The Role of Institutions, Firm Wage Policies, and Working Time for Labor Market Prospects of Employees
-
Essays in Labor Economics

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M.Sc. Theresa Markefke
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Second reviewer: Prof. Dr. Christian Bredemeier
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1 Introduction

1.1 Overview of the Thesis

Work shapes the lives of individuals like little else: For the majority of households in OECD countries, labor income constitutes the main source of income (Garcia-Mandico et al., 2022). Consequently, whether or not one does well in the labor market will have a fundamental impact on one's material well-being and life satisfaction (L. Winkelmann and R. Winkelmann, 1998). Beyond its importance for individuals, the state of the labor market also bears macroeconomic implications, as high levels of unemployment indicate an underutilization of resources, which is why labor market statistics are important indicators of economic slack. For these reasons, understanding the functioning of labor markets is vital for understanding societies. In this thesis, I use German labor market data to study the role of institutions, firm wage policies, and working time for the labor market prospects of employees. Each chapter presents empirical analyses grounded in theory, employing micro-econometric methods. While the thesis consists of three independent research papers, they all focus on aspects of general questions in labor economics that have not been addressed so far, thus contributing to a better understanding of labor markets.

The role of institutions for labor market outcomes is a core issue in labor market research. A key institutional difference between labor markets in different countries is the degree of regulation. Chapter 2, which is joint work with Rebekka Müller-Rehm, examines the cyclicality of involuntary part-time employment work in a labor market with a high level of worker protection, a setting that has not been studied before with regard to involuntary part-time. A worker is involuntarily part-time employed when working part-time despite preferring a full-time job, thus not being able to tap their earnings potential. This situation is detrimental to workers' happiness (Bell and Blanchflower, 2018a) and constitutes an underutilization of labor. In liberal labor markets such as the US and the UK, employers can freely adjust working hours and react to economic downturns by unilaterally reducing working hours, leading to a countercyclical increase in involuntary part-time work. In Germany, hours adjustments are highly restricted by law, allowing us to study the effectiveness of working time regulation in preventing such an increase during recessions. Following the approach by Valletta et al. (2020), we use federal state-level variation to assess the influence of structural and cyclical factors on the development of involuntary part-time employment. We use data from the European Labour Force Survey that we aggregate to the federal state-level, and supplement it with information from the German Socio-Economic Panel as well as official statistics to take particular German employment forms into account. The first main finding is that the incidence of involuntary part-time work develops countercyclically in Germany as well, as it is significantly positively associated with the unemployment rate. However, the association is much weaker than in the US and in the UK. The second main finding is that, despite there being a positive correlation between unemployment and involuntary part-time on the aggregate level, the underlying mechanisms for this correlation are fundamentally different from those in liberal markets. There, the cyclicality of involuntary part-time work is predominantly driven by changes in hours at the same employer (Borowczyk-Martins and Lalé, 2019). In Germany, these play only a minor role. Analyzing transitions between non-employment, unemployment, voluntary and involuntary part-time as well as full-time, we find that movements in involuntary part-time work can be explained by two particular mechanisms: First, an "added hours effect" in the sense that already employed workers are more likely to want a full-time position in economic downturns. Second, a "reservation hours effect" as job seekers make concessions with regards to their desired hours when labor market conditions are bad and the inflow of unemployed into involuntary part-time is higher compared to good times. This work is the first to document these margins of cyclical hours adjustments, providing novel insights into working hours dynamics over the cycle. Thus, we find that institutions seem to have an alleviating impact, but do not prevent an increase in involuntary part-time during recessions.

Another phenomenon that has gained a lot of attention in labor economics is the earnings difference between workers with interruptions of full-time careers and workers without any interruptions. The transition into working part-time is an important career crossroad and is usually perceived as costly in terms of reduced current and future wages (B. T. Hirsch, 2005; Paul, 2016). However, the reasons for the earnings losses are much less clear. Chapter 3, which is joint work with Christian Bredemeier, sheds new light on the underlying causes for the correlation between part-time work and subsequent career stagnation by looking at the effects of working part-time versus working involuntary part-time on labor market outcomes. The distinction between voluntary and involuntary part-time helps to distinguish between the mechanisms that have been proposed in the literature: self-selection into part-time work by workers expecting a career stagnation into part-time, part-time work as a signal of qualities that are disliked by employers, who react by paying less, and lower human capital accumulation due to the fewer hours. Voluntary part-time is labor-supply driven, i.e., it results from the workers' preferences or constraints in their private life such as taking care of a child. As described above, involuntary part-time means that workers would rather work full-time but cannot, either because they could not find a full-time position or working full-time is no longer possible due to firm-specific reasons. It is thus caused on the demand side of the labor market. Therefore, the distinction between the two types is useful to separate the different channels: while the loss of human capital occurs in both voluntary and involuntary part-time, a signaling effect only emanates from voluntary part-time. Also, by definition, involuntary part-time is not subject to dynamic self-selection into this employment state. Thus, the human capital channel can be isolated from the signaling channel and self-selection. To assess the career costs of part-time, we use data from the German Socio-Economic Panel, a representative household panel survey containing detailed information on individuals' characteristics and employment. In an event-study approach, we compare labor market outcomes for workers who work full-time continuously to those of workers who transition into (involuntary) part-time for the first time. Outcomes are normalized to the year prior to the transition, thus the approach takes into account that these workers are already on different trajectories before the transition. We find that a transition into involuntary part-time work compresses a worker's earnings for a total of three years. In comparison, after a transition into part-time independent of its voluntariness, earnings are reduced for over five years, with larger earnings losses during the first three years than those estimated for involuntary transitions into part-time. Thus, considering involuntary part-time reduces longevity and strength of subsequent earnings losses. These results suggest that the human capital channel plays a role for the negative consequences of part-time work, but does not have the importance that has been attributed to it in the literature so far.

Finally, Chapter 4 studies the role of employers for labor market inequality. More specifically, it examines the implications of firm pay heterogeneity for the provision of on-the-job training. On-the-job training is important for firms, workers, and the economy. A skilled labor force is a crucial ingredient for economic growth. This is mirrored not only by economists' consideration of the importance of human capital in their modeling but also by recurring public debate about the ways the skills of workers could be improved. Whether or not firms are willing to provide such training critically depends on the structure of the labor market as has been demonstrated in the theoretical work of Becker (1964) and Acemoglu and Pischke (1998,1999): In a market with frictions, firms can have an incentive to finance general skills training, which they do not have in a perfectly competitive market. By now there is robust evidence that firms pay different wages to equally skilled workers (Abowd et al., 1999; Card et al., 2013; Card et al., 2018) and that these differences are important for wage inequality. In this chapter, I provide new insights into the relative incentives of firms operating in an imperfectly competitive labor market to invest in the skills of their workforce. To this end, I derive empirical hypotheses based on the strategic wage posting models by Manning (2003) and Fu (2011). I then use two high-quality datasets for the German labor market to test these hypotheses: the German Linked employer-employee data set and the National Educational Panel Survey linked with the social security records of the respondents. Using a number of different cross-sectional models, I analyze first how firms' training activities differ depending on firms' position in the wage distribution, and second, how this interacts with individual workers' training activity and wages. I employ a two-stages-least-squares-approach to account for potential biases arising from the endogeneity of the training and wage decision and the measurement error of wages in the administrative data. Consistent with theoretical predictions, I find that higher-paying firms provide more on-the-job training, a finding that is confirmed by the analysis of individuals' training activities. Wages increase after training, but the correlation is not always statistically significant. My findings imply that workers matched with a lower-paying firm will face worse career and income opportunities than workers matched with higher-paying firms due to lower on-the-job training provided by the firm. Thus, initial wage inequality between workers matched with differently high-paying firms is aggravated by the differential incentives of these firms to invest in the human capital of their workers. The findings have immediate relevance for policymakers designing instruments to encourage employer-provided training. These should be targeted at small, low-paying firms who have lower incentives to invest in training according to my results.

1.2 Contribution to Chapters 2, 3, and 4

Chapter 2, "Labor market regulation and the cyclicality of involuntary part-time work" (*Revise & Resubmit* at the Journal for Labour Market Research), is joint work with Rebekka Müller-Rehm. The chapter is based on a research idea developed by Rebekka Müller-Rehm and myself, in close contact with Michael Krause and Christian Bredemeier. The empirical analysis was conducted by myself. We both collected the literature, developed the content of the paper, and selected the output to be included. Also, the first draft as well as all the revisions of the paper were written by both of us.

Chapter 3, "The career costs of part-time: Human capital, signaling, and dynamic self-selection", is joint work with Christian Bredemeier. The research idea for the paper was developed by Christian Bredemeier. I collected the related literature and conducted the empirical analysis, both of which I discussed extensively with Christian Bredemeier. I revised the first draft of the paper, which was written by Christian Bredemeier.

Chapter 4, "On-the-job training under imperfect competition: The additional burden of working for a low-pay firm" is single-authored and thus based on my own research.

2 Labor Market Regulation and the Cyclicality of Involuntary Part-time Work

Authors: Theresa Markefke and Rebekka Müller-Rehm

2.1 Introduction

In many developed economies, a sizable share of the labor force works fewer hours than they would like to. This means that there is an underutilization of labor beyond unemployment, and the rate of involuntary part-time workers has become a useful additional measure of labor market slack.[1] Since workers who work part-time despite preferring a full-time job are already participating in the labor market, they offer the potential to easily increase aggregate working hours. From a welfare perspective, involuntary part-time (IPT) deserves attention too: employees who work part-time involuntarily suffer from not realizing full-time earnings, and working below one's desired hours is detrimental to workers' happiness, as argued by Friedland and Price (2003) and Bell and Blanchflower (2018a).

It is well known that involuntary part-time behaves strongly countercyclically in rather liberal labor markets, such as the US and UK. Thus underemployment rises during recessions along two margins, unemployment and under-utilization of employed workers. The costs of recessions are higher than looking solely at the unemployment rate would suggest, because many workers get an hours' cut in downswings (Borowczyk-Martins and Lalé, 2019; Valletta et al., 2020). In fact, recent evidence by Borowczyk-Martins and Lalé (2019) shows that movements in the share of part-time work in the US and UK are predominantly driven by transitions between full-time and part-time work at the same employer rather than by mobility between jobs. This is possible because in those countries employers are allowed to reduce their workers' hours at will. While this gives employers the flexibility to respond to changes in demand, it means that workers are faced with sudden unwanted changes in hours worked.

In continental European countries, labor markets are often more regulated than Anglo-Saxon labor markets. This raises the question of how the cyclical response of part-time and involuntary part-time work differs when both the intensive and extensive margin of hours adjustment are restricted by legislation. For example, in Germany, workers are more strongly protected both from dismissal and from reductions in paid working hours. Unlike in less regulated labor markets, employers thus *cannot* unilaterally reduce working hours. In this chapter, Rebekka Müller-Rehm and I evaluate the effectiveness of these regulations in preventing a rise in involuntary part-time during downswings. We apply the approach of Valletta et al. (2020) in order to assess the influence of cyclical and structural factors on the variation of the share of involuntary part-time, by exploiting regional variation in these factors. Furthermore, we analyze

[1] In 2014, for example, Janet Yellen, at the time chair of the Board of Governors of the Federal Reserve System emphasized at the Federal Reserve Bank of Kansas City Economic Symposium that involuntary part-time was one of the main reasons *"why the current level of the unemployment rate may understate the amount of remaining slack in the labor market"*.

the transitions of workers between non-participation, unemployment, full-time and voluntary as well as involuntary part-time work.

We find that as in the US and UK, changes in the incidence of involuntary part-time are mainly associated with variations in the unemployment rate, i.e., that involuntary part-time fluctuates countercyclically in Germany, as in those countries. However, the cyclicality is attenuated: In our preferred specification, a one percentage point increase in the regional unemployment rate leads to a change of about 0.17 percentage points in the IPT share, which is less than one-third of the US effect.

By looking at German data, we are able to provide novel insights on working hours dynamics over the cycle. Despite the fact that there is a positive relationship between involuntary part-time work and unemployment on the regional level, the mechanisms underlying this correlation are fundamentally different than in the US and UK. Transitions from full-time to IPT that take place at the same employer play a minor role in Germany. While their share in all IPT inflows accounts for about one third in the US, it is 11% in Germany. This raises the question of which alternative mechanisms contribute to the countercyclical patterns of IPT. To shed light on the matter, we analyze how workers' transitions between employment states vary with the regional unemployment rate. Two interesting patterns emerge.

Transition probabilities suggest that there is an "added hours effect" and a "reservation hours effect". Analogously to the added worker effect, the added hours effect means that some individuals would like to work more in economic downturns. Already employed workers are more likely to want a full-time position compared to a time with good economic conditions. Thus, while the added worker effect refers to the extensive margin, the added hours effect refers to the intensive margin. We are the first to document this dimension of cyclicality in labor supply. The reservation hours effect refers to the observation that job seekers make concessions with regard to their desired hours when labor market conditions are bad. Unemployed individuals are more likely to accept a part-time position even though they prefer a full-time position. It seems that unemployed workers choose a reservation level of hours, which varies over the cycle, just as reservation wages. Our findings contribute to a better understanding of the labor market adjustment in a setting with strict regulation. While the German regulation indeed hampers hours reductions, there are other market mechanisms that lead to an anticyclical pattern of involuntary part-time.

There are some institutional peculiarities in the German labor market that we make sure to sufficiently consider in our analysis as they could affect hours adjustment: marginal employment, working time accounts, and short-time work. Our main finding regarding the link between unemployment and involuntary part-time is not qualitatively affected when we include them in our regression. Interestingly, only the incidence of working time accounts is significantly associated with the development of involuntary part-time. The association is positive, suggesting that employers hire more part-time instead of full-time employees when the firm uses working time accounts and that this comes with a higher incidence of involuntary part-time. An explanation is that employers can ask part-time employees to work full-time hours when needed without paying overtime premia as long as working time accounts are balanced over time. This suggests that employers also use working time accounts as a strategy to adjust workers' hours to varying needs.

The chapter proceeds as follows. In Section 2.2, we give a short overview of our data and key measurement concepts. Section 2.3 provides the theoretical (2.3.1) and institutional (2.3.2) background for our analysis. It also contains descriptive evidence regarding the cyclicality of IPT in Germany and the structural factors associated with it (2.3.3). We turn to our empirical analysis in Section 2.4. After investigating the relationship between cyclical and structural factors with IPT on the macroeconomic level (2.4.1), we turn to the underlying mechanisms (2.4.2). In Section 2.5, we confirm that our key findings do not depend on specific forms of employment. Section 2.6 concludes.

2.2 Data and Key Concepts

In this section, we describe our data and present some key measurement concepts. We primarily use yearly cross-sectional micro data from the European Labour Force Survey (LFS), which collects demographic and employment information on households in European Countries. For Germany, it includes about 830,000 respondents each year. Our analysis covers the time period 2002 through 2017, as information on federal states ("Bundesländer") is only available as of 2002. Since we exploit variation of cyclical, structural, and institutional factors on the federal-state level, this information is crucial.

The LFS provides information on relevant socio-demographic characteristics of employees and on their occupation as well as industry. Most importantly, it allows for the identification of (involuntary) part-time workers. The definition of part-time work varies in the literature. The part-time measure in the LFS is based on self-assessment, but 95% of self-identified part-time workers work 31 hours or less, which is in line with rather restrictive part-time definitions in the literature. To make sure we only rely on plausible self-assessments, we further restrict our definition of part-time work to those working no more than 35 hours in total.[2] Respondents are also asked why they work part-time. Those who are in part-time employment because they "could not find a full-time job" are considered IPT. If instead, respondents state to work part-time for family or school-related reasons, for example, they are working part-time voluntarily.

Our main indicators of interest are the yearly unemployment rate and the share of IPT workers in all workers.[3] Similarly, structural factors will also be measured as the share of a certain

[2] This means that respondents who work more than 35 hours by combining two jobs are not considered involuntary part-timers.

[3] We use non-self-employed, non-agricultural employment for our analysis and further exclude workers producing for their own use and employees of extraterritorial organizations and bodies.

demographic group or industry in the whole population or all employed persons. To have an internationally harmonized measure of unemployment we use the ILO definition. Respective data on unemployment and GDP growth is drawn from Eurostat.

Some steps of our analysis require further information. Additional data is necessary to calculate transition probabilities in Section 2.4.1. Here we use the Mikrozensus, which can be combined into a panel in certain time periods. Since it forms the basis of the LFS, the measurement of IPT is identical in both data sets. We are interested in aggregate-level transition rates between employment states which we relate to federal state-level variation of labor market conditions. This information is available on a yearly frequency. To consider the incidence of particular German employment forms in Section 2.5, we need data on the prevalence of these types of employment on the federal-state level. Analogous to our main analysis, we calculate the share of marginally employed workers, the share of workers on short-time work compensation, and the share of workers using working time accounts relative to all workers to account for them in our empirical analysis. For this, we draw on data from the Federal Employment Agency as well as the Socio-Economic Panel. Appendix A.6 provides an overview of our data sources.

2.3 Involuntary Part-Time Work: Theory and Evidence for Germany

In this section, we first provide some theory on the demand for part-time. We then discuss the institutional setting and present descriptive evidence for Germany. It thereby becomes clear why Germany is a useful example case for a country with rather strict regulation of working hours on the extensive and intensive margin and suitable for evaluating the effectiveness of working time regulation in preventing IPT.

2.3.1 Demand for Part-Time

In this Section, we briefly discuss why employers might prefer part-time employees over full-time employees although using part-time labor will usually be associated with higher overall fixed costs. While fixed costs of employment have decreased over time, they are still relevant for most jobs (see for example Neubäumer and Tretter, 2008). The most important reasons for certain employers wanting to hire part-time employees despite higher overall fixed costs are the following.

Employers hire part-time employees for production requirements. Some firms face regular and predictable demand peaks. Hiring part-time workers allows them to use their work force more flexibly. The need for part-time labor can also stem from opening hours that cannot be adequately covered by full-time staff. Studies on the determinants of part-time demand find that part-time work can increase firm productivity for those reasons (see for example Euwals and Hogerbrugge, 2006; Devicienti et al., 2015). If those industries that require a high degree of flexibility become relatively more relevant compared to those that rely more on full-time work, this will result in a higher share of IPT, all else being equal. Production might not only depend on part-time for organizational reasons. There is a large literature that tries to investigate the effect of working hours on individual productivity, with many studies finding decreasing returns to hours. See Collewet and Sauermann (2017) for recent evidence and an overview of previous studies.

Other reasons for using part-time labor stem from business cycle developments, for example, if employers prefer decreasing working hours over laying off part of their workforce during economic downturns. This is mainly due to employers' incentives to hold on to human capital and to avoid redundancy payments. This reasoning implies a negative relationship between economic activity and the incidence of IPT. In fact, IPT is observed to behave countercyclically in many countries (see for example Bredemeier and Winkler, 2017a; Bell and Blanchflower, 2018b; Borowczyk-Martins and Lalé, 2019; Valletta et al., 2020). Moreover, some employers hire part-time employees to screen them for full-time positions. If they are risk-averse, they will be even more likely to do so in periods of economic downturns to decrease uncertainty (Buddelmeyer et al., 2004).

Employers may also expand their workforce and reduce the number of hours per employee for strategic purposes with regard to wages. Dossche et al. (2019) analyze overhiring strategies in an intra-firm bargaining framework with extensive and intensive margins. Under the assumption that the marginal disutility of working is increasing in the number of hours, firms overhire and reduce hours as they can thereby enforce a reduction in wages.

Depending on the institutional framework, legal requirements might impose additional incentives for using part-time labor or prevent employers from doing so.[4] Therefore, country-specific regulations have to be taken into account as well.

2.3.2 Institutions and the Choice of Working Hours

When negotiating a new employment contract, employers and employees are fairly free in choosing the number of working hours. The framework within which the negotiations can take place in Germany is mainly restricted by laws that limit the maximum permissible working time. Further restrictions may result from collective or works council agreements. Within that scope, negotiation outcomes can be assumed to depend on employers' and employees' preferences as well as their respective bargaining position.

If employers hire part-time employees, they are bound to treat them equally to full-time employees[5] by the European Council Directive 97/81/EC and respective German law, with exceptions

[4] In the US, the Affordable Care Act (ACA) imposes institutional incentives for using part-time work (see for example Jolevski and Sherk, 2014; Garrett, 2014; Even and Macpherson, 2015).

[5] Legally, part-time and full-time work is not clearly defined by a specific working time. Instead, the respective employment relationship is taken into account. The benchmark is a comparable full-time employee of the same

for marginal employment ("minijobs"). Marginal employment is a particular German form of employment that is defined by income limits.[6] Especially with binding minimum wages, those limits imply a maximum number of working hours. Minijobs are partly exempt from social security contributions, which induces incentives for restricting working hours. In 2003, the Hartz I and II reforms inter alia expanded the possibilities to hire marginal employees. In Section 2.5, we examine whether marginal employment plays an important role for the extent of IPT, specifically differentiating between marginal employment as only employment or secondary employment. In many respects, the Hartz reforms can be considered the most important set of reforms of the German labor market in the last decades as they brought about fundamental changes in the regulation of different forms of employment and in unemployment benefits. We therefore come back to those reforms at various points in the analysis, but they are not the main focus of this analysis (for an overview of the reforms and their performance see for instance Jacobi and Kluve, 2006; Giannelli et al., 2016; Jung and Kuhn, 2019).

Once an employment contract is in force, there may be various reasons to change the working hours that employers and employees initially agreed on. From employers' perspective, organizational requirements might change over time. Even more importantly, the economic situation might change. Borowczyk-Martins and Lalé (2019) show that employers in the US and UK adjust employment via the intensive margin. They observe that the share of part-time workers strongly increases during recessions. This rise is due to changes in the transitions between full-time and part-time rather than transitions between unemployment/non-employment and part-time. Moreover, these transitions between full- and part-time work mostly occur at the same employer. In Germany, however, reductions of working hours are usually only possible if employees agree to them unless flexible hours have been stipulated.[7] Unilateral reductions are only admissible in particular circumstances which we explain in the next paragraph. In addition, there is a comparatively high level of protection against dismissal. These major differences to the far more liberal labor markets in the UK and especially in the US motivate our analysis.

In Germany, there are a number of exceptions that allow employers to unilaterally reduce working hours under very restrictive circumstances for a certain time span. The most important are the following two: First, short-time work ("Kurzarbeit") is a government subsidy that firms can apply for when they face short-term demand slumps (firm-specific component) and which is also frequently facilitated during recessions (discretionary component)(Balleer et al., 2016). In short-time work, working hours are reduced and associated losses in income are compensated at a rate of about 60% by the social security system or the state. Whether short-time work results in IPT cannot be predicted easily as it depends on employees' preferences regarding hours/wage combinations. Second, working time accounts ("Arbeitszeitkonten") allow for ad-

company. If an employee regularly works less, he is legally considered a part-time worker.

[6]The income may not regularly exceed 450 euros.

[7]Contracts that stipulate on-call working hours, especially those that do not stipulate a minimum number of working hours, are rare in Germany (see for example Tobsch et al., 2012).

justing working hours dynamically. The basic idea behind working time accounts is that over a certain period of time, employers can have their employees work longer or shorter hours than collectively agreed. Employees thereby collect working time credits or debits in an individual working time account, which are later compensated for by additional free time or work. Theoretically, the use of working time accounts can have opposing effects on the incidence of IPT.[8] In Section 2.5, we also look at the relevance of short-time work and working time accounts for the incidence of IPT.

Not only employers but also employees might want to change their working hours. Employers are usually obligated by the "Teilzeit- und Befristungsgesetz" (TzBfG) to allow for a reduction of working hours unless they qualify for exceptions because of certain firm characteristics. Since the amendment of the "Bundeselterngeld- und Elternzeitgesetz" (BEEG) in 2015, it has been even easier for parents to reduce hours. This should not lead to IPT. However, while part-time employment might be voluntary at first, it can result in IPT if preferences for working hours change again. Until last year, employees had only been allowed to reduce hours, but had not been entitled to increase them again against their employer's will. This is especially relevant for women, who often reduce their working hours after childbirth and want to increase their working time again when the child has reached a certain age.[9]

In summary, unlike in the US, employers' choices of working hours in Germany are restricted in many ways. Reductions in working hours are thus relatively more costly for employers. This raises the question of whether employers adjust differently to economic shocks.

2.3.3 Descriptive Evidence on Involuntary Part-Time Employment in Germany

In this Section, we present some key facts on the incidence of involuntary part-time employment in Germany as well as its cyclicality and variation across demographic groups, occupations, industries, and federal states.

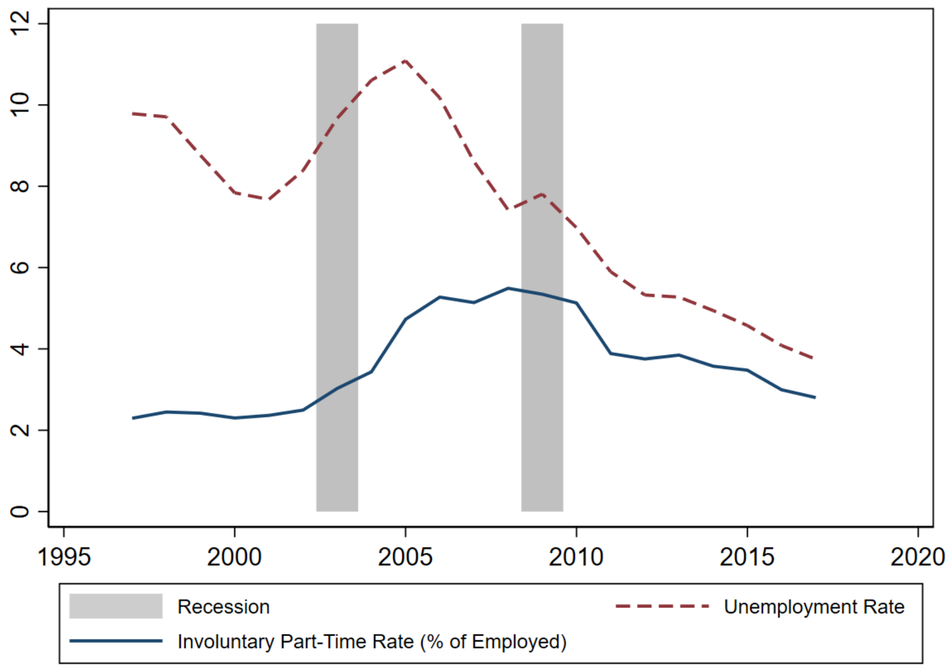
For a first impression, Figure 1 illustrates the aggregate time-series patterns of IPT as a share of total employment and the unemployment rate between 1997 and 2017, and puts them in the context of recession periods. IPT ranges between 2.2% and 5.5%, which is a magnitude quite comparable to other developed countries (see for example Glauber, 2017). In absolute numbers, this means that between 800 thousand and two million people were working involuntarily in part-time during the sample period. IPT and unemployment develop in a somewhat parallel manner but the cyclical patterns of IPT are not entirely evident from an aggregate perspective. There also seems to be no clear response to recessions.[10]

[8] In addition to these two important exceptions, there are working time corridors as a further instrument, which is, however, not widely used (see for example Burda and Hunt, 2011).

[9] However, since 2019, employees can opt for a temporary reduction of hours under certain circumstances ("Brückenteilzeit"). Whether the new law applies, depends mainly on the size of the company and operational and organizational particularities. As our sample period does not include 2019, this does not affect our analysis.

[10] The unemployment rate and GDP growth are not as closely related in the German economy as they are in other countries. While the fall in GDP growth experienced in the crisis of 2009 was the largest since the Second

Figure 1: Involuntary Part-Time Employment and Unemployment in Germany



Notes: This figure shows the evolution of the unemployment rate (dashed red line) and the involuntary part-time rate (solid blue line) over the period 1997 to 2017. Recessionary periods are indicated in gray. Data sources are the European Labour Force Survey and Eurostat. Own calculations using sampling weights of the Labour Force Survey.

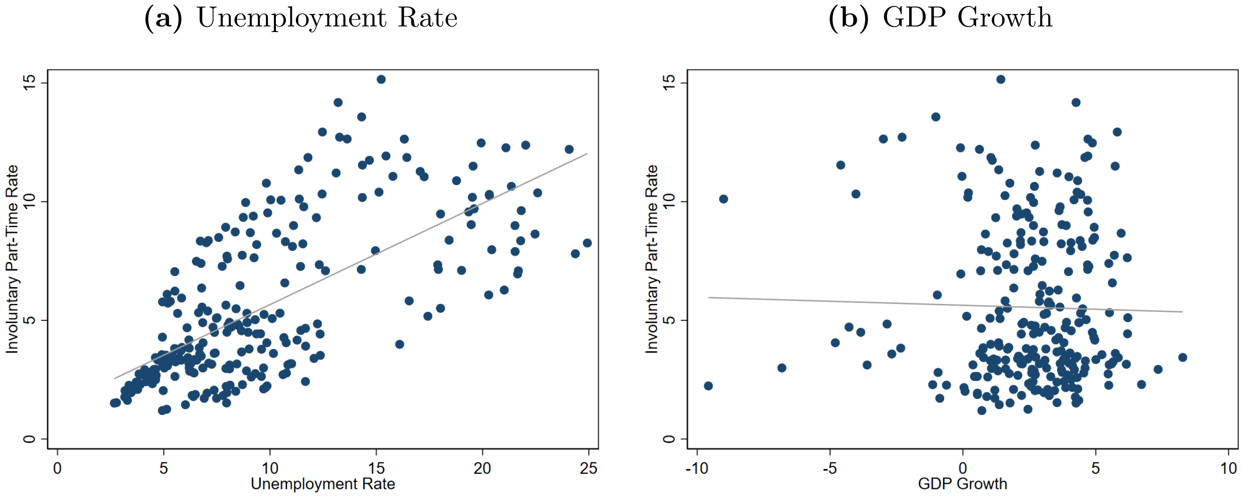
Our analysis does not rely on the aggregate business cycle because we assume that the decisions of labor market agents are determined by local rather than aggregate conditions. We define labor markets by federal states and exploit federal state-level variation in demand and supply factors to assess the determinants of IPT. Supporting this assumption is the fact that mobility between federal states in Germany is rather low. Appendix A.6 shows descriptive statistics on the commuting and moving behavior of people between states. Ideally, we would want to define labor markets by commuting zones. However, crucial information, especially the incidence of IPT, is only available for federal states.

To explore the relationship between cyclical indicators and IPT on the federal-state level, Figure 2 plots the state-specific IPT share and unemployment rate (left panel) as well as GDP growth (right panel) for the years between 2002 and 2017. There is a positive correlation between unemployment and IPT, despite substantial deviation from the fitted line. As there seems to be no relationship between IPT with GDP growth at the federal-state level, we focus on unemployment as the key cyclical indicator in our empirical analysis. We do, however, control for GDP growth.

Based on Figure 2 we do not know whether the stronger relationship between unemployment

World War, there was no equivalent rise in unemployment. The causes of this particularly German phenomenon have been studied extensively by other authors (see for example Burda and Hunt, 2011).

Figure 2: Correlation Between Involuntary Part-Time Employment and Cyclical Indicators in German Federal States



Notes: This figure shows the correlation within German federal states between the involuntary part-time rate and the unemployment rate (left) and GDP growth (right) for the sample period 2002 to 2017. Data sources are the European Labour Force Survey and Eurostat. Own calculations using sampling weights of the Labour Force Survey.

and IPT stems from level differences between federal states or movements over time within states. Table 1 provides detailed information on the incidence of (involuntary) part-time by demographic groups and industries as well as federal states. It reads as follows: for any given row, the table lists the share of the respective group that works part-time (involuntarily) for the years 2002, 2010, and 2017 in order to span our sample period. Additionally, the last three columns show the overall employment share of each group. There are substantial level differences in the shares of IPT employment between states, with IPT being particularly high in Eastern Germany. However, there is also considerable variation within states over time. Our regression analysis only exploits within-state variation.

We now turn to structural factors that are potentially related to IPT. Structural factors pertain to long-term changes in the demographic composition of the population and in the structure of the economy, i.e., changes in the relevance of different industries. There is considerable variation in the incidence of IPT across demographic groups. Both part-time in general and IPT are more prevalent among women. Depending on gender, the share of IPT also differs strongly between age groups. While men are more prone to becoming IPT when they are young, the opposite is true for women. Overall, shifts in the demographic composition of the workforce as well as developments over time within groups can influence the level of IPT, which is why we account for demographics in our regression analysis.

The incidence of IPT also differs greatly between industries (which in turn is related to gender differences, see for example Acosta-Ballesteros et al., 2021). It is particularly prevalent in industries that comprise services, like for example Hotels and Restaurants or Other Services. The high relevance of part-time labor for service industries is frequently highlighted in the literature (see for example Buddelmeyer et al., 2004; Euwals and Hogerbrugge, 2006). Organizational flexibility is often particularly important for service providers, whose businesses rely on certain opening hours and are subject to short-term demand peaks. Variations in industry shares between federal states and over time can be relevant for the prevalence of IPT in a state as both the intensity of part-time work within an industry as well as the relevance of that industry in the whole economy can vary.

Table 1: Incidence of (Involuntary) Part-Time Work by Labor Market Groups

	Involuntary Part-time			**Part-time**			**Employment Share**		
	2002	**2010**	**2017**	**2002**	**2010**	**2017**	**2002**	**2010**	**2017**
All	0.025	0.051	0.028	0.207	0.263	0.279	1	1	1
Demographic Groups									
All 17-26	0.020	0.041	0.019	0.140	0.221	0.258	0.116	0.109	0.096
Men 27-36	0.013	0.040	0.018	0.066	0.111	0.108	0.123	0.106	0.112
Women 27-36	0.033	0.057	0.032	0.318	0.349	0.337	0.100	0.091	0.094
Men 37-56	0.008	0.021	0.015	0.032	0.059	0.064	0.295	0.283	0.252
Women 37-56	0.051	0.088	0.045	0.449	0.519	0.521	0.243	0.248	0.226
All 57-66	0.026	0.061	0.037	0.233	0.267	0.297	0.113	0.147	0.193
All 67+	0.012	0.011	0.007	0.599	0.668	0.711	0.010	0.016	0.027
Occupations									
Clerks	0.021	0.052	0.019	0.292	0.344	0.336	0.130	0.122	0.132
Craft	0.008	0.021	0.012	0.046	0.069	0.082	0.168	0.147	0.125
Elementary Occupations	0.078	0.168	0.099	0.442	0.525	0.562	0.075	0.076	0.074
Managers	0.004	0.008	0.003	0.049	0.085	0.074	0.066	0.064	0.048
Plant & Machine Operators	0.013	0.027	0.017	0.077	0.113	0.134	0.073	0.070	0.061
Professionals	0.019	0.020	0.013	0.162	0.199	0.241	0.139	0.163	0.185
Services and Sales	0.057	0.112	0.065	0.393	0.485	0.454	0.120	0.126	0.142
Technicians	0.020	0.037	0.016	0.209	0.262	0.268	0.214	0.219	0.231
Industries									
Business Activities	0.034	0.074	0.034	0.259	0.322	0.330	0.089	0.107	0.112
Construction	0.012	0.022	0.013	0.078	0.104	0.118	0.078	0.069	0.070
Education	0.053	0.074	0.038	0.349	0.410	0.438	0.058	0.067	0.069
Electricity, Gas and Water	0.003	0.028	0.008	0.063	0.104	0.110	0.008	0.013	0.014
Financial Intermediation	0.007	0.016	0.009	0.170	0.197	0.241	0.038	0.035	0.032
Health and Social Work	0.034	0.064	0.041	0.316	0.395	0.429	0.109	0.125	0.132
Hotels and Restaurants	0.057	0.099	0.069	0.295	0.443	0.456	0.035	0.040	0.038
Manufacturing	0.008	0.017	0.008	0.103	0.118	0.120	0.242	0.210	0.195
Other Services	0.038	0.075	0.043	0.296	0.401	0.428	0.058	0.042	0.043
Public Administration	0.014	0.023	0.012	0.158	0.181	0.209	0.082	0.076	0.070
Logistics & Communication	0.021	0.045	0.024	0.141	0.206	0.206	0.058	0.084	0.083
Wholesale and Retail Trade	0.040	0.085	0.042	0.296	0.351	0.332	0.143	0.131	0.142

Table 1 continued on the next page.

	Involuntary Part-time			Part-time			Employment Share		
	2002	**2010**	**2017**	**2002**	**2010**	**2017**	**2002**	**2010**	**2017**
Federal States									
West									
Schleswig-Holstein	0.021	0.044	0.025	0.232	0.270	0.301	0.034	0.035	0.034
Hamburg	0.025	0.034	0.024	0.222	0.247	0.250	0.022	0.023	0.024
Lower Saxony	0.021	0.049	0.027	0.229	0.279	0.284	0.092	0.091	0.094
Bremen	0.031	0.069	0.039	0.235	0.353	0.319	0.007	0.007	0.008
North Rhine-Westphalia	0.014	0.042	0.025	0.217	0.275	0.285	0.208	0.208	0.207
Hesse	0.018	0.040	0.023	0.216	0.275	0.293	0.078	0.076	0.077
Rhineland-Palatinate	0.017	0.048	0.021	0.220	0.300	0.303	0.049	0.049	0.049
Baden-Württemberg	0.011	0.032	0.019	0.224	0.270	0.290	0.139	0.138	0.142
Bavaria	0.012	0.030	0.014	0.213	0.265	0.269	0.163	0.163	0.167
Saarland	0.019	0.029	0.019	0.224	0.282	0.293	0.012	0.012	0.011
East									
Berlin	0.049	0.073	0.051	0.205	0.263	0.273	0.041	0.041	0.044
Brandenburg	0.066	0.076	0.053	0.143	0.186	0.235	0.030	0.032	0.029
Mecklenburg West. Pomerania	0.074	0.095	0.055	0.146	0.215	0.299	0.019	0.019	0.018
Saxony	0.084	0.130	0.056	0.157	0.223	0.259	0.050	0.049	0.047
Saxony-Anhalt	0.060	0.124	0.069	0.122	0.205	0.225	0.028	0.028	0.024
Thuringia	0.054	0.085	0.054	0.125	0.207	0.247	0.029	0.028	0.025

Notes: This table shows the share of involuntary part-time workers, the share of part-time workers within, and the employment share of different labor market groups clustered by demographic characteristics, occupations, industries, or federal states. Shares are indicated for the years 2002, 2010, and 2017 respectively. Data source is the European Labour Force Survey. Own calculations using sampling weights of the Labour Force Survey.

2.4 Empirical Analysis

As a first step, we investigate whether the apparent positive relationship between IPT and unemployment on the regional level is upheld when we account for the influence of structural factors. To do this, we apply the state panel regression framework by Valletta et al. (2020), which has been proven useful in assessing the importance of both, market and cyclical factors for IPT (see for example MacDonald, 2019). Afterward, we disentangle the mechanisms underlying the association between IPT and unemployment. Among other things, we calculate transition probabilities between employment states at the individual level.

2.4.1 Aggregate Analysis

The state panel regression framework exploits variation in cyclical and structural factors within German federal states over time. This approach allows to jointly account for changes in demand and supply factors. As argued by Valletta et al. (2020), considering those factors together is crucial to properly evaluate their respective roles as different structural changes may be offsetting one another.

We apply state fixed effects to control for unobserved differences between states. We also include year fixed effects which capture unobserved common developments over time. These could be developments due to nationwide regulatory changes such as the Hartz reforms. It also makes sure that the regression results do not simply reflect an overall similarity in the trends of the time series of IPT and explanatory factors. As our dependent variable is a share, we also use the fractional regression method proposed by Papke and Wooldridge (1996) and Papke and Wooldridge (2008). Observations are weighted by employment of the respective state. Standard errors are clustered by state. All tables report marginal effects at the mean, that is, the impact of a one percentage point change in the respective independent variable on the dependent variable, with all other explanatory variables held at their mean values.
The regression model is specified as follows

$$IPT_{st} \quad = \quad \alpha + \beta u_{st} + \gamma u_{st}^2 + \zeta' X_{st} + \varphi_s + \varpi_t + \epsilon_{st} \tag{1}$$

with s indexing states and t indexing years and IPT_{st} being the fraction of the employed population that is involuntarily part-time employed. Variable u_{st} represents the unemployment rate and u_{st}^2 is the square of the unemployment rate. Including the quadratic term controls for a potential non-linearity of the relationship between IPT and unemployment and improves our model fit as shown in Appendix A.2. X_{st} represents a vector of structural variables that includes time and state-dependent industry and demographic group shares.[11] It furthermore includes GDP growth as an additional cyclical control variable. State fixed effects are φ_s and year fixed effects are ϖ_t. In Section 2.4.1, we present additional specifications to consider the

[11] Note that we use population shares of demographic groups as opposed to employment shares as they cover the exogenous differences in labor supply between federal states more accurately. We obtain, however, qualitatively similar results when including employment shares instead.

role of labor force participation and voluntary part-time employment for the assumed relationship between IPT and the explanatory variables.

Table 2 shows the results. In the baseline specification (column 1), we only include the cyclical indicators u_{st} and u_{st}^2 as well as state and time effects. The coefficient of the unemployment rate is positive and precisely estimated. It shows a significant correlation between unemployment and the share of IPT in a region. Interpreting the effect of unemployment requires jointly accounting for the effect of unemployment and the quadratic term, which is negative and significant. Calculated at the weighted sample mean of 7.9%, a one percentage point increase in the regional unemployment rate leads to a change of about 0.17 percentage points in the IPT share in this specification. The maximum difference between the lowest and highest regional unemployment rate in our sample period is 19 percentage points in Mecklenburg Western Pomerania. A change of this magnitude indicates a change in the share of IPT of approximately 3.3 percentage points, an effect which is of economic significance but is less than a third of the effect in the US. The mean of within-state differences is about 10 percentage points in our sample. An increase in the unemployment rate of this magnitude would translate into an increase in the number of involuntary part-time workers of about 600 thousand people. The negative effect of the quadratic term indicates that the marginal effect of unemployment becomes smaller as unemployment increases. Our data are almost entirely within the range where the marginal effect remains positive.

In column (2), we present a specification, which also includes the structural variables, only a few of which have a significant impact on the regional IPT rate.[12] Higher shares of employment in *Wholesale and Retail Trade* and in *Electricity, Gas and Water Supply* are associated with a higher share of IPT. Most of the structural factors are not individually significant, but the overall model fit does improve with their inclusion as indicated by a lower Akaike information criterion and the within R^2.[13] This is probably due to the fact that the demographic group and industry shares have been rather stable within states over the sample period compared to the cyclical indicators. Further, the respective group shares are correlated with each other and the sample size is rather small. However, a Wald test of joint significance indicates that the structural factors as a whole do affect the incidence of IPT, but the effect cannot be attributed to single regressors. More importantly, the marginal effect of unemployment is almost unaffected by the inclusion of structural variables and most importantly in terms of effect size.

[12]This finding is in line with the results of Dietz et al. (2013) who conducted shift-share analyses that show that changes in atypical employment, which includes part-time employment, can hardly be explained by structural change.

[13]The within R^2 is directly calculated from the sum of squares as demonstrated by Valletta et al. (2020).

Table 2: Cyclical and Structural Determinants of Involuntary Part-Time Work

Share IPT	(1)		(2)		(3)	
Unemployment Rate	0.273***	(0.0659)	0.251***	(0.0668)	0.253***	(0.0695)
Unemployment Rate Squared	-0.592***	(0.201)	-0.549***	(0.160)	-0.550***	(0.167)
GDP Growth					0.0432**	(0.0200)
Women 17-26			0.0267	(0.122)	0.0228	(0.120)
Women 27-36			0.00334	(0.134)	0.0337	(0.139)
Women 37-56			-0.0391	(0.131)	-0.0329	(0.130)
Women 57-66			0.0250	(0.148)	0.0365	(0.151)
Women 67+			0.0787	(0.514)	0.123	(0.524)
Men 27-36			-0.180	(0.118)	-0.202*	(0.114)
Men 37-56			0.00757	(0.122)	0.0173	(0.122)
Men 57-66			-0.0561	(0.149)	-0.0444	(0.151)
Men 67+			-0.283	(0.415)	-0.214	(0.436)
Manufacturing			-0.0136	(0.0547)	-0.0101	(0.0541)
Electricity, Gas and Water			0.152*	(0.0840)	0.156*	(0.0875)
Construction			0.0405	(0.0474)	0.0458	(0.0509)
Wholesale and Retail Trade			0.136**	(0.0562)	0.142**	(0.0567)
Hotels and Restaurants			-0.0132	(0.105)	-0.0187	(0.102)
Financial Intermediation			-0.0924	(0.0828)	-0.100	(0.0810)
Business Activities & Real Estate			0.0805	(0.0579)	0.0797	(0.0552)
Public Administration & Defence			-0.0188	(0.0511)	-0.0160	(0.0468)
Education			0.00455	(0.0787)	0.00397	(0.0759)
Health and Social Work			0.00573	(0.0602)	0.0174	(0.0581)
Other Services			0.0571	(0.0613)	0.0557	(0.0592)
AIC	0.2577781		0.2576841		0.2576776	
R^2 within	0.82		0.94		0.94	
N= 256						

Notes: This table shows fractional regressions of Equation 1 with the federal-state-level share of involuntary part-time as dependent variable. All specifications include year and state fixed effects. *Men 17-26* is the omitted demographic group and *Transportation, Storage and Communication* is the omitted industry category. Observations are weighted by the state's employment. Standard errors in parentheses clustered at federal states, $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data sources are the European Labour Force Survey and Eurostat. Own calculations.

In column (3), we further add regional GDP growth to account for the cyclical dynamics in terms of output. The coefficient of the unemployment rate is almost unaffected. The other effects also remain qualitatively unchanged, besides a higher population share of men aged between 27 and 36 now significantly corresponding to a lower share of IPT. The effect of GDP growth itself is positive. A one percentage point increase in output is associated with an increase of IPT of 0.043 percentage points. Bearing in mind that a change of that magnitude in GDP growth would be quite substantial, the effect it has on IPT seems rather negligible. Moreover, as we show in Appendix A.1, it is only prevalent in a few sectors. In most sectors, IPT is rather connected to unemployment. In Appendix A.2, we explore different specifications of the indicators presented here and of alternative indicators. Basically, it seems that the rather strict regulation of the German labor market does not prevent high unemployment reducing the chances of employees realizing their desired full-time positions.

To understand the connection between unemployment and IPT better, we also conduct heterogeneity analyses, which reveal that the connection between unemployment and IPT differs in important dimensions. In Appendix A.3, we focus on macro-level heterogeneity and show that the correlation is larger in Western Germany than in Eastern Germany and it has been larger after the Great Recession than before. This suggests that the relevant labor market mechanisms affect the Western labor market more strongly and have been amplified by the crisis. Meanwhile, our findings do not hint at any relevant changes regarding the connection between unemployment and IPT that could be attributed to the Hartz reforms. In Appendix A.4 we make use of the individual-level dimension of our data. First, we show that our main findings are reinforced when we use IPT status as the dependent variable and control for individual worker characteristics in logit regressions. Thus, this exercise shows that the connection between labor market conditions and IPT is prevalent across workers. Second, we consider the individual probability of being inactive in the labor market as dependent variable which is relevant for understanding the mechanisms underlying the relationship between unemployment and IPT as we explain in the next section. Moreover, we investigate micro-level heterogeneity in the correlation of unemployment and IPT. A notable finding here is that the probability of working in IPT when unemployment is high is much larger for women than for men.

2.4.2 Unemployment and Involuntary Part-Time Employment: Underlying Mechanisms

In this section, we explore the underlying mechanisms of the positive relationship between unemployment and involuntary part-time work. In the US, unilateral adjustments by employers of their workers' hours from full- to part-time play a major role for the countercyclicality of IPT (see for example Warren, 2016; Lariau, 2017; Borowczyk-Martins and Lalé, 2019). Downward adjustments of hours become attractive for firms when the demand for their products weakens in a downturn. At the same time, firms face little resistance from their employees as a slack labor market offers them fewer alternatives. By contrast, German regulation makes reductions in working hours difficult as they usually require employees' consent. Consequently, involuntary hours reductions at the same employer are a less relevant margin of labor adjustment in Germany than in the US. To illustrate this point, Table 3 shows the share of transitions from full-time to IPT in all IPT inflows and the probability of staying with the same employer when transitioning from full-time to IPT for these two countries (the latter is taken from Borowczyk-Martins and Lalé, 2016). The figures show that the share of transitions from full-time to IPT that take place at the same employer in all IPT inflows is about three times higher in the US than in Germany. While it accounts for about one-third of those transitions in the US, it is 11% in Germany. This raises the question of which alternative mechanisms explain the relationship between unemployment and IPT. To find out, we present different channels and investigate their relevance. We first conduct additional regression analyses for a broader set of dependent variables and then second, look at yearly transition rates between employment states.

Table 3: Hours Reductions at the Same Employer in Germany and in the US

	Share Transitions FT-IPT in all IPT Inflows	Probability of Staying with Same Employer at Transition	Share Transitions FT-IPT at Same Employer in all IPT Inflows
Germany	≈ 18 %	≈ 64 %	≈ 11 %
US (BML 2016)	≈ 31 %	≈ 95 %	≈ 29 %

Notes: This table shows the transition rates of workers between full-time and involuntary part-time employment for Germany and the US. Source for the German data: RDC of the Federal Statistical Office and Statistical Offices of the Länder, Mikrozensus 2001-2004 & 2012-2015, own calculations using the sampling weights of the Mikrozensus. Information on the US is taken from Borowczyk-Martins and Lalé (2016), Table 5, and applies to the years 2009-2015 based on monthly CPS data. The numbers are very similar for the longer period 1994-2019 (see Borowczyk-Martins and Lalé (2019), Tables 2 and 6).

Alternative Channels of Labor Adjustment in Regulated Labor Markets

The three candidate explanations we consider are composition effects between sectors that have different intensities in their use of full-time and part-time work, added labor supply effects that result from higher unemployment of a household member leading to higher hours supply by other members, and the effect that a weaker labor market has on jobseekers' and workers' opportunities to gain full-time employment.

Composition effect: A higher unemployment rate could be associated with a higher share of involuntary part-time work due to sectoral reallocation. The argument runs as follows. In Germany, the Great Recession primarily affected employment in manufacturing (see for example Burda and Hunt, 2011). As manufacturing firms use relatively little part-time labor (see Table 1), this could have been responsible for an increase in IPT's share in employment. Not only does a decrease in the employment share of full-time intensive industries lead to a decline in employment without a proportional decrease in IPT in all sectors, but it potentially also leads to additional employment in sectors that are comparatively part-time intensive. However, by controlling for the industry composition in our regression analysis, we rule out that the connection of unemployment and IPT is driven by this kind of interaction between cyclical and sectoral developments.

Added labor supply effect: Another potentially relevant mechanism is based on increased labor supply in times of high unemployment. It has primarily been discussed with regard to the labor supply of married women in the literature (see for example Mincer, 1962; Heckman and MaCurdy, 1980; Stephens, 2002; Bredtmann et al., 2018). In the respective literature, the labor supply of individuals is put in the context of family decision-making. If a household member becomes unemployed, this leads other, formerly inactive household members to enter the labor market in order to compensate for the transitory income loss. This *added worker effect* could explain the positive association of unemployment and IPT if the additional workers were particularly prone to becoming involuntary part-time employed. Given that they were only marginally attached to the labor force, this is not unlikely. By the same reasoning, there could be an *added hours effect* on the intensive margin of those household members who are already employed but have been working part-time and want to increase their hours when their spouse loses his or her job. We present suggestive evidence for this channel on the macro and micro levels.

Reservation hours effect: From the perspective of the search and matching theory of the labor market, it is plausible to expect workers' bargaining positions to positively depend on labor market tightness. That is, the higher the number of vacancies relative to the number of job seekers, the better the position of an employee vis à vis her employer. We therefore expect a negative correlation between unemployment and the probability of a worker realizing her desired hours. Our findings suggest that job seekers actually make concessions with regard to their desired hours when labor market conditions are not in their favor. Analogous to reservation wages, *reservation hours* then appear to be lower. Consequently, unemployed individuals who prefer a full-time position are more likely to accept a part-time position during economic downswings. Along the same lines, those who are already involuntarily part-time employed have fewer opportunities to transition to full-time positions.

Different Dependent Variables

Table 4 shows additional regression results at the same aggregation level as in Section 2.4.1, which help to evaluate whether the above mechanisms of employment adjustment play a role in the German labor market.

In the first column, we repeat the full specification from Table 2 (column 3) but add the labor force participation rate. If workers who are marginally attached to the labor market were especially prone to become involuntary part-time employed, we would expect a positive coefficient of the labor force participation rate. In addition, the inclusion of this variable would affect the coefficient of the unemployment rate if there was an *added worker effect* as described above. However, this is not the case as the variable itself has no explanatory power for the incidence of IPT and the marginal effect of the unemployment rate remains about the same.

Thus, there might be no significant *added worker effect*. Another explanation might be that it is just compensated by a *discouraged worker effect*, implying that groups that often work part-time involuntarily are discouraged in times of high unemployment and completely withdraw from the labor market. In Appendix A.4, we examine the relationship between unemployment and the individual probability of becoming inactive. There is no significant association between the two variables in our data, implying that there is indeed no (predominating) added worker effect.

Table 4: Different Dependent Variables, Regression Results

	(1) Share IPT	(2) Number IPT	(3) Share PT	(4) Share IPT/PT
Unemployment Rate	0.245**	5.066***	-0.181	1.674***
	(0.0960)	(1.699)	(0.170)	(0.239)
Unemployment Rate Squared	-0.550***	-12.56***	-0.218	-2.799***
	(0.167)	(3.088)	(0.520)	(0.504)
Labor Force Participation	0.0146			
	(0.115)			
N= 256				

Notes: This table shows linear or fractional regressions of Equation 1 with different dependent variables indicated in columns. All specifications include year and state fixed effects, controls for 10 gender x age demographic group shares, controls for 12 NACE Rev 1.1 industry shares as well as GDP growth. Observations are weighted by the state's employment. Standard errors in parentheses clustered at federal states, $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data sources are the European Labour Force Survey and Eurostat. Own calculations.

Columns (2) to (4) present the same specification as before, but with different dependent variables. First, we look at the effect of unemployment on the absolute number of IPT workers. If there is a *reservation hours effect*, the number of IPT workers will rise when unemployment increases. As expected, the marginal effect of unemployment on the absolute number of IPT workers in column (2) is positive and precisely estimated. We next look at the share of PT workers in all workers (column (3)) and the share of IPT workers in all part-timers (column (4)). This provides an indication of whether the positive association between unemployment and the share of IPT hinges on the overall relevance of part-time employment or on shifts within the group of part-time employees. The coefficient of the unemployment rate in column (3) is not significant, suggesting that movements in overall part-time are not correlated with unemployment. While this might be surprising, it is consistent with the finding by Carrillo-Tudela et al. (2021) that the role of part-time employment in directly reducing unemployment in Germany was negligible. The share of IPT workers in all part-time employed is, however, significantly positively associated with unemployment. Together, these results suggest that changes in unemployment come with a compositional shift within the group of part-time workers rather than with an overall rise in part-time employment. This speaks to the low relevance of transitions from full-time to part-time as argued at the beginning of this section. Instead, an increase in unemployment is not only associated with an increase in involuntary but also with a decrease in voluntary part-time work. This is in line with added labor supply at the intensive margin (*added hours effect*).

Transitions

On the aggregate level, the results are indicative of a *reservation hours effect* and an *added hours effect*. To inspect both effects in more detail, we look at transitions between different employment states (EMPST), specifically between the different employment states involuntary part-time (IPT), voluntary part-time (VPT), and full-time (FT) and the non-employment states unemployment (U) and non-participation (NE), and how these depend on labor market conditions. For this purpose, we use Mikrozensus data from survey years 2001 to 2004 and 2012 to 2015 which can be combined to panel data sets.[14]

We pool the observations from the two 4-year periods together. While the earlier time period lies only partly within our sample period, using it means that an economic downturn in terms of GDP growth is included in this part of the analysis. This also means that half of the observations are before the Hartz IV reform of 2005 and the other half after the reform. As mentioned before, the Hartz reforms are the most important set of reforms of the German labor market in the last decades. The Hartz IV reform of 2005 fundamentally changed the generosity of the unemployment insurance system, increasing the incentives for unemployed workers to accept jobs. This has caused the unemployment rate to decline substantially, leading to a structural break in the time series of the unemployment rate (see e.g. Krause and Uhlig, 2012). Therefore, the unemployment rates before and after the reform cannot be readily compared. To deal with this problem, we first harmonize the two samples by subtracting the mean unemployment rate of the respective 4-year period. In this way, we remove the level differences in unemployment that are due to the reforms.[15]

We calculate yearly transition probabilities between the five different states and relate them to regional unemployment in the initial year, formally speaking:

$$corr\,(U_{t-1}, P(EMPST_t|EMPST_{t-1})).$$

The *reservation hours effect* implies that workers are more likely to accept a part-time position despite preferring a full-time position when labor market conditions are not in their favor. Unemployed workers who start a job, i.e., transition from unemployment to employment, more often become IPT, indicating that

$$corr\,(U_{t-1}, P(EMPST_t = IPT|EMPST_{t-1} = U)) > 0,$$
$$U_{t-1} \uparrow \Longleftrightarrow P(EMPST_t = IPT|EMPST_{t-1} = U) \uparrow.$$

[14]The German EU-LFS is based on the Mikrozensus, such that this data actually stems from the same source as our main data. Unfortunately, the Mikrozensus allows the construction of a panel only over certain time periods. See Appendix A.6 for further information.

[15]The sample period for our main analysis also includes three pre-reform years. Here, we include year fixed effects to account for common unobserved shifts in the unemployment rate across federal states. In addition, we conduct a robustness check repeating our regression for a sample excluding those years in Appendix A.3. The results are qualitatively similar.

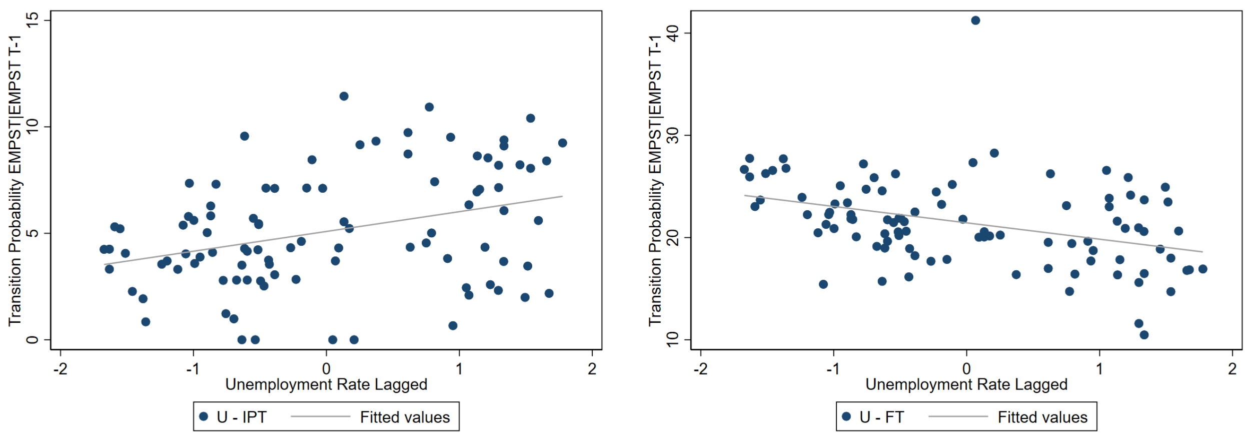
This is all the more remarkable given that outflows from unemployment to employment are lower when unemployment is high. Accordingly, fewer unemployed workers find full-time jobs, i.e., that is

$$corr\,(U_{t-1}, P(EMPST_t = FT|EMPST_{t-1} = U)) < 0.$$
$$U_{t-1} \uparrow \Longleftrightarrow P(EMPST_t = FT|EMPST_{t-1} = U) \downarrow.$$

Figure 3 shows these transition probabilities and corresponding initial unemployment rates. They support the assumed mechanisms for the German labor market.

Figure 3: Reservation Hours Effect

(a) Correlation Unemployment Rate and U-IPT Transition Probability **(b)** Correlation Unemployment Rate and U-FT Transition Probability



Notes: This figure shows correlations between the federal state-level unemployment rate in the previous year and the probability of transitioning from unemployment to involuntary part-time (left) or the probability of transitioning from unemployment to full-time (right). Sources are RDC of the Federal Statistical Office and Statistical Offices of the Länder, Mikrozensus 2001-2004 and 2012-2015. Own calculations using the weighting factor of the Mikrozensus.

In this context, it is also noteworthy that our data confirms that the probability of transitioning between IPT and a full time position is lower when economic conditions are unfavorable. However, the link is rather weak. This again suggests that transitions at the same employer are less crucial for the cyclicality of IPT than they are in less regulated labor markets.

The *added hours effect* implies that part-time workers extend their labor supply in times of high unemployment. If they succeed, this leads to higher transition probabilities from voluntary part-time to full-time, that is

$$corr\,(U_{t-1}, P(EMPST_t = FT|EMPST_{t-1} = VPT)) > 0.$$
$$U_{t-1} \uparrow \Longleftrightarrow P(EMPST_t = FT|EMPST_{t-1} = VPT) \uparrow.$$

If they do not succeed, they will become involuntary part-timers, such that

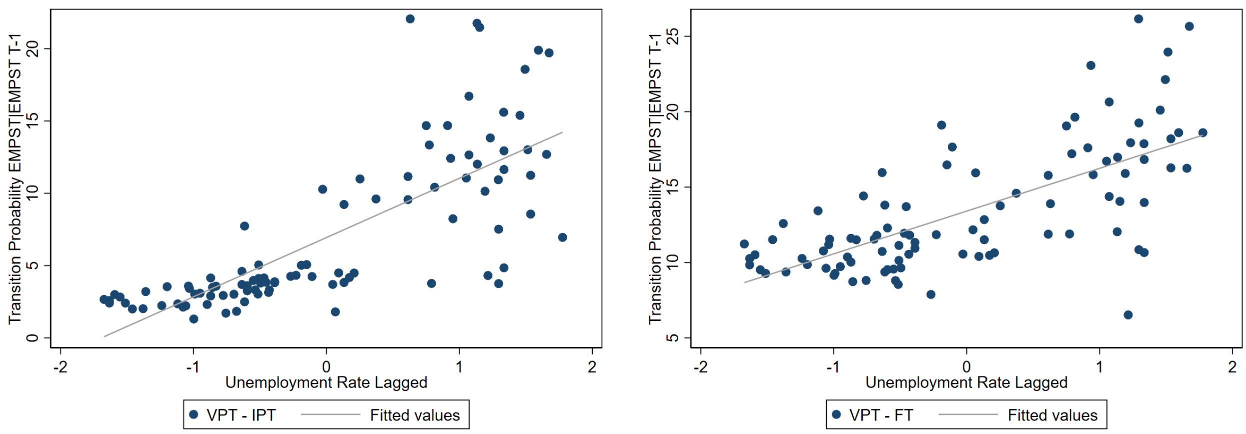
$$corr\,(U_{t-1}, P(EMSPT_t = IPT|EMPST_{t-1} = VPT)) > 0.$$
$$U_{t-1} \uparrow \Longleftrightarrow P(EMPST_t = IPT|EMPST_{t-1} = VPT) \uparrow.$$

Again, the respective scatter plots, which are shown in Figure 4, suggest that both are the case. In fact, 85% of transitions from voluntary to involuntary part-time happen at the same employer, thereby reflecting changes in desired hours under presumably unchanged working circumstances. We furthermore find that there are stronger connections for women between unemployment and the probabilities of transitions from voluntary to involuntary part-time as well as to full-time (figures available from the authors upon request). This is in line with the interpretation that it is often women who try to compensate for a transitory household income loss during economic downswings (see Section 2.4.1).

Figure 4: Added Hours Effect

(a) Correlation Unemployment Rate and VPT-IPT Transition Probability

(b) Correlation Unemployment Rate and VPT-FT Transition Probability



Notes: This figure shows correlations between the federal state-level unemployment rate in the previous year and the probability of transitioning from voluntary part-time to involuntary part-time (left) or the probability of transitioning from voluntary part-time to full-time (right). Sources are RDC of the Federal Statistical Office and Statistical Offices of the Länder, Mikrozensus 2001-2004 and 2012-2015. Own calculations using the weighting factor of the Mikrozensus.

In summary, the transition probabilities between the different relevant employment states are convincing indications of procyclical dynamics in the reservation level of hours and anticyclical patterns in labor supply on the intensive margin.

2.5 The Influence of Institutions on the Cyclicality of Involuntary Part-Time Work

Our analysis so far stresses the importance of institutions for the incidence of involuntary part-time work in Germany. As mentioned in Section 2.3.2, there are further institutional particularities that might be worth controlling for as the association between IPT and unemployment could in fact (also) be driven by changes in these particular forms of employment. Since labor market regulation is mandated at the national level, there are no relevant differences in regulation at the federal state level. However, the incidence of relevant forms of employment differs between federal states and over time. We again exploit within-state variation to evaluate the relevance for the share of marginally employed for IPT, the share of employees using working time accounts, and the share of short-time workers. Adding the additional variables does not qualitatively change our findings from Section 2.4.1.

In 2003, the Hartz reforms expanded the possibilities to hire marginal employees, which means lower non-wage labor costs for the employer than for other employees (see Section 2.3.2). Some suspect that marginal employment has been used as a substitute for non-marginal employment. However, there has not been a clear trend in the use of marginal employment since the early 2000s and its role remains controversial (see for example Burda and Hunt, 2011). A priori, the effect of the share of marginal employment on IPT is unclear. A positive effect would be expected if a relatively large share of minijobbers was seeking full-time employment. However, it is also conceivable that minijobbers are satisfied with a small number of working hours or that they use an additional minijob to achieve the desired number of hours. We therefore differentiate between those who have a minijob in addition to a regular job and those who are exclusively marginally employed. The LFS does not include information on marginal employment as this is a form of employment specific to Germany. Therefore, we use administrative data from the Federal Employment Agency on the year and state-specific shares of marginal employment.
Moreover, we control for the incidence of working time accounts. If a firm uses working time accounts, the distribution of employees' working hours over the business cycle becomes more flexible. On the one hand, an increase in the spread of this instrument could lead to a heavier use of (involuntary) part-time as employers can ask part-time employees with working time accounts to work full-time hours when needed without paying overtime premia as long as the accounts are balanced over time. On the other hand, employers might be more willing to employ full-time labor, when working time can be saved that is not needed at the moment. Again, the LFS does not provide information on working time accounts. We use data from the Socio-Economic Panel, a representative survey with about 30,000 respondents, to calculate the year- and state-specific shares of employees who use those accounts.

Lastly, we control for the incidence of short-time work using respective data from the Federal Employment Agency. As mentioned in Section 2.3.2, it cannot be predicted easily whether short-time work results in IPT because this depends on employees' preferences regarding hours/wage combinations. As the incidence of short-time work is a rather countercyclical phenomenon overall (Balleer et al., 2016), it appears worth controlling for. It is important to note that our IPT measure most probably does not capture those employees who are (involuntarily) in a short-time work scheme. Short time workers who usually work full-time hours will not report being part-time employed because contractual working hours do not change due to short-time work.

Table 5 shows the regression results using the full specification from before (structural variables and GDP growth are not shown), additionally including (1) the share of exclusively marginally employed, (2) the share of all marginally employed, (3) the share of employees using working time accounts, (4) the share of short-time workers and (5) variables (1), (3) and (4). Some of the variation in the incidence of IPT can be attributed to the use of working time accounts. The positive marginal effect suggests that employers hire more part-time instead of full-time employees when the firm uses working time accounts and that this comes with a higher incidence of IPT. The effect of unemployment remains comparable in magnitude and significance to our findings from Section 2.4.1.

Table 5: Involuntary Part-Time and Particular Employment, Regression Results

Share IPT	(1) Mini (Excl.)	(2) Mini (All)	(3) WTA	(4) STW	(5) All
Unemployment Rate	0.264***	0.225***	0.236***	0.256***	0.249***
	(0.0718)	(0.0813)	(0.0711)	(0.0711)	(0.0728)
Unemployment Rate Squared	-0.582***	-0.463**	-0.486***	-0.555***	-0.523***
	(0.189)	(0.202)	(0.176)	(0.168)	(0.192)
Share Minijobbers (Excl.)	-0.0549				-0.0569
	(0.158)				(0.160)
Share Minijobbers (All)		-0.0548			
		(0.131)			
Share Working Time Accounts			0.0197***		0.0197**
			(0.00764)		(0.00780)
Share Short-Time Workers				-0.0476	-0.0397
				(0.120)	(0.113)
AIC	0.258	0.262	0.258	0.258	0.258
N= 256					

Notes: This table shows linear or fractional regressions of Equation 1 with different dependent variables indicated in columns. All specifications include year and state fixed effects, controls for 10 gender x age demographic group shares, controls for 12 NACE Rev 1.1 industry shares as well as GDP growth. Observations are weighted by the state's employment. Standard errors in parentheses clustered at federal states, $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data sources are the European Labour Force Survey, Eurostat, the Federal Employment Agency, and the Socio-Economic Panel. Own calculations.

2.6 Conclusion

In Germany, labor market regulation interferes with the adjustment of labor at the intensive and extensive margins. Workers are protected both from dismissals and from reductions in paid working hours. In contrast to less regulated labor markets, employers cannot unilaterally reduce working hours to adjust to business cycle fluctuations. We evaluate the effectiveness of these regulations in protecting the workforce from involuntary part-time employment during economic downswings.

We first assess the relevance of cyclical and structural factors for the incidence of IPT by applying a state panel regression approach to data on the German labor market. The incidence of IPT is associated with the unemployment rate, i.e., it behaves anticyclically in Germany as well. However, the effect is less than one-third of the US effect at respective sample means. The connection is not driven by specific industries but is prevalent across the economy. Given the institutional constraints firms and workers face we investigate the underlying mechanisms which lead to the positive connection between unemployment and IPT, as we suspect them to be very different from those in less regulated labor markets. In a first step, we show that transitions from full-time to IPT at the same employer indeed only play a minor role. In the second step, we analyze transition probabilities between relevant employment states to provide an alternative explanation. We find convincing indications of procyclical dynamics in the reservation level of hours ("reservation hours effect") and anticyclical patterns in labor supply on the intensive margin ("added hours effect"). The reservation hours effect refers to the observation that job seekers make concessions with regard to their desired hours when labor market conditions are not in their favor. Unemployed individuals are hence more likely to accept a part-time position even though they prefer a full-time position. The added hours effect refers to the phenomenon that some individuals would like to work more in economic downturns. Apparently, recessions increase the probability of former voluntary part-timers preferring full-time positions. We are the first to document these margins of cyclical hours adjustments. Lastly, we incorporate the incidence of particular forms of employment into the analysis and find that our main results remain unaffected. Of the employment forms considered, only working time accounts are relevant for the incidence of IPT.

It appears that the rather strict regulation of the German labor market does not prevent unemployment from reducing the chances of employees realizing their desired working hours. From a welfare perspective, it is not clear that preventing IPT is an appropriate policy goal. On the one hand, IPT can be assumed to come with substantial disadvantages. Involuntary part-time jobs are associated not only with overall lower income but also lower hourly wages compared to workers in similar full-time jobs (see for example Golden, 2016; Glauber, 2017). On the other hand, the welfare effects of regulation that aims to prevent IPT cannot be assessed without knowing the resulting outcomes for all labor market participants. In particular, without the option of (involuntary) part-time, the alternative might be unemployment for some workers.

There are a number of interesting open questions to address in future research. One is whether there is a relationship between IPT and other macroeconomic variables in Germany, such as wage growth. Hong et al. (2018) find that IPT has recently weakened wage growth across countries, even in economies where unemployment rates are now at or below their averages before the Great Recession, like Germany. Nevertheless, special analysis for Germany seems worthwhile because of its institutional peculiarities. Another is the assessment of the impact of recent reforms that directly target the incidence of IPT. Since 2019, employees can opt for a temporary reduction of hours under certain circumstances ("Brückenteilzeit"). The right to return to full-time work could prevent involuntary part-time work in some cases.

Not only in rather liberal labor markets but also in a regulated labor market like Germany, market mechanisms lead to a countercyclical occurrence of IPT. Apparently, working time regulation is not entirely effective. While reductions in hours at the same employer play a much smaller role, other mechanisms lead to an increase of IPT in downswings, as we have explored in this chapter.

3 The Career Costs of Part-Time: Human Capital, Signaling, and Dynamic Self-Selection

Authors: Christian Bredemeier and Theresa Markefke

3.1 Introduction

The transition into working part-time is an important career crossroad and is usually perceived as costly in terms of reduced current and future wages. To begin with, part-time workers earn less per hour than full-time workers (B. T. Hirsch, 2005; Manning and Petrongolo, 2008; Goldin, 2014). In addition to this immediate earnings loss, they face lower subsequent wage growth over the life cycle (Blundell et al., 2016; Adda et al., 2017). Relatedly, it has been shown that earnings increase in full-time – but not part-time – experience (Francesconi, 2002; Bertrand et al., 2010; Blundell et al., 2016). Consequently, the concentration of women in part-time contributes to the persistent gender gap in wages (Blau and Kahn, 2017).

In this chapter, Christian Bredemeier and I use the German household survey data to shed light on the underlying causes of the documented correlation between part-time experience and subsequent career stagnation. In general, there are three possible mechanisms that can give rise to this correlation. First, it is often argued that on-the-job accumulation of human capital is weakened when one works part time, which reduces future earnings. Second, employers may perceive the choice to work part time as a signal about characteristics they dislike in a worker, which is why they pay less to workers for whom they observed such a signal, i.e., ex-part time workers. Third, there might be dynamic self-selection into part-time. That is, workers may decide to go into part-time work in response to advance information about future career stagnation observable to them but not the econometrician, which would also lead to a correlation between part-time work and subsequent earnings growth through reverse causality.[16]

We distinguish between voluntary and involuntary transitions into part-time to isolate the human capital channel from the signaling channel and dynamic self-selection. Voluntary part-time work (VPT) is labor-supply driven, i.e., it results from constraints in the workers' private life, such as family obligations like having to take care of children. Involuntary part-time work (IPT), on the other hand, is caused on the demand side of the labor market. It arises when a worker can find no full-time position or when it is no longer possible for them to work full-time for firm-specific reasons, such as a demand shortage. The distinction between these two types of part-time work is useful to separate the different channels: While a loss of human capital takes place in both involuntary and voluntary part-time, a signaling effect only emanates from voluntary part-time employment, but not from involuntary part-time.[17] Furthermore, if a worker

[16]While it has long been accepted that there is selection into part-time along time-invariant unobservable characteristics, dynamic self-selection concerns the timing of transitions into part time and the response to changes in relevant determinants. Put more technically, the (self-) selection into part-time extends beyond time-invariant characteristics and is hence dynamic.

[17]The vast majority of transitions to involuntary part-time take place within the same firm and the management thus knows who it has sent to part-time against her or his will.

is in part-time involuntary, it can by definition be ruled out that they chose part-time themself, and hence the part-time spell cannot be a reaction of the worker to developments associated with lower future earnings. Focusing on involuntary part-time work as a refined measure of part-time freed from the endogeneity problems arising from self-selection is similar in spirit to the identification of the consequences of unemployment through considering displacements and plant closures which are arguably more involuntary than transitions into unemployment in general which include, e.g., quits.

We consider first transitions in an event-study approach where we normalize outcomes to the year prior to the transition. This approach compares a worker who transitions into part-time to themself prior to the transition and hence takes into account that, already before the transition, these workers are on different trajectories than workers who will remain in full-time. We find that a transition into involuntary part-time work compresses a worker's earnings for a total of three years. The earnings drop amounts to about €3,100 (roughly $4,300) in the first year and a total of about €5,500 (roughly $7,700) over the three. As a comparison, if we considered a transition into part-time independent of its voluntariness, it would appear that the transition reduced earnings for over five years with a cumulated earnings loss of over €9,400 (roughly $13,100), of which €7,400 (roughly $10,300) occur in the first three years. While the immediate earnings drop is similar, considering involuntary part-time reduces the longevity and strength of subsequent earnings losses.

The working-time margin seems to be the dominant driver of our results. Most involuntary part-time workers return to full-time work relatively quickly, but they continue to work shorter hours than their peers without part-time experience. These reduced hours are important for the observed drops in earnings with earnings per hour not substantially affected by the past part-time spell. Our approach allows us to compare the consequences of part-time experience across population groups. Interestingly, we find substantial effects for men while the literature has so far focused on women. We do not find sizable effect differences between age groups or between Eastern and Western Germany. We also analyze in how far the type of the transition affects the consequences of part-time. We find no discernible impact of whether the transition coincided with the birth of a child. Yet, the adverse consequences of part-time experience are stronger when only a few hours are worked during part-time and when the worker changes firm or job.

An aspect worth discussing when relying on involuntary transitions into part-time as arguably more exogenous events compared to voluntary transitions is the potential selection through the employer. Concentrating on involuntary transition eliminates biases through dynamic *self*-selection of workers into part-time, but a potential challenge remains the possibility of selection by employers whom to send into involuntary part-time. Employers may use advance information about workers' productivity to select workers for IPT who are expected to perform worse in the future which would induce a spurious negative correlation between IPT experience and earnings. This thought implies that an estimate based on involuntary transition remains an upper bound for the costs of foregone human capital during part-time with the true costs being (even) smaller. We can use our results to gauge the importance of this issue. If selection by employers were important, we should find a negative connection between hourly wages and IPT experience but we do not. This indicates that dynamic selection through the employer, though theoretically a challenge to our identification, is not overly important.

Thus, our results on involuntary part-time work are a fairly clean measure of the importance of the human capital mismatch channel. They suggest that this channel plays a role but should not be overestimated. That foregone human capital accumulation matters is indicated by the fact that we also observe earnings losses after involuntary part-time work. In line with this, we also see in the data that the larger the reduction in hours, i.e., the smaller the amount of work in which skills and knowledge can be accumulated, the stronger the negative effects. However, even when involuntary part-time work is 15 hours or less, the earnings differences to the control group are small and blur within the statistical error three years down the road. Our results thus put the importance of the human capital channel for the consequences of part-time into perspective. In this sense, our analysis confirms Manning's (2011) conjecture that earnings differences between people with and without interruptions of their full-time careers are disproportionately large to be explained by human capital differences alone. Our study shows that when the effects of signaling and dynamic self-selection are accounted for, the remaining effects of part-time to be attributed to the human capital effect fall substantially.

The remainder of this chapter is organized as follows. Section 3.2 discusses the related literature, 3.3 describes our data, and presents descriptive statistics. Section 3.4 outlines our econometric strategy and presents its results. Section 3.5 concludes.

3.2 Related Literature

This chapter is mainly related to two strands of the literature. The first is the literature on the effects of part-time. In his seminal paper, B. T. Hirsch (2005) finds large wage differentials between part-time and comparable full-time workers in the US which can be fully explained by lower worker-specific skills among part-time workers. In their study of women in the UK, Connolly and Gregory (2008) find that a part-time pay gap remains even when accounting for unobserved differences in characteristics between both types of workers. Using administrative data from Italy, Devicienti et al. (2020) document that workers transitioning within the same firm see their earnings drop by less than their hours, conditional on worker, firm, and match fixed effects. Overall, the estimated effect of part-time work on wages depends on the country, the sample, and the definition of part-time used.[18]

[18]For Germany, E. Wolf (2002) finds a penalty for women in West Germany who work less than 20 hours per week but no negative effect on current wages for those with 20 or more hours a week. This finding is corroborated in a more recent analysis for both men and women in all of Germany, where the author finds that men incur a penalty while women do not (E. Wolf, 2014).

Going beyond the immediate effects of part-time on wages, the literature has documented extensively the employment and earnings trajectories of workers after a transition into part-time. Fernández-Kranz and Rodríguez-Planas (2011) and Fernández-Kranz et al. (2015) document that female ex part-time workers in Spain have persistently lower earnings growth than workers without part-time experience. Connolly and Gregory (2008) and Manning and Petrongolo (2008) document similar results for British women and show that women switching to lower-pay occupations after entering part-time plays a large role for their deteriorating earnings growth.

While these results are of first-order importance to understanding the nexus between part-time and subsequent career trajectories, the documented career slowdowns cannot be interpreted directly as the causal consequences of the transitions into part-time. Most closely related to this chapter are Aaronson and French (2004) and Paul (2016) who apply explicit identification strategies to isolate exogenous variation in the propensity to work part-time. Both papers use instruments derived from the institutional background. Aaronson and French (2004) exploit that social security rules in the US reduce the costs of going into part-time in terms of foregone pension claims. Exploiting this instrument for workers' willingness to work part-time, they find that halving one's working time reduces the hourly wage by 25% for men. Paul (2016) exploits regional variation in German parental leave regulations and related family policies that facilitate working part time. Her results suggest that moving into part-time has persistent and sizeable negative consequences for women's subsequent careers.

Both papers have similarities as well as differences to our approach. The major similarity is that they too focus on the voluntariness of the full-time/part-time status of the worker. Aaronson and French (2004) and Paul (2016) compare part-time workers with similar workers who presumably would also choose part-time work if they faced similar institutional costs of part-time. Through the lens of our approach, these papers compare part-time workers with involuntary full-time workers. By contrast, we compare involuntary part-time workers with full-time workers. A main difference is how these papers determine voluntariness in the data. While they apply instruments, we use respondents' direct expressions of desired working time. This has implications for external validity and puts limits on the scope of the analysis. While the instrument of Aaronson and French (2004) can only be applied to the elderly and Paul (2016) only considers women, our approach can be applied to the population at large as we observe involuntary part-time spells for, e.g., both genders and all age groups. Further, our approach needs to assume less structure than imposed by, e.g., Paul (2016).

Second, this chapter is related to the literature on involuntary part-time work. Interest in this form of employment has been sparked by the substantial increase in involuntary part-time work during the Great Recession (a phenomenon which has repeated itself during the Covid-19 recession) and the slow decline afterward. There are three papers that assess the aggregate determinants of involuntary part-time work. Valletta et al. (2020) emphasize shifts in the industry composition of employment as the most important factor with employment shifting into the more part-time intensive service sector. Even and Macpherson (2019) and Dillender et al. (2020) provide evidence that healthcare mandates for employers with sufficiently many full-time employees under the Affordable Care Act ("Obamacare") contributed to the rise in involuntary part-time employment in the US. While these papers focus on the causes of involuntary part-time, we are interested in its consequences. The fact that the literature has identified important aggregate, rather than individual-specific, causes of involuntary part-time strengthens our identification approach, again similar to the costs-of-unemployment literature which also understands layoffs as mostly driven by non-worker-specific factors.[19]

3.3 Data and Descriptive Facts

We use data from the German Socio-Economic Panel (SOEP), a large-scale household survey with rich information on socio-demographic and household characteristics as well as the employment situation of the respondent and their employment history. In addition, the data set has two properties that are critical for our analysis. First, it is a panel survey, which allows us to track individuals over time and, thus, to account for unobserved constant differences between workers. Second, it is possible to distinguish between voluntary, and involuntary part-time. We construct indicators for full-time, voluntary and involuntary part-time using the following questions on actual and desired working hours. Respondents are asked to state how many hours they would like to work at the given wage rate: *"If you could choose your own working hours, taking into account that your income would change according to the number of hours: How many hours would you want to work?"*. In a subsequent question, they are asked to state how many hours they work on average per week, including any overtime: *"And how many hours do you work per week on average, including any overtime?"*[20]

We use a sample of non-self-employed workers aged 20 to 60 from the sample period 1985-2017. We exclude observations below the 1st and above the 99th percentile of the hourly earnings distribution.[21] We further do not use the migration and refugee samples of the SOEP. In total, our sample consists of 150,491 person-years, for which we observe labor market status as well as a wide range of person and job characteristics. Following standard convention, we define full-time work as working at least 35 hours per week and part-time work as working less than 35 hours. Those respondents who state that they work less than full-time hours but wish to work 35 hours or more are considered involuntary part-time workers. To make sure that our results are not driven by small differences around the full-time threshold of 35 hours, we also use alternative definitions of IPT which impose a higher gap in actual and desired hours.[22]

[19] From a more macroeconomic perspective, Borowczyk-Martins and Lalé (2019) show that the volatility of part-time employment is an important driver of the cyclicality in hours worked. Consistently, Bredemeier and Winkler (2017b) show that the full-time/part-time ratio is strongly pro-cyclical.

[20] This is a more literal translation of the German question than the official translation provided by the SOEP.

[21] Hourly earnings are calculated using actual weekly hours worked and monthly labor income.

[22] In contrast to our indicator, the IPT indicator used by national statistics offices such as the U.S. Bureau of Labor Statistics or the German Federal Statistical Office is based on a question on the *reason* for part-time work. If respondents state to work part-time either because of "slack work" or because they "could not find a full-time job", they are considered involuntary part-timers. The surveys using this more direct measure of IPT, however, do not have a sufficient longitudinal dimension to analyze the effect of working in IPT on workers'

In our sample, the average part-time rate is about 23% and the average rate of involuntary part-time is 4%, so about one in six observations (person-years) of part-time work is involuntary.

In a first exercise, we verify that our involuntary part-time indicator indeed captures demand-driven discrepancies between actual and desired hours which cannot be explained by the factors which are typically associated with part-time in general. This exercise is motivated by the possibility that survey respondents understand the desired working hours question differently than it is intended and refer to a counterfactual situation in their private life where they, for example, have fewer family obligations which would allow them to supply more labor. To corroborate that this is not the case, we compute linear regressions with part-time status and involuntary part-time status as dependent variables, conditional on being employed. As explanatory variables, we include socio-demographic characteristics such as gender, age, income, and household composition as well as an indicator for individuals' health status. We start with a rather sparse specification and subsequently add more variables. For a detailed description of variable definitions see Appendix B.1.

Already in the sparse specification in column (1) of Table 6, there is a large difference in the predictive power of the variables: about 30% of the variation in part-time status can be explained by this simple model, but only about 3% of the variation in the involuntary part-time status. When we add lagged income in column (2), the R^2 increases to 46% for part-time, but for involuntary part-time, it increases only by 1 percentage point. Thus, past labor market success is highly predictive of the probability of working reduced hours, but only if this is voluntary. When we add part-time status last year as a regressor, two facts stand out. First, past part-time status is highly predictive of part-time status this year, and thus adds sizably to the predictive power of the model. Again, this is not the case for involuntary part-time status this year. Adding part-time status last year increases the R^2 only by a few percentage points. Second, the fact that we find significant effects of past income even when we control for part-time last year points to a relevant dynamic component of selection into part-time. The coefficient for income last year is of the opposite sign and smaller by factor 10. Adding part-time status last year increases the R^2 only by a few percentage points. In the last two columns, we control for whether a respondent cares for a relative during a weekday. This information is only available as of 2001, which is why we have fewer observations in these specifications. It is significant with the expected sign for part-time but is not significantly related to involuntary part-time. In sum, while part-time is in general quite predictable, it is difficult to predict involuntary part-time with standard variables, which makes us confident that our measure of involuntary part-time is indeed driven by demand shortages of the employer, difficulties in finding a full-time job, or other determinants outside the employee's private sphere.

subsequent careers. This is a clear advantage of the SOEP. We validate our definition of IPT by comparing our indicator to that of the European Labor Force Survey, in which IPT is measured using the above definition. We find that both the level and the development of IPT are quite comparable, as can be seen in Figure B.5 in the Appendix.

Table 6: Linear Prediction of Part-Time and Involuntary Part-Time Status

	(1) PT Status	(2) IPT Status	(3) PT Status	(4) IPT Status	(5) PT Status	(6) IPT Status	(7) PT Status	(8) IPT Status
Age	0.00379	0.000220	0.00613	0.000450	0.00171	-0.000131	0.00176	-0.000214
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.012)	(0.000)	(0.001)
Female	0.286	0.0322	0.144	0.0183	0.0462	0.00537	0.0478	0.00554
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Children in HH Age 2-7	-0.00825	-0.000772	0.0331	0.00331	0.00763	-0.0000445	0.00690	0.00000633
	(0.000)	(0.403)	(0.000)	(0.000)	(0.000)	(0.961)	(0.000)	(0.995)
Children in HH Age 8-15	-0.0196	-0.000853	0.0117	0.00224	0.00158	0.000904	0.00145	0.00103
	(0.000)	(0.277)	(0.000)	(0.004)	(0.120)	(0.243)	(0.213)	(0.254)
Female with Children Age 0-7	0.176	0.00390	0.101	-0.00349	0.0338	-0.0123	0.0304	-0.0126
	(0.000)	(0.035)	(0.000)	(0.059)	(0.000)	(0.000)	(0.000)	(0.000)
Female with Children Age 8-15	0.189	0.00572	0.101	-0.00288	0.0264	-0.0127	0.0278	-0.0138
	(0.000)	(0.000)	(0.000)	(0.020)	(0.000)	(0.000)	(0.000)	(0.000)
Health Status	0.00139	-0.00144	0.00531	-0.00105	-0.000748	-0.00185	-0.00131	-0.00191
	(0.207)	(0.006)	(0.000)	(0.045)	(0.272)	(0.000)	(0.095)	(0.002)
Partner in Household	0.0586	-0.000281	0.00729	-0.00534	0.00650	-0.00544	0.00770	-0.00532
	(0.000)	(0.644)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Ln Income Partner	-0.0737	-0.0323	0.0853	-0.0166	0.0166	-0.0256	0.0148	-0.0293
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Ln Labor Income Last Year			-0.221	-0.0217	-0.0315	0.00319	-0.0311	0.00332
			(0.000)	(0.000)	(0.000)	(0.048)	(0.000)	(0.071)
Ln Labor Income Two Years Ago			-0.108	-0.0107	-0.0432	-0.00222	-0.0452	-0.000582
			(0.000)	(0.000)	(0.000)	(0.146)	(0.000)	(0.737)
PT Status Last Year					0.723	0.0949	0.718	0.0991
					(0.000)	(0.000)	(0.000)	(0.000)
Care for Relative Week Day							0.00368	0.000546
							(0.000)	(0.472)
Constant	0.0123	0.134	2.011	0.331	0.451	0.126	0.468	0.131
	(0.129)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	143529	143529	143529	143529	143529	143529	114192	114192
R^2	0.293	0.027	0.458	0.037	0.729	0.065	0.730	0.069

Notes: This table shows linear regressions with part-time or involuntary part-time status as dependent variables, conditional on being employed. Time period is 1985-201X The variable *Care for Relative Week Day* is only available as of 2001. p-values in parentheses. Data source is the SOEP v34.1, 1985-2017. Own calculations.

Our empirical analysis focuses on first transitions from full-time into part-time work, distinguishing between voluntary and involuntary transitions. As it is not possible to predict involuntary part-time status well with worker and household characteristics, a *transition* into this employment state can be plausibly viewed as a rather exogenous event from the worker's point of view. To understand better the group of workers who undergo this transition and how they might differ from workers who work in full-time or voluntary part-time respectively, we compare descriptively three groups of workers.

In Table 7, we present summary statistics for full-time workers who in the subsequent year (1) remain in full-time (FT-FT), (2) transition into part-time (FT-PT), and (3) transition into involuntary part-time (FT-IPT). Note that the third group is a subset of the second. In our sample, we observe about 3,100 transitions into part-time, about 1,050 of which (or about one in three) are involuntary. For the group of workers who experience at least one transition into involuntary (voluntary) part-time in our sample, we have a total of 6,546 (18,582) person-year observations.

Our three groups are similar in terms of household characteristics, although workers subsequently transitioning into IPT live alone somewhat more often. Workers moving into part-time (both voluntarily and involuntarily) are more often female and somewhat younger than workers who remain in full-time. Further, the share of highly educated individuals is lower among workers moving into part-time and these workers are relatively more concentrated in industries such as *Trade and Business* and *Services*. Also, part-time is more common in smaller firms and in East Germany. Regarding earnings, the main outcome variable of our analysis, subsequent part-time workers earn substantially less (both in total and per hour) than workers who remain in full-time, and work sometimes shorter hours, but earnings differences between the two groups of subsequent part-time workers are small. Hence, there are differences between the groups that have to be taken into account in an estimation of the causal consequences of (involuntary) part-time. Yet, importantly, subsequent involuntary part-time workers are not so different from subsequent voluntary part-time workers descriptively. In terms of external validity, this implies that conclusions drawn for involuntary part-time workers are informative also for part-time workers.

Table 7: Characteristics of Full-Time Workers With and Without Transitions to Part-Time the Following Year.

	(1) FT-FT	(2) FT-PT	(3) FT-IPT
Household characteristics			
No Partner	0.23	0.27	0.34
Cohabiting	0.12	0.13	0.15
Married, spouse present	0.64	0.60	0.51
Number of Persons in HH	3.00	2.92	2.89
Number Children U14 in HH	0.57	0.57	0.50
Demographic characteristics			
Female	0.33	0.75	0.64
Age 20-29	0.13	0.15	0.20
Age 30-54	0.74	0.72	0.69
Age >= 55	0.10	0.10	0.08
Education			
Low	0.11	0.11	0.13
Medium	0.57	0.57	0.64
High	0.32	0.32	0.23
Industry			
Manufacturing	0.34	0.16	0.19
Utilities, Transport, Storage and Communication	0.08	0.05	0.06
Construction	0.08	0.03	0.05
Wholesale and Retail	0.09	0.16	0.18
Hotels and Restaurants	0.01	0.04	0.05
Financial Intermediation	0.04	0.03	0.03
Business Activities	0.06	0.07	0.07
Public Administration	0.10	0.06	0.06
Education	0.06	0.14	0.09
Health and Social Work	0.09	0.20	0.17
Other Services	0.03	0.05	0.04
Occupation			
Managers	0.06	0.03	0.03
Professionals	0.18	0.20	0.12
Technicians	0.22	0.26	0.24
Clerks	0.11	0.12	0.10
Services and Sales	0.07	0.19	0.20
Skilled Agriculture	0.01	0.01	0.01
Craft	0.19	0.07	0.11
Plant and Machine Operators	0.11	0.06	0.10
Elementary Occupations	0.05	0.07	0.10
Occupational position			
Manual Laborer	0.36	0.23	0.34
Employee	0.55	0.68	0.61
Civil Service	0.09	0.09	0.05
Tenure at Firm	11.62	8.58	7.89
Firm size			
5-20	0.17	0.28	0.30
20-200	0.29	0.31	0.29
200-2000	0.26	0.18	0.17
> 2000	0.28	0.23	0.24
Labor market experience			
Experience Full-Time Employment	17.70	12.02	12.73
Experience Part-Time Employment	1.08	4.07	3.36
Experience Unemployment	0.40	0.68	0.97
Region			
West	0.79	0.77	0.68
East	0.21	0.23	0.32
Regional Unemployment Rate	9.70	10.05	10.71
Labor market outcomes			
FT Status	1	1	1
Gross Labor Income Last Month (annualized 2011 €)	35,970	25,569	22,954
Gross Hourly Wage (annualized 2011 €)	16.02	12.31	11.00
Actual Weekly Hours	43.18	40.19	40.52
Observations	137,933	3,108	1,059

Notes: This table shows summary statistics for three different groups of full-time workers: those who work in full-time in the subsequent year (1), those who transition into part-time (2), and those who transition into involuntary part-time (3) (i.e., a subgroup of the second group). Data source is the SOEP v34.1, 1985-2017. Own calculations.

3.4 Econometric Strategy and Results

3.4.1 Methodology

We use the three groups of workers described in the section above as our sample for an event study. Thus, we compare labor market outcomes of workers who switch from full-time to (involuntary) part-time relative to those who stay in full-time and to themselves. This design allows us to analyze both the direct and longer-term effects of transitioning into IPT. Equation (2) specifies our model.

$$y_{i,t} = \alpha_i^y + \lambda_t^y + \delta^y X_{i,t} + \sum_{r \neq -1} \mu_r^y + \varepsilon_{i,t}^y, \tag{2}$$

where $y_{i,t}$ is an outcome variable, α_i^y are individual fixed effects, λ_t^y represents calendar time dummies, and $X_{i,t}$ are time-varying individual, household, and job characteristics (see Table 7) which could influence the propensity to work reduced hours and outcome variables. μ_r^y are coefficients that indicate time relative to treatment for those who transition into IPT and are always zero for the continuously full-time employed. Relative treatment time is denoted by r and the treatment defining transition takes place between treatment times -1 and 0. Hence, μ_1 refers to relative treatment time one, i.e., the first year after a transition. As a baseline specification, we include two pre-treatment periods ($r = -2$ and $r = -3$) and four post-treatment ($r = 1$ through $r = 4$). We chose this time frame because it is the longest possible period in which the number of observations per period in the unbalanced panel is relatively stable, as the average time a household remains in our panel is 6.5 years. In robustness checks, we vary the time frame to analyze how sensitive our results are to this specification choice. We exclude the period period before treatment, as is common in event studies (Kleven et al., 2019; Sun and Abraham, 2020).

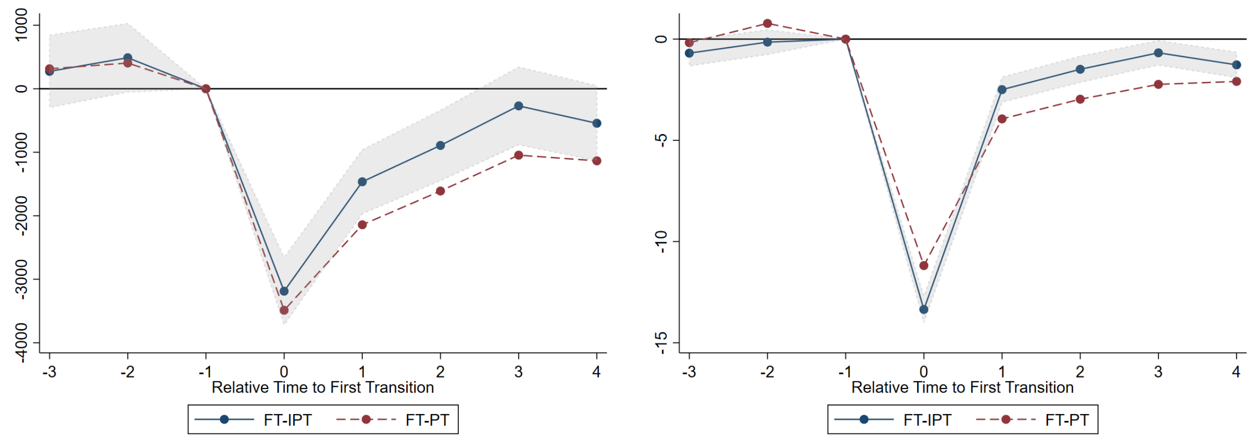
The validity of the causal interpretation of the coefficients in an event study with two-way fixed effects is subject of an ongoing discussion. Especially the presence of heterogeneous treatment effects between cohorts which are treated at different times can lead to a contamination of coefficients with effects from other periods. To address this concern, we use the estimator proposed by Sun and Abraham (2020). We implement it by using the Stata program *eventstudyinteract* provided by the authors. However, it turns out that in our case, treatment effect heterogeneity is rather limited as the estimated coefficients are very similar to that of a standard two-way fixed effects regression.

3.4.2 Results

Figure 5 shows the results of a set of two event studies where the considered events are transitions into involuntary part-time and into part-time in general. The dependent variables $y_{i,t}$ are annualized labor income last month (panel a) and actual working hours per week (panel b). The graphs show the coefficients μ_r resulting from estimating (2). The gray areas show 95% confidence intervals based on robust standard errors.

Figure 5: Labor Income (in €) and Actual Working Hours Before and After a Transition Into (Involuntary) Part-Time Relative to Year Before Transition.

(a) Annualized Labor Income **(b)** Actual Working Hours



Notes: This figure shows estimated coefficients μ_r in regression (2) with annualized real labor income last month (2011 €) as dependent variable. Vertical intervals are 95% intervals based on robust standard errors. Solid blue: transition into involuntary part-time. Dashed red: transition into part-time. Data source is the SOEP v34.1, 1985-2017. Own calculations.

The thick solid blue lines show the results for an involuntary transition. For both outcomes, we see no significant pre-event coefficients, indicating that our approach satisfies the common trend requirement. With regard to earnings, one can see that they are significantly affected by the event. On impact, they drop by an annualized €3,000 ($ 4,200). In the aftermath of the transition, however, earnings recover relatively quickly. Already at relative treatment time $r = 3$, there is no significant difference in earnings relative to the counterfactual. Put differently, three years after the transition, the affected workers' earnings are indistinguishable from what they would have made had they not transitioned into IPT in the first place. The earnings losses in the four years after the transition into part-time amount to about €3,300.

The thin dashed red lines show earnings developments around a transition into part-time without a distinction between voluntary and involuntary events. This line is provided for comparison and should not be interpreted as causal effects. The underlying transitions include voluntary, and hence endogenous, transitions into part-time. Comparing the two lines in panel (a) of Figure 5 shows that a look at earnings differences between workers with and without part-time experience (i.e., the red dashed line) would lead to a substantial overestimation of the negative effects of part-time on future earnings. This is particularly so during the recovery which is substantially quicker than a broad view on part-time in general would suggest. After the average transition into part-time, earnings remain significantly below the counterfactual for over four years while they do so after an involuntary transition only for two years.[23] In terms of the underlying channels, this finding suggests that signaling and dynamic self-selection are

[23] A formal test of differences between the lines shows that voluntariness impacts significantly the earnings losses of transitions into part-time, as can be seen in Figure 8 in Section 3.4.3.

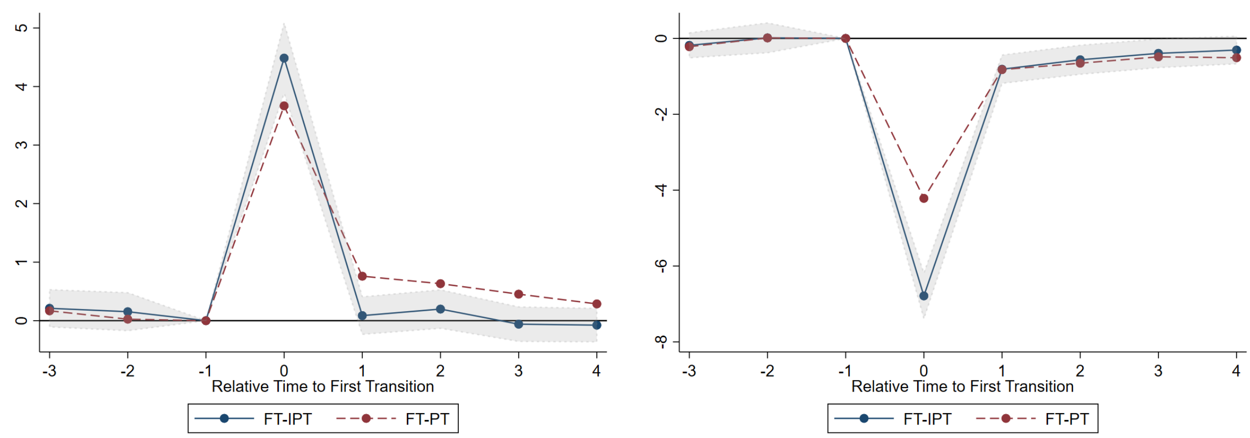
important mechanisms for the negative correlation between part-time and subsequent career developments. The part of the correlation that is due to lower human capital accumulation is lower than previous findings suggest.

Panel (b) shows that workers with involuntary part-time experience also work comparable amounts of hours as workers without such experience relatively early after their transitions into IPT. Already in the first year after the transition, the hours difference is only 2.5 hours per week. However, the difference persists until the fourth year after the part-time experience.

Figure 6 decomposes the documented dynamics further as it shows the development of hourly earnings and overhours. The key insights from the figure are drawn from the blue solid lines, that again summarize dynamics around involuntary transitions. Panel (a) shows that hourly earnings are not impacted in the first year after the transition into involuntary part-time and beyond. Hence, the documented earnings drop comes from a reduced likelihood to work somewhat shorter hours, and not from earnings conditional on working time. In the transition year, we see the well-known spikes in hourly earnings (Paul, 2016; Devicienti et al., 2020). Put differently, earnings do not fall one-to-one with hours. A possible part of the mechanism is shown in panel (b): workers considerably reduce their numbers of overhours (presumably a good share of which are unpaid) upon a transition into part-time.

Figure 6: Hourly Earnings (in €) and Overhours Before and After a Transition Into (Involuntary) Part-Time Relative to Year Before Transition.

(a) Hourly Earnings **(b)** Overhours per Week



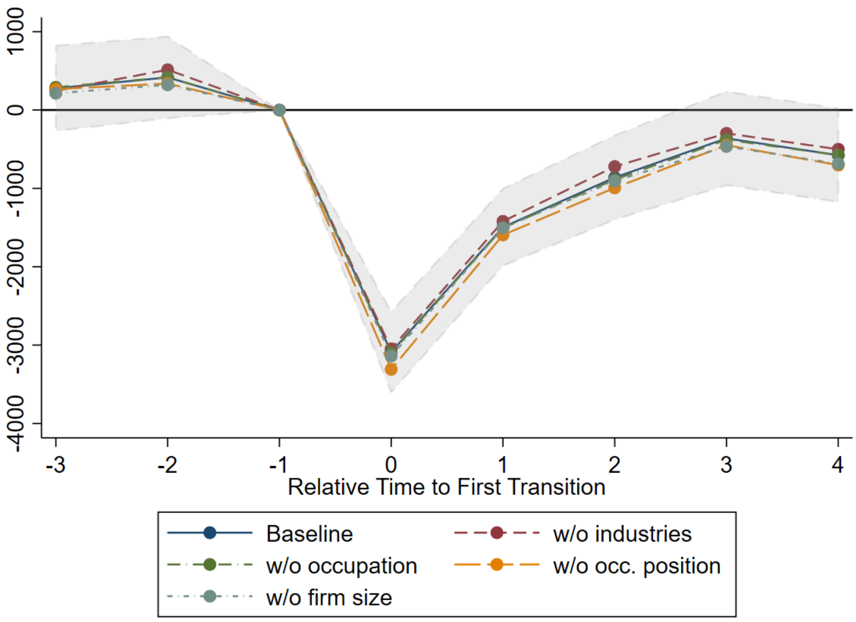
Notes: This figure shows estimated coefficients μ_r in regression (2) with full-time dummy, usual weekly working hours, real hourly earnings (2011 €), and overtime (actual minus contractual hours) as dependent variables. Vertical intervals are 95% intervals based on robust standard errors. Solid blue: transition into involuntary part-time. Dashed red: transition into part-time. Data source is the SOEP v34.1, 1985-2017. Own calculations.

After the transition, the hourly earnings of workers with IPT experience are not discernably different from the earnings of workers without PT experience. This has two important implications. First, as discussed above, the medium-run income consequences of IPT are due to working in full time less often and shorter hours on average, not from a reduction in pay per hour. Second, to the extent that we can use hourly pay as a proxy of productivity, firms do not seem to select those workers into IPT from whom they expect declining productivity.

Potentially Endogenous Control Variables

The results shown in Figure 7 refer to regressions where we leave out sets of control variables from our baseline specification one by one. These exercises serve two purposes. First, they reveal whether our results rely on specific control variables. Second, they are informative about potentially endogenous responses of control variables and their effects on our variables of interest. Thus, we can gain some insights about the mechanisms behind the effects of transitions into part-time. Specifically, we address potentially endogenous reactions in industry, occupation, and firm characteristics because transitions into part-time may cause workers to switch industry, occupation, or firm. The figure shows that our results are not overly sensitive to changes in the set of control variables. Endogenous reactions of the respective variables seem to be rare or to have no substantial impact on earnings as the estimated coefficients differ only slightly and lie well in the range of the confidence bands.[24]

Figure 7: Robustness Checks and Inspection of Mechanisms.



Notes: This table shows estimated coefficients μ_r in regression (2) with annualized real labor income last month (2011 €) as dependent variable. Gray area indicates 95% confidence bands based on robust standard errors for the baseline specification including a full set of control variables. Solid blue: full set of controls. Dashed dark red: excluding industry dummies. Dashed-dotted green: excluding occupation dummies. Dashed orange: excluding dummies for occupational position. Shortdashed-dotted: excluding dummies for firm size. Data source is the SOEP v34.1, 1985-2017. Own calculations.

In summary, our results show that the average transition into involuntary part-time has only moderate and not overly long-lasting consequences for earnings. This suggests that the causal career costs of part-time are limited.

[24]This might be different for voluntary transitions where workers may deliberately change occupation or industry in order to be able to work part-time.

3.4.3 Types of Transitions

In this subsection, we analyze in how far different types of transitions into part-time vary in their consequences. We do so by interacting relative event time with an indicator variable I_i for whether the transition fulfills a certain condition,

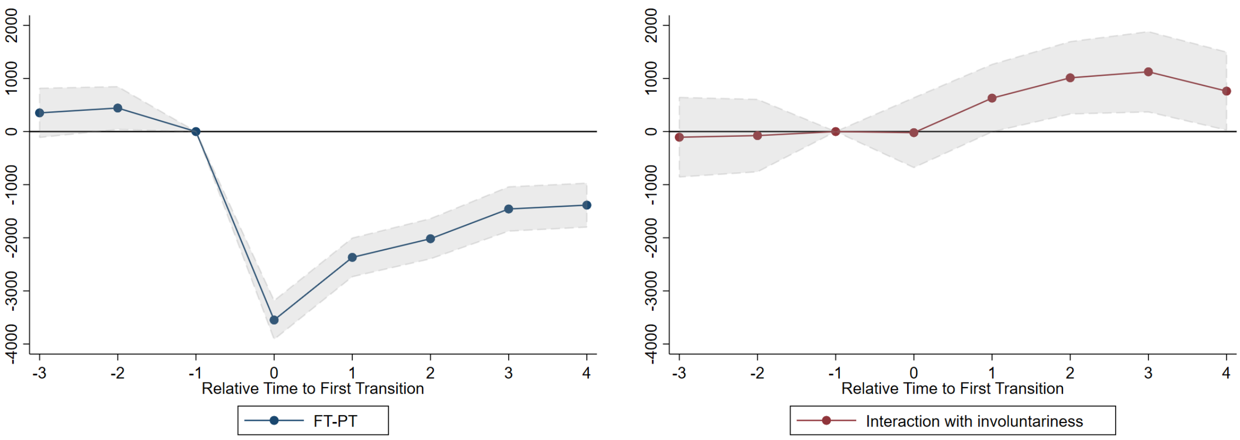
$$y_{i,t} = \alpha_i^y + \lambda_t^y + \delta^y X_{i,t} + \sum_{r \neq -1} \mu_r^y + \sum_{r \neq -1} \nu_r^y I_i + \beta I_i + \varepsilon_{i,t}^y. \tag{3}$$

In such regressions, the coefficients μ^r give the average change in the outcome variable upon a transition r years ago that does not fulfill the condition measured by I_t whereas the marginal effect of a transition that fulfills the condition is given by $\mu_r + \nu_r$ with ν_r measuring the difference in the consequences of the two types of transitions.

First, we consider transitions into part-time as the underlying event and involuntariness as defined before as the characteristics measured by I. This exercise serves the purpose of testing whether the differences between the consequences of voluntary and involuntary transitions documented in Figure 5 are statistically significant. Panel (a) of Figure 8 shows the coefficients μ^r associated with the transition into part-time. Panel (b) plots the coefficients ν_r which measure the impact on earnings if the transition occurs involuntarily. While the immediate earnings changes upon voluntary and involuntary transitions are statistically indistinguishable (i.e., the coefficients on the interaction are insignificant), the difference becomes highly significant starting from year two after the transition. P-values for the interaction coefficients ν_2 through ν_4 are below 0.04.

Figure 8: Voluntary and Involuntary Transitions Into Part-Time.

(a) Voluntary Transition **(b)** Difference Involuntary-Voluntary



Notes: This table shows estimated coefficients μ_r (blue solid) and ν_r (red solid) in regression (3) with annualized real labor income (in 2011 €) as dependent variable. The event is a transition into part-time, interaction with involuntariness. Gray areas are 95% intervals based on robust standard errors. Data source is the SOEP v34.1, 1985-2017. Own calculations.

In the following four evaluations, we consider only involuntary transitions into part-time and distinguish between further characteristics of such transitions. The results are presented in Table 8. First, we analyze whether the consequences of an involuntary transition are different when the transition coincides with the birth of a child. With this, we want to make sure that our results are not confounded by the effects of a concurrent birth event, which has been shown to have substantial effects on earnings (Adda et al., 2017; Lundborg et al., 2017; Kleven et al., 2019). Specifically, we use as the indicator variable I_t whether a dependent child was born in the transition year or the year after. Column (1) shows that the birth of a child does not impact the consequences of an involuntary transition into part-time in a discernible way, the coefficients ν_1 through ν_4 are all insignificant. The immediate drop in earnings is somewhat more pronounced when the transition is accompanied by childbirth, likely because the drop in hours is sharper. For completeness, we also interacted with voluntary transitions into part-time with child birth and found that, intuitively, these transitions are indeed associated with more pronounced and longer-lasting drops in earnings when they are accompanied by childbirths. See Figure B.8 in Appendix B.3.3.

Next, we distinguish between differently strong drops in working hours during transitions into involuntary part-time. This follows Paul (2016) who has shown that what she calls "long part-time", i.e., working less than 35 but 16 or more hours has only moderate consequences on future wages while "short part-time" (5-15 hours) has more drastic career effects. We follow her cut-off for defining short part-time and use a thus defined indicator I in interaction with the involuntary transition event indicator. Column (2) shows the results. Transitions into short part-time induce sharper drops in earnings. This is a rather mechanical result as hours (not shown) drop by about 9 hours in transitions into long part-time and by an additional 22 in transitions into short part-time, such that the additional earnings drop when the transition is into short part-time is roughly proportional. Hourly earnings (also not shown) are slightly reduced following transitions into short part-time (in line with Paul, 2016) but not following transitions into long part-time. The proportionality of the earnings losses is in line with our interpretation of involuntary part-time as a clean measure for the human capital channel as more capital accumulation is foregone if the decrease in hours worked is larger.

Finally, we consider changes between jobs or firms upon involuntary transitions into part-time. Such changes are usually understood to be costly in terms of lost job-specific or firm-specific human capital and we can thus expect part-time to be more costly in terms of current and future earnings if the transition into it is associated with job or firm changes. Columns (3) and (4) reveal that this is indeed the case. Changes of firm and/or job boost the extent and longevity of the earnings consequence of part-time experience. Put differently, earnings drops are even less severe than on average when the transition into part-time takes place within the same firm. Regarding earnings per hour, we do see some negative consequences of transitions that entail job or firm changes but not for those where the worker remains on the same job and/or in the same firm.

To summarize, our results indicate that, while part-time work does not seem overly costly once self-selection is accounted for, the career costs of part-time are even lower when the part-time worker still works relatively long hours and when they remain in the same firm or job than before the transition into part-time.

Table 8: Interaction of Transitions Into Involuntary Part-Time With Type of Transition.

t	(1) Childbirth	(2) Short PT	(3) Job change	(4) Firm change
-3	23.48	16.77	27.44	22.63
	(22.95)	(23.72)	(25.43)	(24.36)
-2	38.91*	28.20	46.54**	35.30
	(22.08)	(22.29)	(23.58)	(23.07)
0	-248.6***	-180.9***	-178.2***	-205.9***
	(21.86)	(20.58)	(22.60)	(21.99)
1	-118.1***	-90.96***	-89.26***	-97.24***
	(20.93)	(20.87)	(22.61)	(21.54)
2	-71.58***	-50.04**	-37.43	-40.11*
	(22.86)	(23.12)	(23.19)	(22.19)
3	-28.65	-16.96	-0.892	-11.52
	(26.06)	(28.25)	(27.89)	(26.30)
4	-46.14*	-52.16**	-21.89	-24.09
	(25.11)	(26.21)	(26.32)	(24.54)
-3× interaction	-24.57	58.23	-29.29	-13.36
	(158.66)	(78.52)	(58.22)	(70.89)
-2× interaction	-27.01	78.86	-44.41	2.725
	(145.26)	(78.26)	(60.34)	(70.79)
0× interaction	-275.3**	-475.6***	-400.9***	-406.6***
	(109.19)	(75.12)	(58.20)	(71.62)
1× interaction	-116.0	-207.7***	-180.7***	-224.5***
	(112.30)	(71.65)	(54.36)	(69.93)
2× interaction	-15.20	-134.8*	-183.6***	-278.3***
	(126.18)	(74.00)	(68.56)	(94.01)
3× interaction	50.39	-49.34	-129.5**	-124.0
	(88.32)	(59.10)	(63.16)	(85.47)
4× interaction	91.09	63.80	-96.20	-145.0
	(116.50)	(70.88)	(68.00)	(103.61)
N	138721	138721	138721	138721

Notes: This table shows estimated coefficients μ_r (1-7) and ν_r (8-14) in regression (3) with annualized real labor income (in 2011 €) as dependent variable. The event is a transition into involuntary part-time, interactions differ by type indicated in column headers. Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data source is the SOEP v34.1, 1985-2017. Own calculations.

3.4.4 Heterogeneity Across Demographics

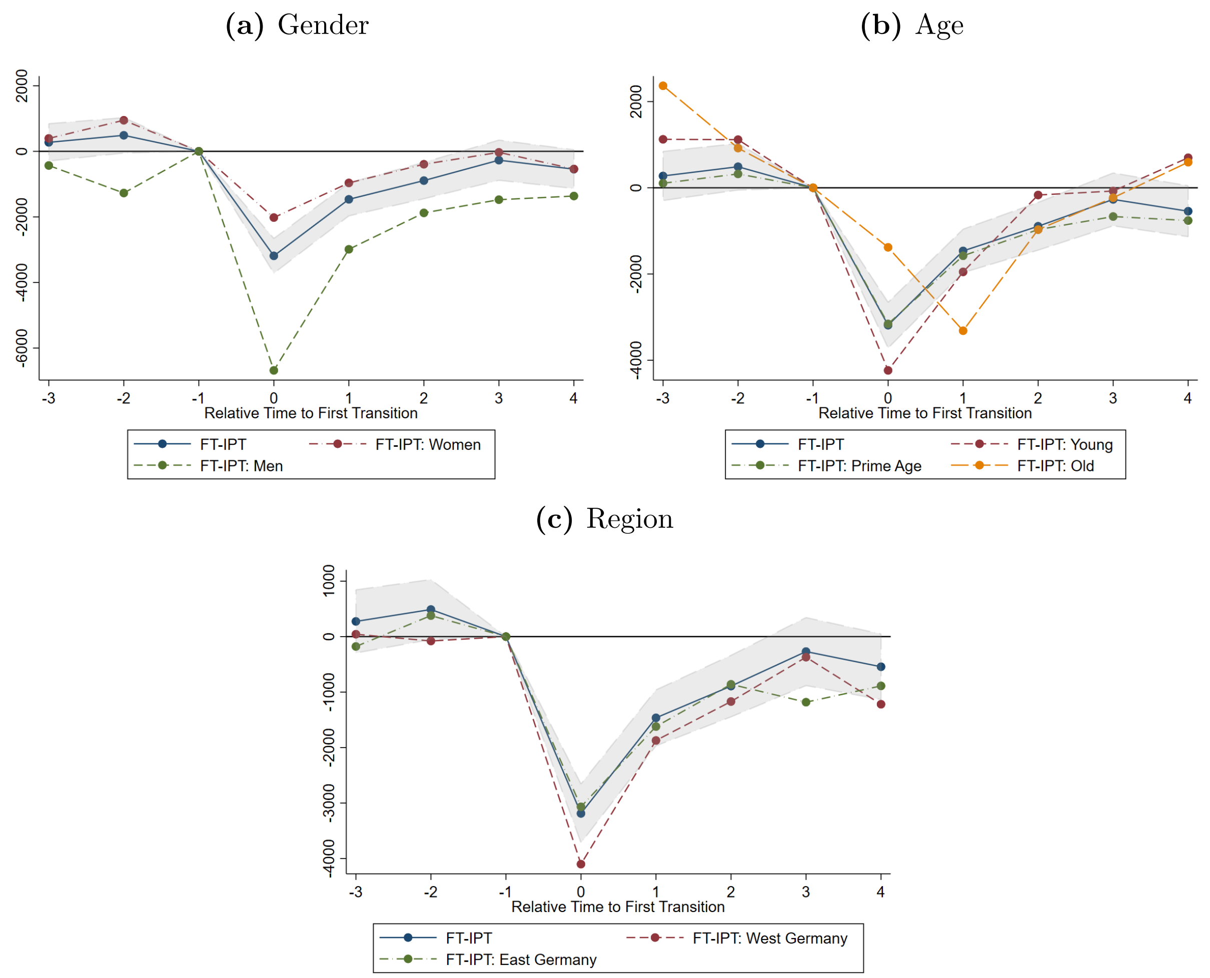
In this Section, we analyze potential heterogeneity in the effects of involuntary transitions into part-time. We do so by splitting our sample into different groups. Specifically, we distinguish between men and women, between workers of different ages, and between workers living in different regions of Germany. The first two sample splits are motivated by the approaches chosen by Paul (2016) and Aaronson and French (2004). Due to their specific instruments, these papers focus on women (Paul, 2016) and older workers close to retirement (Aaronson and French, 2004). We want to assess in how far their results may be driven by the effects of part-time being different in these groups than in the population at large. The third sample split is motivated by German economic history. It is well documented that the formerly communist Eastern Germany differs from Western Germany in terms of norms, e.g., regarding female labor force participation and the availability of child care and the resulting possibilities for mothers to work full-time (Alesina and Fuchs-Schündeln, 2007). It therefore appears plausible that working part-time has different implications in the East than it has in the West.

In Figure 9, we show the event study coefficients for the groups of interest together with the baseline specification for the sake of comparison. Panel (a) reveals that involuntary part-time has negative earnings consequences for workers of both genders. Interestingly, the drop in earnings is stronger and lasts somewhat longer for men. This is striking given the literature's focus on women in the context of part-time work. For neither men nor women, we find significant drops in earnings per hour (not shown). For the results of Paul (2016), the stronger earnings drops we find for men suggest that, if anything, her focus on women leads to estimating smaller effects of part-time than they accrue to the part-time average worker. Yet, of course, the majority of part-time workers are women which reduces the importance of this bias.

Panel (b) draws a similar picture for workers of different age groups. Young (age below 30), old (age above 55), and prime age (between 30 and 55) workers all experience a moderate and relatively short-lived drop in earnings upon a transition into involuntary part-time. The similarity across the age groups suggests that the results of Aaronson and French (2004) are not strongly reflective of their sample of rather old workers.

Lastly, panel (c) shows that earnings drops upon involuntary part-time are remarkably similar in East and West Germany. This indicates that the impact of norms and child-care availability on the consequences of working part-time might be limited.

Figure 9: Heterogeneity in the Consequences of Involuntary Transitions Into Part-Time



Notes: This figure shows estimated coefficients μ_r in regression (2) with annualized real labor income last month and real hourly earnings (both in 2011 €) as dependent variables. Vertical intervals are 95% intervals based on robust standard errors. Solid blue: baseline specification. Other: specifications for subgroups. Data source is the SOEP v34.1, 1985-2017. Own calculations.

3.5 Conclusion

We have used German panel data to shed light on the underlying causes of the documented correlation between part-time experience and subsequent career stagnation. We have argued that the distinction between involuntary and voluntary part-time is helpful in separating the channels of foregone human capital accumulation, signaling, and dynamic self-selection. Using an event study with first transitions from full-time into involuntary part-time as a treatment indicator, we find that this approach reduces both the estimated magnitude and the longevity of the penalty for part-time experience as compared to an approach where part-time in general is considered. Earnings losses upon involuntary transitions into part-time are moderate and relatively short-lived. This is especially true for part-time workers who work more than 15 hours, remain in their jobs and firms, and are female. As such, the results suggest that a significant part of the negative correlation between part-time and negative career developments is driven by signaling effects and dynamic self-selection. Once these are accounted for, the part of the negative relation ascribable to lost human capital accumulation is relatively small.

4 On-the-Job Training Under Imperfect Competition: The Additional Burden of Working for a Low-Pay Firm

4.1 Introduction

The question of how to foster life-long learning of workers as a response to a permanently changing economic environment is an ongoing concern for policymakers and a recurrent topic of labor market research as a skilled labor force is an important contributor to innovation and growth (Acemoglu and Pischke, 1999a). Particularly, on-the-job training by firms plays a crucial role for maintaining and improving the human capital of their workers.

In this chapter, I study the role of firm heterogeneity for the provision of on-the-job training. To this end, I derive empirical hypotheses based on the models by Manning (2003), Fu (2011), and Quercioli (2005), who extend the strategic wage posting model of Burdett and Mortensen (1998) to include employer-provided training. The models predict that firms higher up the wage distribution will invest more in the training of their workers as they have less reason to fear that workers will be poached by a rivaling firm. However, empirical evidence on this relationship is scarce. Combining survey data, which includes information on firm-level on-the-job training with estimates of firm wage fixed effects from matched employer-employee data for the German labor market, I analyze first whether and to which extent higher-paying firms invest more in the human capital of their workers and then, second, how firm wage fixed effects interact with individual workers' training participation and wages.

I find first, consistent with the theoretical predictions of the models, that higher-paying firms provide more on-the-job training. This applies both for the incidence of training and for the extent of training as measured by the share of trained workers relative to all employees. This result is confirmed in a second analysis conducted at the employee level, where the probability of receiving employer-financed training is significantly higher when an employee is employed in a higher-paying firm. The amount of training received by individuals, as measured by the length of training courses, is also higher in high-wage firms. With regard to wages, the results confirm the hypothesis insofar as training that occurred in preceding periods is positively associated with wages. However, depending on the specification, the coefficient is not statistically significant. This finding is in line with the literature on the returns to on-the-job training in Germany, in which the returns are often found to be positive but only small and weakly significant.

With my analysis, I provide new insights into the *relative* incentives of an individual firm operating in an imperfectly competitive labor market to engage in training. This dimension of employer-provided training has not been studied before in the empirical labor market literature. Economists have long been interested in the implications of the market structure for the provision of on-the-job training. While the standard competitive theory approach formulated by Becker (1964) predicts that firms only provide firm-specific training, Stevens (1994) and Acemoglu and Pischke (1997, 1998, 1999a), showed that firms in frictional labor markets have incentives to also provide general training. This is noteworthy because it is assumed that general training improves the skills of workers in both the current and an alternative firm, thus possibly benefiting a competing firm. Because the interests of other employers are not featured in the training decision of the current firm, the level of investments in general training will be sub-optimally low, potentially giving cause for government intervention. This type of inefficiency does not arise in the case of specific training, as an investment in specific skills has no externalities (see e.g. Hashimoto, 1981). Therefore, I focus on general skills training in my analysis, which is also the most common form of training in Europe and the United States (see e.g. Barron et al., 1997 for the US, Booth and Bryan, 2005 for the UK and Bilger et al., 2017 for Germany). I discuss the implications of imperfect competition for the provision of specific training as well.

Many studies have tried to discriminate between the alternative theories of training on an empirical basis by looking at specific aspects for which the models predict opposing outcomes (Booth and Bryan, 2007; Bassanini and Brunello, 2008; Konings and Vanormelingen, 2015). The bulk of findings is consistent with theories of training in an imperfectly competitive market. A number of researchers have used measures of frictions in the labor market, providing a more direct test of such theories (Picchio and Van Ours, 2011; Muehlemann and Wolter, 2011, e.g.). However, none of the studies consider the role of firm heterogeneity in shaping the incentives to train workers on the job. This study presents an alternative approach that directly tests the theories while taking into account firm heterogeneity.

As has been shown by Card et al. (2013), firm heterogeneity has been a major driver of recent wage inequality. Fu (2011) has explicitly modeled the correlation between pay rate and human capital accumulation of workers. In his model, the correlation is positive, implying that wage dispersion of ex-ante identical workers is magnified by the endogenous training choices of firms, and workers matched with a lower-paying firm will face worse career and income opportunities due to lower human capital accumulation. The findings I present suggest that there is indeed an additional burden for workers working at lower-paying firms: they are less likely to receive on-the-job training. Thus, the careers of workers employed at firms at different ends of the wage distribution can be expected to diverge in other dimensions than just pay, namely, their skill development. While I find no robust evidence that wages increase after training, others have estimated statistically significant returns for firm-provided training in Germany (e.g. Ehlert, 2017; Ruhose et al., 2019). Even if there was no direct monetary return, skills acquired through on-the-job training matter for workers' careers in the long run as they significantly increase employment prospects (Picchio and Van Ours, 2013; Ebner and Ehlert, 2018).

To encourage training, many governments have established subsidy programs targeted directly at firms. Therefore, the question of which firm is more likely to invest in training is of immediate relevance to policymakers. My results imply that policy instruments should target those firms that are further down the wage distribution. The reason is that a worker with a given ability who happens to work at a lower-paying firm will be trained less than a worker with the same ability who works for a higher-paying firm, resulting in a divergence of their careers and an increase in inequality due to the different skill developments of workers. While there are a range of possible instruments that could alleviate the market efficiency, none of them stands out because all have advantages as well as disadvantages. Policymakers designing instruments should pay attention to the needs of small firms, as these are, according to my results, lower-paying and therefore less likely to invest in training.

To derive empirical results, I use high-quality data provided by the Institute for Employment Research (IAB) and the Leibniz Institute for Educational Trajectories (LIfBi). To study firms' decision to engage in on-the-job training, I use the LIAB cross-sectional model. The LIAB links survey information from the IAB Establishment Panel with the social security records of workers employed at the surveyed establishments. The Establishment Panel is a representative survey of German establishments, which has been carried out annually since 1993 and covers a wide range of aspects of firms' employment policies. It has been one of the major sources to investigate questions concerning employer-provided training. Crucially for my research question, Card, Heining and Kline (2013), have estimated firm wage fixed effects from the social security records, which can be linked to individual firms. With the data on training, I can thus relate a firm's relative position in the wage distribution to its training investments. In my econometric specification, I estimate cross-sectional models of training activities on the firm-specific wage components, controlling for a rich set of controls. To address potential biases arising from the simultaneous determination of training and wages as well as the measurement of wages in administrative data, I employ an instrumental variable approach where I use the past value of the firm-specific wage component as an instrument for the current value. While the use of the LIAB allows me to consider many firm characteristics that are relevant for training investments in the analysis, it is not possible to observe who is being trained and what the consequences of training are for workers' careers. Therefore, I complement the firm-level analysis with a worker-level analysis using the NEPS-SC6-ADIAD, a particularly well-suited data set for this purpose.

The National Educational Panel Study (NEPS) is a longitudinal survey with a special focus on educational processes, including work-related training. I use data from the starting cohort number 6, which follows individuals from working age to retirement. The survey information are linked to the administrative data of the IAB for those respondents who agreed to it. The linked NEPS-SC6-ADIAB data set does not only contain precise information on wages and employment but also identifies the firm, which allows me to merge it with the firm wage fixed effects estimates. I can therefore observe who is trained by a firm with a particular firm-specific wage component. I can furthermore track workers' employment outcomes very precisely, which is an advantage over previous studies using only survey data. With these data at hand, I estimate a number of different cross-sectional models. The first set of outcomes I consider are individual training activities as a function of the firm's position in the wage distribution. The second outcome I study is the log hourly wage depending on the firm wage component and employer-provided training.

The chapter proceeds as follows. Section 4.2 gives an overview of the related literature and highlights the contributions of the chapter. In Section 4.3, I describe the theoretical models of imperfect competition and on-the-job training and derive my empirical hypotheses. Section 4.4 contains the analysis of firms' training engagement and firm wage components. I turn to the worker-level analysis in Section 4.5. In Section 4.6, I discuss policy instruments. Section 4.7 concludes.

4.2 Related Literature and Contribution

This chapter relates to three different strands of labor market literature. First and foremost, I contribute to the literature on the implications of the labor market structure for the provision of on-the-job training. Theoretical examination of the role of market imperfections in the provision of training was initially spurred by the observation that employers frequently seemed to pay for general skills training. This was at odds with the competitive market approach formulated by Becker (1964) in which workers are the residual claimants of any training investments and thus should bear the costs. Without frictions, the resulting level of training in the market would be efficient without any policy intervention. If, however, markets are imperfectly competitive, firms can have an incentive to finance the general training of their workers, not just training in specific skills. This is because, in a market with frictions, workers are paid a wage below their productivity, enabling employers to recover the investment costs made upfront. In addition, in models of training in imperfect labor markets, it is typically assumed that wages are compressed along the training dimension, i.e., that productivity increases more through training than wages. Without wage compression, employers would not gain anything from the training of their workers, but would simply pay a wage below marginal productivity.

The literature has identified several potential sources of wage compression. These are summarized and discussed in Acemoglu and Pischke (1999a). One reason why the wage structure might be compressed are search and matching frictions. If workers and firms cannot easily find alternatives to the current employment relationship, there is a bilateral monopoly situation in wage determination. This creates a match-specific rent, which is typically shared by both parties through bargaining. A compressed wage structure then arises because the match-specific surplus is higher when the worker is trained, and the firm will obtain a share of this larger surplus (see also Acemoglu, 1997). Another source considered by Katz and Ziderman (1990), might be the difficulty of outside employers to judge the content and amount of training precisely. If there is asymmetric information about the quality of training, the outside employer might be unwilling to pay a wage that would be appropriate for the productivity increase gained through training. This enables the current employer to also pay a wage below the productivity of the worker. A variation of the asymmetric information argument is formulated by Acemoglu and Pischke (1998) and Autor (2001). They assume that current employers have better information on the ability of a worker, which creates ex-post monopsony power and thus incentives to invest in the training of those workers. A third source of wage compression is the presence of minimum wages, either due to regulation or because it is the necessary amount of remuneration to induce effort when faced with the threat of moral hazard. If a worker's productivity is below the minimum wage level, it can be increased without having to increase the wage. Similar reasoning applies when wages are compressed due to the presence of unions (e.g. Dustmann and Schönberg, 2009). Finally, interactions between general and firm-specific training can lead to firm-sponsored general training. When general and specific skills are complements, general skills training will also raise the value of firm-specific skills, again generating a compressed wage structure and encouraging firms to invest in such training.

A compressed wage structure across the market means that all other firms will also benefit from a skilled worker. The interests of potential future employers are, however, not represented in the training decision of the worker and the current employer. Because of this positive externality on future employers, the market level of training will be sub-optimally low and there may be a case for government intervention. The extent of under-investment depends on the extent of monopsony. As Manning (2003) and Fu (2011) argue, in a very monopsonistic labor market, the level might be close to the efficient one as there are few potential other employers. In a competitive labor market, the level will also be efficient as workers earn their marginal products at current and future employers. In the in-between case, firms that are higher up the wage distribution have less reason to fear that workers move to another firm, reducing the size of the externality and thus increasing the incentives to invest in general skills training for these firms relative to those with a lower level of pay. This argument will be set out in detail in Section 4.3.

A number of different papers have empirically examined the implications of a non-competitive labor market on the provision of general training empirically. A lot of the evidence consistent with a non-competitive view is indirect as researchers have looked at specific aspects for which the theories predict opposing outcomes. Booth and Bryan (2007), for example, estimate the impact of training on wages at current and future firms. The gains from training for the worker should be larger at a future firm than at the current firm in the model with frictions but not according to standard theory. Bassanini and Brunello (2008) and Konings and Vanormelingen (2015) focus on wage compression as an explanation for firms' engagement in training. Using data from the European Community Household Panel, Bassanini and Brunello (2008) quantify the wage gain from training - the so-called wage premium - in clusters of homogeneous workers and relate this to the amount of training provided for this group. They find a negative relationship, meaning that a more compressed wage structure leads to a higher willingness of firms to finance training. Konings and Vanormelingen (2015) use panel data on Belgian firms to estimate the impact of training and productivity simultaneously. They find that the productivity premium is substantially higher compared to the wage premium. More direct evidence is provided by studies using a measure for frictions in a local labor market. Muehlemann and Wolter (2011) and Rzepka and Tamm (2016) use the number of firms in a sector and a regional labor market defined by commuting zones as indicator of competition. While Muehlemann and Wolter (2011) find that the number of hired apprentices by Swiss firms is significantly negatively impacted by regional density, Rzepka and Tamm (2016) find the same relation for individual on-the-job training in Germany, but the effect is not always precisely estimated. Picchio and Van Ours (2011) approximate market imperfections by estimating an index of search frictions based on the job offer arrival and destruction rate within the manufacturing sector in the Netherlands. According to their results, firms' training expenditures are significantly reduced if labor market flexibility increases.

While all of these papers offer meaningful validations for the theory of training in imperfectly competitive markets, they do not account for the role of firm heterogeneity. By now there is robust evidence that firms pay differently high wages for equally skilled workers (Abowd et al., 1999; Card et al., 2013; Card et al., 2018) and that these differences are relevant for the development of wage inequality. The natural question that arises is "What does this imply for the provision of general training by employers?". At the same time, it opens up a new avenue to test the implications of a frictional labor market for employer-provided training. Most crucially, it can provide insights into the *relative* incentive of an individual firm in a given market to engage in training, a dimension that has not been studied before. Therefore, the first question I want to answer is "What is the correlation between a firm's position in the wage distribution and its training investment?". Because receiving training is significant for workers' careers, the second question I want to answer is "What does this imply for individual training and wages?".

By answering the second question, I also contribute to the literature investigating the impact of training on individual labor market outcomes. Many studies have attempted to estimate the effect of on-the-job training on wages, and have produced ambiguous results (see e.g. Schøne, 2004; Frazis and Loewenstein, 2005; Leuven and Oosterbeek, 2008). For Germany, the evidence is also mixed. While some studies find no or very small positive effects (Pischke, 2001; Görlitz, 2011; Backhaus, 2022) using data from the SOEP, WeLL, and NEPS respectively, others find significant positive effects that are economically relevant using the same data sets (Ruhose et al., 2019; Steffes and Warnke, 2016a; Ehlert, 2017). The different results might be due to differences in estimation strategy, sample period, and types of training measures analyzed. For example, while Görlitz (2011) analyzes the effect of participation in the number of courses using random non-participation, Steffes and Warnke (2016a) differentiate between employer-sponsored and worker-sponsored training in terms of time and find that wages are positively affected mainly by worker-sponsored training. Similarly, Ehlert (2017) differentiates between types of courses along the dimension mandatory/voluntary and employer/worker-financed. He finds that only mandatory employer-financed courses in large establishments or those operating in the public sector positively affect wages, whereas all other combinations do not have any significant effect. As such, the effect of training on wages remains an open question.

From the analysis, I can further draw conclusions about the relevance of training for wage differentials for observably similar workers, and thus add to the literature investigating the sources of firm-wage-heterogeneity. In their wage regressions, Card et al. (2013) account for differences in workers' human capital and unobservable skills. All other factors that could potentially lead to differences in wages are subsumed in the firm wage fixed effect. An increase in the dispersion of these fixed effects can account for a quarter of the wage increase in West Germany between 1985 and 2009. The sources for firm wage premia are currently discussed in the economic literature. Card et al. (2018) relate differences in relative pay to differences in productivity between firms exercising rent-sharing. Building on this, B. Hirsch and Mueller (2020) estimate the differential impact of institutions that increase the workers' bargaining power on firm wage premia conditional on productivity. According to this view, firms' training investments represent an additional benefit to the wage received by the worker and should be positively correlated with firm-level pay. An alternative explanation is proposed by Sorkin (2018), who argues that compensating wage differentials are a significant source of firm wage premia, as otherwise, the flow of workers to systematically lower-paying firms could not be explained. In this view, workers are compensated for relatively low pay with non pay characteristics that they value, and training can be seen as one of these characteristics. Thus, training and firm wage premia would be negatively correlated if compensating wage differentials were the predominant form of compensation.

4.3 Model and Hypotheses

I use the model by Manning (2003) to guide my analysis. It is suitable for my endeavor because it discusses the implications of imperfect labor market competition for the provision of employer-provided general training and allows me to derive empirically testable predictions. Manning keeps the model rather simple. Fu (2011) provides a more elaborate formulation and arrives at the same predictions regarding the relationship between a firm's position in the wage distribution and its incentives to invest in the human capital of its workers. In this section, I begin by outlining the generalized model of monopsony that Manning uses as the basis for his applications which is outlined in Chapter 2 of Manning (2003) (Section 4.3.1). Building on this, in Section 4.3.2 I summarize the model of the provision of general on-the-job training, providing the key formulae for the derivation of my hypotheses. These are specified in Section 4.3.3.

4.3.1 A Generalized Model of Monopsony

The basis for Manning's considerations is a wage-posting model in the style of Burdett and Mortensen (1998). In that model, employed workers engage in search for a job that pays a higher wage, while unemployed workers search for a job with a wage at or above their reservation wage. Search is random in the sense that workers cannot direct their search to a particular job, but receive random offers from firms with openings. Employers post a wage conditional on workers' search behavior and wages posted by other firms. Given the wages offered by other employers and the distribution of workers' reservation wages, the labor supply curve to an individual employer positively depends on the wage it posted. The higher the wage the larger the steady-state employment at the firm, because firms with relatively higher wages find more workers who accept their job offers and lose fewer workers to other employers. Thus, in the "wage posting game" (Burdett and Mortensen, p.258) employers set wages to maximize steady-state profits conditional on the wages offered by other employers and the reservation wages demanded by workers.

Workers are assumed to behave in the following way. They have identical marginal products, p, and attach equal value to leisure, b. They can be in three labor market states, employed, non-employed, and out of the labor force. Both employed and non-employed workers receive job offers drawn randomly at rate λ from the set of firms. There are exogenous job separations, which occur at rate δ. An employed worker will move to another job when they receive a wage offer above her current wage, and a non-employed worker will accept an offer if the wage is at least as high as the reservation wage. The decision to accept a job has no influence on future job offers, therefore the worker accepts an offer if the wage is at least as high as the value for leisure b. Thus, while the job offer and destruction rate are set exogenously, the actual flow rates depend on the wage offer distribution.

Consequently, the employment level of the firm is determined by the flows in and out of the firm. Inflows will be higher the higher the wage offered, as a firm paying wage w will recruit from non-employment (if $w \geq b$) and from firms paying a wage lower than w. It will lose workers because of exogenous job separations and because they move to higher-paying firms. Even though firms are assumed to be identical, they choose different wages that yield the same level of profits, such that there is a continuous cumulative density function of wages denoted by $F(w)$ in equilibrium. Starting from a situation where a mass of firms pays wages equal to productivity $w = p$, it is optimal for a firm to pay a wage lower than p as long as it can retain some workers in steady state, i.e., if workers' labor supply to the firm is not infinitely elastic. Then, if there is a mass of firms paying $w < p$, a firm that pays an infinitesimally higher wage will recruit a significant number of additional workers from the firms paying w while only incurring an infinitesimally small drop in profits per worker. Thus, the initial condition could not have been in equilibrium. The offered wages then lie in the interval,

$$b \leq w \leq p - \left(\frac{1}{1+\frac{\lambda}{\delta}}\right)^2 (p-b) \leq p \tag{4}$$

which implies that, in equilibrium, all workers get paid a wage equal to or above the reservation wage b and below their marginal product p. The markdown on the wage relative to marginal product depends on the parameters that govern the flows of workers in the labor market, the job offer arrival rate, and the job destruction rate. The higher the rate at which job offers arrive relative to their destruction, the more mobile workers become and the lower the markdown.

The result that wages are dispersed in equilibrium is, of course, in contrast to the model of a perfectly competitive labor market in which all workers get paid the same wage equal to their marginal product. The two models can be aligned by adapting the ratio of the job offer arrival rate to the job destruction rate (λ/δ). If job offers would arrive infinitely fast for employed workers, $(\lambda/\delta) \to \infty$, the distribution of wages across workers would collapse to the perfectly competitive equilibrium in which all workers get paid their marginal product. As such, it depends on the degree of frictions in the labor market where on the continuum the offered wages lie. Assuming that all firms are identical is extreme, and the result that there is nonetheless a distribution of wages in equilibrium might be counterintuitive. If one relaxes this assumption to differences between firms, one would arrive at qualitatively similar results, as long as one assumes an upward-sloping supply curve of labor to an individual firm.

For my research question, it is important that the model can explain the fact that equally productive workers receive different wages depending on the firm they work for. Central for the relationship between a firm's position in the wage distribution and its incentives to invest in the human capital of its workers is the feature that a firm offering a higher wage will have a lower separation rate and a higher recruitment rate.

4.3.2 Employer-Provided General Training

To introduce training, there are now two types of labor, unskilled and skilled, with the marginal product of the skilled being higher than that of the unskilled. Both types of labor are paid a wage below their marginal product. It costs c to convert an unskilled worker into a skilled worker. As unskilled workers derive value from becoming skilled, an employer who offers training can reduce the unskilled wage by the expected gain from training. To maximize profits π, employers choose the wages paid for both skill types, w_0 and w_1, and the number of trained workers T.

$$\pi = Y(N_0(w_0), N_1(w_1)) - w_1 N_1(w_1) - w_0 N_0(w_0) - [c - (V_1(w_1) - V_0(w_0))]T \tag{5}$$

The gap between the value of a worker of being trained V_1, relative to the value of being untrained, V_0 in Equation (5) is paid by the workers to the firm and therefore reduces the cost of training.[25] The numbers of unskilled and skilled workers at the firm, N_0 and N_1, will depend on how many workers separate, how many will be recruited, and how many are moving from being untrained to trained, T. Thus, firms maximize their profits subject to

$$s_0(w_0; F)N_0 = R_0(w_0; F) - T \tag{6}$$

$$s_1(w_1; F)N_1 = R_1(w_1; F) + T \tag{7}$$

The separation rate $s_i(w_i; F)$ endogenously depends on the wage paid by the firm and the endogenous distribution of wages offered by other firms. Likewise for the recruitment rate

[25] Manning assumes that workers can be either implicitly or explicitly charged for training, depending on the contract.

$R_i(w_i;F)$. The parameters that affect the wage posted by an individual employer capture how responsive the worker flows are to wage-setting policies, as given by the resulting wage elasticities. The first-order conditions for the wages are given by the following expressions:

$$w_0 = \frac{\epsilon_0^R - \epsilon_0^s(1-\frac{T}{R_0})}{1+\epsilon_0^R - \epsilon_0^s(1-\frac{T}{R_0})} p_0 \tag{8}$$

$$w_1 = \frac{\epsilon_1^R - \epsilon_1^s(1+\frac{T}{R_1})}{1+\epsilon_1^R - \epsilon_1^s(1+\frac{T}{R_1})} p_1 \tag{9}$$

where $\epsilon_i^R = \frac{\partial R}{\partial w_i}\frac{w_i}{R}$ is the elasticity of recruits of a worker type with respect to the wage, $\epsilon_i^s = \frac{\partial s}{\partial w_i}\frac{w_i}{s}$ is the elasticity of workers' separation rate with respect to the wage, $\frac{T}{R_0}$ is the ratio of the flow of trainees to the flow of unskilled recruits, and $\frac{T}{R_1}$ is the ratio of skilled workers recruited internally through training to those externally recruited.

As one can see, the relationship between wages and productivity is determined by the wage elasticities of the recruitment and separation rates. The higher these elasticities, the closer the offered wage is to the marginal product of the workers. In addition, the training intensity of the firm affects the wages of both types of workers. The unskilled wage is lower the higher the fraction of unskilled recruits becoming trained as the firm needs to worry less about deterring them the more likely it is for an unskilled worker to become skilled. The skilled wage, on the opposite, is increasing in the fraction of workers that are recruited through training because only for internally recruited workers (i.e., trained) it can recoup part of the cost of training from the unskilled wage.

The optimal number of trained workers depends on the returns to training for the worker and the firm, as well as the cost of training. The first-order condition for training is

$$[\frac{p_1 - w_1}{s_1} + V_1] - [\frac{p_0 - w_0}{s_0} + V_0] - c = 0 \tag{10}$$

and represents the marginal returns to training. It implies that a worker is trained if the returns from training exceeds its costs. The terms $\frac{p_i - w_i}{s_i}$ denote the expected gain for the employer, who will earn a rent if the productivity increase is not met by a proportional increase in the worker's pay. The difference is scaled by the expected duration of the employment relationship. V_i is the return for the worker. Overall, the sum of the returns must be greater than the training costs for the training decision to be positive. Note that returns to all parties are internalized in the firm, but the interests of future employers are not represented and thus investment in training might be inefficient.

The differences between the perfectly competitive case and the case in which employers have some labor market power can be illustrated using the last three formulae. If there were no frictions in the labor market, separation and recruitment elasticities would be infinite and both skill types would be paid their marginal wage. Equation (10) then implies that only the workers receive the benefits from training and employers have no incentives to bear the costs. If this is not the case, employers might have an incentive to pay for the general training. To derive predictions with regard to which employer is more likely to invest in human capital, one has to make assumptions. For simplification, Manning assumes that the separation and recruitment rates of unskilled and skilled workers are the same within a firm, and argues that "one might think of this as the case where employers choose the same position in the skilled and unskilled workers' wage distribution"(p.309). Crucial to the decision to train or not to train is then whether there is wage compression, that is, whether productivity increases more through training than wages, i.e., $p_1 - p_0 > w_1 - w_0$. If there is wage compression, it means that workers cannot fully recoup the costs from training, so that the firm will pay for it.

If wages are compressed in all firms, then there will be under-investment in training in the labor market. This follows from the fact that it is profitable for all employers in the market if a worker is trained but does not have to be paid a wage proportional to the increase in productivity. However, the interests of potential future employers are not internalized in the training decisions of workers and firms, resulting in under-investment. As described in Section 4.2, scholars have identified several reasons why wages might be compressed, such as match-specific rents or information asymmetries regarding the quality of training. By now, there are also a number of studies that empirically test whether this phenomenon exists and arrive at the conclusion that wages are indeed compressed along the training dimension (Booth and Bryan, 2007; Dustmann and Schönberg, 2009; Konings and Vanormelingen, 2015).

Using Equation (10), Manning derives a prediction for the relationship between a firm's position in the wage distribution and the likelihood of providing general training. His prediction is that training is more likely in firms that are further up the wage distribution, as firms and workers are "better able to internalize the benefits from training" (p.311). With this statement, he refers to the positive externality which accrues to any future employer of a worker who received financed general training from their current employer. The more likely a worker stays with the employer who provided the training, the smaller becomes this externality. As the separation rate decreases in the firm's position in the wage distribution, the threat of the worker leaving the firm after training is lower and hence the expected benefits from training are greater. Equations 11 and 12 formalize the relationship.

$$\Omega(F) = \frac{\Delta p(F) - \Delta w(F)}{s(F)} + \Delta V(F) \tag{11}$$

$\Omega(F)$ is a function that relates the returns to training to the firm's relative position in the unskilled and skilled wage distribution F.[26] Assuming that the separation rate only depends

[26] For exposition, Manning assumes here that the firm chooses the same point in the unskilled and the skilled wage distribution, which is, of course, a critical assumption as this might not be the case in reality. Another approach is chosen by Fu (2011) who assumes that a firm offers a job consisting of a pay rate and training. Thus, there exists only one wage distribution, however, at the cost of assuming a firm trains all the workers it hires. In practice, I measure a firm's position in the wage distribution using the firm-specific wage component

on F and doing some rearrangements, one can write the marginal returns to training as in (12).

$$\Omega'(F) = \frac{\Delta p'(F)}{s(F)} - \frac{s'(F)}{s(F)}[\Delta p(F) - \Delta w(F)] \tag{12}$$

The first term captures how large the productivity increase through training is as a function of the firm's position in the wage distribution. Manning conjectures that $\Delta p'(F)$ is positive as he thinks it's plausible that higher-paying firms have a higher marginal product for everyone, a conjecture that is supported by Fu (2011). Since $s'(F)$ is negative, the second term is also positive if there is wage compression, rendering the whole expression positive. Thus, firms higher up the distribution will have higher incentives to invest in the human capital of their workers.

This means that if a worker happens to work at a firm further down the wage distribution, they do not only get paid a lower wage but is also less likely to receive general skills training. One concern with this conclusion might be that higher-paying firms have lower separation rates, and if training was primarily concerned with the induction of new employees, they might actually train less for this reason. I come back to this in my empirical analysis.

4.3.3 Empirical Hypotheses

Based on these theoretical considerations, I derive empirically testable predictions at the worker and firm level. The first hypothesis is straightforward:

Hypothesis 1: Firms higher up the wage distribution are more likely to provide general training.

I operationalize it by the following three hypotheses:

Hypothesis 1a: The probability of providing any training should increase with the firm's position in the wage distribution.

Hypothesis 1b: Firms higher up the wage distribution train a larger share of their workers.

Hypothesis 1c: The individual probability to be trained is higher for workers employed at higher-paying firms.

Manning considers only the number of trained workers in his model and does not look at training intensity per worker. In an extension of his model, Fu (2011) endogenizes firms' choice of training intensity and conjectures that firms offering a higher pay rate will also offer a higher training intensity. The reasoning behind this prediction is the same as for the decision to train or not train a worker: firms invest more in training per worker the longer the worker is expected to stay with the firm, which positively depends on the wage offered. Thus, my second hypothesis is:

from a two-way fixed effects model, i.e., I do not observe pre-training wages directly. Therefore, I extract them in a decomposition exercise (see Hypothesis 3 below).

Hypothesis 2: Training intensity will be higher in higher-paying firms.

In practice, many firms provide both general as well as firm-specific training and it is not easily possible to distinguish between the two types of training in the data. This could make it difficult to test the above hypotheses if theory would predict that higher-paying firms invest more in general training but are less likely to provide training that is firm-specific. However, the main prediction of the models also applies to specific training, i.e., that firms higher up the wage distribution have a greater incentive to train their worker because the worker is expected to stay with the firm for longer. A formalization of this argument is provided by Quercioli (2005). Thus, a positive correlation between employer-provided training and the firm's position in the wage distribution can be seen as validation of Hypotheses 1 and 2 as long as training is partly general.

With regards to wages, it follows from Hypothesis 1c that an employee of a higher-paying firm is more likely to receive the skilled wage due to training. That means that in a regression of wages on firm-specific wage components, the coefficient of the firm-specific wage component encompasses a direct and an indirect effect of the firm's relative wage position on training. The direct effect captures the fact that a worker with a given qualification will get paid more by a firm that is further up the wage distribution. If the worker was trained by said firm due to the higher incentives to do so, part of the higher wage observed by the empiricist is due to training. Thus, the firm-specific wage component can be decomposed into the direct effect and the indirect part due to higher training. Hypothesis 3 is the following:

Hypothesis 3: The coefficient of the wage premium on a worker's wage should decrease if training is accounted for in the model and the coefficient of training is positive.

4.4 Firm-Level Analysis

To test hypotheses 1a and 1b, I use the LIAB cross-sectional model, a linked employer-employee data set for Germany. Using firm-level data allows me to consider relevant firm characteristics that are not captured in worker surveys. In this section, I first describe the data set in general and the way training and firm-specific wage components are measured in particular (Section 4.4.1). I also provide some descriptive evidence on firms' training engagement and its correlation with firm wage fixed effects in that Section. Then, I analyze the relationship in more detail in Section 4.4.2.

4.4.1 Data and Descriptive Evidence

The LIAB consists of survey information from the IAB Establishment Panel, which is linked to the social security records of the workers employed at those establishments.[27] The Establishment Panel is an annual representative survey of establishments in Germany, which has been conducted since 1993 in West Germany, and since 1996 in East Germany, too. Near to 15,500 establishments from all branches of the economy and of all sizes are surveyed from end

[27] For a description see Ruf et al. (2021).

of June to October. Therefore, most of the answers pertain to the first half of the survey year. It includes questions on firms' engagement in on-the-job training on a biennial basis from 1997 until 2013 when the mode was changed to an annual basis. It also covers a wide range of other topics on employment policy, allowing me to take into account firm-level factors relevant to the provision of on-the-job training that are not covered in individual or household surveys such as the composition of the workforce, wage-setting institutions, or the role of productivity differences between firms. To combine all waves from the survey into a panel and to construct consistent variables over the sample period, I use the code provided by Umkehrer (2017). I use the linked social security records to add information on the age structure of the workforce, which is not covered by the survey. I exclude firms from the financial and public sectors as well as firms from the agricultural and mining sectors. In total, my sample for the period of 1997 to 2017 consists of 31,954 firms in 116,963 observation-years.

Measuring Firms' Position in the Wage Distribution

Crucial to my research question is the fact that the firm information can be linked with the firm wage fixed effects derived from the decomposition approach of Abowd et al. (1999) (also known as AKM framework). For the German data, they have initially been estimated by Card, Heining and Kline (2013), henceforth CHK, and since then been updated by the research data center. I use the CHK firm wage effects as a measure of a firm's position in the wage distribution, which is the central parameter in the theoretical models for firms' relative incentives to invest in training. The estimation of the firm wage premia requires a "connected set" of establishments that are linked by worker mobility. This means that they are estimated over time intervals of several years, not for each year individually. Currently, they are available for the five overlapping time intervals 1985-1992, 1993-1999, 1998-2004, 2003-2010, and 2010-2017. In each interval, they are normalized by the omission of the last establishment dummy, such that firm wage effects have to be interpreted relative to the last in the sample and estimated values from different intervals cannot be readily compared (Card et al., 2013). To be able to pool observations from several intervals, I follow B. Hirsch and Mueller (2020) and subtract the mean of the firm wage fixed effects in each time interval before using them. As a robustness check, I also run separate regressions for each of the four time intervals.

Measuring Firms' Training Engagement

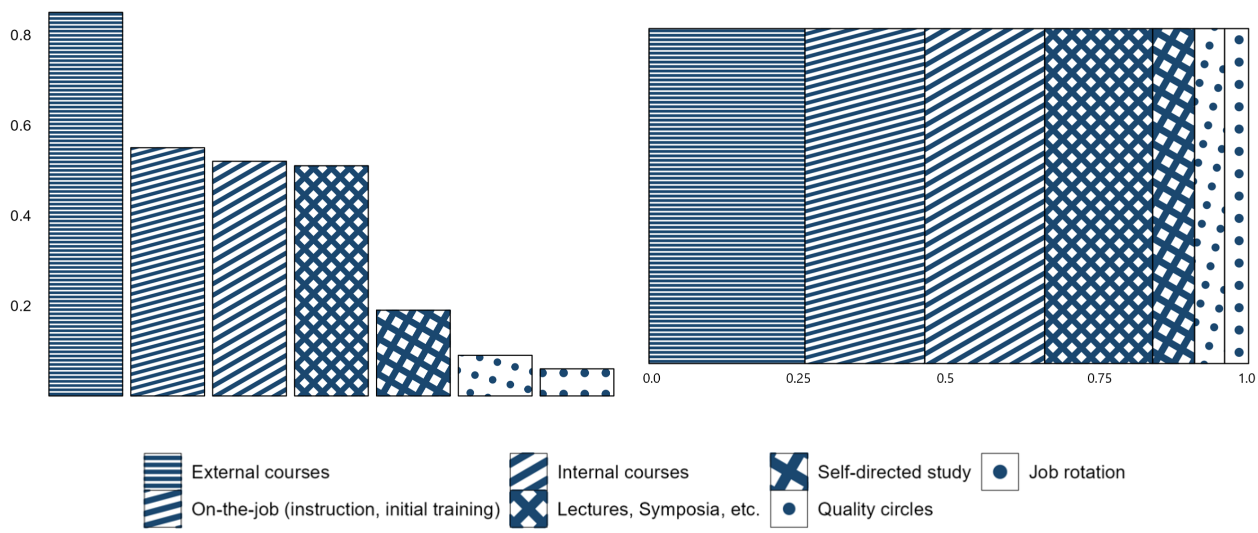
The questions on training participation in the Establishment Panel allow the construction of an extensive and intensive measure. Filter question to the module is the following: *"Did your establishment support training in the first half of the year? To be more precise, did you release staff for the purpose of participating in internal or external training courses and did your establishment cover the expense for these in full or at least in part?"* If the answer is affirmative, questions about the number of trained workers and the form of training are asked. With regards to training extent, the question was changed over the years. From 1997 to 2013 respondents could choose to report the number of trained workers or the number of training measures, from 2014 they could only report the number of trained workers. To deal with this problem, I use the code provided by Hinz and Stegmaier (2018) which converts the number of training measures to the number of trained workers through comparison with similar establishments that indicated the number of workers.

Since I am mainly interested in general employer-provided training, ideally I would like to differentiate between general and specific training in my analysis. Unfortunately, this is not feasible with the Establishment Panel. Respondents are asked to categorize the training measures used, however, these are non-mutually exclusive dummy categories that cannot be related to the number of employees who underwent a specific measure. Nonetheless, they can provide some indicative information that helps to assess the generality versus specificity of training activities, as some of them will naturally be more general than others. Figure 10 shows the relative importance of the different forms of employer-provided training. Panel (a) depicts the share of firms that indicate to have used a certain measure. Here, *External courses* is by far the measure most frequently indicated by firms with more than 80%. *On-the-job training*, *Internal courses*, and *Lectures and symposia* are used by around 50% of training firms. Other measures such as *Self-directed study*, *Quality circles*, and *Job rotation* are used much less often. A similar picture emerges from panel (b) of Figure 10, which shows the share a training measure was used relative to the sum of all measures used. *External courses* is the most common measure, accounting for 26 percent of training measures indicated, followed by *internal courses*, *lectures and symposia*, and *on-the-job training* with about 20 percent each. Again, other forms of training are of much lower significance.

I argue that training taking place in courses can be seen as general training, whereas training that takes place on the job via instruction of colleagues, through job rotation or quality circles is less general in nature. Supporting evidence for this assumption can be found in Steffes and Warnke (2016b). The authors analyze worker- and firm-level determinants of participation in formal training courses using the WeLL dataset. This is a linked employer-employee data set that provides comprehensive course information for workers employed at a subset of 149 firms randomly drawn from the Establishment Panel. Among other things, course participants were asked whether the skills learned in the course are transferable to other firms. For more than 85% of the courses, this was the case (p.7). Thus, a substantial part of employer-provided training reported by firms can be categorized as general.

This is reflected in the responses of workers to a representative survey, which confirms the predominance of formal training courses over other forms of training. The Adult Education Survey (AES) is an obligatory survey on the participation and non-participation in life-long learning of adults in the European Union. When asked about the training measures they have participated in during the last 12 month, *formal training courses* is the measure indicated most often, followed by *lectures, seminars and workshops. Instruction on the job, job rotation, or quality circles* are indicated to a much lesser extent, and only a few report having taken *private lessons* (Bilger et al., 2017, p. 28).

Figure 10: Forms of Further Training



Notes: This figure shows descriptive evidence of the training measures used by establishments in Germany. Data source is the LIAB. Own calculations using sample weights of the Establishment Panel.

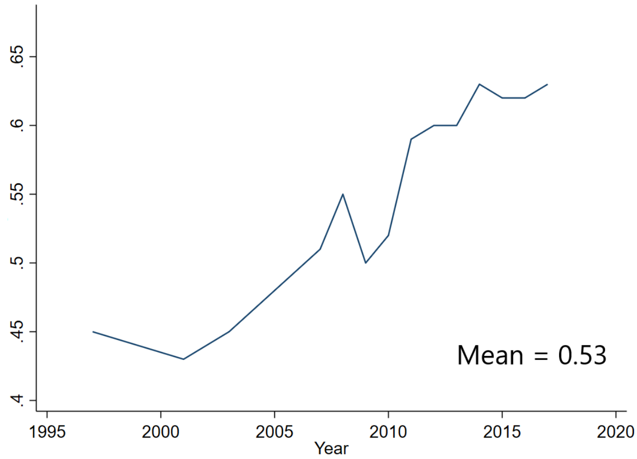
Firms' Training Engagement and its Correlation to Firm Wage Premia

To give an impression of firms' training engagement in general and its correlation with firms' position in the wage distribution in particular, Figure 11 provides some descriptive evidence. The upper panels show developments over time. Every year, about half of the surveyed managers state that the firm promoted further training by releasing employees for a training measure during the first half of the year or by covering the costs. There is a clear upward trend over the years, with a discernible temporary downturn during the recession. On average, around a quarter of employees receive on-the-job training each year. Again, this share is rising over the sampled period, suggesting that the importance of on-the-job training has increased over the last two decades.

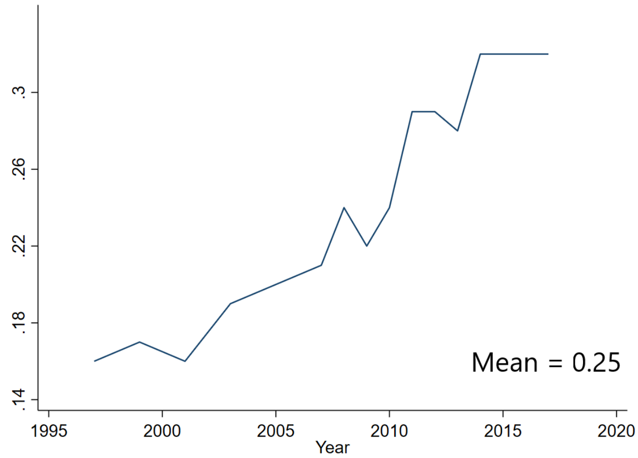
The bottom panels display the correlation between the two indicators with a firm's position in the wage distribution, which is aggregated to vigintiles. There is a positive correlation for both the probability of providing any training as well as for the extent of training. The share of participating firms ranges from 29 percent in the lowest-paying group to 63 percent in third- and second-highest paying groups of firms. With regards to training extent, the relationship is less pronounced. While the difference in the share of trained employees between the lowest- and highest-paying-group of firms is noticeable with 18 versus 27 percent, there are some firms in the lower half of the distribution with relatively high shares of trained workers and some in the upper half with relatively low shares.

Figure 11: Firms' Training Engagement Over Time and its Correlation With Firm Wage Premia

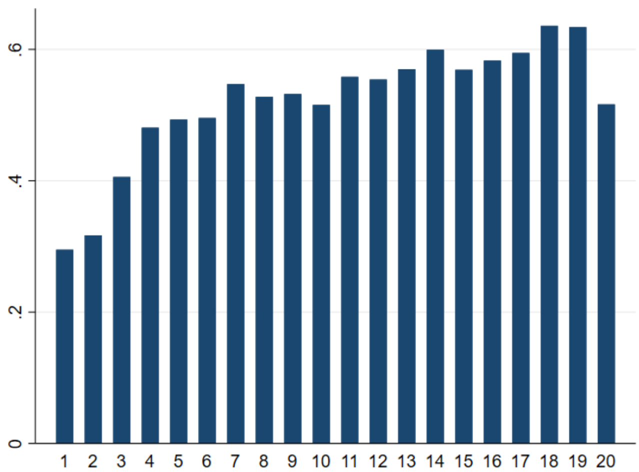
(a) Share of Training Firms Over Time



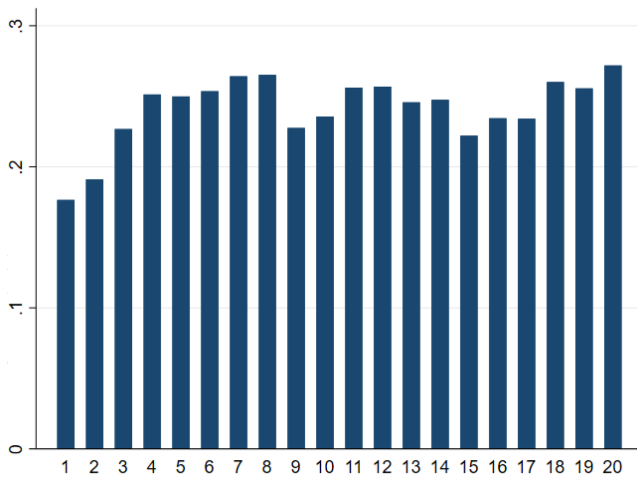
(b) Share of Trained Workers Over Time



(c) Share of Training Firms by Position in the Wage Distribution



(d) Share of Trained Workers by Position in the Wage Distribution



Notes: This figure shows descriptive evidence of the training engagement of establishments in Germany over time and its correlation with firm-specific wage components. Data sources are the LIAB and CHK firm wage fixed effects from Card, Heining and Kline (2013). Weighted data using sample weights of the Establishment Panel. Own calculations.

4.4.2 Cross-Sectional Regression Models of Firms' Training Engagement on Firm Wage Components

To analyze the relationship between firm wage premia and on-the-job training more thoroughly, I estimate cross-sectional models of firms' position in the wage distribution, represented by the firm-specific wage component estimated by CHK, on firms' training activities. The dependent variables are either a dummy for training activity or the share of trained workers relative to all employees. In the first step, I estimate a parsimonious specification where I simply regress the dependent variable on the firm-specific wage component and a set of fixed effects. Then, I successively extend the regression model to account for various factors that influence firms' training engagement. The full model is given in Equation (13),

$$T_{jt} = \beta F_j + X'_{jt}\gamma + \omega_c + \lambda_s + \nu_t + \epsilon_{jt} \tag{13}$$

where T_{jt} represents the measure of training activity of firm j at a given time t, F_j is the CHK firm fixed wage fixed effect associated with that firm and thus, β is the coefficient of interest. X'_{jt} is a rich set of controls that vary across firms and over time. The choice of variables is guided by the literature that studies firms' decisions to engage in on-the-job training. Lastly, ω_c, λ_s, and ν_t account for unobserved region-, industry, and time-specific effects in the provision of training across firms.

The variables I add to the model in the next step account for the composition of the workforce in terms of gender, contract type, age, and skill. These are all factors that influence the expected benefits of a training investment as they directly or indirectly affect the duration over which an investment can be amortized. If a worker is employed on a part-time basis, it will take longer until the investment has paid off and thus a higher share of part-time employees is likely to reduce the likelihood of training engagement of the firm. For a similar reason, female workers, who have a lower labor market attachment, might receive less training than male workers (Rzepka and Tamm, 2016; Steffes and Warnke, 2016a). Regarding age, there is an inversely u-shaped relationship with the probability of being trained. The reasoning behind it is that the need to update skills increases with time since initial education and training while at the same time, the scope for amortization decreases with age. Therefore, the training probability is expected to be highest for middle-aged workers (Grund and Martin, 2012). In terms of skills, measured by education, higher-educated workers are expected to receive more training because there is a complementarity between skills and the ability to learn. Thus, a firm employing a higher share of high-skilled workers is more likely to provide training as a given amount of training will be more beneficial (Grund and Martin, 2012; Steffes and Warnke, 2016a). The firm-specific wage components estimated by CHK are identified controlling for age and education of the worker. Thus, they represent the relative pay of a firm *net of* the human capital of the workers in terms of these variables. Nevertheless, it is helpful to take into account the relative importance of a skill or age group in the firm's workforce, as there may be non-random sorting of workers to firms with a given relative pay along the dimensions of age and education, and not taking this selection into account would lead to omitted variable bias. Descriptive sample statistics for the included variables are shown in Appendix C.1.

While the inclusion of control variables helps to identify the correlation between training engagement and firm-specific wage components more precisely, the coefficient in the OLS regression might be confounded for two different reasons. First, there is a potential endogeneity bias due to a simultaneous equation problem. In the model, wage levels for unskilled and skilled (i.e., trained) workers are simultaneously decided and in turn, determine productivity levels. Therefore, a regression of current firm wage fixed effects on current training decisions might be biased upwards as productivity increases from training, leading to a higher firm wage effect. To address this concern, I add value added per worker as a measure of firms' productivity to the model. This captures the part of the firm-specific wage component that is systematically related to productivity. The remaining variation could stem from differences in wage-setting strategies either in general or due to company taxes, as well as monopoly power in the product market, which are not influenced through training. Unfortunately, the variable "value added" has a lot of missing values, so using it significantly reduces my sample size. Therefore, as an alternative, I implement a two-stage least squares approach, which also helps to deal with the second concern.

Besides simultaneity bias, there could be a bias arising from the measurement of wages in the administrative data. The log daily wage that CHK use as the dependent variable in their two-way fixed effects model is computed by dividing earnings by the number of days employed. If workers are released for training during the period for which the firm-specific wage components are estimated, they will not contribute to production during the training days but still receive the agreed-upon wage. Thus, there will be a difference between the wage given in the data and the wage per day actually worked. The latter - the wage per day actually worked - is what matters to a profit-maximizing firm and what I would ideally like to measure. However, due to the administrative nature of the data, this is not possible and the estimated firm-specific components per construction will be underestimated whenever a worker is trained, potentially leading to a downward bias in β. To address this problem, I instrument the current value of the firm-specific wage component with its lagged value, i.e., that of the previous interval. Even though the lagged value is subject to the same measurement error, it is not *systematically* related to training taking place in the current period and will therefore be an improvement over the current value. Using the lagged value helps to address the endogeneity bias as well, because it is predetermined to the observation period, and allows me to analyze differences in training decisions given a firm's position in the wage distribution.

Results

Table 9 shows the results of linear probability models with the dummy for training engagement as dependent variable and standard errors clustered at the firm level. The coefficient of the CHK firm-specific wage component is positive and significant at the one percent level in all specifications.

The inclusion of control variables capturing the composition of the workforce in relevant dimensions in column (2) somewhat reduces the size of the firm wage coefficient, and the control variables themselves mostly have the expected signs. The higher the share of part-time workers, the lower is the probability of providing training. The positive coefficient of the share of female workers is surprising given previous results in the literature. With the data at hand, it is difficult to investigate this correlation further as it is not possible to distinguish between different types of training. Steffes and Warnke (2016a) show that women are less likely to receive training taking place exclusively during working hours, however, they are more likely to undergo training that at least partially takes place during leisure hours. As would be expected, a higher share of skilled workers is associated with a higher probability of training provision and a higher share of workers aged 36 and above reduces the likelihood of training.

Table 9: Regression of Firm Training Dummy on Firm-Specific Wage Component

Dep. var: *Training dummy*	OLS			2SLS
	(1)	(2)	(3)	(4)
	Base	Composition	VA/N	Lagged value
Firm-specific wage component	0.461***	0.387***	0.376***	0.630***
	(0.0110)	(0.0111)	(0.0130)	(0.0191)
Share female empl.		0.0871***	0.0919***	0.109***
		(0.0108)	(0.0124)	(0.0109)
Share part-time workers		-0.137***	-0.145***	-0.107***
		(0.0104)	(0.0122)	(0.0108)
Share skilled workers		0.258***	0.274***	0.213***
		(0.00891)	(0.0105)	(0.00934)
Share aged 36-50		-0.106***	-0.100***	-0.126***
		(0.0125)	(0.0145)	(0.0129)
Share aged 51-65		-0.170***	-0.175***	-0.192***
		(0.0135)	(0.0157)	(0.0138)
Value added per worker			0.00537***	
			(0.00188)	
Observations	116963	116963	85832	116963
Adjusted R^2	0.118	0.144	0.139	0.131

Notes: This table shows linear probability models of Equation 13 with a dummy for training participation of the firm as dependent variable. All specifications include year dummies, federal state dummies and 16 one-digit-sector dummies (NACE 2). Data sources are the LIAB and CHK firm wage fixed effects from Card, Heining and Kline (2013). Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01. Own calculations.

When value added per worker is added to the model to account for the contemporaneous increase in productivity through training in column (3), the coefficient of the firm wage component decreases only slightly, even though value added per worker is statistically significant. Such it seems that productivity is little affected by contemporaneous training decisions and the coefficient of the firm-specific wage component is not significantly biased upwards for that reason. This is also true if I compare the coefficient with that of the reduced sample of firms for which value added per worker is non-missing, which is 0.380.

This notion is supported by the two-stage-least-squares-approach, which I implement as an alternative strategy to deal with the potential simultaneity bias and with the bias due to the measurement of wages. The coefficient of the firm-specific wage component in column (4) is larger than the coefficient using OLS for the same model in column (2), implying that the downward bias from the measurement of wages in administrative data exceeds the positive bias from simultaneity. Thus, productivity increases do not seem to carry over to the wage levels directly such that the estimated firm-specific wage components are affected, but possibly materialize with some lag.

The downward bias due to the measurement of wages, however, seems quite relevant, as the coefficient of the firm-specific wage component is double the size of the coefficient from the OLS regression. The increase in the coefficient of around 0.3 appears rather large but is plausible given the nature of the bias. The downward bias arises because the daily wage given in the administrative data is calculated based on days employed, and not days actually worked, leading to an underestimation of firm-specific wage components whenever a firm trains its workers. As the number of training days on the left-hand side of the regression is negatively correlated with the denominator of the right-hand-side variable, this is a form of division bias as characterized by Borjas (1980).

To quantify the division bias in my context, I do the following thought experiment. Consider an alternative scenario in which there is a true negative effect of training on wages but the reverse relationship does not exist. Firms pay their workers a wage per day actually worked. Workers value training and are therefore not paid during training days. On average, a worker is trained 3 days per year in Germany, so a worker's absence from the production process weighs only little considering a typical working year of around 250 days. The yearly wage of a trained worker would be 1.2% lower than the wage of an untrained worker. If that was the only relation between training and wages, one would observe firms doing more training paying lower wages, with the negative effect of training on wages being very small. If one would then conduct the reverse regression of training provision on wage differences, however, the negative coefficient would be large as it is identified by dividing differences in training intensity by the very small differences in wages. To get a feeling for the magnitude, assume that 1% of firms reduce the yearly wage to account for the fewer days in production. This would lead to a coefficient of training on wages of -0.83. As I assumed that wages do not influence training in this scenario, the true coefficient is zero, and any deviation from it captures the extent of the bias. Thus, a small but systematic reverse causality relationship can lead to large biases, making a difference in estimated coefficients with and without the bias of 0.3 plausible.

In the case of division bias, any alternative wage measure used as an instrument helps to get closer to an unbiased estimate, including the lags. Naturally, there will be measurement errors in the lagged value of the firm-specific wage component as well. However, these are not systematically related to training decisions in the current period in the same way and therefore the lagged value can serve as an instrument as also argued by Borjas (1980). Evidence that it works as an instrument and not just captures a permanent shock of training on wages is provided by the fact that using the second lag of the firm-specific wage component as an instrument yields a slightly higher, and not a smaller coefficient of wages on training as shown in Appendix C.2.

Thus, the larger coefficient of the firm-specific wage component from the two-stage least squares estimation shown in column (4) is a more credible estimator of the relationship between firms' position in the wage distribution and their training engagement. It implies that an interquartile range change in the firm-specific wage component raises the probability of training by about 20 percentage points. Compared to the mean training participation of 0.53, the size of this correlation is of considerable economic relevance, especially considering that other influential factors such as sectoral differences in training provision are controlled for in the model. In absolute terms, the interquartile range in firm-specific wage components means an earnings difference of approximately €691 per month. On top of this sizable earnings difference, the worker incurs the additional burden of being less likely to receive employer-provided training.

With regard to the extent of training, measured by the share of trained employees relative to all employees, the results shown in Table 10 are qualitatively similar. The correlations between firms' training engagement and the control variables have the same sign. As before, the firm-specific wage component is positive and the correlation is statistically significant at the one percent level. The inclusion of value added per worker seems to have a stronger effect on the coefficient of interest, but most of the change is due to differences between the full sample and the reduced sample of firms with non-missing values for value added per worker, for which the coefficient on the firm-specific wage component is 0.0864. The downward bias from measurement error is again more relevant, as the coefficient estimated in the two-stage least squares regression in column (4) is larger than the OLS coefficient. In terms of magnitude, the coefficient from the two-stage-least-squares approach implies that an interquartile range increase in the firm-specific wage component is associated with an increase in the share of trained employees of 5. Considering that the average share of trained employees in the sample is 0.25, the magnitude of this association is quite meaningful.

Summarizing, the results of the firm-level analysis confirm Hypotheses 1a and 1b, i.e., firms higher up the wage distribution are more likely to provide general training, and the extent of training measured by the share of trained workers relative to all employees is also higher. The correlation between a firm's position in the wage distribution and its training engagement is positive, statistically significant, and of economic relevance, even when a wide range of factors determining firms' training decisions are accounted for.

For the regressions in Table 9 and Table 10, I pool the estimates of firm-specific wage components from different intervals after subtracting the mean value of the respective interval. To make sure that the results are not affected by this procedure, I run separate regressions for each of the four time intervals. Results are very similar across the different periods (see Appendix C.3).

Table 10: Regression of Firm Training Extent on Firm-Specific Wage Component

Dep. var: *Share trained workers*	OLS			2SLS
	(1)	(2)	(3)	(4)
	Base	Composition	VA/N	Lagged value
Firm-specific wage component	0.130***	0.0975***	0.0836***	0.160***
	(0.00688)	(0.00701)	(0.00786)	(0.0131)
Share female empl.		0.0778***	0.0815***	0.0834***
		(0.00724)	(0.00819)	(0.00729)
Share part-time workers		-0.0516***	-0.0529***	-0.0439***
		(0.00672)	(0.00767)	(0.00687)
Share skilled workers		0.149***	0.146***	0.137***
		(0.00574)	(0.00658)	(0.00603)
Share aged 36-50		-0.0382***	-0.0337***	-0.0431***
		(0.00791)	(0.00884)	(0.00798)
Share aged 51-65		-0.113***	-0.121***	-0.118***
		(0.00866)	(0.00952)	(0.00873)
Value added per worker			0.00435**	
			(0.00203)	
Observations	114273	114273	84308	114273
Adjusted R^2	0.112	0.131	0.123	0.129

Notes: This table shows linear regressions of Equation 13 with the share of trained workers as dependent variable. All specifications include year dummies, federal state dummies, and 16 one-digit-sector dummies (NACE 2). Data sources are the LIAB and CHK firm wage fixed effects from Card, Heining and Kline (2013). Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01. Own calculations.

Underlying Mechanisms

Having established that firms higher up the wage distribution are indeed more likely to invest in the human capital of their workers, I turn to the analysis of the mechanisms underlying this relationship. I account for a number of candidate factors. While the relevance of some factors is directly derived from the model, the inclusion of other factors is motivated by previous findings in the literature on the determinants of training. However, these factors are linked to the mechanisms implied by theory.

According to the models, higher-paying firms have more incentives to train their workers because they have a lower separation rate and thus the expected benefits from training are higher, all else being equal. The separation rate is lower because there are fewer firms that make a more attractive offer if the current wage received by the employee is already high. Thus, a firm offering a higher wage will be larger and thus have a higher market share, making it more likely that the worker is employed at that firm. Therefore, firm size and turnover are mediators of the correlation between firm wage components and training, and adding them to the model captures the channel through which the correlation between firm wage components and training is expected to run. Note that turnover and firm size are closely linked in the model, but that the relationship between employment and the separation is not linear (see Equations 6 and 7), which is why they can have separate effects on training and should both be considered as mediator variables. By estimating models with and without these mediator variables, one can learn about the importance of this transmission channel, as the coefficient of firm wage components on training in the model with those variables then represents the "controlled direct effect", holding constant firm size and turnover (Cinelli et al., 2020, p.8). Note that besides the theoretically implied mechanism, there might be other reasons why a larger firm might invest more in training such as better organizational possibilities or the distribution of fixed costs over a larger number of workers (Grund and Martin, 2012; Lallemand et al., 2007). As is common, I use the log number of employees as a measure of firm size. To measure turnover, I follow Bellmann et al. (2018) and calculate yearly separation rates by dividing separations by the average employment between t and $t-1$. For both, firm size and turnover, I use the averages over the last three years prior to the observation year as a control variable in the analysis.[28] Further, I exclude the first and the hundredth percentile of the separation rate distribution to rule out the extreme cases of firm founding and firm closure.

Results for the likelihood of training provision are shown in Table 11. As I am interested in the change in the coefficient when different control factors are included, I first repeat the two-stage least squares estimation for the subset of observations with non-missing values in all control variables in column (1). In column (2), I add the average separation rate and the average firm size to the model. This markedly reduces the size of the coefficient of the firm-specific wage component: 71% of the correlation between firm-specific wage components and training provision goes through the mechanisms proposed in the theoretical models. The coefficients of the control variables themselves are of the expected sign and statistically significant. Thus, the results are very much in line with the theoretical predictions of the model and confirm the relevance of firm size and turnover as mediators for the association between firms' position in the wage distribution and their training investment.

Besides the mechanisms considered in the model, there are other factors that influence the training provision of firms, such as the presence of institutions that shape workers' bargaining power. Collective wage agreements increase the incentives to train because they are usually associated with wage compression, thus making it more beneficial for the firm to invest in human capital (Dustmann and Schönberg, 2009). Works councils also have a positive impact on firm-provided training, because they are an instrument for collective rather than individual voice. They facilitate the resolution of conflicts and thus promote long-term commitments, which again, are a prerequisite for training (Stegmaier, 2012). Both types of institutions are themselves positively correlated with firm size and negatively correlated with turnover. Thus, they present an alternative test for the channels proposed in the wage posting model. Adding the dummies for works council and collective agreement in column (3) also substantially reduces the coefficient of the firm wage component, albeit not as much controlling directly for size and turnover. Again, the control variables themselves have the expected positive correlation with

[28] Ideally, I would want to measure both firm size and turnover over the same interval as the lagged value of the firm-specific wage components are estimated. However, that would severely reduce my sample size and introduce new biases due to sample selection.

training, and the correlation is of statistical significance.

Table 11: Regression of Firm Training Dummy on Firm-Specific Component - Underlying Mechanisms

Dep. var: *Training dummy*	2SLS				
	(1)	(2)	(3)	(4)	(5)
	Base	Size & sep.	Institutions	Strategies	All
Firm-specific wage component	0.636***	0.186***	0.378***	0.465***	0.115***
	(0.0216)	(0.0237)	(0.0247)	(0.0215)	(0.0250)
Separation rate (mean last 3 years)		-0.238***			-0.195***
		(0.0186)			(0.0181)
Log firm size (mean last 3 years)		0.0944***			0.0720***
		(0.00192)			(0.00194)
Works council			0.181***		0.0476***
			(0.00624)		(0.00586)
Coll. agreement			0.0410***		0.0245***
			(0.00519)		(0.00473)
Multi-branch				0.107***	0.0448***
				(0.00472)	(0.00432)
Investments in physical capital				0.203***	0.142***
				(0.00436)	(0.00411)
Observations	95161	95161	95161	95161	95161
Adjusted R^2	0.129	0.234	0.177	0.191	0.257

Notes: This table shows linear probability models of Equation 13 with a dummy for training participation of the firm as dependent variable. All specifications include year dummies, federal state dummies, and 16 one-digit-sector dummies (NACE 2). Data sources are the LIAB and CHK firm wage fixed effects from Card, Heining and Kline (2013). Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01. Own calculations.

Further relevant differences in firm characteristics that the model abstracts from are management type and investment strategies. As Manning (Manning, 2003, p.39) also acknowledges, it was "reasonable to assume that firm heterogeneity exists". Differences in training investments between firms paying differently high wages could stem from differences in investment strategies of firms. If a firm has invested in physical capital, it will be more likely to train its workers to become accustomed to new equipment. Similarly, human resource management strategies are likely to differ depending on whether the firm is managed locally or internationally as an international firm has more resources to develop a training scheme (Lynch and Black, 1998; Jost, 2022). Again, both types of investments are positively correlated with firm size. Therefore, in the last step, I add a dummy of whether the firm is part of a multi-branch and a dummy for investments in physical capital in the previous business year to the model. This leads to a reduction in the magnitude of the coefficient on the firm-specific wage component. A firm that has bought new equipment is more likely to train its workers, as is a firm that belongs to a multi-branch compared to a firm that is managed locally. There remains an economically meaningful association between firms' position in the wage distribution and likelihood of providing training even when these factors are accounted for.

Considering the mediators suggested by theory jointly with the other underlying factors in column (5) brings about a further reduction of the firm wage coefficient relative to column (2) when only the separation rate and firm size are included. Compared to the initial reduction, however, the additional gain is rather small, confirming the robustness of the relationship derived from the theoretical model alone.

Heterogeneity analysis

It is likely that the relationship between a firm's position in the wage distribution and its training engagement is not homogeneous across firms. In my analysis so far, I abstract from differences in training between sectors and only consider the correlation between firm-specific wage components and training within sectors. To test whether the results differ depending on the sector, I repeat the regression analyses from above for the group of manufacturing firms (including construction) and the group of service sector firms separately. Table 12 shows the results. The positive association between firm-specific wage components and training investments is present in both groups of sectors, as indicated by the positive and significant coefficients. The association is stronger in the manufacturing sectors, where the incidence of training is generally lower than in the service sectors. Thus it appears that considerations of the profitability of a training investment are more important in sectors where training is less common.

Table 12: Sectoral Heterogeneity of the Association Between Firm Training Engagement and Firm-Specific Wage Component

Dependent variable	Training participation		Training extent	
	Manufacturing	Services	Manufacturing	Services
Firm-specific wage component	0.794***	0.552***	0.274***	0.125***
	(0.0309)	(0.0221)	(0.0198)	(0.0162)
Observations	53560	63403	52372	61901
Adjusted R^2	0.131	0.098	0.066	0.108

Notes: This table shows linear probability models/linear regressions of Equation 13 with either a dummy for training participation of the firm or the share of trained workers as dependent variable. Manufacturing includes the *Manufacturing* and *Construction* sector according to NACE 2 while Services encompasses all other sectors. Data sources are the LIAB and CHK firm wage fixed effects from Card, Heining and Kline (2013). Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01. Own calculations.

4.5 Worker-Level Analysis

To test Hypothesis 1c, 2, and 3, I use the NEPS-SC6-ADIAB data, a linked survey-administrative data set for Germany. With these data, I can observe which worker is being trained by a firm with a given firm-specific wage component and can thus control for relevant worker and job characteristics that are not available in the LIAB. In this section, I begin again with a description of the data set in Section 4.5.1, providing detailed information on the measurement of training activities and wages, as well as some descriptive evidence on the training involvement of workers in Germany. I turn to the regression analysis in Section 4.5.2.

4.5.1 Data and Descriptive Evidence

The National Educational Panel Study (NEPS) is a survey carried out by the Leibniz Institute for Educational Trajectories (LIfBi) with the target of collecting longitudinal data on the development of competencies, educational processes, educational decisions, and returns to education. The study is organized in different cohorts. I use data from the starting cohort number 6, i.e., the "Adult education and lifelong learning" substudy of individuals born between 1956 and 1986. It tracks persons starting from working age to retirement and beyond. Due to the focus on education, information on educational processes of all kinds, including work-related training, are gathered in detail, alongside a broad set of biographical and socio-demographic characteristics. The survey data is linked to the administrative data of the IAB for the 85% of respondents who agreed to it.[29] The combined data does not only contain precise information on wages and employment, but also the identification of the firm, allowing me to merge the CHK firm wage fixed effects estimates and some aggregate firm characteristics. I can thus observe who is trained by a firm with a given wage premium, something which is not possible with the Establishment Panel, where firms only report the aggregate number of trained workers. It also enables me to characterize firms' training engagement in more detail and to control for a wide range of individual and job characteristics that determine workers' training participation. The use of the NEPS-SC6-ADIAB is therefore an ideal complement to the analysis at the firm level, as I can ensure that the results obtained so far are still valid when taking into account worker and job characteristics.

For my analysis, I construct a sample of employed individuals aged up to 60 years. For the preparation of the administrative data, I rely on the guidelines of Dauth and Eppelsheimer (2020) and their programming examples. The sample period ranges from 2010 to 2017. Analogously to the firm level, I exclude workers in the public and the agricultural and mining sectors. I further exclude workers in occupations where it is obligatory to undergo work-related training such as workers in legal occupations, medical personnel, architects, and persons working in real estate.[30] My final sample includes 3,998 individuals working in 4,355 firms in a total of 16,897 observation-years. There are some cases in which several workers in the sample are employed at the same firm. This concerns only 5 percent of the firms, while for the other 95 percent, I only observe one worker per firm.

Measuring Employer-Provided Training of Surveyed Individuals

In the NEPS, respondents are asked whether they have participated in any training courses since the last interview. This means that only part of the training activities provided by employers are covered by the survey. As described in Section 4.4.1, this is the most relevant form of training in Germany. In fact, it is very uncommon that a firm reports training through other measures, such as lectures, instructions on-the-job, job rotation, or quality circles but

[29] For a description of the dataset see Bachbauer and Wolf (2022).

[30] To be precise, I exclude workers with KldB 2010 3-digit codes 731, 722, 723, 813, 814, 815, 816, 817, 818, 613, 21, 311, and 932.

not through either external or internal courses. In the sample of firms surveyed in the IAB Establishment Panel, 95 percent of those doing any training indicate courses as a measure, and formal training activities in terms of courses and workshops account for two-thirds of all used training measures. Furthermore, they can be viewed as training in general skills, i.e., the type of training I am interested in. I can thus draw definite conclusions regarding the relationship between the firm's position in the wage distribution and general training in terms of the most important training form.

If people state to have participated in a training course, information on the content, duration, and motivation (private or occupational) is recorded for up to five of these courses. Additional information is asked for two courses selected randomly from the reported courses. These include whether costs are incurred by the employer, whether they led to a certificate or license, whether they took place during work or leisure time and whether they were supplied by internal or external staff. I use this randomly selected subset to get insights into the nature of courses. Table 13 shows descriptive statistics for work-related training of individuals in my sample. The upper part contains information on the frequency of training. On average, 20 percent of workers attended at least one training course. Those who did participate, report on average 2 courses, resulting in an unconditional mean of 0.4 courses per person. The five courses that are recorded in detail had a mean length of 0.63 workweeks, i.e., about three working days. The unconditional mean length of training courses is thus 0.12. I will use the recorded training length as a measure of training intensity even though some workers state to have participated in more than five courses. However, these are only 5 percent of the sample, such that this restriction should not affect my results in a substantive manner.

Table 13: Work-Related Training of Individuals

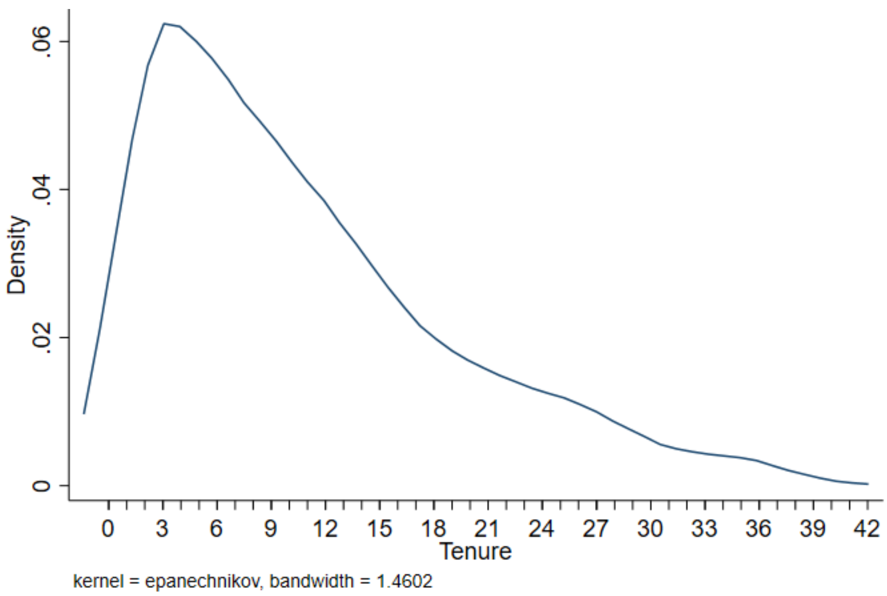
Frequency of training	Mean
Training participation during the last 12 months	0.20
Number of work-related courses	0.40
Length of courses (up to five) in work weeks	0.12
Observations	16,868
Characteristics subset of randomly chosen courses	
Financed by employer	0.96
During working hours	0.88
Supplied by external staff	0.56
Attendance certified	0.78
Certificate or license	0.24
Observations	6,340

Notes: This table shows descriptive evidence of the training participation and intensity of workers in the sample. Further, it shows the characteristics of the courses respondents participated in. Data source is the NEPS-SC6-ADIAB. Own calculations.

Further characteristics of the courses are shown in the lower part of Table 13. Almost all courses are financed by the employer and the vast majority take place during working hours. More than half of the courses are supplied by external staff and attendance is certified in four out of five times. About a quarter of courses lead to a specific certificate or license. Taken together, these statistics lend support for the assumption that the training recorded in the data is indeed employer-provided training in the sense of the model and that a significant part of it can be characterized as general. The fact that more than half of the courses are supplied by external staff and attendance is certified more often than not makes it plausible that workers learn transferable skills and that they can prove it vis-à-vis a potential future employer.

One objection to the hypothesized positive correlation between a firm's position in the wage distribution and its training provision voiced by Manning is that higher-paying firms have fewer separations and less often recruit new workers. If training mainly serves to induct new employees, the relationship might be the other way around. Figure 12 shows the distribution of years on the job at the time of training. While on-the-job training is more frequent in the early years of a job, it is also pervasive in the later years.

Figure 12: Tenure on the Job at Time of Training



Notes: This figure shows the tenure on the job at time of training participation in years. Data source is the NEPS-SC6-ADIAB. Own calculations.

Measuring Wages in the NEPS-ADIAB

I construct hourly wages using the information on weekly working time from the NEPS and on the daily wage from the social security records, assuming a workweek of five days. Fortunately, only 1 percent of respondents had missing values on their working time and dropped out of the sample. The daily wages from the administrative data are topcoded at the social security threshold. As is common, I impute the values of wages above the threshold. For this, I use the code provided by Dauth and Eppelsheimer (2020) who implement a two-step imputation procedure building on Gartner et al. (2005), Dustmann et al. (2009) and Card et al. (2013). In the first step, Tobit wage equations are fitted in clusters of observations by year, education, and East/West, controlling for worker characteristics. In an intermediate step, so-called "leave-one-out means" of wages of each worker over time and of all workers within a firm in a given year are calculated, without considering the respective observation. Then, the Tobit wage regressions are repeated including those two types of averages, which mimic controlling for worker and plant fixed effects. To exploit as much information as possible, I carry out the wage imputation on the entire employment biographies of the respondents, not just the wages at the time of the interview used in the final sample.

4.5.2 Cross-Sectional Regression Models of Workers' Training Participation and Wages

With this data at hand, I estimate a number of different regressions. First, I analyze how workers' training activities depend on firm-specific wage components. More precisely, I test Hypothesis 1c, according to which the individual probability of being trained should be higher for a worker employed at a higher-paying firm. Then I test Hypothesis 2 regarding the positive relationship between individual training intensity and firm-specific wages. I use a similar approach as in the firm-level analysis, going from a parsimonious specification to a model in which I control for other relevant individual and firm factors. Equation (14) specifies the final model.

$$T_{it} = \beta F_{j(it)} + X'_{j(it)t} + Z'_{it}\delta + \phi A_i + \omega_{c(j(it))} + \lambda_{s(j(it))} + \theta_{o(it)} + \nu_t + \epsilon_{it} \tag{14}$$

Here, T_{it} is the individual's probability to participate in training or the length of training respectively. $F_{j(it)}$ is the firm-specific wage component of the firm where the worker i is employed in year t. $X'_{j(it)t}$ is the vector of firm variables available in the data, i.e., the share of part-time and marginal workers. The vector Z'_{it} contains the individual-level factors that, according to the literature, are relevant for participation in training. These include socio-demographic characteristics of the worker such as gender and household size, as well as job characteristics like contract type and tenure on the job. As said before, women have been found to undergo less employer-sponsored training, possibly due to their lower labor market attachment. Along the same line, household size might matter, as more family-orientated workers may be seen as less motivated to develop their career, or less able to participate in training due to lack of time. A worker's contract type is relevant because it has direct effects on how likely a training investment will amortize itself. Job tenure matters because training is more likely to happen at the beginning of a job.

Z'_{it} further includes variables that capture individuals' observable human capital encompassing age, education, and labor market experience. As at the firm level, even if the firm-specific wage components are estimated conditional on age and education, it is a good idea to control for the worker's human capital to be on the safe side, as there may be non-random sorting of workers into firms along these dimensions. Also, I only have a small sub-sample of the one Card et al. used for their estimations, making it possible that there is an omitted variable bias in my sample not accounted for in their estimates. Thus, I can make absolutely sure that an identified positive correlation is not confounded by the omission of these control variables.

Besides observable human capital, differences in training participation could stem from unobserved heterogeneity between workers. To deal with this, I follow Steffes and Warnke (2016a) who use the worker's fixed effect from a log wage regression in the pre-survey period as a proxy for unobserved worker productivity. In my case, the proxy is the CHK person-specific wage component for the period before the NEPS survey, represented by A_i. The inclusion of the proxy in the regression should not make a big difference in the results as the firm-specific wage components are identified conditional on the person fixed effects, which are highly correlated over estimation intervals. But again, by including it I can make absolutely sure that there is no omitted variable bias due to sorting or sample differences. In addition to time-, region- and sector-specific differences ν_t, $\omega_{c(j(it))}$, and $\lambda_{s(j(it))}$, I account for differences in the training participation between occupations $\theta_{o(it)}$.

As before, the OLS regression could be biased due to simultaneity and due to the measurement of daily wages in the administrative data. Therefore, I also employ a model in which I use the lag of the CHK firm-specific wage component to predict its current value to deal with the potential biases.

Second, I examine log hourly wages. As conjectured in Hypothesis 3, the coefficient of the wage premium in a log wage regression should decrease if I account for training in the model and the coefficient of training is positive. To test this hypothesis, I proceed in three steps. In the first, step, I estimate a specification that mimic the model Card et al. (2013) use to estimate the firm and person fixed effects as shown in Equation 15. It includes only the firm wage fixed effect, $F_{j(it)}$, human capital measures in terms of age and experience, H'_{it}, a set of year dummies, ν_t, and the proxy for ability, i.e., the pre-determined value of the CHK person effect, A_i. In the next step, I add a measure for training. Following Ehlert (2017), I assume that wage gains from training are realized with some lag and that training has a permanent effect on wages. Therefore, the independent variable in Equation 16 is the sum of past training relative to the first survey wave 2009/2010, $\sum_{z=2011}^{t-1} T_{it}$, and the sample period for this part of the analysis is 2012 to 2017. Thus, I can analyze what happens to individual wages if a worker with a given productivity is trained. I am also able to trace out the importance of the training channel for the observed firm-specific wage components. In the last step, I extend the model in order to align it with the literature on wage returns to training. To that end, I include control variables for gender and contract type, Z'_{it}, and firm size $S_{j(it)t}$. I also include state, sector, and occupation fixed effects: $\omega_{c(j(it))}$, $\lambda_{s(j(it))}$, and $\theta_{o(it)}$. Descriptive sample statistics for the considered variables are shown in Appendix C.4.

$$log(W_{it}) = \beta_0 F_{j(it)} + H'_{it}\delta + \phi A_i + \nu_t + \epsilon_{it} \tag{15}$$

$$log(W_{it}) = \beta_1 F_{j(it)} + \beta_2 \sum_{z=2011}^{t-1} T_{it} + H'_{it}\delta + \phi A_i + \nu_t + \epsilon_{it} \tag{16}$$

$$log(W_{it}) = \beta_1 F_{j(it)} + \beta_2 \sum_{z=2011}^{t-1} T_{it} + H'_{it}\delta + \phi A_i + \nu_t + Z'_{it}\gamma + \mu S_{j(it)t} + \omega_{c(j(it))} + \lambda_{s(j(it))} + \theta_{o(it)} + \epsilon_{it} \tag{17}$$

Results

Results of linear probability models with a dummy for individual training participation as dependent variable are presented in Table 14. Standard errors are clustered by individual. It would also be conceivable to cluster standard errors by firm or to use a two-way clustering approach. As described above, 95% of the workers in the sample are employed at a firm where no one else from the sample is employed, therefore I expect this dimension of cross-individual correlation to be of little importance. Using standard errors clustered by individual and firm does not change the results.

Like in the firm-level analysis, the coefficient of the firm-specific wage component is positive and highly significant in all specifications. When I extend the baseline model to control for the worker characteristics contained in Z'_{it} in column (2), the coefficient of the firm-specific wage component becomes smaller, but not by a lot. For the sake of readability, the variables already in the estimation of the firm-specific wage components are not shown. Of the other control variables, only a few are statistically significant. Socio-demographic characteristics are not significantly correlated with individual training probability. As expected, working part-time or on a fixed-term basis is associated with a lower likelihood of being trained. Tenure on the job is not significantly correlated with the likelihood of participation in training, suggesting that it does not seem to have any additional explanatory power conditional on the other variables in the model.[31] It is also possible that tenure on the job is more important for on-the-job training from colleagues, which is not covered by the NEPS.

The inclusion of the variables capturing the composition of the workforce in column (3) shows that it is relevant to consider firm-level factors next to individual-level determinants, as a higher share of marginal part-time workers in the firm reduces the individual likelihood of being trained even conditional on individual part-time status, implying that the overall volume of working hours at the firm influences how well a firm can reorganize the workload when people are being trained. Overall, the results in columns (1) to (3) point to the importance of job and firm characteristics rather than individual characteristics.

When I account for potential biases in column (4), the coefficient on the firm-specific wage com-

[31] This is also found by Steffes and Warnke (2016b), whose estimation strategy is closest to mine, but use a different data set.

ponent is again larger than its counterpart in the OLS regression. Considering an interquartile range change in the firm-specific wage components, the coefficient in (4) implies that if a worker would move from a firm at the lowest quartile of the wage distribution to a firm at the highest quartile, the probability of receiving firm-sponsored training is 0.055 percentage points higher. Given that the mean training participation in the sample is 0.2, this difference is of high economic relevance. Thus, I can confirm the validity of Hypothesis 1 with individual-level data: higher-paying firms are more likely to provide general training for their workers (Hypothesis 1c).

Table 14: Regression of Individual Training Participation on Firm-specific Wage Component

	OLS			2SLS
	(1)	(2)	(3)	(4)
	Base	Worker	Firm	Lagged value
Firm-specific wage component	0.181***	0.155***	0.131***	0.204***
	(0.0193)	(0.0201)	(0.0206)	(0.0334)
Female		0.0112	0.0102	0.0121
		(0.0109)	(0.0109)	(0.0109)
Household size		0.00347	0.00376	0.00375
		(0.00363)	(0.00363)	(0.00363)
Number of children		-0.00194	-0.00235	-0.00204
		(0.00634)	(0.00635)	(0.00633)
Part-time		-0.0262**	-0.0211*	-0.0234*
		(0.0112)	(0.0120)	(0.0120)
Fixed-term contract		-0.0278**	-0.0289**	-0.0307**
		(0.0122)	(0.0121)	(0.0122)
Tenure on the job		0.000941	0.000815	0.000745
		(0.00146)	(0.00146)	(0.00146)
Tenure on the job sq.		-0.000559	-0.000565	-0.000550
		(0.000552)	(0.000552)	(0.000552)
Share part-time workers			0.0354	0.0405
			(0.0301)	(0.0303)
Share marginal workers			-0.135***	-0.107***
			(0.0358)	(0.0369)
Human capital variables		Yes	Yes	Yes
Proxy ability		Yes	Yes	Yes
Observations	16897	16897	16897	16897
Adjusted R^2	0.049	0.052	0.053	0.052

Notes: This table shows linear probability models of Equation 14 with individual training participation as dependent variable. All specifications include year and federal state dummies, 16 one-digit-sector dummies (NACE 2), and 11 occupation dummies according to Blossfeld. Data sources are the NEPS-SC6-ADIAD and CHK firm- and person wage fixed effects from Card, Heining and Kline (2013). Standard errors in parentheses are clustered at the individual level. * p<0.10, ** p<0.05, *** p<0.01. Own calculations.

Results for linear regression models with individual training length as dependent variable are shown in Table 15. Training intensity is significantly positively associated with the firm-specific wage component. Only a few of the control variables at the worker level added in column (2) are significant. Women tend to receive fewer hours of training. This finding is congruent with the study by Steffes and Warnke (2016a) who differentiate between employer- and worker-sponsored training in terms of hours and establish that women are more likely to undergo training that happens during leisure time but receive less training taking place during working hours, which is what I measure here. While job tenure was not significantly correlated with training participation, it matters for the length of courses once a worker is trained. Those who have been on the job longer tend to receive fewer hours of training, which would be expected from a theoretical point of view. Again, the more marginal workers are employed at a firm relative to all employees, the lower the average training length.

Turning to the two-stage least squares model in column (4), the coefficient on the firm-specific wage component implies that an interquartile change raises the training length by 0.05. Considering that the average training length in the sample is 0.12, this is a sizable increase. For those workers who are being trained, it corresponds to an increase in training from about three working days to four and a half. Hence, Hypothesis 2 is also confirmed by the data, that is, training intensity is higher in higher-paying firms.

Table 15: Regression of Individual Training Length on Firm-specific Wage Component

	OLS			2SLS
	(1)	(2)	(3)	(4)
	Base	Worker	Firm	Lagged value
Firm-specific wage component	0.154***	0.126***	0.103***	0.181***
	(0.0219)	(0.0214)	(0.0222)	(0.0340)
Female		-0.0329***	-0.0337***	-0.0316***
		(0.0103)	(0.0104)	(0.0104)
Household size		0.00176	0.00203	0.00202
		(0.00344)	(0.00346)	(0.00346)
Number of children		0.00293	0.00249	0.00189
		(0.00701)	(0.00697)	(0.00695)
Part-time		-0.0103	-0.00324	-0.00573
		(0.0151)	(0.0133)	(0.0135)
Fixed-term contract		0.000443	-0.0000802	-0.00201
		(0.0270)	(0.0274)	(0.0272)
Tenure on the job		-0.00302**	-0.00311**	-0.00318**
		(0.00148)	(0.00148)	(0.00148)
Tenure on the job sq.		0.000774	0.000768	0.000783
		(0.000539)	(0.000540)	(0.000540)
Share part-time workers			0.0155	0.0210
			(0.0412)	(0.0411)
Share marginal workers			-0.114**	-0.0837*
			(0.0455)	(0.0468)
Human capital variables		Yes	Yes	Yes
Proxy ability		Yes	Yes	Yes
Observations	16897	16897	16897	16897
Adjusted R^2	0.018	0.020	0.020	0.020

Notes: This table shows linear regressions of 14 with individual training length as dependent variable. All specifications include year and federal state dummies, 16 one-digit-sector dummies (NACE 2), and 11 occupation dummies according to Blossfeld. Data sources are NEPS-SC6-ADIAB and CHK firm- and person wage fixed effects from Card, Heining and Kline (2013). Standard errors in parentheses are clustered at the individual level. * p<0.10, ** p<0.05, *** p<0.01. Own calculations.

Having confirmed that a higher firm-wage effect is strongly positively associated with the individual likelihood of participation in training courses and the length of these courses, the question is what this implies for individual wages and whether differences in training behavior of firms contribute to differences in estimated firm-specific wage components. As described above, I analyze the interaction between wages, training, and firm wage fixed effects by successively estimating the models in Equations 15, 16, and 17. Results are shown in Table 16, columns (1)-(3) respectively.

Table 16: Log Hourly Wage Regressions on Firm-Specific Wage Component and Past Training

	(1) Human capital	(2) Training	(3) Controls
Firm-specific wage component	1.279***	1.272***	0.832***
	(0.0828)	(0.0828)	(0.0791)
Person-specific wage component	0.606***	0.602***	0.397***
	(0.0309)	(0.0309)	(0.0298)
Past training length		0.0147**	0.00522
		(0.00631)	(0.00479)
Part-time			-0.256***
			(0.0299)
Fixed-term contract			-0.102***
			(0.0346)
Female			0.00988
			(0.0226)
Labor market experience			0.00115
			(0.00192)
Tenure			0.00514**
			(0.00252)
Tenure sq.			-0.000866
			(0.000811)
Log Number of employees			0.0650***
			(0.00545)
Human capital variables	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
State fixed effects			Yes
Sector fixed effects			Yes
Occupation fixed effects			Yes
Observations	12999	12999	12999
Adjusted R^2	0.478	0.478	0.551

Notes: This table shows linear regression models of Equations 15, 16, and 17 with log daily wage as dependent variable. Data sources are the NEPS-SC6-ADIAB and CHK firm- and person wage fixed effects from Card, Heining and Kline (2013). Standard errors in parentheses are clustered at the individual level. * p<0.10, ** p<0.05, *** p<0.01. Own calculations.

Column (1) presents the raw specification where I only include controls for human capital in terms of age and education, pre-period unobserved worker ability, and macroeconomic trends captured by year fixed effects. This model is very similar to the model used by Card et al.

(2013), but it is not exactly the same, with the dependent variable being hourly wages instead of daily wages and unobserved worker heterogeneity being measured in the pre-period and not in the current period due to my interest in training. Also, of course, I only have a sub-sample of their sample. Thus, a coefficient for the firm-specific wage component of around one, but not exactly one, as in column (1), is quite plausible.

In column (2), I add an indicator for past training in workweeks to the model. According to Hypothesis 3, the coefficient of the firm-specific wage component should decrease if I account for training and the coefficient of training should be positive. While past training is significantly positively associated with wages, the inclusion of it in the model only marginally decreases the magnitude of the coefficient of the firm-specific wage component, implying that differences in pay for equally skilled workers are in part driven by differences in training decisions between high- and low-paying firms, but that these are quantitatively not so important.

When I add relevant worker and firm characteristics in column (3), past training is no longer significantly correlated with wages. As discussed in Section 4.2, many studies have found low or insignificant wage returns to firm-provided training in Germany. But some have also identified statistically significant positive returns. Ehlert (2017) for example also uses data from the NEPS and finds significant monetary returns for on-the-job training in firms with an internal labor market. These are either large firms or firms belonging to the public sector. In my analysis, I only consider private sector firms, something that could account for the difference in results. Thus, it strongly depends on the data, the sample, and the type of training measures considered whether or not statistically significant wage returns from training can be identified. In the NEPS, I only have information on formal training in terms of courses that primarily take place during working hours. While this captures the most important part of training activities of German establishments, there are other forms that are also relevant for wages, such as training taking place during leisure time (Steffes and Warnke, 2016a) and training on-the-job, which will possibly impart general as well as firm-specific skills. Consideration of the entirety of training activities in the analysis or a longer observation period might plausibly render positive and significant wage returns that might also affect the firm-specific wage components more.

In summary, the results regarding wage responses to training and the relevance of training as a mechanism underlying firm wage differentials are not as clear-cut as those regarding the relationship between the firm's position in the wage distribution and individuals' training. Even if participation in training does not lead to large wage increases, it still matters for workers' careers as it significantly improves employment prospects (Picchio and Van Ours, 2013; Ebner and Ehlert, 2018). Being matched with a higher-paying firm is thus still more beneficial because workers can expect to enjoy a relatively higher wage for longer than comparable workers at lower-paying firms due to the expected duration of the employment relationship being longer at a higher-paying firm.

4.6 Policy Implications

The positive correlation between a firm's position in the wage distribution and its investment in training shown above has immediate implications for policymakers. It means that workers who are matched with a lower-paying firm will be likely to incur the additional burden of receiving less on-the-job training. Thus, the careers of equally skilled workers matched with firms at different ends of the wage distribution will not only diverge in terms of pay, but also in terms of further skill development. Even though direct wage returns are not evident in the data, a lower level of human capital accumulation through training comes with a higher level of career instability for those workers employed at lower-paying firms. Thus, an initial inequality in terms of wages between workers matched with differently high-paying firms will be magnified by the differential incentives of these firms to invest in training. In addition to the increasing inequality between workers, the results also suggest that there is underinvestment in employer-provided general training in the market due to the poaching externality. The question therefore arises as to whether and how economic policy measures could improve the incentives for the provision of employer-financed training. There are a number of different instruments that could be used to address the problem. The appropriateness of an instrument ultimately depends on the policy objective, i.e., whether the objective is the internalization of the poaching externality or an additional objective such as the internationalization of the positive external effects of training on innovation.

An effective way of preventing newly trained workers from being poached by rival firms would be to use a non-compete clause, i.e., prohibiting employees from joining a direct competitor for a period of time after leaving the current employer. Such a clause obviously makes it less attractive if not impossible for workers to change jobs, thus ensuring that a training investment would not be lost. In an empirical study of the US labor market, where non-compete clauses are widespread, Starr (2019) finds that indeed, higher enforceability of a non-compete clause is associated with a 14% increase in training that is most often firm-sponsored and meant to upgrade or teach new skills. On the downside, the fact that non-compete clauses limit workers' mobility also means that they probably have negative effects on wages. In a market with frictions, where firms have wage-setting power, the use of non-compete clauses would further strengthen employers' market power, making it easier to suppress wages, which is why the general application of such clauses does not seem advisable from a welfare perspective.

An alternative way of addressing the market failure in training could be to impose a training levy on firms. A levy is a financial contribution to employer-provided training paid by all firms in the market. Ideally, this would be designed to increase incentives to invest in those training activities where the poaching externality is most likely to exist. That means training in general skills whose value is identifiable and quantifiable for outside employers. Firms that train disproportionately few of their own employees and instead recruit from other firms would then share the training costs through the levy - removing the incentive to poach. While the transferability of skills may be at least qualitatively ascertainable, the value of training in those skills to another company may be difficult to measure. For this reason, the literature emphasizes the need to involve employers in the design process of the levy so that it is based on the real needs of firms and is actually taken up by them (Müller and Behringer, 2012). In practice, such mandatory levies are widespread, and, if designed appropriately, are a suitable means of increasing the overall level of employer-provided training, according to Müller and Behringer (2012). Depending on the design of the levy, however, it makes hiring employees more expensive. This could prove to be an additional obstacle for small and new firms entering the market in competition with large and established firms that can pay higher wages (Greenhalgh, 2002). Thus, whether or not such a levy would actually lead to a leveling of training investments and wages between firms or, conversely, lead to a further widening of the observable gap in wages, would have to be carefully weighed up in advance and thoroughly evaluated in retrospect.

Another possible instrument to increase incentives to invest in training is repayment obligations for employer-financed training measures. These offer firms a hedge against the risk of losing an employee before the investment in their training has paid off. For example, it would be contractually agreed on how long a person must be employed in a company after completing the training. In the case of an early termination by the employee, the company can reclaim the costs of the training. This makes poaching less attractive for competing firms, as they would have to offer a higher wage to compensate potential employees for the repayment. Since the implementation of such obligations is associated with administrative costs, repayment clauses are most suitable for particularly expensive and longer measures, and less so for training measures of a few days, which make up the majority of training activities. Fixed administrative costs of setting up and negotiating repayment contracts also mean that those firms that have a particular interest in repayment clauses according to the results reported above are less likely to use them. These are mainly low-paying and thus typically smaller firms that usually have fewer resources for the implementation. In fact, such clauses are mainly used by larger firms (Cedefop, 2012). In summary, while repayment clauses may be a suitable instrument in theory, in practice they are unlikely to reliably mitigate the inequality in training behavior between low and high-paying firms if there is no additional support for smaller firms in the implementation process.

Government intervention beyond the instruments considered so far, for example in the form of subsidies, can be justified if one expects additional positive external effects of providing general training for society, which are not internalized by the firms. For example, Acemoglu (1997) argues that a high level of skill has an overall positive effect on the innovation activity in a country, and the more firms innovate in the market, the more worthwhile training becomes. In political practice, government funding of employer-provided training is often motivated by equity considerations and aims to support people who are disadvantaged in the labor market and less so by expected positive external effects (Brunello and Wruuck, 2020). There may also be other economic motives for subsidizing employer-provided training, but these are independent of the incentive dynamics between heterogeneous firms presented here. For example, it may be worthwhile to invest in a qualification of a worker in order to avoid greater burdens on social security systems if it is likely that the worker would become unemployed without further training. Accordingly, public subsidies for employer-provided training in Germany are primarily aimed at low-skilled workers and workers in sectors that are particularly affected by technological or structural change. This alone would not necessarily reduce the inequality in workers' career prospects as it might be that both lower- and higher-paying are operating in these sectors. However, the requirements for receiving the subsidies are less stringent for employees in small firms and the cost-sharing obligation of firms is staggered according to firm size. In light of the results shown here, such a staggered cost-sharing rule is to be supported, as it is the smaller, low-wage firms that provide too little training. However, only 2 percent of firms made use of government grants at all in 2015 (BIBB, 2022). This suggests that the existing instruments are not effective in addressing the inequality in employer-provided training between firms with different levels of pay. The reasons for the low take-up rate are an interesting topic for further research that would give further insights on how one can effectively enable lower-paying, i.e., small firms, to invest more in training.

In summary, there is not one instrument that stands out, but the advantages and disadvantages have to be considered. When designing an instrument, policymakers should pay special attention to the needs of small firms. Small firms are likely to be firms located at the lower end of the wage distribution and therefore have, according to my results, fewer incentives to invest in the human capital of their workers.

4.7 Discussion and Conclusion

Using high-quality data for the German labor market, I analyzed how firms' training activities differ depending on their position in the wage distribution, and how this interacts with individual workers' training participation and wages. I used the strategic wage posting models by Manning (2003) and Fu (2011) to derive empirically testable hypotheses. To measure a firm's position in the wage distribution, I use the firm wage fixed effects from an AKM model estimated by Card, Heining and Kline (2013).

Consistent with the theoretical predictions, I find that higher-paying firms provide more on-the-job training. They engage in training more often and train a larger share of their workforce. The positive correlations are highly statistically significant and economically meaningful. Investigation of the underlying mechanisms behind this correlation confirms the relevance of the channels proposed by the theoretical models: higher-paying firms have a lower separation rate and are larger, and train more for this reason.

The findings are corroborated by the analysis of individual workers' outcomes. Workers employed at higher-paying firms are significantly more likely to receive employer-financed training and the training measures at those firms are on average longer. With regards to wages, I find that past training is positively associated with wages, which is in line with the theoretical expectations, but the correlation is not always statistically significant.

My findings shed light on the relative incentives of firms operating in an imperfectly competitive labor market to invest in the human capital of their workers. In line with theoretical models on on-the-job training in frictional labor markets, a firm's position in the wage distribution matters for firms' training decisions. That implies that ex-ante equally skilled workers matched with firms at different points in the wage distribution will experience different levels of skill development.

By looking at the interaction between firm-specific wage components and on-the-job training through the lens of the strategic wage posting model, I focus on one of the possible interpretations of firm-pay heterogeneity. A common alternative interpretation is that differences in firm-specific pay premiums stem from productivity differences between firms that share part of their rents with workers (Card et al., 2018; B. Hirsch and Mueller, 2020). While the strategic wage posting model presented in Section 4.3.1 assumes firms to be identical, it could be enhanced to include productivity differences between firms and would be able to generate the positive correlation between profits per worker and wages observed in empirical data (Manning, 2003, Chapter 8.1). From the perspective of this interpretation, my results would have the same implication, namely that it is beneficial for a worker to be matched with a higher-paying, more productive firm as one earns a higher wage relative to other firms and receives the benefits of training.

A competing interpretation of firm pay heterogeneity measured by the AKM framework is voiced by Sorkin (2018). He posits that firm-specific wage effects in an imperfectly competitive labor market with job posting by firms could represent either rents or compensating wage differentials. In his model, firms post a job offer that consists of a wage and nonpay characteristics. These nonpay characteristics can be desirable or non-desirable from the worker's perspective. Using worker transitions between firms as revealed preferences for the desirability of a firm, he examines whether high-paying firms also offer more desirable nonpay characteristics or whether a high wage actually compensates for something workers dislike. According to his results, for more than half of the firms in the US labor market, firm pay components reflect compensating differentials, while for the rest it represents a rent. In the context of this chapter, employer-provided training can be regarded as a nonpay characteristic that is valued by workers as it increases the chances to transition to a better-paying employer, i.e., being trained generates utility that is not reflected in the wage paid by the current firm. With my analysis, I can thus check whether the firm-specific wage components represent compensating wage differentials with respect to this specific nonpay characteristic. As I find a positive correlation between firm-level pay and training, the results suggest that this is not the case in the German labor market: firms that pay less do not train their workers more. Of course, this might happen in some cases, but, not to the extent that it affects the average correlation estimated here. Thus, the results are in line with a strategic wage setting or rent-sharing interpretation of firm pay heterogeneity which implies that workers matched with a lower-paying firm incur the additional burden of receiving less on-the-job training.

Even though direct monetary returns from on-the-job training seem small in Germany, skills acquired through training still matter for workers' careers as they significantly increase employment prospects. As many governments have established subsidy programs to encourage firm-sponsored training in order to maintain the skills of the workforce, my results suggest that these should be designed to take into account the different incentives of firms to invest, in order to avoid a divergence in skills development between workers employed at lower- and higher-paying firms.

Appendix

A Appendix to Chapter 2

A.1 Sectoral Analysis

From the literature, it is well known that sectors are differently affected by cyclical developments (see for example Burda and Hunt, 2011). By accounting for industry employment shifts in Section 4.1 of our paper, we do not cover sectoral differences in cyclical dynamics. In the following, we conduct the same analysis within industries. That is, we regress the regional involuntary part-time share within a specific sector on regional characteristics, including the regional unemployment rate and regional GDP growth. Table A.1 shows the results. Demographic shares and state as well as year fixed effects are included in all specifications but not shown.

In the majority of sectors, we find an effect of unemployment which is even larger than on the aggregate level. The positive GDP growth effect is only significant in three sectors: (1) *Manufacturing*, (3) *Construction*, and (6) *Transportation, Storage and Communication.* These are industries in which GDP growth has been rather volatile. Especially for (3) and (6), the effect is larger than the aggregate GDP growth effect (Table 2 of our paper). Within those two industries, we do not find a significant unemployment effect. In (10) *Education*, the effect of GDP growth is negative. This is consistent with cyclical volatility being rather low in this sector.

There are different potential explanations for the positive effect of GDP growth on IPT in particular sectors. One hypothesis is that part-time labor is preferably hired during booms. This gives employers more flexibility in adjusting working hours as they cannot reduce employees' hours but can have them work overtime when necessary. Moreover, employees who only find IPT jobs as the GDP growth is high, might be the first to lose their jobs when the economy turns down. This has been investigated as the "last hired, first fired" phenomenon in a large body of literature.[32] Other reasons for high IPT in economic upturns could lie in expansions of labor supply. If individuals who were previously satisfied with part-time jobs suddenly offer full-time hours, this will likely result in additional IPT. Such adjustments in desired working hours seem particularly likely when an upswing is accompanied by wage increases.

In summary, the positive effects of unemployment and GDP growth apply to distinct sectors. In most sectors, variation in IPT can rather be attributed to changes in unemployment. The effect of GDP only plays a role in a few sectors and remains rather negligible in scope. In our paper, we therefore focus on unemployment as the cyclical indicator of interest.

[32] For Germany, e.g., Kogan (2004) investigates the labor market spells of immigrants against this background.

Table A.1: Involuntary Part-time Within Industries, Regression Results

Share IPVT by NACE (Rev. 1.1)	Manufacturing	Electricity, Gas	Construction	Wholesale, Retail	Hotels, Restaurants	Transportation	Financial Interm.	Business Activities	Public Admin.	Education	Health, Social Work	Other
Unemployment Rate	0.164**	-0.299**	0.128	0.429**	0.769	0.0534	0.474***	0.277	0.553***	0.602**	0.0382	0.728*
	(0.0771)	(0.136)	(0.143)	(0.196)	(0.571)	(0.220)	(0.104)	(0.199)	(0.0957)	(0.241)	(0.204)	(0.388)
Unemployment Rate Sq.	-0.613***	0.917**	-0.0336	-0.687	-3.702***	-0.701	-1.108***	-0.428	-1.179***	-0.939	-0.0838	-1.680**
	(0.231)	(0.389)	(0.405)	(0.507)	(1.196)	(0.626)	(0.246)	(0.498)	(0.262)	(0.595)	(0.585)	(0.690)
GDP Growth	0.0589***	0.0569	0.153***	0.0658	0.0813	0.157*	0.00975	0.109	0.0438	-0.148*	0.00445	-0.117
	(0.0225)	(0.0551)	(0.0375)	(0.0607)	(0.109)	(0.0818)	(0.0516)	(0.0731)	(0.0566)	(0.0846)	(0.0873)	(0.155)
N = 256												

Notes: Table shows fractional regressions of Equation 1 with the federal-state-level x industry share of involuntary part-time as dependent variable. All specifications include year and state fixed effects and controls for 10 gender x age demographic group shares. Observations are weighted by the state's employment. Standard errors in parentheses clustered at federal states, $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data sources are the European Labour Force Survey and Eurostat. Own calculations.

A.2 Different Cyclical Indicators

In this subsection, we separately consider different specifications of the model in order to gain some additional insights and to evaluate the robustness of our results. Specifically, we use different indicators to describe the state of the labor market and estimate different variants in each case. In summary, this exercise provides some interesting findings but does not produce any conflicting results.

Table A.2 shows the results. Again, state and year fixed effects are included in all specifications. Apart from that, we first only include the unemployment rate (column 1). Column (2) repeats the first specification of the analysis in Section 4.1 of our paper. Comparing the first two columns shows that adding squared unemployment improves the model fit with regard to R^2. As mentioned before, we thereby account for non-linear effects of unemployment. Controlling for demographic group and industry shares in column (3) does not change the estimated effects much but again increases the overall model fit substantially. Additionally adding GDP growth (column 4) also does not bring about any relevant changes. These findings could be expected given the comparison between the different specifications in Table 2 in Section 4.1 of our paper.

We consider the lagged unemployment rate as a potential alternative explanatory variable in specifications (5)-(8). It is conceivable that certain effects of high unemployment will only become apparent in the following year. We find a significant positive effect of lagged unemployment once we account for its non-linearity in column (6). The negative effect of the squared term suggests a decreasing effect of lagged unemployment on the incidence of IPT. Again, not much changes when controlling for demographic group and industry shares (column 7) and GDP growth (column 8). The positive effect of lagged unemployment suggests that unemployment is not only relevant for the incidence of IPT as an indicator of current business conditions but that it also determines labor market outcomes in terms of IPT in subsequent periods (for various possible effects of unemployment on the incidence of IPT see Section 4.2 of our paper).

In the literature, the employment-to-population ratio has been highlighted as an interesting alternative measure to the unemployment rate, because when the US economy recovered from the Great Recession, the decline in unemployment did not go along with a correspondingly large increase in the employment-to-population ratio (see for example Bitler and Hoynes, 2016). As emphasized above, the German unemployment rate barely responded to the crisis. This is similarly true for the German employment-to-population ratio. We nevertheless consider this ratio as an alternative explanatory variable in specifications (9)-(12). It does not have any significant effect.

Overall, considering different cyclical indicators provides some interesting insights. In particular, note that the connection of none of the indicators considered here to IPT depends on GDP growth which supports our focus on unemployment as the main cyclical indicator.

Table A.2: Different Cyclical Indicators, Regression Results

Share IPT	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Unemployment Rate	0.0645**	0.273***	0.254***	0.254***								
	(0.0318)	(0.0659)	(0.0594)	(0.0639)								
Unemployment Rate Sq.		-0.592***	-0.546***	-0.545***								
		(0.201)	(0.154)	(0.162)								
Lag. Unemployment Rate					0.0361	0.320***	0.336***	0.329***				
					(0.0334)	(0.0613)	(0.0660)	(0.0686)				
Lag. Unemployment Rate Sq.						-0.785***	-0.797***	-0.783***				
						(0.166)	(0.182)	(0.187)				
Empl.-to-Pop. Ratio									-0.0450	0.228	0.615	0.661
									(0.0761)	(0.631)	(0.477)	(0.476)
Empl.-to-Pop. Ratio Sq.										-0.338	-0.855	-0.906
										(0.776)	(0.594)	(0.597)
Demographic Group Shares			Yes	Yes			Yes	Yes			Yes	Yes
Industry Shares			Yes	Yes			Yes	Yes			Yes	Yes
GDP Growth				Yes				Yes				Yes
R^2 within	0.70	0.82	0.94	0.94	0.72	0.84	0.94	0.94	0.21	0.22	0.89	0.90
N= 256												

Notes: Table shows fractional regressions of Equation 1 with federal state level involuntary part-time share as dependent variable. All specifications include year and state fixed effects. Observations are weighted by the state's employment. Standard errors in parentheses clustered at federal states, $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data sources are the European Labour Force Survey, Eurostat, the Federal Employment Agency and the Socio-Economic Panel. Own calculations.

A.3 Heterogeneity Analysis

In this subsection, we analyze whether the effect of unemployment on the incidence of involuntary part-time work varies in relevant macro dimensions: between Eastern and Western Germany, before and after the Great Recession or after the Hartz reforms. We find that the marginal unemployment effect is larger in Western Germany and that it has been larger since the Great Recession. Contrary to common belief, the Hartz reforms apparently did not increase the effect of unemployment on IPT.

In Table A.3, we report the results from different exercises that we further describe in the following. In each specification, we include year and state fixed effects and control for demographic group and industry shares as well as GDP growth. The first column of Table A.3 shows the main results from Section 4.1 (specification (3) of Table 2) for a convenient comparison.

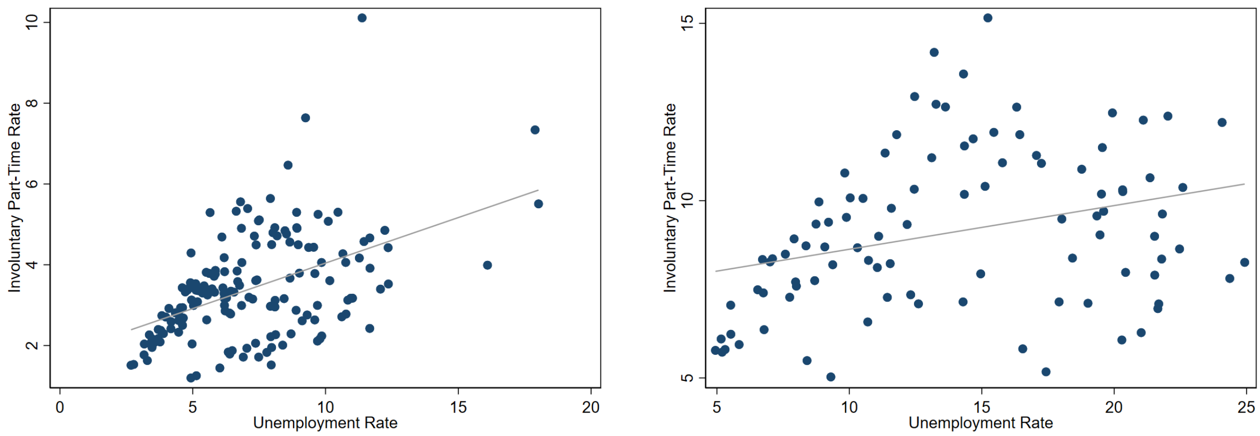
First, we investigate whether the association between IPT and unemployment differs between Eastern and Western Germany. This is motivated by the observation that there are substantial differences in the incidence of (involuntary) part-time work between the two regions. The share of IPT is higher in Eastern Federal States, although the share of part-time work is generally lower. These differences have often been highlighted in the literature and are mainly attributed to different working time preferences of women: Not only is the labor market participation rate of women in Eastern Germany higher than in Western Germany, women in the East are also more likely to work full-time and are less likely to be content with part-time hours than women in the West (see for example Wanger, 2011).

To inspect whether there are regional differences regarding the relationship between cyclical indicators and the incidence of IPT we present some descriptive evidence in Figure A.1. It shows the correlation between IPT and unemployment separately for Eastern and Western German states. For both regions, there is a positive correlation between unemployment and IPT, with the slope in Western Germany being steeper than in the East.

Column (2) of Table A.3 confirms that there is a stronger relationship between unemployment and IPT in Western Germany. In this specification, we interact unemployment with the distinction between Eastern and Western states. Calculated at the respective sample means, the marginal effect of unemployment on the incidence of IPT is about twice as large in Western Germany than it is in Eastern Germany. This suggests that the relevant labor market mechanisms discussed in Section 4.2 affect the Western labor market more strongly. The result furthermore reflects the finding that the marginal effect of unemployment is generally decreasing: As the level of the unemployment rate is usually higher in Eastern Germany (see Figure A.1), differences in the unemployment rates are less relevant for the incidence of IPT. In other words, it seems that in view of Eastern German employees' poorer situation, it does not matter as much how bad the situation actually is.

Figure A.1: Correlation Between Involuntary Part-Time Employment and Unemployment in Eastern and Western German Federal States

(a) West **(b)** East

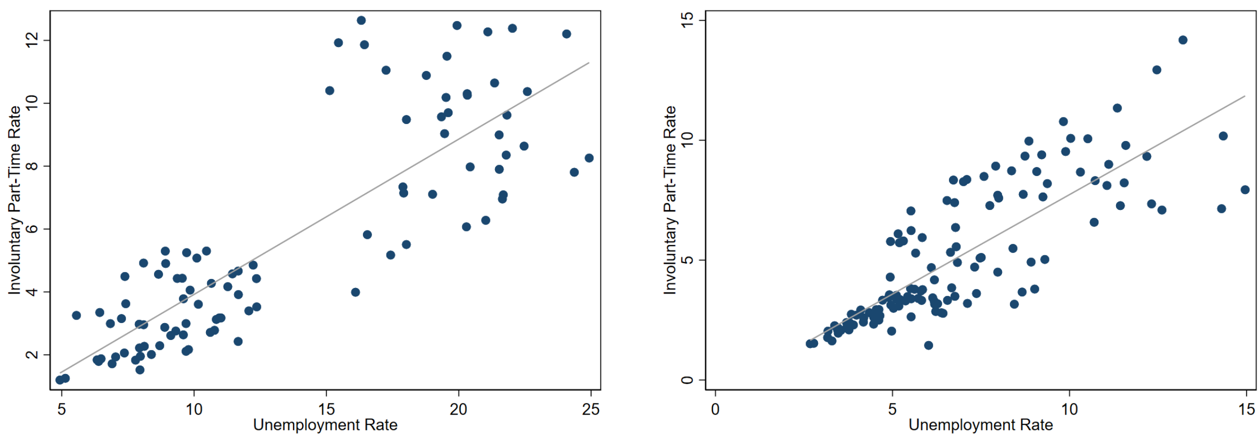


Notes: This figure shows the correlation within German federal states between the involuntary part-time rate and the unemployment rate in West Germany (a) and East Germany (b) for the sample period 2002 to 2017. Data sources are the European Labour Force Survey and Eurostat. Own calculations using sampling weights of the Labour Force Survey.

We now turn to potential differences between the periods before and after the Great Recession. This distinction seems particularly relevant with regard to the literature: For the US labor market, Valletta et al. (2020) show that while the level of IPT used to behave strongly countercyclically until the Great Recession, its recent development can be increasingly explained by structural factors. Here, we investigate whether there are differences in the cyclical dynamics of IPT before and after the Great Recession in Germany as well.

Figure A.2: Correlation Between Involuntary Part-Time Employment and Unemployment Before and After the Great Recession

(a) Before (2002-2007) **(b)** After (2010-2017)



Notes: This figure shows the correlation within German federal states between the involuntary part-time rate and the unemployment rate for the period 2002-2007 (left) and the period 2010-2017 (right). Data sources are the European Labour Force Survey and Eurostat. Own calculations using sampling weights of the Labour Force Survey.

Similar to Figure A.1, Figure A.2 shows the correlation between IPT and unemployment separately for the periods before (2002-2007) and after (2010-2017) the Great Recession. The correlation appears to be slightly stronger after the Great Recession. Column (3) of Table A.3 confirms that the impact of unemployment on the incidence of IPT has actually been much stronger in recent years. In this specification, we interact the cyclical indicators with the distinction between the periods before and after the Great Recession. The marginal effect of the latter is more than twice as large (0.19 compared to 0.07 before the recession). This could hint at a regime change in employers' hiring behavior after the recession. With the experience of the crisis, the same unemployment rate now leads to a higher rate of IPT, implying either a shift in the bargaining positions or in workers' preferences regarding full-time hours.

Table A.3: Heterogeneity, Additional Regression Results

	(1)	(2)	(3)	(4)
Share IPT	FS from Section 2.4.1	Eastern/Western Ger.	Before/After GR	After Hartz Reforms
Unemployment Rate	0.253***			0.186**
	(0.0695)			(0.0779)
Unemployment Rate Sq.	-0.550***			-0.433**
	(0.167)			(0.206)
GDP Growth	0.0693**			0.0895**
	(0.0314)			(0.0402)
Unemployment Rate West		0.578***		
		(0.124)		
Unemployment Rate Sq. West		-2.117***		
		(0.643)		
Unemployment Rate East		0.276***		
		(0.0759)		
Unemployment Rate Sq. East		-0.506**		
		(0.210)		
Unemployment Rate Before			0.167***	
			(0.0324)	
Unemployment Rate Sq. Before			-0.444***	
			(0.110)	
Unemployment Rate After			0.466***	
			(0.114)	
Unemployment Rate Sq. After			-2.231***	
			(0.622)	
N	256	256	256	208

Notes: This table shows fractional regressions of Equation 1 with the federal state level involuntary part-time share as dependent variable. All specifications include year and state fixed effects, controls for 10 gender x age demographic group shares, controls for 12 NACE Rev 1.1 industry shares as well as GDP growth. Observations are weighted by the state's employment. Standard errors in parentheses clustered at federal states, $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data sources are the European Labour Force Survey and Eurostat. Own calculations.

We furthermore examine whether our findings regarding the relationship between IPT and unemployment depend on the Hartz reforms. These major reforms had various implications for the German labor market. As briefly outlined in Section 5 of our paper, the Hartz reforms expanded, among other things, the opportunities for marginal employment ("minijobs"). However, the incidence of minijobs has no significant impact on IPT, as shown in the same section. Another relevant change was the reduction in unemployment benefits generosity: the payment period of income-dependent benefits was shortened, making unemployment a much worse option for employees since then. Theoretically, this suggests that unemployment has had a stronger influence on the incidence of IPT since the implementation of the Hartz Reforms.

We do not show separate scatter plots and do not use interaction terms to investigate this dimension, because there are too few observations before the Hartz reforms in our sample for them to be analyzed separately. Instead, we consider a separate sample that contains only observations after the Hartz reforms, that is from 2005-2017. The results are shown in column (4) of Table A.3. In comparison to column (1), there are no qualitative differences. Contrary to the theoretical prediction, the effect is smaller when restricting the analysis to the period after the Hartz reforms.[33].

[33]This result is consistent with other findings in the literature which show that the Hartz reforms did not have the expected clear-cut effect on atypical employment, including part-time, but rather reinforced the positive trend in a short transitory period right after the reforms, see for example Walwei et al., 2017

A.4 Individual-Level Analysis

In this subsection, we make use of the individual-level dimension of our data. Similarly to Valletta et al. (2018), we first check whether our finding is robust to the inclusion of individual worker characteristics. Second, we consider the individual probability of being inactive in the labor market as dependent variable which is relevant for understanding the mechanisms underlying unemployment and IPT as we explain in Section 4.2.2 of our paper. Moreover, we investigate potential heterogeneity in the correlation of unemployment and involuntary part-time across groups of workers.

For the first step, we conduct logit regressions with IPT status as the dependent variable. It takes the value one if a person is in IPT and zero otherwise. Thus, we consider all employed, unemployed, and inactive individuals in this analysis. As before, the interest lies in the variables capturing macroeconomic cyclical and structural factors. In addition to these, we also include a number of variables that control for individual-level demographic characteristics: interactions of age (five groups) with sex and marital status and four educational levels based on ISCED. The full model is described by the following equation,

$$Pr(Empl_{it} = IPT) \quad = \quad \alpha + \beta u_{s(i,t)} + \gamma u^2_{s(i,t)} + \delta' X_{it} + \omega' Z_{s(i,t)} + \varphi_s + \phi_t + \epsilon_{it} \tag{18}$$

with X_{it} being the vector of individual-level variables that we now control for and $Z_{s(i,t)}$ being the state-level structural factors. The first four columns of Table A.4 show the results. The table displays average marginal effects. For better readability, the coefficients for the individual-level controls are not shown as we are interested in the macroeconomic determinants. The results reinforce our findings based on the state panel regression. The unemployment rate is significantly positively related to the individual probability of working in IPT. The quadratic term is negative and significant, suggesting a non-linear relationship between the unemployment rate and the propensity to be involuntary part-time employed. As before, the structural indicators capturing long-term changes in the labor market are in general not significantly correlated, with the few exceptions of the *Electricity, Gas and Water Supply* and *Other Services* sectors and some demographic groups, for example, the share of women aged 17-26.

We then repeat the same analysis with inactive status as the dependent variable. This is motivated by an observation that we highlight in Section 4.2.2 of our paper: We do not find a relationship between IPT and labor force participation, suggesting that there is no added worker effect underlying the relationship between unemployment and IPT. This argument is corroborated by the results presented in columns (5) to (8): the likelihood to become inactive is not significantly correlated with the regional unemployment rate. Together these results support the argumentation against the presence of an added worker effect, at least against one that is not offset by a discouraged worker effect.

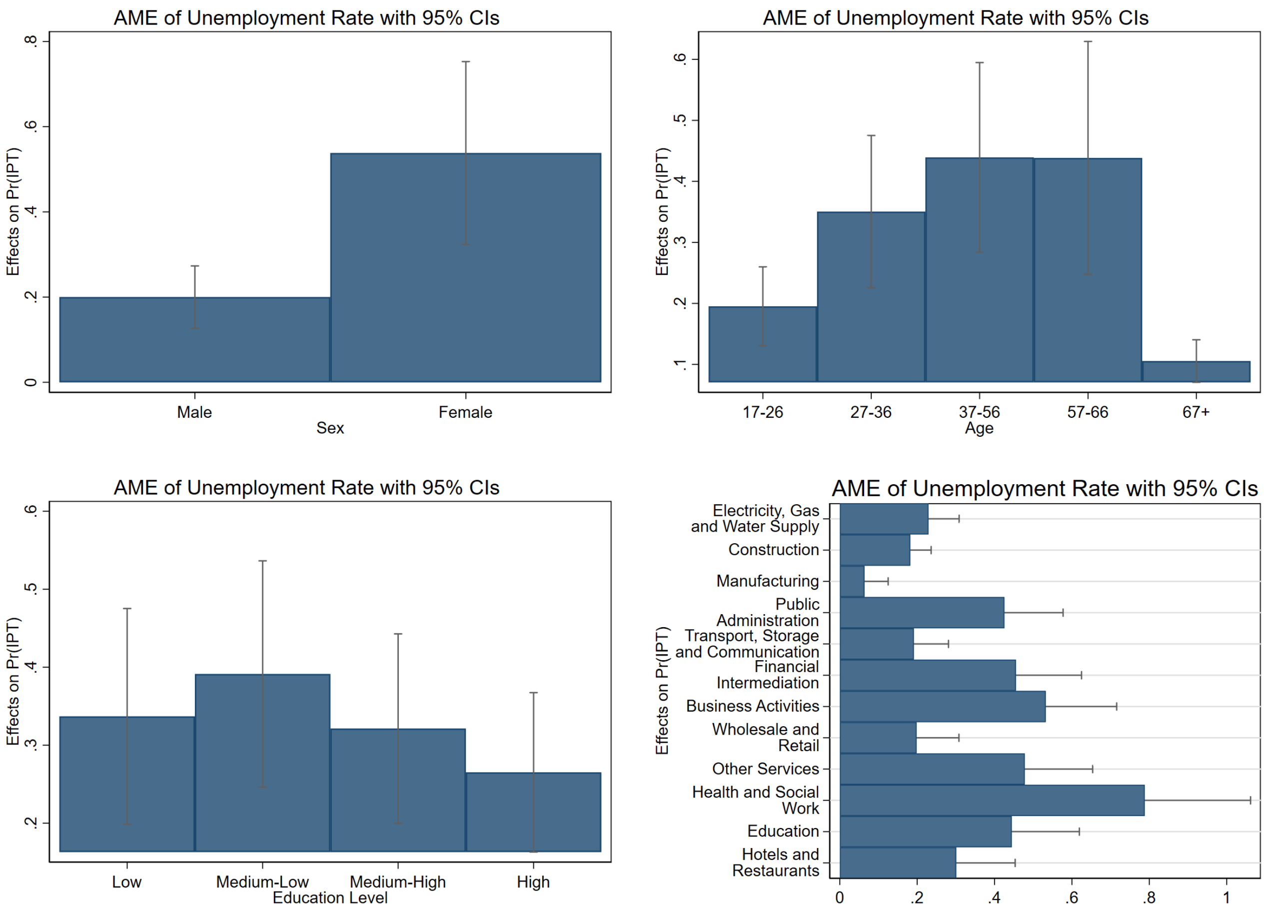
Table A.4: Logit Regressions with IPT or Inactive Status as Dependent Variable

Dependent variable	IPT Status				Inactive Status			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unemployment Rate	0.183***	0.195***	0.173***	0.172***	-0.124	0.0253	0.0228	0.0285
	(0.0424)	(0.0456)	(0.0365)	(0.0360)	(0.136)	(0.109)	(0.0885)	(0.0784)
Unemployment Rate Squared	-0.445***	-0.464***	-0.417***	-0.415***	-0.499	-0.472	-0.355	-0.363*
	(0.128)	(0.138)	(0.0947)	(0.0931)	(0.372)	(0.301)	(0.236)	(0.213)
Manufacturing			-0.0176	-0.0184			-0.159***	-0.159***
			(0.0251)	(0.0245)			(0.0451)	(0.0421)
Electricity, Gas and Water Supply			0.132*	0.136*			0.120	0.184
			(0.0715)	(0.0731)			(0.135)	(0.135)
Construction			0.0126	0.0115			-0.158*	-0.160*
			(0.0263)	(0.0263)			(0.0948)	(0.0961)
Wholesale and Retail Trade			0.0680**	0.0669**			-0.109	-0.111*
			(0.0305)	(0.0299)			(0.0695)	(0.0634)
Hotels and Restaurants			-0.00612	-0.00780			0.0230	0.00942
			(0.0363)	(0.0355)			(0.0757)	(0.0751)
Financial Intermediation			0.0204	0.0199			-0.174	-0.174
			(0.0362)	(0.0361)			(0.115)	(0.106)
Business Activities			0.0390	0.0376			-0.0679	-0.0682
			(0.0343)	(0.0334)			(0.0614)	(0.0564)
Public Administration and Defence			-0.0205	-0.0202			-0.106*	-0.0845
			(0.0211)	(0.0212)			(0.0602)	(0.0578)
Education			0.00243	0.00225			0.143*	0.158**
			(0.0280)	(0.0280)			(0.0748)	(0.0659)
Health and Social Work			-0.000497	-0.000626			-0.125***	-0.111***
			(0.0240)	(0.0239)			(0.0459)	(0.0425)
Other Services			-0.0225	-0.0247			-0.0716	-0.0836
			(0.0234)	(0.0227)			(0.0622)	(0.0661)
Women 17-26			0.242***	0.242***			-1.242***	-1.233***
			(0.0628)	(0.0626)			(0.187)	(0.188)
Women 27-36			0.119**	0.121**			-0.858***	-0.832***
			(0.0553)	(0.0576)			(0.112)	(0.0964)
Women 37-56			-0.0255	-0.0264			-0.345**	-0.368***
			(0.0557)	(0.0555)			(0.137)	(0.128)
Women 57-66			0.0213	0.0246			-0.0156	0.0309
			(0.0763)	(0.0760)			(0.202)	(0.181)
Women 67+			0.115	0.115			-1.221**	-1.229**
			(0.275)	(0.279)			(0.601)	(0.586)
Men 27-36			-0.0506	-0.0534			-0.0579	-0.0743
			(0.0603)	(0.0600)			(0.159)	(0.153)
Men 37-56			0.0431	0.0431			0.0441	0.0562
			(0.0500)	(0.0500)			(0.213)	(0.213)
Men 57-66			0.0683	0.0676			-0.347**	-0.366**
			(0.0756)	(0.0750)			(0.165)	(0.159)
Men 67+			0.326*	0.334*			-1.886***	-1.805***
			(0.181)	(0.184)			(0.694)	(0.671)
GDP Growth				0.00561				0.0631***
				(0.00720)				(0.0216)
Demographic group dummies	No	Yes	Yes	Yes		Yes	Yes	Yes
Education dummies	No	Yes	Yes	Yes		Yes	Yes	Yes
N	4022632	4022632	4022632	4022632	4020276	4020276	4020276	4020276

Notes: Table shows logit regressions of Equation 18 with either IPT status or inactive status of workers as dependent variable. All specifications include year and state fixed effects. Demographic group dummies are interactions of age (5 groups) × sex × marital status. Education dummies are 4 categories based on ISCED 97. Standard errors in parentheses clustered at federal states, $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data sources are the European Labour Force Survey and Eurostat. Own calculations.

For the second step, we again compute logit regressions as described above, but now we interact the unemployment rate with categorical variables of interest to see whether the relationship between macroeconomic conditions and IPT differs depending on sex, age, education or industry. Figure A.3 plots the average marginal effects of the unemployment rate on IPT status for groups of workers in different categories. From the top-left panel, it becomes clear that the probability to work in IPT when unemployment is high is much larger for women than for men. This is in line with the interpretation that it is mainly women who expand their labor supply on the intensive margin during economic downswings, which we offer in Section 4.2.3 of our paper. Interestingly, the marginal effects for different age and education groups are not statistically different from each other. With regards to industries, the figure in the bottom-right panel shows that there is a large variation in the connection between unemployment and the individual propensity to work in IPT. It is much higher in those industries which generally have a high share of part-time work.

Figure A.3: Effects of Unemployment on Probability to Work IPT for Different Individual-Level Dimensions



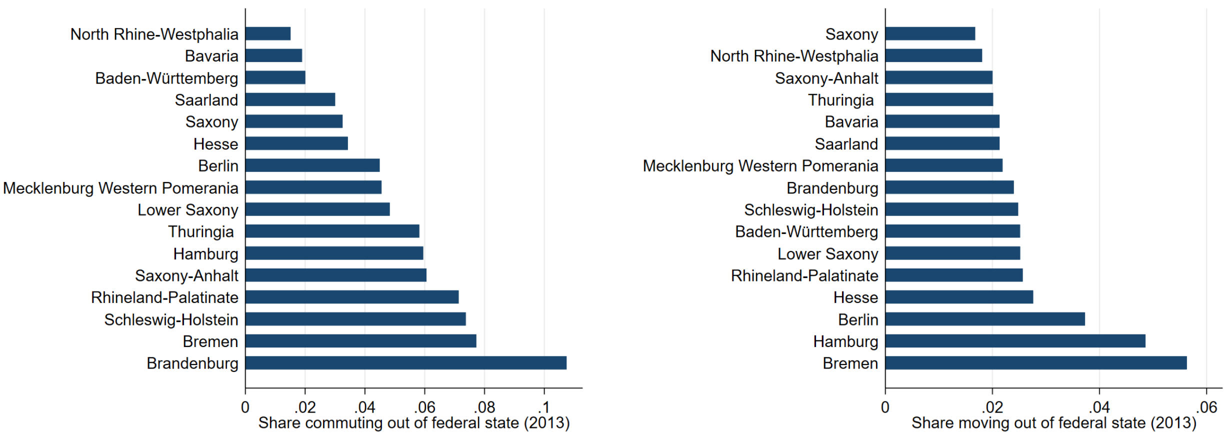
Notes: Graphs show the coefficients of logit regressions of Equation 18 with IPT status as dependent variable for different individual-level dimensions. Bars indicate the magnitude of the coefficient of the unemployment rate at the federal-state level, and vertical/horizontal lines indicate 95% confidence intervals. Specifications include year and state fixed effects, dummies for age group, gender, education according to ISCED 97, marital status, occupation according to ISCO-88 and industry according to NACE Rev 1.1. if not dimension of interest. Data source is the European Labour Force Survey. Own calculations.

A.5 Moving and Commuting Behavior Between Federal States

In this subsection, we provide descriptive evidence that illustrates commuting and moving behavior between states according to the commuting and migration statistics of the Federal Statistical Office. Statistics refer to the year 2013. Panel (a) of Figure A.4 shows that the share of employed people commuting out of their residential state for work is below 10% in all states but one (Brandenburg). Panel (b) of Figure A.4 furthermore shows that the annual moving shares between federal states are mostly below 3% and somewhat higher for the three city states. This share can be understood as an upper bound as it includes all inter-state moves, not just those related to work. For further insights we also looked at the Socio-Economic Panel: According to that data, only 4.72% of job changes between 1985 and 2017 were associated with moving between states. Thus, overall, work-related mobility between federal states is very low in Germany, supporting our assumption that federal states are the relevant unit for our analysis as we discuss in more detail in Section 3.3 of our paper.

Figure A.4: Inter-State Mobility

(a) Commuting Share by Federal States **(b)** Moving Share by Federal States



Notes: Figure shows the share of workers commuting to a different federal state than their residential federal state for work (left) and the share of people moving out of a given residential federal state (right) by federal state. Data sources are the Commuting Statistic and Migration statistic of the Federal Statistical Office.

A.6 Data Description

In this subsection, we describe our different data sources. We primarily use yearly micro data from the European Labour Force Survey (LFS). However, additional data sources are needed for information on GDP growth and particular employment forms as well as for the calculation of transition probabilities. Table A.5 provides an overview of our data sources.

For Germany, the European Labour Force Survey (LFS) provides cross-sectional information on about 830,000 respondents per year. Our main analysis covers the time period 2002 through 2017, as information on federal states ("Bundesländer") is crucial for our analysis and only available as of 2002 (variable *region*).

The LFS provides information on respondents' employment status. We use the variable *ilostat* which is based on the respective ILO definition[34] to determine whether respondents are employed or unemployed. We exclude all self-employed from our sample, using the variable *stapro*. Most importantly, the LFS allows for the identification of part-time workers (variable *ftpt*). This part-time measure is based on self-assessment. We further restrict our definition of part-time work to those who usually do not work more than 35 hours in total using the variable *hwusual*. Variable *ftptreas* determines whether we consider employees as involuntary part-time workers. Part-time employees are only considered as IPT if *Could not find a full-time job* applies. If instead respondents state to work part-time for one of the following reasons, they are assumed to work part-time voluntarily: *Person is undergoing school education or training*, *Of own illness or disability*, *Looking after children or incapacitated adults*, *Other family or personal reasons* (from 2006) or *Other reasons*.

The LFS furthermore allows the assignment of employees to occupations and industries. For industries, we primarily use variable *na111d*, which is based on the NACE Rev 1.1 classification. As of 2009, the LFS provides information on respondents' industry only based on the newer NACE Rev. 2 classification. We use guidelines by the European Communities (2008, chapter 5) to translate this information into NACE Rev. 1.1 (on the one-digit level). We exclude respondents from our data set who are assigned to *Agriculture, hunting and forestry*, *Activities of households* or *Extra-territorial organizations and bodies*. We use variables *is883d* (2002-2010) and *isco3d* (2011-2017) to identify occupations. Again, our analysis is based on the one-digit level. In line with the restriction relating to industries, we exclude respondents who are *Skilled agricultural, forestry and fishery workers*. The LFS also provides information on relevant socio-demographic characteristics. We use variables *age* and *sex* to define demographic groups.

For information on GDP, we use additional Eurostat data. We calculate regional GDP growth

[34] *"Persons in unemployment or Unemployed population are defined as all those of working age who were not in employment, carried out activities to seek employment in a recent period (comprising the previous 4 weeks or month) and were currently available to take up employment (in the reference period or within a short subsequent period not exceeding two weeks in total)."* https://www.ilo.org/global/statistics-and-databases/statistics-overview-and-topics/WCMS_470306/lang--en/index.htm, accessed October 11th 2019.

based on yearly GDP in Euros on German federal state level.

For the calculation of transition probabilities in Section 4.2 of our paper, we use Mikrozensus data covering the time period 2012-2015. In general, this data comprises the same information as the LFS, because the German LFS data is collected from the Mikrozensus. However, unlike the LFS data, the original Mikrozensus data can be linked to a panel. This possibility exists for the years 2001-2004 and 2012-2015, but not for a longer time period.[35] We follow instructions by Herter-Eschweiler and Schimpl-Neimanns (2018) to longitudinally combine survey years . We use the same definitions for the different employment states as in the main analysis.

Additional information is required to assess the incidence of particular employment forms in Section 5 of our paper. The Federal Employment Agency provides time series quarterly data on employees' characteristics by federal states ("Beschäftigte nach ausgewählten Merkmalen"), including information on the number of marginally employed. This data source allows to distinguish between those who have a minijob in addition to a regular job and those who are exclusively marginally employed. It only provides information on the incidence of marginal employment as of 2003. For 2002, we use data that is separately available from the Federal Employment Agency. Since marginal employment has only been allowed alongside another job since the Hartz reforms in 2003, this additional source only covers individuals who are exclusively marginally employed.

For information on short-time work, we also use data from the Federal Employment agency ("Angezeigte und realisierte Kurzarbeit"). We use the annual time series data on the actual number of short-time workers by federal states (as opposed to planned numbers which are often reported). This data covers all legal bases for claiming short-time work subsidies.

For the measurement of the incidence of working-time accounts and the calculation of moving shares conditional to job changes in additional file 6, we rely on data from the Socio-economic panel (SOEP). This data set is based on an annual representative survey of about 30,000 individuals in about 14,000 private households in Germany. Respondents who work overtime are asked whether their additional working time can be recorded in a working time account. For a discussion of data sources in which working time in Germany is covered see Zapf (2012).

[35] Mikrozensus panel data is furthermore available for the time period from 1996 to 1999 which does not overlap with the time period of our main analysis.

Table A.5: Data Sources

Source	Information	Period
European Labour Force Survey	Main data source, incl. information on employment states	1997-2017
https://ec.europa.eu/eurostat/web/lfs/overview		
Release 2018, extracted October 2018, no DOI available		
Eurostat	GDP at current market prices by NUTS 2 regions	1997-2017
http://appsso.eurostat.ec.europa.eu/nui/show.do?dataset=nama_10r_2gdp		
Mikrozensus	Panel data on employment states	2001-2004 and 2012-2015
http://www.forschungsdatenzentrum.de/de/haushalte/mikrozensus		
(RDC of the Federal Statistical Office and Statistical Offices of the Länder Mikrozensus)		
2012 DOI: 10.21242/12211.2012.00.00.3.1.0, 2013 DOI: 10.21242/12211.2013.00.00.3.1.0		
2014 DOI: 10.21242/12211.2014.00.00.3.1.0, 2015 DOI: 10.21242/12211.2015.00.00.3.1.0		
Socio-Economic Panel	Incidence of working time accounts	2002-2017
https://www.diw.de/de/diw_01.c.576627.de/soep.v33.1.htmc		
DOI: 10.5684/soep.v34		
Federal Employment Agency	Incidence of marginal employment and of short-time work	2002-2017
https://statistik.arbeitsagentur.de/DE/Navigation/Statistiken		
Federal Statistical Office	Inter-state commuting and moving behavior	2013
https://www.pendleratlas.de		
https://www.statistikportal.de/de/bevoelkerung/raeumliche-bevoelkerungsbewegung/wanderungen-ueber-die-grenzen-der-bundeslaender		

Notes: The results and conclusions are ours and not those of Eurostat, the European Commission or any of the national statistical authorities whose data have been used.

B Appendix to Chapter 3

B.1 Data and Variable Definitions

The following list describes how we construct our variables from the SOEP data:

- FT: usual weekly hours worked ≥ 35
- PT: usual weekly hours worked < 35 but > 0
- IPT: PT and desired weekly hours ≥ 35
- PT: PT and desired weekly hours < 35
- Annualized labor income: $12 \times$ labor income last month $\times$ CPI˙2011/CPI_t
- Hourly earnings: labor income last month / ($4.33 \times$ usual weekly hours) $\times$ CPI˙2011/CPI_t
- Overhours: usual weekly hours minus contractual weekly hours
- Birth: Birth of a child in interview year.
- Job change: This indicates a job change (in German "Stellenwechsel") since the previous interview. It can be at the same employer or be accompanied by a firm change.
- Firm change: A change in employer since the previous interview. This information is only available as of 2004 and is provided as a subsequent question given a respondent indicates a job change since previous interview.
- Young: Below 30
- Prime Age: 30 to 55
- Old: above 55
- East Germany: Constructed from federal state the respondent is living in. Berlin is considered as East Germany.
- Education: 3 categories based on ISCED97
- Occupation: 9 categories based on ISCO88 1 digit
- Industry: 14 categories based on NACE, Rev.1.1 1 digit
- Occupational Position: constructed from SOEP variable STIB, which enables to differentiate between the positions "Manual Labourer", "Employee", "Civil Servant" and "Self-Employed"
- Labor market histories: Variables for experience in full-time, part-time and unemployment are provided by the SOEP and are constructed from the monthly calendar on employment status in the survey.

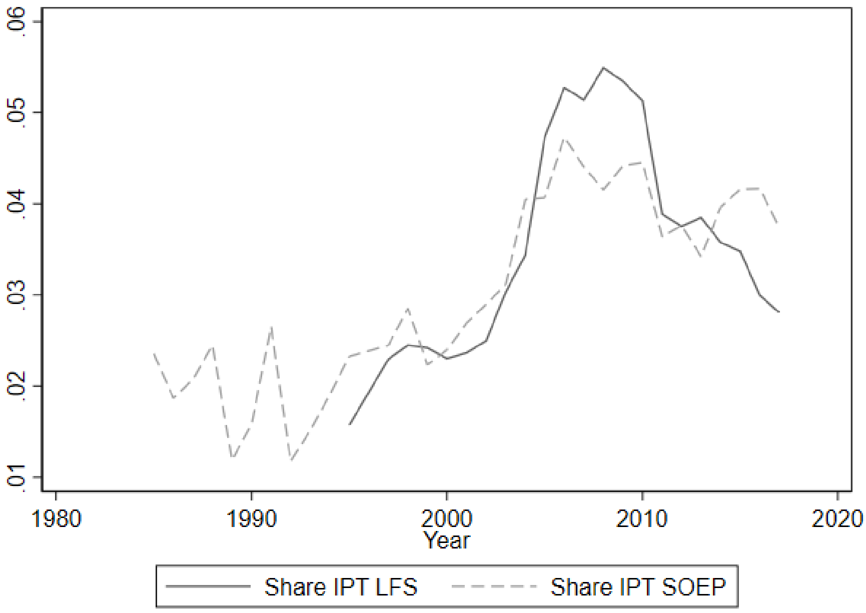
We use weekly hours worked and gender as provided in the SOEP.

Titel: Sozio-oekonomisches Panel (SOEP), Daten der Jahre 1984-2017
DOI: 10.5684/soep.v34
Link to SOEP website

B.2 Involuntary Part-Time in SOEP and Labour Force Survey

Figure B.5 shows the development of the share of involuntary part-time employed in all employed persons over time using the two different data sources available on the topic. In the EU-LFS, involuntary part-time is measured by asking respondents the reason for not working full-time. Those who indicate "could not find full-time" are considered involuntarily in part-time. In the SOEP, respondents are asked the amount of actual weekly hours worked and desired hours worked. We use these questions to construct an indicator of involuntary part-time (working less than 35 hours but wanting to work 35 hours or more). Overall, the development of the two indicators over time looks very similar. This makes us confident that we capture involuntary part-time work as intended with our self-constructed indicator.

Figure B.5: Comparison of Involuntary Part-Time Indicator in SOEP and EU-LFS



Notes: This figure shows the share of involuntary part-time workers over time, as measured by reason for not working full-time in the EU-LFS and the difference in desired and actual weekly working hours in the SOEP. Solid black line: Share IPT LFS, gray dashed line: Share IPT SOEP. Data sources are the EU-LFS, 1996-2017 and the SOEP v34.1, 1985-2017. Own calculations.

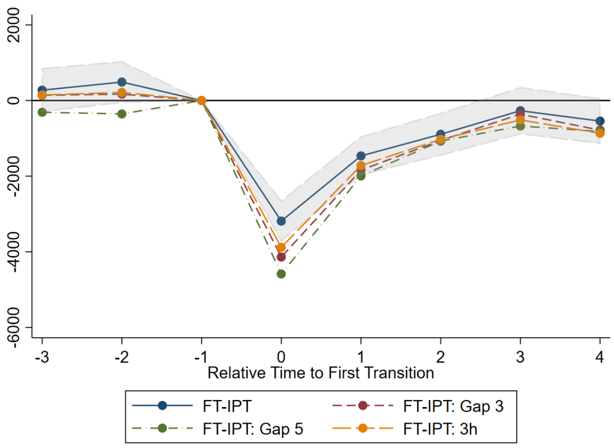
B.3 Robustness

In this section, we present some robustness checks for various aspects of the analysis. First, to make sure that our results do not hinge on our definition of involuntary part-time, we use some alternative, more strict definitions and repeat our analysis in Section B.3.1. Second, to verify whether our choice of the relative time period, i.e. the "effect window" is reasonable, we include further leads and lags to see if there are treatment effects beyond the window used in Section B.3.2 . Third, we show that the role of child birth is different for voluntary part-timers, which strengthens our approach to use involuntary transitions into part-time, in section B.3.3. Fourth, we control more flexibly for previous labor market experience of workers who transition into involuntary part-time in section B.3.4.

B.3.1 Vary IPT Definition

Our analysis crucially depends on the definition of involuntary part-time used. The indicator we used is coherent with hours conditional on self-reported part-time vs. full-time status as the 90^{th} percentile of actual weekly working hours reported by workers who consider themselves to be part-timers is 35, and conversely, the 5^{th} percentile of hours worked by self-classified full-timers is also 35. However, since we use transitions from full-time into involuntary part-time as event, it might be that we catch differences in reported actual hours at the threshold of 35 hours with the definition above. In order to check whether our results are driven by workers who report changes in hours around this threshold rather than a more sizable reduction in hours we repeat our analysis using three alternative, more strict definitions. The "3 hours" indicator requires a difference of at least 3 hours between actual and desired hours in addition to the existing indicator. "Gap 3" imposes that involuntary part-timers work at most 32 hours but want to work at least 35, and "Gap 5" that they work 32 hours but want at least 37. Figure B.6 shows the results.

Figure B.6: Alternative Treatment Definitions



Notes: This figure shows estimated coefficients μ_r in regression (2) with annualized real labor income last month (2011 €) as dependent variable. Gray areas are 95% intervals based on robust standard errors. Solid blue: transition into involuntary part-time ("Gap 3"). Dashed red: transition into involuntary part-time ("Gap 5"). Dashed-dotted green: transition into involuntary part-time ("3 Hours"). Data source is the SOEP v34.1, 1985-2017. Own calculations.

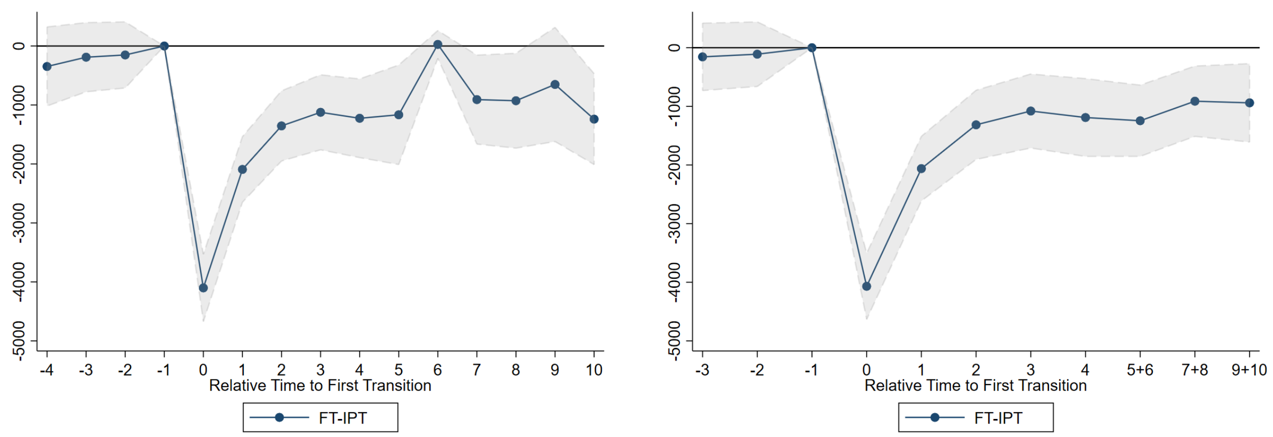
While there are some small differences in the coefficients of the different treatment specifications, the results are qualitatively unchanged. Reassuringly, there are also no significant coefficients in the pre-event time periods for the most strict treatment definition “Gap 5”.

B.3.2 The Role of the Effect Window

In an event study, the choice of the relative time period around the event is crucial for the identification of dynamic treatment effects. In a recent paper, Schmidheiny and Siegloch (2020) discuss important properties of panel-data event study designs, especially the identification assumptions one makes when choosing what they call the “effect window”, i.e. the number of relative periods before and after an event. Essentially, one assumes that the treatment effect drops to zero outside this window and the observations of the treatment group that lie outside the effect window serve as additional control observations. With an unbalanced panel as in our case, the choice of more relative periods always comes at the cost of fewer observations. We choose to look at three periods before and four periods after the event because observation numbers are relatively stable over that time period and we do not observe treatment effects outside these periods. So far, our results support this approach. In order to check whether we missed potential treatment effects in more distant leads and lags, we include more of them in a robustness check. Panel (a) of Figure B.7 expands the effect window using an additional lead and six additional lags. There seems to be no significant effect of the event four years prior to the transition, but small negative effects also in the longer period after the event. However, the confidence bands become quite large due to the smaller number of observations and the treatment effect itself is not very stable. To alleviate the problem of small observations somewhat, we bin the more distant lags in panel (b). Nor the effect is much more stable over the whole time period and becomes insignificant in periods 9 and 10. This suggests that the negative effect of a transition into involuntary part-time is slightly more persistent than assumed so far. However, with an effect size of around -500€ per year, it is rather small.

Figure B.7: Include More Leads and Lags

(a) Individual Periods **(b)** Lags Binned

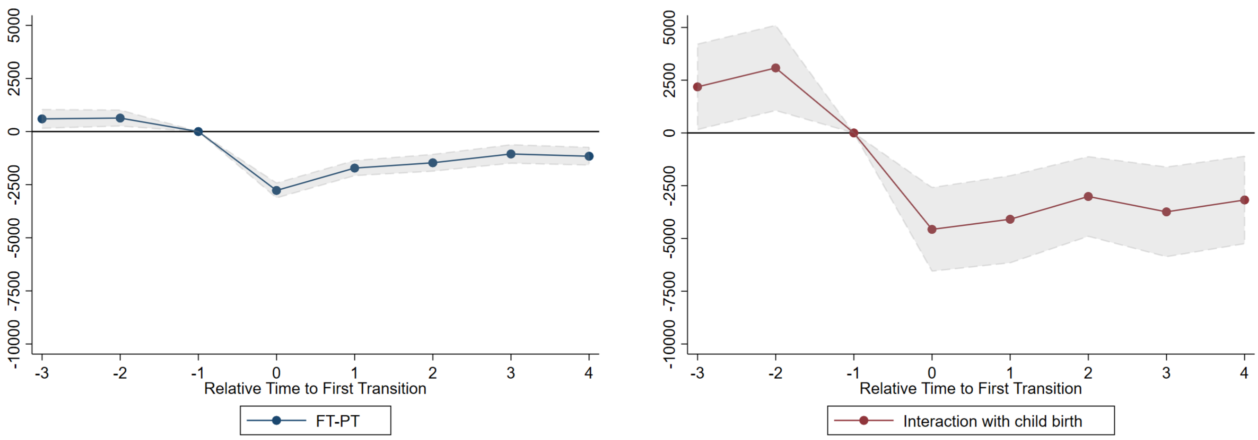


Notes: This table shows estimated coefficients μ_r in regression (2) with annualized real labor income last month (2011 €) as dependent variable. Gray areas are 95% intervals based on robust standard errors. Solid blue: transition into involuntary part-time. Data source is the SOEP v34.1, 1985-2017. Own calculations.

B.3.3 Interaction of Voluntary Part-Time and Births

Figure B.8 shows, analogously to column (1) of Table 8 how the effects of a voluntary transition into part-time differ if it coincides with the birth of a child. The large negative coefficients of the interaction terms show that birth increases the negative effects of a transition into voluntary part-time sizably, with the effect of the interaction on earnings being higher than the main effect. This finding strengthens our motivation for using transitions into involuntary part-time, for which childbirth does not play a role.

Figure B.8: Voluntary Transitions Into Part-Time With and Without Childbirths.

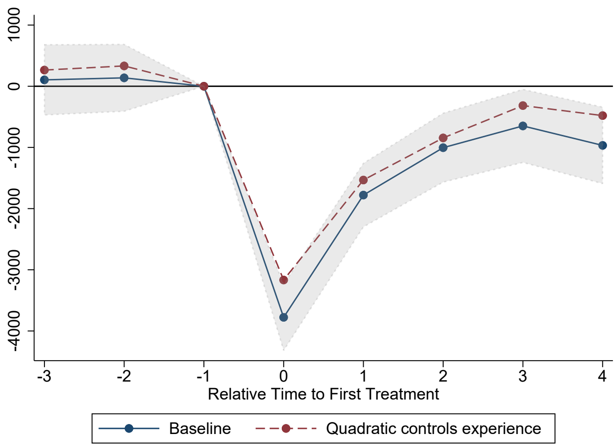


Notes: This figure shows estimated coefficients μ_r (blue solid) and ν_r (red dashed) in regression (3) with annualized real labor income and real hourly earnings (both in 2011 €) as dependent variables. Event is transition into involuntary part-time, interaction with childbirth at relative event times $r = 0$ or $r = 1$. Gray areas are 95% intervals based on robust standard errors. Data source is the SOEP v34.1, 1985-2017. Own calculations.

B.3.4 The Role of Prior Labor Market Experience

Figure B.9 shows how our baseline results would change if we included quadratic controls for prior labor market experience. This captures potential non-linear effects of labor market experience for the development of subsequent earnings. When we account more flexibly for prior labor market experience, the impact of a transition into involuntary part-time is somewhat reduced, however, the difference in estimations is not statistically different from the baseline.

Figure B.9: Quadratic Controls for Experience



Notes: Estimated coefficients μ_r in regression (2) with annualized real labor income last month (2011 €) as dependent variable. Gray areas are 95% intervals based on robust standard errors. Solid blue: transition into involuntary part-time. Dashed red: transition into involuntary part-time including quadratic controls for labor market experience. Data source is the SOEP v34.1, 1985-2017. Own calculations.

C Appendix to Chapter 4

C.1 Descriptive Sample Statistics Firm-Level Analysis

Table C.6: Descriptive Sample Statistics Firms

	No training Mean	Training Mean
Independent variable		
Firm-specific wage component	-0.05	0.03
Firm characteristics		
Share skilled workers	0.54	0.68
Share female empl.	0.39	0.45
Share part-time workers	0.40	0.38
Share aged 21-35	0.30	0.32
Share aged 36_50	0.39	0.38
Share aged 51-65	0.24	0.23
Number of employees	11	34
Separation rate (mean last 3 years)	0.25	0.20
Works/staff council	0.04	0.14
Collective agreement	0.37	0.43
Multibranch	0.09	0.21
Any investments in facilities	0.48	0.68
Sectors		
Manufacturing	0.16	0.14
Water supply & Waste Management	0.00	0.01
Construction	0.20	0.12
Wholesale & Retail	0.23	0.25
Transport	0.07	0.05
Hospitality	0.10	0.02
Information & Communication	0.02	0.03
Financial intermediation	0.00	0.01
Real estate	0.02	0.01
Prof., scientific & technical services	0.08	0.13
Administrative & support activities	0.05	0.05
Education	0.01	0.01
Health & social work	0.04	0.13
Other services	0.02	0.03
Observations	35080	81883

Notes: This table shows descriptive sample statistics for the firm-level analysis in Section 4.4. Data sources are the LIAB and CHK firm wage fixed effects from Card, Heining and Kline (2013). Weighted data using sample weights of the Establishment Panel. Own calculations.

C.2 Regression of Firm Training Participation on Firm-Specific Wage Components Using Different Lags

Table C.7: Regression of Firm Training Dummy on Firm-Specific Wage Component - Different Lags

	OLS	2SLS	
	(1)	(2)	(3)
Dep. var: Training dummy		1st lag	2nd lag
Firm-specific wage component	0.403***	0.647***	0.694***
	(0.0148)	(0.0241)	(0.0269)
Observations	82599	82599	82599
Adjusted R^2	0.136	0.124	0.119

Notes: This table shows linear probability models of Equation 13 with a dummy for training participation of the firm as dependent variable. All specifications include year dummies, federal state dummies and 16 one-digit-sector dummies (NACE 2). Data sources are the LIAB and CHK firm wage fixed effects from Card, Heining and Kline (2013). Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01. Own calculations.

C.3 Regression Analysis of Firm-Level Training Activities for Separate Intervals of Firm-Specific Wage Component Estimation

Table C.8: Regression of Firm Training Dummy on Firm-Specific Wage Component - Separate Intervals

	(1)	(2)	(1)	(2)
Dep. var: Training dummy	1997-1999	2000-2004	2005-2010	2011-2017
Firm-specific wage component	0.884***	0.707***	0.578***	0.636***
	(0.120)	(0.0324)	(0.0266)	(0.0296)
Observations	4037	18077	40900	53949
Adjusted R^2	0.119	0.117	0.125	0.135

Notes: This table shows linear probability models of Equation 13 with a dummy for training participation as the dependent variable. All specifications include year dummies, federal state dummies, 16 one-digit-sector dummies (NACE 2) as well as controls for the composition of the firm's workforce. Data sources are the LIAB and CHK firm wage fixed effects from Card, Heining and Kline (2013). Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01. Own calculations.

Table C.9: Regression of Firm Training Extent on Firm-Specific Wage Component - Separate Intervals

	(1)	(2)	(1)	(2)
Dep. var: Share trained workers	1997-1999	2000-2004	2005-2010	2011-2017
Firm-specific wage component	0.141***	0.113***	0.110***	0.213***
	(0.0508)	(0.0166)	(0.0156)	(0.0220)
Observations	3831	17860	39980	52602
Adjusted R^2	0.082	0.104	0.099	0.116

Notes: This table shows linear regressions of Equation 13 with the share of trained workers as the dependent variable. All specifications include year dummies, federal state dummies, 16 one-digit-sector dummies (NACE 2) as well as controls for the composition of the firm's workforce. Data sources are the LIAB and CHK firm wage fixed effects from Card, Heining and Kline (2013). Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01. Own calculations.

C.4 Descriptive Sample Statistics Worker-Level Analysis

Table C.10: Descriptive Sample Statistics Workers

	Not trained Mean	Trained Mean
Socio-demographic characteristics		
Female	0.31	0.32
Age	45	44
Household size	2.8	2.8
Number of children under 14 in household	0.4	0.51
No degree	0.02	0.01
Low education	0.29	0.23
Medium education	0.41	0.37
High education	0.28	0.40
Job characteristics		
Part-time	0.16	0.14
Fixed-term contract	0.08	0.05
Labor market experience	20	20
Tenure on the job	11	11
Log hourly wage	2.54	2.71
Person-specific wage component	-0.08	0.01
Firm characteristics		
Share part-time workers	0.27	0.24
Share marginal workers	0.09	0.06
Occupations		
Unskilled manual workers	0.15	0.08
Qualified manual workers	0.20	0.12
Technicians	0.07	0.11
Engieneers	0.04	0.06
Elementary services	0.13	0.09
Qualified services	0.02	0.04
Semi-professionals	0.03	0.06
Professionals	0.01	0.01
Unskilled clerks	0.07	0.08
Qualified clerks	0.23	0.29
Managers	0.04	0.07
Sectors		
Manufacturing	0.42	0.36
Construction	0.07	0.04
Wholesale & Retail	0.14	0.16
Transport	0.06	0.04
Hospitality	0.02	0.01
Information & Communication	0.05	0.08
Financial intermediation	0.04	0.06
Real estate	0.01	0.00
Prof., scientific & technical services	0.05	0.07
Administrative & support activities	0.06	0.04
Education	0.01	0.02
Health & social work	0.04	0.07
Other services	0.02	0.01
Independent variable & mediators		
Firm-specific wage component	0.07	0.13
Observations	13545	3351

Notes: This table shows descriptive sample statistics for the worker-level analysis in Section 4.5. Data sources are the NEPS-SC6-ADIAB and CHK firm- and person wage fixed effects from Card, Heining and Kline (2013). Weighted data using sampling weights of the NEPS. Own calculations.

C.5 Data Description

LIAB

Ruf, Kevin; Schmidtlein, Lisa; Seth, Stefan; Stüber, Heiko; Umkehrer, Matthias; Graf, Tobias; Grießemer, Stephan; Kaimer, Steffen; Köhler, Markus; Lehnert, Claudia; Oertel, Martina; Schneider, Andreas (2021): "Linked-Employer-Employee-Daten des IAB (LIAB): LIAB-Querschnittmodell 2 1993-2019, Version 1". Forschungsdatenzentrum der Bundesagentur für Arbeit (BA) im Institut für Arbeitsmarkt- und Berufsforschung (IAB).
DOI: 10.5164/IAB.LIABQM29319.de.en.v1
Link to LIAB website

Documentation

Ruf, Kevin; Schmidtlein, Lisa; Seth, Stefan; Stüber, Heiko; Umkehrer, Matthias (2021): Linked-Employer-Employee-Daten des IAB: LIAB Querschnittmodell 2 (LIAB QM2) 1993–2019. FDZ-Datenreport, 03/2021 (de), Nürnberg.
DOI: 10.5164/IAB.FDZD.2103.de.v1

NEPS-SC6-ADIAB

NEPS-Netzwerk (LIfBi); Bachbauer, Nadine; Wolf, Clara; Graf, Tobias; Grießemer, Stephan; Kaimer, Steffen; Köhler, Markus; Lehnert, Claudia; Oertel, Martina; Schneider, Andreas (2022): "Erhebungsdaten des Nationalen Bildungspanels (NEPS), Startkohorte 6 (SC6) verknüpft mit administrativen Daten des IAB (NEPS-SC6-ADIAB) – Version 7520 v1". Forschungsdatenzentrum der Bundesagentur für Arbeit (BA) im Institut für Arbeitsmarkt- und Berufsforschung (IAB).
DOI: 10.5164/IAB.NEPS-SC6-ADIAB7520.de.en.v1
Link to NEPS-SC6-ADIAB website

Documentation

Bachbauer, Nadine; Wolf, Clara (2022): NEPS-SC6-Erhebungsdaten verknüpft mit administrativen Daten des IAB (NEPS-SC6-ADIAB 7520). FDZ-Datenreport, 01/2022 (de), Nürnberg.
DOI: 10.5164/IAB.FDZD.2201.de.v1

AKM Effekte/CHK Effects

Bellmann, Lisa; Lochner, Ben; Seth, Stefan; Wolter, Stefanie (2020): AKM effects for German labour market data. FDZ-Methodenreport, 01/2020 (en), Nürnberg

Der Datenzugang erfolgte über einen Gastaufenthalt am Forschungsdatenzentrum der Bundesagentur für Arbeit im Institut für Arbeitsmarkt- und Berufsforschung (FDZ) und anschließend mittels kontrollierter Datenfernverarbeitung beim FDZ.

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Curriculum Vitae

markefke@wiso.uni-koeln.de
0221 470 5355
Institute for Economic Policy at the University of Cologne
Pohligstraße 1, 50969 Cologne

Experience

Research Associate *Since Sep. 2018*
Institute for Economic Policy at the University of Cologne, Cologne

Student Research Assistant *Feb. 2016 - June 2018*
IZA Institute of Labor Economics, Bonn

Research Internship *Jan. - March 2015*
ZEW Centre for European Economic Research, Mannheim

Public Sector Internship *March 2014*
Federal Ministry of Education and Research, Berlin

Education

PhD Student *Since Sep. 2018*
University of Cologne

M.Sc. Economics *Oct. 2015 - May 2018*
University of Cologne

B.A. Politics and Economics *Oct. 2011 - Jan. 2015*
University of Münster
Corvinus University Budapest

Eidesstattliche Erklärung

nach § 8 Abs. 3 der Promotionsordnung vom 17.02.2015.

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