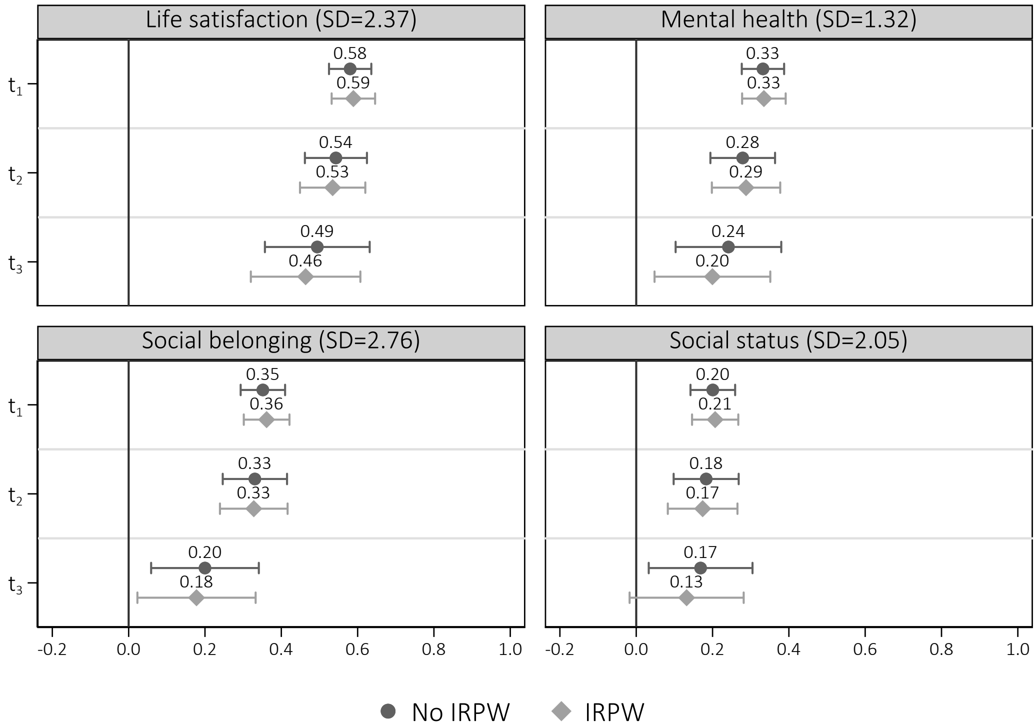

*Notes:* SD: standard deviation. The figure shows the estimated ATTs at different program durations (mean duration of 7 months at $t_1$, 18 months at $t_2$, 29 months at $t_3$) from pooled OLS regressions (see equation 1) based on the final estimation sample (see Section 4). Control variables consist of sociodemographics and the individual employment history (see Table C.1.1 in the Appendix). To account for the dynamic assignment into the program, the sample is restricted to matches where the control individual had stayed unemployed until the program entry of the matched participant. For the baseline model, the total number of observations amounts to 8,344 (5,062 at $t_1$, 2,382 at $t_2$, 900 at $t_3$). The total number of observations for the model with accounting for dynamic assignment amounts to 7,054 (4,310 at $t_1$, 1,978 at $t_2$, 766 at $t_3$). Standard errors are clustered at the individual level. Whiskers represent 95% confidence intervals.
*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

**Figure C.10.:** ATTs on Standardized Outcomes, accounting for Matches of later Participants in SILM or the ESF Federal Program in the Control Group




*Notes:* SD: standard deviation. The figure shows the estimated ATTs at different program durations (mean duration of 7 months at $t_1$, 18 months at $t_2$, 29 months at $t_3$) from pooled OLS regressions (see equation 1) based on the final estimation sample (see Section 4). Control variables consist of sociodemographics and the individual employment history (see Table C.1.1 in the Appendix). We test whether our results are sensitive to how we deal with control individuals who join either SILM or the ESF federal program. In the baseline model we exclude them, alternatively we right-censor a match when the control individual enters either of these programs or we keep them in the sample. For the baseline model, the total number of observations amounts to 8,344 (5,062 at $t_1$, 2,382 at $t_2$, 900 at $t_3$), for the model with right-censored matches to 8,606 (5,250 at $t_1$, 2,430 at $t_2$, 926 at $t_3$) and for the model with no restriction to 8,734 (5,304 at $t_1$, 2,476 at $t_2$, 954 at $t_3$). Standard errors are clustered at the individual level. Whiskers represent 95% confidence intervals.
*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

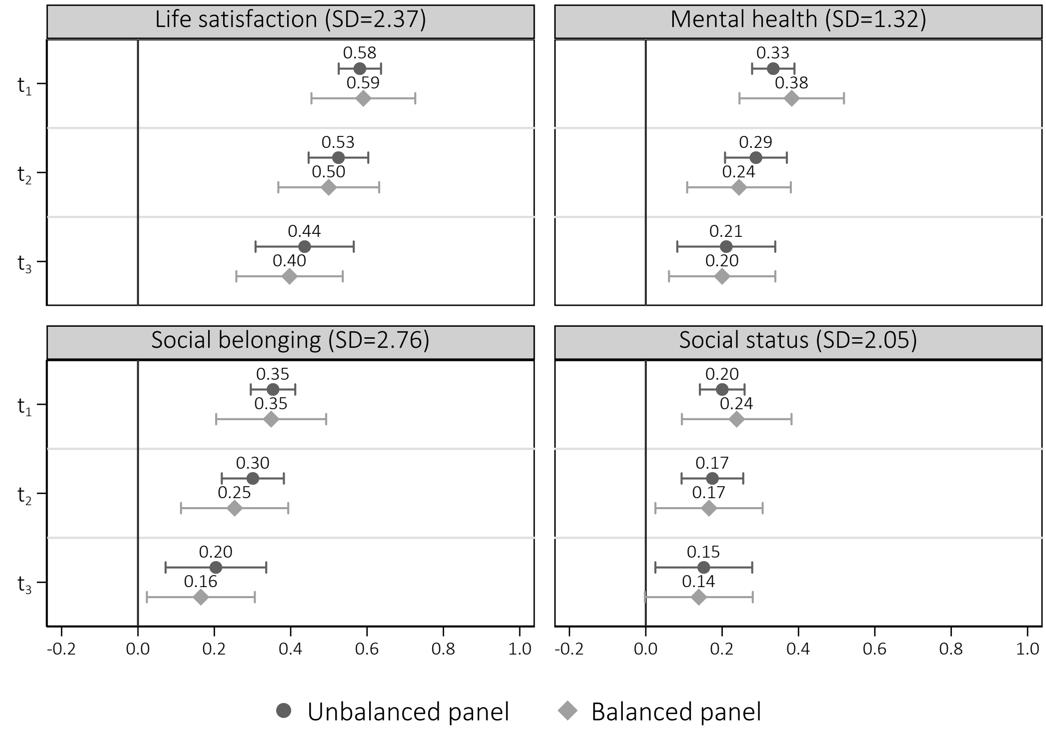
**Figure C.11.:** ATTs on Standardized Outcomes, with and without Inverse Response Probability Weighting




*Notes:* IRPW: inverse response probability weighting, SD: standard deviation. The figure shows the estimated ATTs at different program durations (mean duration of 7 months at $t_1$, 18 months at $t_2$, 29 months at $t_3$) from pooled OLS regressions (see equation 1) based on the final estimation sample (see Section 4). Control variables consist of sociodemographics and the individual employment history (see Table C.1.1 in the Appendix). The model is estimated with and without IRPW. The probability to respond to wave 1 of the survey is estimated based on pre-treatment characteristics and a drop-out indicator from the administrative data. The probability of responding to the follow-up surveys is estimated based on additional information from survey data such as information on the need for support in different life domains, and lags of the outcome variables. The total number of observations amounts to 7,962 (5,056 at $t_1$, 2,142 at $t_2$, 764 at $t_3$). The sample is restricted to observations with non-missing estimated response probability. Standard errors are clustered at the individual level. Whiskers represent 95% confidence intervals.

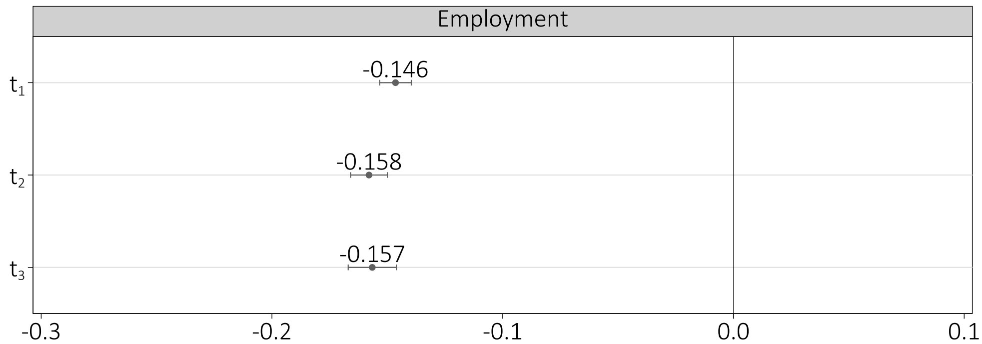
*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

**Figure C.12.:** ATTs on Standardized Outcomes, unbalanced and balanced Panel




*Notes:* SD: standard deviation. The figure shows the estimated ATTs at different program durations (mean duration of 7 months at $t_1$, 18 months at $t_2$, 29 months at $t_3$) from pooled OLS regressions (see equation 1) based on the final estimation sample (see Section 4). Control variables consist of sociodemographics and the individual employment history (see Table C.1.1 in the Appendix). The model is estimated in the unbalanced and balanced panel. The total number of observations in the unbalanced panel amounts to 8,344 (5,062 at $t_1$, 2,382 at $t_2$, 900 at $t_3$). For the balanced panel, the sample is restricted to matches with non-missing values in outcomes and control variables. This results in a total of 2,370 observations (784 in each wave). Whiskers represent 95% confidence intervals.
*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

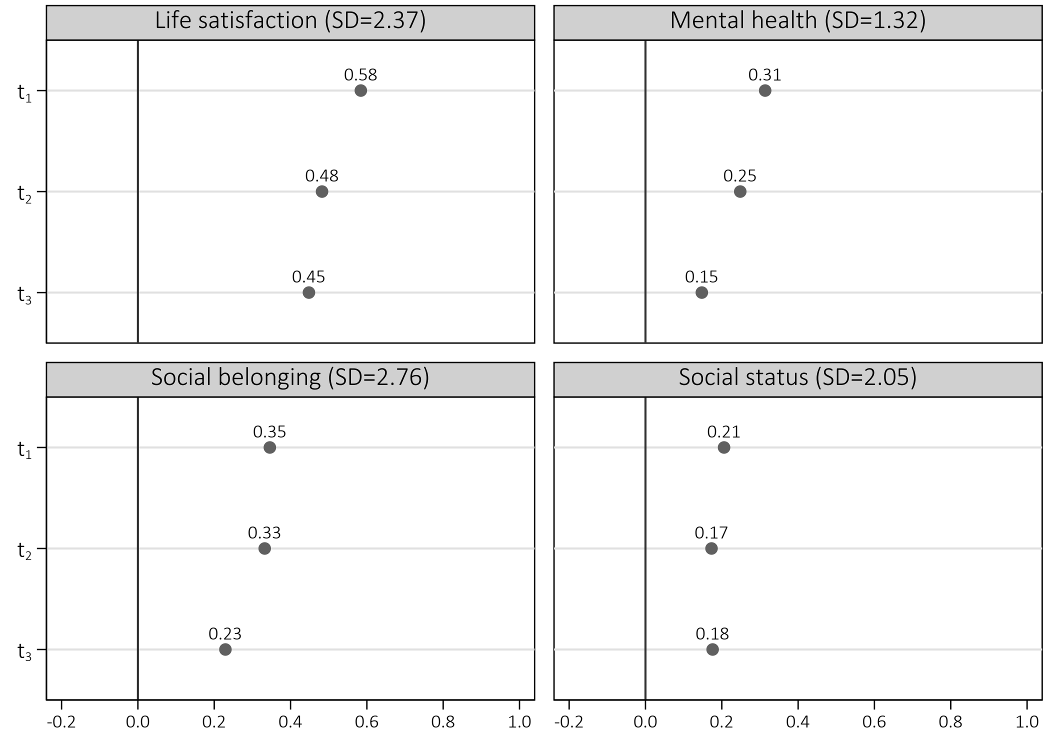
**Figure C.13.:** ATTs on the Employment Probability, Full Administrative Sample




*Notes:* The figure shows the estimated ATT on the probability of finding non-subsidized employment at different program durations (mean duration of 7 months at $t_1$, 18 months at $t_2$, 29 months at $t_3$) from pooled probit regressions (see equation 1) based on the full administrative sample (see Section 4). Control variables consist of sociodemographics and the individual employment history (see Table C.1.1 in the Appendix). The total number of observations amounts to 64,676 (24,610 at $t_1$, 23,862 at $t_2$, 16,204 at $t_3$). Standard errors are clustered at the individual level. Whiskers represent 95% confidence intervals.

*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

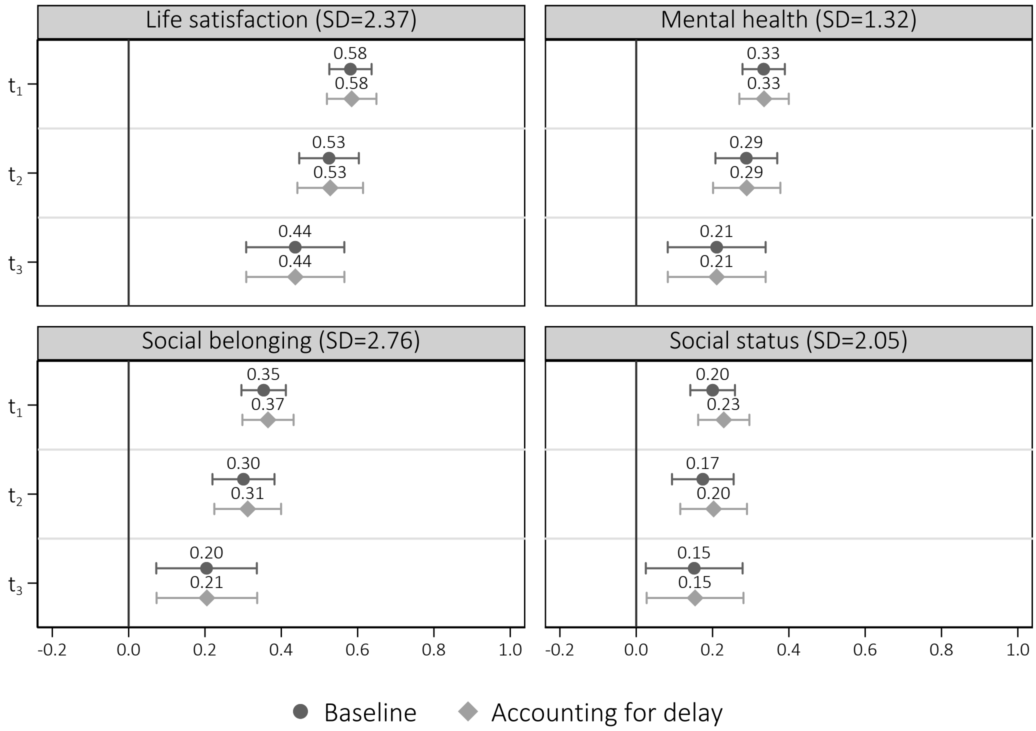
**Figure C.14.:** ATTs on Standardized Outcomes, Imputation with Cell Means from the Survey Sample




*Notes:* SD: standard deviation. The figure shows the estimated ATTs at different program durations (mean duration of 7 months at $t_1$, 18 months at $t_2$, 29 months at $t_3$) from pooled OLS regressions (see equation 1) based on the full sample of participants (see Section 4). The outcomes of survey non-respondents are imputed survey means conditional on treatment status, survey cohort, survey wave and employment status. Control variables consist of sociodemographics and the individual employment history (see Table C.1.1 in the Appendix). The total number of observations amounts to 63,972 (24,501 at $t_1$, 23,594 at $t_2$, 15,877 at $t_3$). Confidence intervals are not shown, because standard errors are not adjusted for additional error from imputing outcomes.

*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

**Figure C.15.:** ATTs on Standardized Outcomes, accounting for Delay in Survey Dates between Participants and Non-participants

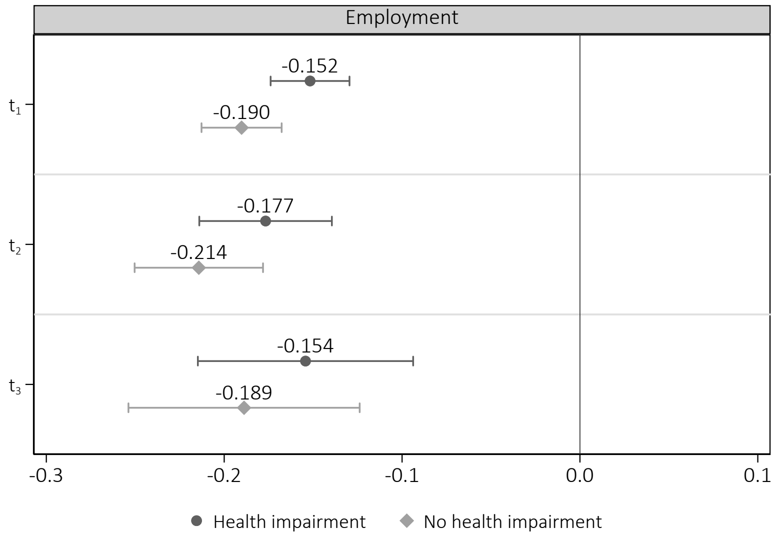



*Notes:* SD: standard deviation. The figure shows the estimated ATTs at different program durations (mean duration of 7 months at $t_1$, 18 months at $t_2$, 29 months at $t_3$) from pooled OLS regressions (see equation 1) based on the final estimation sample (see Section 4). Control variables consist of sociodemographics and the individual employment history (see Table C.1.1 in the Appendix). To account for the delay in the survey dates between participants and non-participants, the duration between the start of the program (11/2015 for early entrants and 01/2017 for late entrants) and the survey date of each wave was added as a control variable. The total number of observations amounts to 8,344 (5,062 at $t_1$, 2,382 at $t_2$, 900 at $t_3$). Standard errors are clustered at the individual level. Whiskers represent 95% confidence intervals.

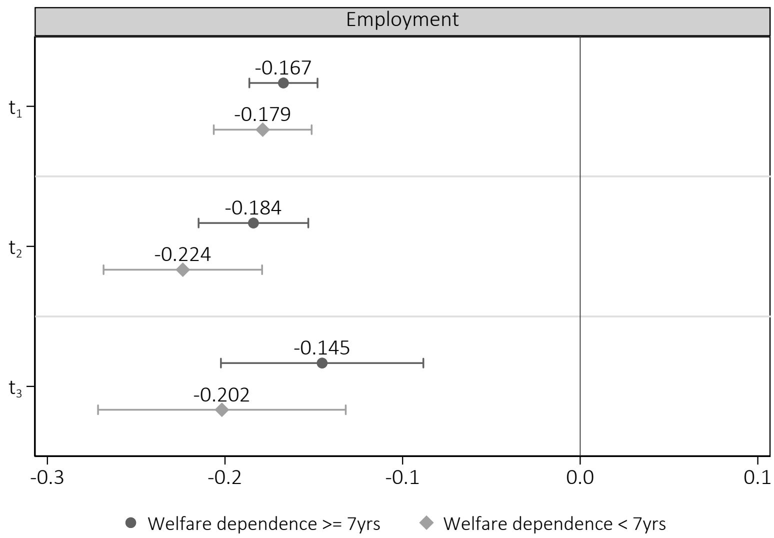
*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

**Figure C.16.:** ATTs on the Employment Probability for Participants with low Employment Prospects

**(a)** Participants with and without Health Impairments




**(b)** Participants with < and ≥ 7 Years of prior Welfare Dependence

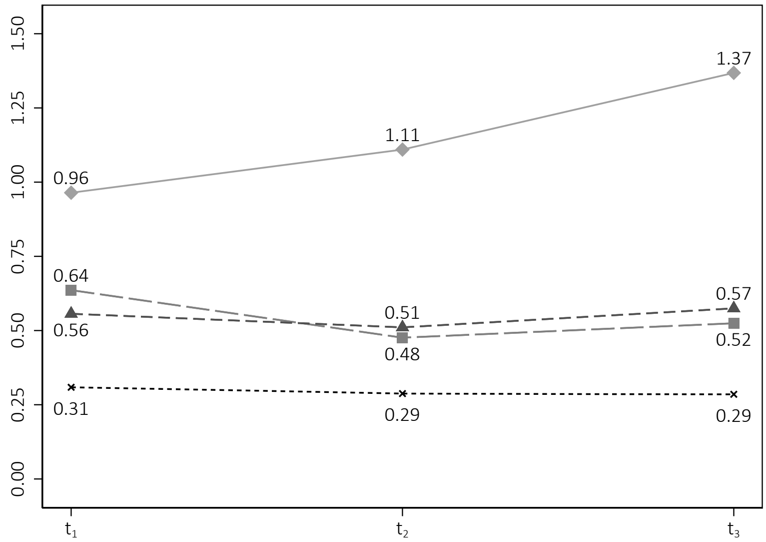



*Notes:* The figure show the estimated ATT on the probability of finding non-subsidized employment at different program durations (mean duration of 7 months at $t_1$, 18 months at $t_2$, 29 months at $t_3$) from pooled probit regressions (see equation 1) for different subsamples of participants and their matches in the final estimation sample (see Section 4). Control variables consist of sociodemographics and the individual employment history (see Table C.1.1 in the Appendix). Sub-figure (a) shows the ATTs separately for participants with and without health impairments and their matched control individuals. The number of observations for individuals with health impairments amounts to 4,306 (2,552 at $t_1$, 1,276 at $t_2$, 478 at $t_3$). The number of observations for individuals without health impairments amounts to 4,038 (2,510 at $t_1$, 1,106 at $t_2$, 422 at $t_3$). Sub-figure (b) shows the ATTs separately for participants with a prior welfare dependence duration below and above the sample mean of seven years and their matched control individuals. The number of observations for individuals with above average welfare dependence amounts to 5,142 (3,110 at $t_1$, 1,498 at $t_2$, 534 at $t_3$). The number of observations for individuals with below average welfare dependence amounts to 3,202 (1,952 at $t_1$, 884 at $t_2$, 366 at $t_3$). Standard errors are clustered at the individual level. Whiskers represent 95% confidence intervals.

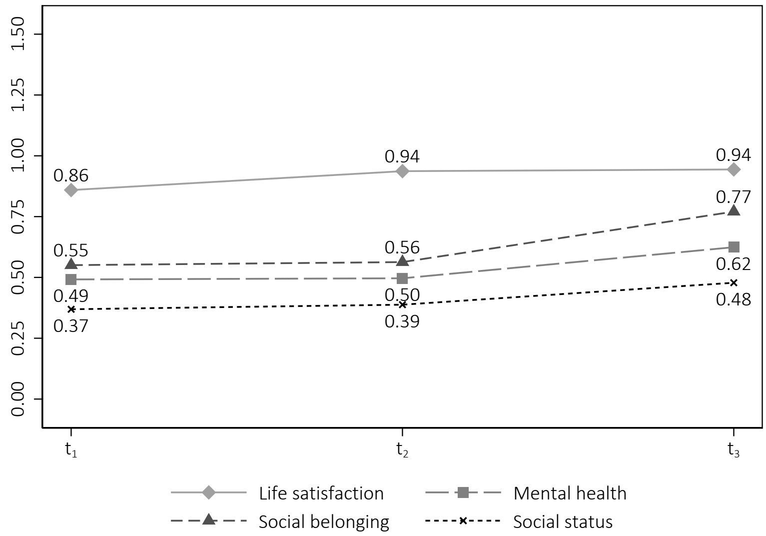
*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).

**Figure C.17.:** Normalized ATTs on Standardized Outcomes for Participants with low Employment Prospects

**(a)** Participants with Health Impairments




**(b)** Participants with $\geq$ 7 years of prior Welfare Dependence




*Notes:* The figure show the estimated ATTs at different program durations (mean duration of 7 months at $t_1$, 18 months at $t_2$, 29 months at $t_3$) from pooled OLS regressions (see equation 1) for different subsamples of participants and their matches in the final estimation sample (see Section 4). Control variables consist of sociodemographics and the individual employment history (see Table C.1.1 in the Appendix). The estimates are normalized by the difference between the share of active participants in the treatment group and the share individuals in non-subsidized employment in the control group. Sub-figure (a) shows the normalized ATTs separately for participants with health impairments and their matched control individuals. The number of observations for individuals with health impairments amounts to 4,306 (2,552 at $t_1$, 1,276 at $t_2$, 478 at $t_3$). Sub-figure (b) shows the normalized ATTs separately for participants with a prior welfare dependence duration above the sample mean of seven years and their matched control individuals. The number of observations for individuals with above average welfare dependence amounts to 5,142 (3,110 at $t_1$, 1,498 at $t_2$, 534 at $t_3$).

*Data:* SILM Evaluation Dataset, see Brussig et al. (2019).