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## 7|2025 The impact of the Covid-19 pandemic on worker careers: do different job opportunities matter?

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# The impact of the Covid-19 pandemic on worker careers: do different job opportunities matter?

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# Abstract

This paper exploits that the Covid-19 pandemic came as an unexpected shock that temporarily reduced the ratio of vacancies to seekers. We use this unique setting to understand the importance of job opportunities for the impact of unemployment on workers' careers. Compared to individuals who became unemployed under more benign conditions, we find greater and lasting adverse effects on earnings. We provide evidence that lower job opportunities lead unemployed individuals to take up jobs that are further down the occupation-specific wage distribution. Finally, we substantiate the importance of job prospects by using exogenous variation in the pandemic's effect on occupations.

# Zusammenfassung

In diesem Papier wird die Tatsache ausgenutzt, dass die Covid-19 Pandemie ein exogener Schock war, der temporär das Verhältnis von offenen Stellen zu Jobsuchenden verringert hat. In diesem einzigartigen Setting wird die Bedeutung von Beschäftigungsmöglichkeiten untersucht und inwiefern diese Erwerbskarrieren, die von kürzlicher Arbeitslosigkeit betroffen sind, beeinflussen. Im Vergleich zu Arbeitslosen, die **unter** günstigeren **Bedingungen** arbeitslos geworden sind, sind die Effekte auf **das** Einkommen größer und anhaltend negativ. Es zeigt sich, dass geringere Beschäftigungsmöglichkeiten dazu führen, dass **A**rbeitslose Beschäftigung weiter unten in der berufsspezifischen Lohnverteilung aufnehmen. Die Größe dieser Effekte hängen invers mit der unterschiedlichen Fähigkeit von Berufen, während Lockdowns zu operieren, zusammen.

# JEL

J23, J62, J64

# Keywords

Economic shocks, Covid-19 pandemic, unemployment, worker careers, occupations

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# 1 Introduction

It is well established that exposure to temporary economic shocks can have long-lasting scarring effects on individuals and their careers (Ruhm, 1991; Arulampalam, 2001). Prime examples include being displaced from one's job (Jacobson/LaLonde/Sullivan, 1993; Lachowska/Mas/Woodbury, 2020), being exposed to a recession as a labour market entrant (Oreopoulos/von Wachter/Heisz, 2012; Schwandt/von Wachter, 2019) or throughout one's career (Davis/von Wachter, 2011; Huckfeldt, 2022) as well as experiencing youth unemployment (Mroz/Savage, 2006; De Fraja/Lemos/Rockey, 2021). More recently, the literature has started to focus on how the conditions that prevail at the time of the shock determine the magnitude of its effect. For example, Schmieder/von Wachter/Heining (2023) show that the costs of job loss vary with the business cycle: individuals who are displaced under unfavourable conditions tend to experience greater losses compared to individuals who lost their job under more benign conditions.

In this paper, we contribute to the strand of literature that is concerned with evaluating the conditions at the time of a shock. Specifically, we use the fact that the Covid-19 pandemic led to an unexpected, but pronounced drop in the ratio of vacancies to job seekers. Individuals who became unemployed shortly before the start of the pandemic were thus exposed to a less favorable environment within which to find a new job compared to earlier cohorts of newly unemployed individuals. We argue that, as a result of worse employment prospects, the former group is likely to experience less favourable employment trajectories. Moreover, we aim to elicit which mechanisms translate a worsening of job opportunities into depressed labour market outcomes.

We propose that the emergence of the Covid-19 pandemic represents an exogenous shock which limits concerns about selection. For example, individuals are unlikely to have avoided becoming unemployed before the start of the pandemic in anticipation of its detrimental effect on their careers. This contrasts with the approach of using mass layoffs as proxies for unexpected job loss, which relies on the assumption of (high-quality) workers not anticipating the event and leaving the firm early or the firm choosing to only lay off lower quality workers. Similarly, the literature on entry conditions has to deal with the fact that individuals may choose when to enter the labour market to avoid negative consequences of unfavourable conditions.

Moreover, the nature of the Covid-19 pandemic was such that it affected some occupations more than others (e.g., Forsythe et al., 2020; Cortes/Forsythe, 2022). We document that the ratio of vacancies to job seekers dropped even more among occupations that were less suited to operating under lockdown conditions, thereby limited the job opportunities in those occupations. We propose that comparing the employment trajectories of individuals who were employed in occupations that were more or less adaptable to lockdown conditions represents an additional approach to identify the consequence of differences in job opportunities at the time of becoming unemployed.

To analyse the effects of the economic shock that was caused by the pandemic, we use detailed administrative data from Germany and compare the employment trajectories of "treated" individuals who became unemployed shortly before the start of the pandemic with a control group of individuals who became unemployed three years earlier. As a result of the pandemic, the treatment group is faced with exogenously depressed job opportunities. This setting therefore allows us to use an event-study difference-in-differences approach, around the time of becoming unemployed, to identify how exogenous variation in workers' job prospects affects the development of employment careers after becoming unemployed.

To further assess the hypothesis that the state of job opportunities affects the development of employment trajectories, we make use of the fact that the pandemic posed a greater adverse shock for occupations that were less able to operate under lockdown conditions. We measure adaptability to lockdown conditions at the occupational level using the lockdown work ability index (LWA) developed by Palomino/Rodríguez/Sebastian (2020). Specifically, we adopt a difference-in-difference-in-differences approach using the LWA of the occupation which an individual was initially employed in as a measure of treatment intensity.

Our empirical analysis shows that treated individuals experienced significantly greater earnings losses. Over the whole period of observation, we estimate an excess earnings loss of €4,900 (corresponding to a proportional increase of 15%) compared to the control group. Earnings losses are most pronounced between March and May, but remain statistically significant until the end of the period of observation. In the short run, earnings losses can be primarily ascribed to a loss of employment. In the longer run, we no longer find a significant difference in the development of employment in the treatment and the control group. By contrast, we document that, conditional on being employed, the pandemic led to a lasting decrease in wages. Hence, in the longer run, the larger earnings loss of the treatment group is also due to a higher probability of receiving a lower wage after finding a new job. These results provide first evidence that the development of individual labour market trajectories are closely related to the state of job prospects at the time of becoming unemployed.

We take care to rule out alternative explanations for why employment trajectories developed less favourably among the treatment group. On the one hand, we provide evidence that the magnitude of the negative effects on earnings, employment and wages cannot simply be explained by a general worsening of aggregate labour market conditions that took place before the start of the pandemic. On the other hand, we show that lost earnings and wages do not merely reflect the wide-spread use of short-time work during the pandemic.

To better understand the sources of the wage losses in the treatment group, we apply a decomposition based on Gelbach (2016). The results suggest that wage losses can primarily be ascribed to treated individuals being more likely to take up jobs that are further down the occupation-specific wage distribution. This finding is especially remarkable as the same individuals are at the same time found to be more likely to move to occupations with higher average wages. Consistent with Braakmann/Eberth/Wildman (2022) and Forsythe et al. (2022), we also document that treated individuals are more likely to switch to jobs with a higher LWA index, which offered better job opportunities. These results provide suggestive evidence that treated individuals used occupational mobility as a way to reduce their exposure to the economic shock caused by the pandemic. However, our findings also suggest that changing occupations came at a cost. Descriptive analyses indicate that the negative impact on occupational rank and wages is more pronounced for occupational movers than for stayers, which would be consistent with existing evidence that human capital is partly occupation-specific (Gathmann/Schönberg, 2010).

Finally, we estimate greater treatment effects for individuals who used to be employed in occupations with a lower LWA. Treated individuals who used to work in occupations whose LWA index was lower by 0.1 units than the mean experienced an additional pandemic-induced earnings loss amounting to about 12% compared to individuals from occupations with a mean LWA. While these individuals also experienced a greater reduction in employment, the additional wage loss stands out. These losses can primarily be ascribed to finding jobs that are further down the occupational wage distribution. To further support the result that the size of the adverse effects depends on the LWA of a worker's pre-unemployment occupation, we provide evidence that the difference in effect size cannot be explained by differences in other characteristics between workers who used to be employed in low- or high-LWA occupations. These findings provide further evidence for our main hypothesis that how employment trajectories develop after becoming unemployed depends to a large extent on the job opportunities that individuals are exposed to.

Our study relates to different strands of literature. First, it contributes to the literature on exposure to temporary economic shocks which analyses the short- and long-term effects on earnings and employment histories after an unexpected job loss (see, e.g., Raposo/Portugal/Carneiro, 2019; Lachowska/Mas/Woodbury, 2020; Bertheau et al., 2023), during the financial crisis (Yagan, 2019; Campos-Vazquez et al., 2023) or while entering the labour market during a recession (see, e.g., Arellano-Bover, 2022; Rothstein, 2023). Overall, these studies find that individuals who are exposed to economic shocks experience long-lasting reductions in earnings. There is evidence that the earnings losses are highly cyclical, resulting mainly from wage declines during recessions (Forsythe, 2022; Schmieder/von Wachter/Heining, 2023). We also document a long-lasting negative wage effect during the economic downturn caused by the Covid-19 pandemic. However, while much of the literature points to losses in firm wage premiums as an important mechanism (see, e.g., Gulyas/Pytka, 2020; Fackler/Mueller/Stegmaier, 2021), we rather find that in the course of the pandemic downward movements in occupational rank are an important driver of wage losses. Whereas papers on the role of recessions often struggle with endogenous inflow problems and the fact that labour market conditions rarely deteriorate suddenly, our paper offers a compelling identification strategy due to the unforeseeable and severe nature of the pandemic-induced shock.

Second, the unexpected occupation-specific change in employment prospects provides an interesting environment in which the jobs considered by the unemployed are affected differently. Since alternative occupations and related jobs that they would usually consider may also be affected, unemployed workers from occupations severely affected by lockdown restrictions were more likely to consider other options. In turn, job seekers might have redirected their search to occupations which are less affected (see, e.g., Hensvik/Le Barbanchon/Rathelot, 2021; Bauer et al., 2023), but where they lack experience. However, Carrillo-Tudela et al. (2023), for instance, show that a large proportion of unemployed individuals also continued targeting declining occupations and industries during the pandemic. Fewer job offers and a potentially worse bargaining position might explain the documented wage losses due to the pandemic. In this way, our second contribution refers to the broader literature on outside options[1] by examining a situation in which the portfolio of suitable jobs changes exogenously.

Third, our paper contributes to the narrower Covid-19 literature by extending individual-level analyses to the longer term, addressing gaps in studies that primarily focus on the immediate aftermath of the pandemic and the rise of remote work (e.g., Dingel/Neiman, 2020; Forsythe et al., 2020; Alipour/Fadinger/Schymik, 2021; Cortes/Forsythe, 2022). Compared to the studies by Huttunen/Pesola (2022) and Adermon et al. (2024) which use a comparable control group design to our paper, we emphasize the moderating role of occupational lockdown work ability and the mechanisms underlying the impact of the pandemic on individual labour market trajectories. Additionally, we build on research into occupation-specific effects of the pandemic (e.g., Beland/Brodeur/Wright, 2020; Albanesi/Kim, 2021; Cortes/Forsythe, 2022; Petroulakis, 2023) which documents more adverse effects for lower-paying occupations and occupations with a higher contact

[1] For studies on the impact of information about job opportunities on job search see, e.g., Altmann et al. (2018); Belot/Kircher/Muller (2019); Gee (2019). For studies looking explicitly at outside options in the labour market see, e.g., Caldwell/Danieli (2024); Schubert/Stansbury/Taska (2024) and for studies using worker flows to determine the size of the relevant labour market see, e.g., Manning/Petrongolo (2017); Nimczik (2023).

intensity and where working from home was not feasible. However, those studies focus on the short-term effects of the pandemic on an aggregated occupational level, whereas this paper analyses individual employment trajectories.

This paper is structured as follows: Section 2 gives an overview of the development of the Covid-19 pandemic in Germany. Section 3 describes the data and Section 4 the empirical strategy. Section 5 analyses the effect of the pandemic on earnings, employment and wages and assesses potential mechanisms, while Section 6 evaluates whether the size of the effects differs by occupation. Section 7 concludes.

# 2 The Covid-19 pandemic in Germany

Although the first Covid-19 case in Germany was registered in January 2020, the beginning of the pandemic can be assigned to early March 2020, when the number of Covid-19 cases started to increase and the first social distancing measures were implemented. Officially, the pandemic ended in April 2023. The early phase of the pandemic was also characterised by the implementation of a lockdown, which had so far not been experienced and which lasted from the 22$^{th}$ of March until the 4$^{th}$ of May 2020. The lockdown imposed restrictions with respect to social distancing measures and closing of facilities.[2] These restrictions therefore affected occupations differently: while employees in occupations with high contact intensity or without the possibility to work from home were less likely to be able to work during the lockdown, employees in occupations of systemic relevance or with the possibility to work from home were more likely to be able to continue working.

During the beginning of the pandemic, the German economy was hit by the strongest shock since the financial crisis (Adams-Prassl et al., 2020). This can also be seen from a negative impact on individual employment prospects as demand for labour declined and the number of job seekers increased, especially among those occupations for which working from home was less possible.[3] Figure 1 shows the ratio of registered vacancies to unemployed job seekers between January 2017 and December 2022.[4] Figure 1 presents the total series (black diamonds) as well as the series for occupations with a high and a low lockdown work ability (LWA) index (blue dots and red triangles). The LWA index, defined by

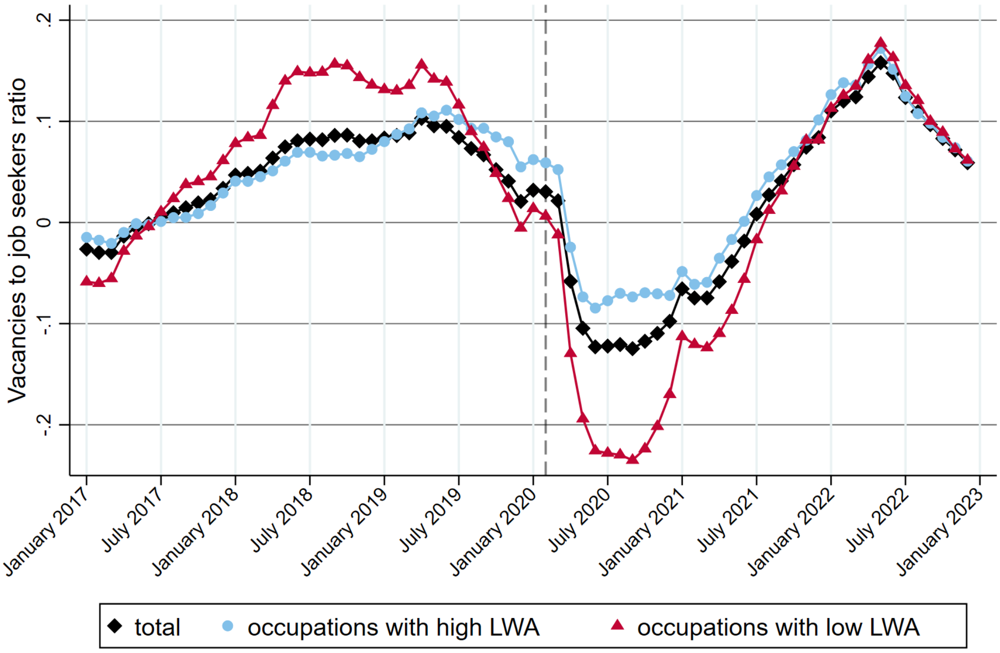
[2] Shops, schools, businesses in hospitality, hairdressers and leisure facilities were closed, whereas facilities of systemic relevance such as pharmacies or supermarkets remained open. Additionally, the government implemented the obligation that all employees who were able to do so should work from home.

[3] Evidence from Germany suggests that search intensity has hardly been affected by the Covid-19 pandemic (Bauer et al., 2023).

[4] The series is shown in residualised form to account for seasonal fluctuation. Specifically, we regress the series on calendar year and month dummies and subtract the latter.

Palomino/Rodríguez/Sebastian (2020), measures the possibility of a specific occupation to operate during a lockdown (see Section 3.2.2 for a detailed description). Occupations with a high LWA are, for example, occupations in IT or healthcare services, while occupations in hospitality or construction have a low LWA.

**Figure 1.: The effect of the Covid-19 pandemic on vacancies and job seekers**




*Note: Figure 1 shows the development of the ratio of registered vacancies to unemployed job seekers in Germany. The series has been residualised to account for seasonal fluctuation. For this purpose, the series is regressed on dummies for calendar years and months and the estimated coefficients of the latter are then subtracted. The reference period is January 2017. Occupations with a low (high) lockdown work ability (LWA) correspond to the first (last) quartile of the (unweighted) LWA distribution across occupations. Note that for unemployed job seekers the targeted occupation is used for any occupational analysis.

Source: Federal Employment Agency, 2023.

During the onset of the Covid-19 pandemic between March and May 2020, the ratio of vacancies to job seekers fell sharply, with the reduction being stronger for occupations with a low LWA. Even though the ratio of vacancies to job seekers subsequently started to recover and eventually exceeded its pre-pandemic level during the year 2022,[5] this upward trend is reversed from June 2022, which might be related to the Russian invasion of Ukraine and the ensuing effect on the global economy. Furthermore, the development of the vacancies to job seekers ratio appears to have been quite similar for low-LWA and high-LWA occupations in the pre-pandemic years and only began to diverge in the second half of 2019. The pandemic, however, exacerbates this divergence which suggests that the differential development in labour market opportunities by occupational LWA does not only reflect a continuation of diverging pre-pandemic trends.

[5] During this period, governmental containment measures were gradually eased, for example by the reopening of hairdressers, gastronomy and schools.

Overall, the figure illustrates that the pandemic created (temporarily) unfavourable conditions for those searching for employment in the spring of 2020, especially for occupations with a low LWA. This situation may have led to longer periods of job search or a higher incidence of occupational mobility of unemployed individuals from lower LWA occupations, which can be costly due to longer unemployment periods or human capital being only partly transferable (Gathmann/Schönberg, 2010).

# 3 Data

## 3.1 Defining treatment and control group

The empirical analysis is based on administrative social security data provided by the Institute of Employment Research (IAB), the research institute of the German Federal Employment Agency. Specifically, the analysis uses data from the Integrated Employment Biographies (IEB).[6] The IEB contains all labour market participants in Germany except for the self-employed, civil servants and military service members. In addition to individual characteristics (e.g., gender, age, skill and nationality), the data include daily information on employment relationships (e.g., the average daily wage, a person's occupation or skill level as well as the type of employment) as well as information on unemployment spells, participation in measures of active labour market policy or receipt of transfer payments. Detailed information on establishment characteristics such as industry and number of employees stem from the Establishment History Panel (BHP). We also have monthly information on the total number of workers in short-time work in each establishment based on records of the Federal Employment Agency.

Our analysis focuses on individuals who became unemployed shortly before the start of the pandemic and were therefore subsequently exposed to an economic shock.[7] In particular, we consider individuals who became unemployed during the first half of February 2020 as the treatment group and compare them to a control group of individuals who became unemployed in the same month in 2017. We choose February 2020, as there were only 18 confirmed cases of Covid-19 infection by the end of that month and (lockdown-)restrictions were not expected at that time. Hence, it is likely that firms did not anticipate the subsequent development of the pandemic and did not lay off their employees. Moreover,

[6] We use IEB version V17.00.00-202212. For a description of the IEB, see Oberschachtsiek et al. (2009).

[7] We define unemployed individuals as those for whom the status "unemployed and searching for work" is recorded. Individuals who are sick for more than *six* weeks during unemployment, only registered as "searching for work" but not unemployed or without a status are excluded.

February 2020 is chosen rather than an earlier month to ensure that as many unemployed individuals as possible in the treatment group are exposed to the pandemic while they are unemployed.[8] The control group is selected so that the time of unemployment is as close as possible to that of the treatment group, while ensuring that individuals in the control group are themselves not exposed to the Covid-19 pandemic.

Furthermore, the sample is restricted to individuals with a certain degree of labour market attachment (see, e.g., Jacobson/LaLonde/Sullivan, 1993; Schmieder/von Wachter/Heining, 2023). In detail, we only retain those unemployed individuals who were employed at least from November in year $t-1$ to the 31st of January in year $t$ in the same occupation and same establishment, so that the first possible day in unemployment is the 1st of February in year $t$ ($t$ refers to the year 2020 for the treatment group and 2017 for the control group). Focusing on individuals with a stable employment pattern ensures that unemployment represents a potentially severe disruption. Further details on the construction of the sample are described in Appendix Section A.1.

In total, the sample consists of 66,199 individuals in the control and 66,070 individuals in the treatment group and the daily information is aggregated to a panel of half-monthly periods ranging from September $t-1$ to December $t+2$. Robustness checks with respect to data preparation are discussed in Section 5.3.3.

## 3.2 Variables

### 3.2.1. Main outcome variables

In our empirical analysis, we focus on earnings, employment and wages. The administrative data allow us to record all relevant outcomes (and control variables) within each half-month period, so that employment is measured as the number of days an individual is employed in that period. Depending on the period, the maximum value of employment is between 13 and 16 days. Information on the wage is available in the form of an average daily wage, as the IEB do not contain information on working hours. Whenever a person is not employed, we set the wage to missing. Moreover, we deflate wages using the consumer price index as in Dauth/Eppelsheimer (2020).[9] Earnings are derived as the product of the number of days

8 As the pandemic officially started in March 2020, inflows into unemployment during that month may already have been due to the pandemic.

9 The consumer price index is additionally adjusted, since high changes in the inflation rate lead to a drop in the estimated wage development at the turn of the year from 2021 to 2022 (see Figure B14 in the Appendix). To avoid these jumps, which reflect changes in consumer prices rather than wage changes, we use the index from 2017 to deflate wages after 2017 for individuals in the control group and the index from 2020 for wages after 2020 for individuals in the treatment group.

in employment in the respective half-month period and the daily wage. Individuals who are not employed or who leave the labour market in a given period receive earnings of zero.

### 3.2.2. Lockdown work ability (LWA)

To measure the extent to which individual labour market prospects are affected by the pandemic, we use the "lockdown work ability" (LWA) index for occupations proposed by Palomino/Rodríguez/Sebastian (2020). This index consists of three components: the possibility to work from home, whether occupations were considered systemically relevant ("essential") or had to close during the lockdown. Thus, the LWA index has the advantage that it does not only consider the ability to work from home, but also takes into account whether people in an occupation were allowed to continue working during the pandemic. For example, some occupations, such as medical and health care occupations, offer only limited possibilities to work from home, but, at the same time, those occupations remained open during lockdown because of their systemic relevance. These components then form an index that ranges from zero (low LWA) to one (high LWA) for each occupation. For details on the construction, see Appendix Section A.4.

## 3.3 Comparing treatment and control group

To ensure comparability between treated and control individuals, we apply inverse propensity score weighting on pre-unemployment characteristics (see Appendix Section A.3 for further details). The weighting variables include socio-demographic characteristics, job and firm characteristics as well as variables from the employment biography. All of these variables are measured in the first half of November of the year $t-1$, i.e. three months before the transition into unemployment, when, by definition, every individual in the sample is employed. Table A2 in the Appendix contains the full list of weighting variables and balancing tests.

Table 1 shows selected descriptive statistics for treated (column (1)), weighted control (column (2)) and unweighted control individuals (column (3)) who became unemployed in the first half of February.[10] Additionally, the standardised differences between the means of the treatment and weighted control group are displayed in column (4).[11] The differences between treated and control individuals are already relatively small before applying the weighting procedure. These differences are further reduced after weighting. The

[10] Note that the variables shown in Table 1 are not necessarily all used in the weighting procedure.

[11] The standardised difference is defined as $\Delta_X = \left(\bar{X}_1 - \bar{X}_0\right) / \left((S_1^2 + S_0^2)/2\right)^{0.5}$, where $\bar{X}_D$ is the sample mean of the treated ($D=1$) or control ($D=0$) individuals and $S_D^2$ are the respective sample variances.

standardised difference is relatively small and below the rule of thumb of 0.1 as suggested by Austin (2011), indicating that a balance between the treatment and the control group is achieved. For example, individuals in the treatment as well as in the (weighted) control group are, on average, 39 years old, around 60% are male and are mostly middle-skilled.[12] We also find that treated and control individuals are comparable in terms of the firm size distribution, which has been shown to be a relevant determinant of employment responses to economics shocks (Xu, 2022).

Although the (consumer price-adjusted) daily wage rate differs between both groups by approximately €4, which corresponds to a relative difference of around 5.6%, we argue that this difference reflects real wage growth that took place between the years 2016 and 2019 rather than structural differences between the treatment and the control group (see Appendix Section A.3.1). The absence of these differences in the two groups' wages is also supported by the fact that measures of unobserved worker and firm quality, which are derived from an AKM wage decomposition (Abowd/Kramarz/Margolis, 1999)[13], do not show any significant differences. This suggests that individuals in the treatment and the control group differ neither with respect to unobserved worker quality nor to the unobserved quality of the firm at which they worked before becoming unemployed.

# 4 Empirical strategy

## 4.1 Estimation: baseline model

We use a difference-in-differences (DiD) event-study design combined with inverse propensity score weighting to identify the effect of the Covid-19 pandemic on the employment trajectories of newly unemployed workers. Thereby, we focus on the average treatment effect on the treated (ATT), i.e. the effect of the pandemic for individuals who became unemployed shortly before its onset. This approach is based on the assumption that these individuals were exposed to an unexpected worsening of their labour market prospects compared to individuals who became unemployed three years earlier.[14] It is

[12] The skill groups are defined as follows: low-skilled individuals have no vocational degree, middle-skilled have a vocational degree and high-skilled have a tertiary degree (e.g., university degree).

[13] Firm fixed effects are estimated on the basis of the full-time male working population in Germany for the years 1995 to 2019. Since this method created too many missings for worker fixed effects, we use the estimation procedure by Bellmann et al. (2020) for worker fixed effects, which includes the years 2014 to 2021.

[14] Formally weights for the control group are given by $\frac{\hat{p}(x_i)}{1-\hat{p}(x_i)}$, where $\hat{p}(x_i)$ is the predicted probability of treatment conditional on observed characteristics $x_i$ (see Appendix Section A.3).

## Table 1.: Descriptive statistics

| | (1) Treatment | (2) Control (weighted) | (3) Control (unweighted) | (4) Standard. diff. |
|---|---|---|---|---|
| **Socio-demographic characteristics (at the time of matching)** | | | | |
| Age | 39.194 | 39.207 | 39.674 | -0.001 |
| | (12.422) | (12.258) | (12.259) | |
| Male (fraction) | 0.612 | 0.613 | 0.592 | -0.002 |
| | (0.487) | (0.487) | (0.491) | |
| Foreign (fraction) | 0.244 | 0.246 | 0.180 | -0.005 |
| | (0.429) | (0.431) | (0.384) | |
| Low skilled (no completed apprenticeship, fraction) | 0.153 | 0.154 | 0.132 | -0.003 |
| | (0.360) | (0.361) | (0.339) | |
| Middle skilled (completed apprenticeship, fraction) | 0.594 | 0.592 | 0.673 | 0.005 |
| | (0.491) | (0.492) | (0.469) | |
| High skilled (tertiary education, completed) | 0.172 | 0.171 | 0.148 | 0.001 |
| | (0.377) | (0.377) | (0.356) | |
| **Current employment (at the time of matching)** | | | | |
| Current wage | 77.658 | 73.751 | 71.414 | 0.084 |
| | (47.204) | (46.009) | (43.509) | |
| Current earnings | 1,164.863 | 1,106.262 | 1,071.214 | 0.084 |
| | (708.061) | (690.128) | (652.642) | |
| In regular employment (fraction) | 0.933 | 0.930 | 0.928 | 0.012 |
| | (0.250 ) | (0.255) | (0.258) | |
| In full-time employment (fraction) | 0.654 | 0.652 | 0.657 | 0.003 |
| | (0.476) | (0.476) | (0.475) | |
| Very small establishment (less than 10, fraction) | 0.205 | 0.202 | 0.216 | 0.008 |
| | (0.404) | (0.402) | (0.411) | |
| Small establishment (10-49, fraction) | 0.303 | 0.302 | 0.287 | 0.001 |
| | (0.459) | (0.459) | (0.453) | |
| Medium-sized establishment (50-249, fraction) | 0.285 | 0.287 | 0.261 | -0.004 |
| | (0.452) | (0.452) | (0.439) | |
| Large establishment (more than 250, fraction) | 0.201 | 0.203 | 0.169 | -0.005 |
| | (0.400) | (0.402) | (0.375) | |
| Estimated AKM firm effect | -0.157 | -0.173 | -0.186 | 0.059 |
| | (0.264) | (0.263) | (0.261) | |
| **Employment biography** | | | | |
| Work experience | 11.961 | 11.757 | 12.357 | 0.021 |
| | (10.098) | (9.723) | (9.532) | |
| Tenure in current establishment | 3.017 | 3.072 | 3.186 | -0.011 |
| | (5.177) | (5.101) | (5.130) | |
| Tenure in current occupation | 5.732 | 5.802 | 5.941 | -0.010 |
| | (7.164) | (7.201) | (7.153) | |
| Number of job changes | 3.259 | 3.008 | 3.112 | 0.069 |
| | (3.677) | (3.666) | (3.566) | |
| Being unemployed before (fraction) | 0.759 | 0.752 | 0.779 | 0.016 |
| | (0.428) | (0.432) | (0.415) | |
| Employed in manufacturing sector (fraction) | 0.394 | 0.395 | 0.404 | -0.002 |
| | (0.489) | (0.489) | (0.491) | |
| Employed in service sector (fraction) | 0.598 | 0.597 | 0.584 | 0.002 |
| | (0.490) | (0.490) | (0.493) | |
| Estimated AKM worker effect | 4.364 | 4.372 | 4.359 | -0.022 |
| | (0.376) | (0.373) | (0.353) | |
| N | 66,070 | 66,199 | 66,199 | |

Note: Columns (1) to (3) show the mean value and standard deviation (in parentheses) of individual characteristics that are measured at the first half of November $t-1$ (the point for the weighting). Column (4) reports the standardised difference between columns (1) and (2), which is defined as $\Delta_X = (\bar{X}_1 - \bar{X}_0) \,/\, ((S_1^2 + S_0^2)/2)^{0.5}$, where $\bar{X}_D$ is the sample mean of the treated ($w=1$) or (weighted) control ($w=0$) individuals and $S_w^2$ are the respective sample variances. Note that the observations for the AKM worker and firm fixed effects are smaller than the reported number of observations. Not all shown characteristics, such as current wages, establishment size or AKM firm effects, are used in propensity score weighting. For the full list of propensity score weighting variables see Table A2 in the Appendix.
Source: IEB, BHP, own calculations.

crucial that the individuals entered unemployment *before* the beginning of the pandemic, because this ensures that any effects on labour market outcomes are only due to the subsequent exposure to the pandemic. We estimate the following model:

$$y_{i,p} = \alpha_i + \sum_{\tau \neq -1} \gamma_\tau I(\tau = p) + \sum_{\tau \neq -1} \beta_\tau I(\tau = p) I(D_i = 1) + \varepsilon_{i,p}. \tag{1}$$

$y_{i,p}$ is the outcome of individual $i$ at time $p$, $\alpha_i$ is an individual fixed effect, $D_i$ represents the treatment dummy which takes the value 1 if the individual became unemployed in February 2020 (and 0 otherwise) and $\varepsilon_{i,p}$ is a random error term. $p$ measures half-month periods and runs from -10 to 68 with $p = 0$ referring to the time period in which the individual became unemployed (February 2017 and 2020 for the control and the treatment group, respectively). The treatment group is observed from September 2019 to December 2022 and the control group from September 2016 to December 2019. For a fixed point in time $\tau$, $\gamma_\tau$ represents the average change in the outcome for the control group relative to the reference period (conditional on fixed effects) and $\beta_\tau$ is the average difference in the change of this outcome between the treatment and the control group at that point in time. The inclusion of individual fixed effects ensures that identification of the parameter of interest, $\beta_\tau$, is based on the within-variation in the outcome variables for treated and control individuals. In a dynamic setting, $\sum_{\tau>0} \gamma_\tau$ provides an estimate of the cumulative expected deviation of the outcome variable from its value at the reference period over the whole treatment period for individuals in the control group. This quantity provides the counterfactual change in the outcome for the treatment group if the pandemic had not taken place. Correspondingly, $\sum_{\tau>0} \beta_\tau$ shows by how much the cumulative deviations differ between the treatment and the control group. The latter quantity, therefore, provides a measure of the cumulative effect of the pandemic on the corresponding outcome. In the following section, we will use average measures computed over the whole treatment period as well as over different sub-periods to quantify the effect of the pandemic on different outcomes.

The first identifying assumption of our empirical approach is that from the perspective of the individuals the Covid-19 pandemic and its timing were unforeseen. We therefore assume that becoming unemployed during the first half of February 2020 is not the result of strategic behaviour on the part of individuals. The second identifying assumption is that the observed labour market trajectories of the control group provide a valid approximation of the counterfactual trajectories for treated individuals that would have occurred in the absence of the pandemic. To provide support for this approach, we sample inflows into unemployment from the same month which should reduce compositional differences related to seasonal fluctuations. We also show that, even without applying weighting, individuals in the treatment and the control group are already very similar with respect to the composition of observable individual, firm and job characteristics. Applying inverse propensity score weighting further reduces any differences between the two groups (see Section 3.3). Moreover, our event-study approach allows us to examine differences in outcome trends during the pre-unemployment period. To support the assumption of parallel trends, $\beta_\tau$ should be close to zero for $\tau < 0$. A potential challenge to identification is that individuals in the treatment group were exposed to less favourable labour market conditions - regardless of the Covid-19 pandemic - compared to individuals in the control group. While this concern is not considered in our baseline event-study model, we explicitly address it in Section 5.3.1 and show that our estimated effects of the Covid-19 pandemic cannot be explained merely by a general worsening of labour market opportunities.

## 4.2 Estimation: heterogeneous effects model

To assess the hypothesis that the size of the economic shock differed between occupations, Equation 1 is extended to a difference-in-difference-in-differences model in which LWA serves as a measure of treatment intensity. The extended model reads as follows:

$$\begin{aligned} y_{i,p} =& \alpha_i + \sum_{\tau \neq -1} \gamma_\tau I(\tau = p) + \sum_{\tau \neq -1} \beta_\tau I(\tau = p) I(D_i = 1) + \\ & \sum_{\tau \neq -1} \delta_\tau I(\tau = p) \overline{LWA^*}_{o(i)} + \sum_{\tau \neq -1} \phi_\tau I(\tau = p) I(D_i = 1) \overline{LWA^*}_{o(i)} + \varepsilon_{i,p} \end{aligned} \tag{2}$$

In particular, Equation 2 includes additional interactions with $\overline{LWA^*}_{o(i)}$ which vary by the occupation $o(i)$ in which individual $i$ was employed before becoming unemployed. To ease the interpretation of the results, we first transform the LWA index by defining $LWA^* = 1 - LWA$, so that higher values indicate a lower lockdown work ability.[15] Second, we adjust the transformed variable by subtracting the mean over all occupations ($\overline{LWA^*}$), so that $\overline{LWA^*}$ takes a value of zero for occupations with the mean value of LWA.

The coefficients $\gamma_\tau$ and $\beta_\tau$ (and their sums) now represent the effects for individuals who used to work in occupations with a mean value of LWA. For a fixed point in time $\tau$, $\delta_\tau$ captures the effect of a marginal increase in the inverse LWA on the respective outcomes for individuals in the control group. $\phi_\tau$ is the average difference between treated and control individuals for those who used to be employed in marginally more exposed occupations. We refer to this quantity as the excess effect of the pandemic as it measures by how much the effect of the pandemic is predicted to change for a marginal reduction in LWA of an individual's pre-unemployment occupation. In a dynamic setting, $\sum_{\tau>0} \phi_\tau$ describes the cumulative differential development of outcomes between treated and control individuals initially employed in marginally more exposed occupations.

[15] Our hypothesis is that workers who used to be employed in occupations with a lower LWA experienced greater (negative) effects from being exposed to the pandemic. After transforming the LWA variable, the estimated coefficients directly show the additional effect associated with a *reduction* in LWA.

When estimating Equation 2, we use variation in the assignment of workers to occupations before becoming unemployed. While sorting into occupations is itself not random, we assume that selection into occupations in the treatment group was not driven by expectations concerning the heterogeneous effect of the pandemic on different occupations. Moreover, since LWA is a continuous variable, estimation of Equation 2 has to fulfill stronger parallel trend assumptions (Callaway/Goodman-Bacon/Sant'Anna, 2024): not only do the treatment and control groups have to display parallel trends on average, but also the trends of individuals from lower and higher LWA occupations have to be similar. Section 6.3.1 provides analyses on the validity of the stronger parallel trends assumption.

# 5 The labour market effects of the pandemic

## 5.1 Earnings, employment and wages

The estimated effects of the Covid-19 pandemic on earnings over time are shown in panel (a) of Figure 2. The horizontal axis measures event time, where $t$ indicates the year in which the transition into unemployment occurs. The dashed vertical line indicates the period in which individuals became unemployed (first half of February 2017 for the control group and 2020 for the treatment group). The vertical axis displays the estimated difference in the change in earnings at every period $p$ (relative to the reference period) between the treatment and the control group, $\hat{\beta}_p$ of Equation 1.

Before becoming unemployed, earnings of the treatment and the control group appear to have developed along a similar path, which provides support for the parallel trends assumption. At the peak of the pandemic between March and May 2020, when the first lockdown was implemented, there is a substantial drop in earnings for the treatment group relative to the control group.[16] This earnings gap between treated and control individuals

[16] This drop is already visible around the transition into unemployment. Since the pandemic sets in during the first half of March 2020 (March $t$), the difference between treated and control individuals suggests that unemployed individuals in 2020 already faced less favourable job opportunities compared to the control group.

then starts to decrease steadily, but does not fully disappear by the end of the observation period.

Column (1) of Table 2 contains the average treatment effect and the average effect of becoming unemployed for the control group over the whole period. It also shows the treatment effects averaged over five separate time periods: pre-pandemic (September 2019 to January 2020), from February to May 2020, from June to September 2020, from October to December 2020 and for the year 2021 and the year 2022. As can be seen, the adjustment stops in 2022 and an average earnings gap of about €20 or 6.1%[17] still remains throughout the year. This suggests that the sudden worsening in job opportunities had a lasting negative earnings effect. In total, the estimated cumulative earnings loss amounts to almost €4,900 over the whole treatment period (70 periods meaning 35.5 months) or about €70 per half-month. This translates into an average earnings gap of 15% ($\frac{-69.65}{-451.306}$).

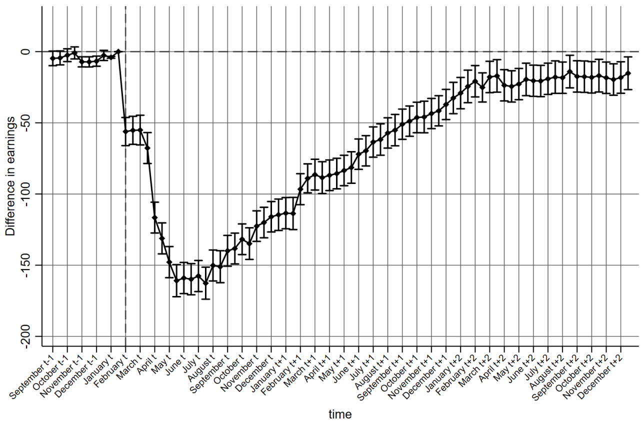
One explanation for the earnings loss is a reduction in employment. Panel (b) of Figure 2 shows the effects of the pandemic using the number of days in employment as the dependent variable. The pandemic significantly reduced the number of days in employment, with the effect being most pronounced between May and July 2020. This development is similar to the evolution of earnings, though employment recovers faster than earnings and reaches the same level as the control group towards the beginning of 2022. In total, treated individuals experienced, on average, a loss of about 40 days in employment over the treatment period compared to the control group, which corresponds to a loss of almost 0.6 days per half a month (see column (2) of Table 2). Thus, becoming unemployed under more benign labour market conditions increased the average loss in employment by about 8.2% ($\frac{-0.573}{-7.024}$) compared to the control group. Figure B5 and Table B6 in the Appendix provide further information on the different labour market states that are responsible for the initial decline in employment. The main finding is that the flip-side of the pandemic's negative effect on employment is an increase in time spent unemployed rather than exiting the labour market or taking part in a policy measure.

In contrast, wages do not follow the same downward trend at the onset of the pandemic, as can be seen from panel (c) of Figure 2.[18] Instead, the wage effects fluctuate in sign during the first weeks following the transition into unemployment before turning positive between April and June 2020. Subsequently, the coefficient estimates steadily decrease in magnitude, turning negative in August 2020, leading to a significant wage penalty in 2021, before stabilising at a level of an average wage loss of 1.1% in 2022 (see column (3) of Table 2). These findings suggest that, in the longer run, the pandemic-induced change in job opportunities significantly reduced wages and that this loss contributed to the reduction in earnings.

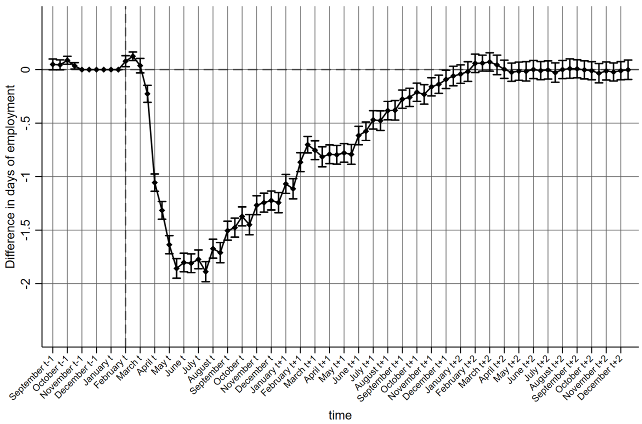
[17] Average earnings loss in 2022: $\frac{-19.84}{-324.379} = 0.061$, where 324.379 is the average earnings loss for the control group in 2022.

[18] Wages are conditional on employment. Estimation of the wage effects is therefore restricted to those observations where individuals are employed for at least one day during a half-month period.

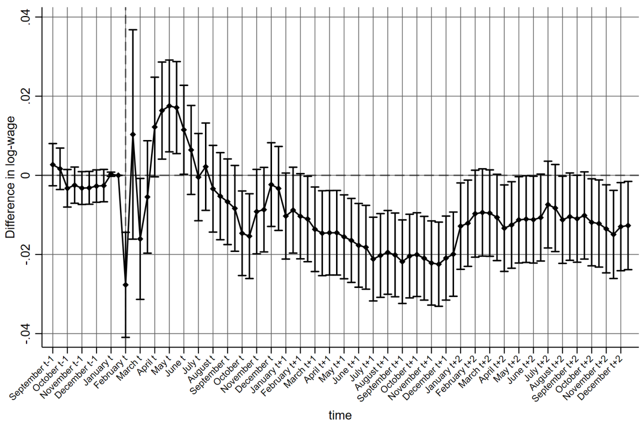
**Figure 2.: The effect of the Covid-19 pandemic on employment and wages**




**(a) Earnings**




**(b) Days in employment**




**(c) Log wages**

Note: Figure 2 shows the estimated coefficients $\hat{\beta}_p$ from Equation 1 with earnings (panel (a)), days in employment (panel (b)) and log wages (panel (c)) as dependent variables using data on individuals who became unemployed in February 2020 and 2017, respectively. The estimation is weighted by the inverse propensity score. $t$ denotes the year in which the individuals in the sample became unemployed. This means that individuals in the treatment group are observed from September 2019 until December 2022, while the control group is observed from September 2016 until 2019. The vertical bars represent the 95% confidence intervals which are based on standard errors that are clustered at the individual level.

Source: IEB, BHP, own calculations.

We argue that the initially positive wage effects can be attributed to a positive selection of individuals in the treatment group who quickly find new employment. We assess this selection into employment by using the estimated worker fixed effects from an AKM wage decomposition as the dependent variable in the estimation of Equation 1.[19] Figure B3 in the Appendix shows that, for the period from April to June 2020, which coincides with the positive wage effects in panel (c) of Figure 2, the average AKM worker fixed effect is significantly higher in the treatment group than in the control group (relative to the period before becoming unemployed). This suggests that selection of individuals with higher unobserved wage components among the treatment group is likely to explain the temporary wage increase. Note that the positive and significant impact on the AKM worker fixed effect disappears in later periods which indicates that selection into employment is unlikely to explain wage differences in the longer run.

**Table 2.: The effect of the Covid-19 pandemic on the main outcomes**

| | (1) Earnings | (2) Days in employ-ment | (3) Log wages | (4) Hypo-thetical earnings |
|---|---|---|---|---|
| Average | | | | |
| Treatment period | -69.654*** | -0.573*** | -0.010** | -57.155*** |
| | (4.871) | (0.033) | (0.005) | (3.906) |
| Treatment period ($\hat{\gamma}$) | -451.306*** | -7.024*** | 0.052*** | -519.439*** |
| | (3.824) | (0.025) | (0.004) | (3.090) |
| Pre-treatment period | -4.015** | 0.022*** | -0.001 | -2.100*** |
| | (1.553) | (0.007) | (0.002) | (0.556) |
| Feb-May 2020 | -98.829*** | -0.731*** | 0.003 | -94.035*** |
| | (5.010) | (0.030) | (0.006) | (4.681) |
| Jun-Sep 2020 | -152.317*** | -1.705*** | -0.001 | -147.438*** |
| | (5.412) | (0.042) | (0.005) | (4.876) |
| Oct-Dec 2020 | -123.294*** | -1.300*** | -0.009* | -116.238*** |
| | (5.425) | (0.044) | (0.005) | (4.805) |
| 2021 | -68.779*** | -0.533*** | -0.017*** | -48.833*** |
| | (5.203) | (0.039) | (0.005) | (4.224) |
| 2022 | -19.838*** | 0.000 | -0.011** | -8.318** |
| | (5.398) | (0.039) | (0.005) | (4.132) |
| Cumulative | | | | |
| Treatment period | -4,875.759*** | -40.079*** | -0.719** | -4,000.820*** |
| | (340.975) | (2.289) | (0.332) | (273.441) |
| N | 10,583,520 | 10,583,520 | 6,747,890 | 10,583,520 |

Note: Table 2 shows the estimated coefficients $\hat{\beta}_p$ from Equation 1, with earnings, days in employment, log wages (conditional on employment) as well as hypothetical earnings as the dependent variable using data on individuals who became unemployed in February 2020 and 2017, respectively. The estimation is weighted by the inverse propensity score. The table displays the averaged $\hat{\beta}_p$ for specific time periods, the (treatment) effect averaged over the whole period and the baseline estimate for the control group averaged over the whole period ($\hat{\gamma}$). Standard errors clustered at the individual level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: IEB, BHP, own calculations.

[19] As the AKM effects do not vary over time for a given worker, the individual fixed effects have to be dropped from the model.

To get a deeper understanding of the relative contributions of employment and wages to the earnings losses, we perform two analyses. First, we compare the developments of earnings and "hypothetical" earnings. Second, we conduct a formal decomposition of earnings losses into an employment, a wage and a covariance component based on the corresponding analysis in Schmieder/von Wachter/Heining (2023).

Hypothetical earnings are computed by holding a worker's wage constant at the pre-pandemic value of November $t-1$ (as observed in period $p=-6$) and multiplying it with the observed days in employment for all observations in the treatment and the control group. The closer the coefficient estimates of earnings and hypothetical earnings are, the larger the part of the reduction in earnings among the treatment group that can be ascribed to a reduction in employment (vis-á-vis the control group). In contrast, a gap between the two coefficient estimates would indicate that the reduction in earnings is due to changes in wages. Comparing columns (1) and (4) of Table 2 shows that throughout 2020 the average effects of the pandemic on earnings and hypothetical earnings are close to each other. Specifically, the effect on hypothetical earnings amounts to about 95% of the effect on earnings in each of the three periods in 2020. However, from 2021 onward, the estimated effects start to diverge more substantially: the ratio of the two average effects amounts to about 71% in 2021 and falls to 42% in 2022. Hence, earnings losses are mainly explained by a reduction in employment during 2020, but as the subsequent recovery of employment is much faster than that of earnings, a greater part of the earnings loss can be ascribed to wage losses during the years 2021 and 2022.

The results of the formal decomposition of the earnings loss into the part related to employment, the part related to wages and the part related to the covariance between the two are displayed in Figure B4 in the Appendix. Following Schmieder/von Wachter/Heining (2023), the analysis is restricted to employed individuals, which means that the coefficient estimates of earnings might differ from the development of actual earnings in Figure 2. In 2020, the average earnings loss conditional on employment is smaller ( €40) than when periods of employment and unemployment are considered ( €130). The results of the decomposition indicate that only for a short period immediately after the transition into unemployment a large share of the decrease in earnings is the result of employment losses, while throughout the following period, the loss in earnings is mainly due to wage reductions. The covariance component does not play a substantial role in explaining earnings losses. Therefore, for employed individuals, the development of earnings is mostly explained by wages.

These findings are qualitatively similar to findings from the job displacement literature (see, e.g., Jacobson/LaLonde/Sullivan, 1993; Schmieder/von Wachter/Heining, 2023): while in the short-run the earnings loss after displacement is relatively high, earnings tend to recover in the longer run, though without reaching the earnings level of their counterparts in the control group. This literature has typically found a persistent earnings loss of 10 to 20% even five years after displacement. Although the evolution of the earnings loss is comparable to our study, effect sizes are substantially higher. However, in this literature displaced workers are compared to a group of individuals who are not displaced, whereas in our paper the unemployed of the treatment group are compared to another cohort of unemployed individuals. Furthermore, we document the same driving factors of earnings: in the short run, the observed earnings loss can be attributed to a decrease in employment, while lower wages are more relevant in the longer run.

## 5.2 Wage mechanisms

The previous section has shown that the pandemic led to negative and significant wage effects throughout 2021 and partly also in 2022. This section assesses possible mechanisms behind these wage losses which are based on evidence from the displacement literature: (i) occupational mobility (Huckfeldt, 2022), (ii) transitions to lower-paying occupations, firms or sectors (Schmieder/von Wachter/Heining, 2023), (iii) the loss of firm-specific wage premia (Fackler/Mueller/Stegmaier, 2021), (iv) finding employment further down the occupational wage distribution (Blien/Dauth/Roth, 2021) and (v) downgrading to lower-paying forms of employment, such as part-time employment (Farber, 2017). We evaluate the role of these mechanisms in two ways: first, we estimate Equation 1 separately using a measure of each of these mechanisms as the dependent variable. The results of these estimations are summarised in Table 3, while the event-study plots can be found in Figure B6 in the Appendix. Second, we take into consideration that these measures are correlated and conduct a Gelbach-decomposition (Gelbach, 2016) to quantify the relevance of each measure for the pandemic-induced wage losses.

According to Huckfeldt (2022), the costs of job loss can be primarily ascribed to finding a new job in a lower-quality occupation. In a first step, we therefore assess whether the pandemic has led to increased occupational mobility. Column (1) of Table 3 shows the estimated effects on the probability of being employed in a different 2-digit occupation compared to the occupation prior to unemployment. Over the treatment period, the pandemic has significantly increased the probability of being employed in a different occupation by about 3.1 percentage points per half-month period. This implies that the probability of working in a different occupation is about 6% ($\frac{0.031}{0.497}$) greater among the treatment group than the control group. Moreover, the size of this effect appears to be roughly constant throughout the whole treatment period, which suggests that moving to a different occupation is unrelated to the fact that finding employment is initially more selective among the treatment group (see the discussion in Section 5.1).

To further investigate whether becoming unemployed during worse labour market conditions caused reallocation towards better- or lower-paying occupations, we show results for the time-invariant occupational mean wage in column (2) of Table 3.[20] Since the mean wage does not change over time within the treatment and the control group, the results exclusively reflect differences in the allocation to occupations between the treatment and the control group. Throughout the whole treatment period, individuals in the treatment group tend to be employed in occupations that pay a mean wage that is higher by about 0.8 percentage points, on average, than among individuals in the control group. This effect is relatively large as it implies that the average change in the occupational mean wage is greater by more than 70% ($\frac{0.008}{0.011}$) compared to the change in the control group. Thus, these results suggest that the wage losses experienced by the treatment group are not due to reemployment in lower-paying occupations during the pandemic. To better understand the type of occupations in which individuals find employment again, panel (a) of Figure B19 in the Appendix shows the estimated coefficients with LWA as the dependent variable. The results suggest that treated individuals are more likely to be employed in an occupation with a higher LWA than control individuals. This finding is consistent with evidence presented earlier that a decrease in the ratio of vacancies to job seekers in low-LWA occupations negatively affected the prospects of finding a new job in these occupations.

We evaluate the question of reallocation along two further dimensions in columns (3) and (4) of Table 3, where we show results for the mean wage by firm and by sector. These results are less clear-cut than in the case of occupational mean wages. During the year 2020, the pandemic appears to have led to reallocation towards better-paying firms and sectors. A possible explanation for this is that individuals in the treatment group who find a job are initially positively selected compared to the control group. If there is positive assortative matching between workers and firms in the labour market, as recent evidence suggests (Dauth et al., 2022), one would expect higher-quality workers to be working at better-paying firms or sectors. From 2021 onwards, the point estimates turn negative and, in most cases, statistically significant indicating that the pandemic eventually led to a reallocation towards lower-paying firms and sectors.

Additionally, column (5) of Table 3 shows that treated individuals do not only tend to move to lower-paying firms but also to firms that pay a lower wage premium, especially in the years 2021 and 2022. Overall, the findings in columns (3) to (5) suggest that changes in the composition of firms and sectors which workers are employed in after entering unemployment, may explain part of the negative wage effects in the longer run, whereas

[20] Mean wages for occupations, firms and sectors are computed from the universe of employees using data from 2016 (control group) and 2019 (treatment group), respectively. Choosing these years ensures that mean wages are not affected by pandemic-induced mobility. Columns (1)-(6) of Table B7 in the Appendix show the corresponding results when mean wages are computed from either the year 2016 or 2019 for both groups. For the firm-level mean wage, the estimated coefficients are partly larger in magnitude when mean wages refer to the year 2019, but otherwise the results are very similar to those shown in Table 3.

the pattern of reallocation across occupations appears to be at odds with the estimated wage effects.

The loss of occupation-specific human capital or the deterioration of bargaining positions due to the reduced job opportunities might lead to individuals finding new jobs further down the occupational wage distribution. To evaluate this, we use the universe of employees to, first, compute wage distributions for each 2-digit occupation in a reference year and to, second, identify the percentile of each worker's job in the occupational wage distribution.[21] While the pandemic led to reallocation to occupations with higher mean wages, the results in column (6) of Table 3 show that it also pushed treated individuals further down the wage distribution within occupations. Over the whole treatment period, the average change in the position in the occupational wage distribution is about 1.2 percentiles lower than in the control group per half-month period, while larger effects are observed especially for the year 2021.

The question now arising is whether these losses in the occupational wage rank can be attributed only to those individuals who change their occupation or whether they are also found among individuals who stay in their occupation. To this end, we estimate Equation 1 using log wages, occupational mean wage and occupational wage rank as dependent variables, separately for individuals who are observed in a different occupation in January 2022 compared to their pre-unemployment occupation ("movers") and those who are employed in the same occupation ("stayers").[22] The event-study plots for occupational movers and non-movers are shown in Figure B7 in the Appendix. The wage losses and losses in the occupational wage rank are more pronounced for occupational movers which provides suggestive evidence that this group is driving the negative effect on wages. The positive impact on the occupational mean wage is also larger for movers.

Finally, we investigate whether the negative wage effects might be related to downgrading into lower-paid forms of employment. Column (7) of Table 3 shows results for downgrading into marginal employment. The dependent variable takes the value one if the person was initially regularly employed, but is marginally employed in the current period and zero otherwise. Downgrading into marginal employment is in 2020 and 2021 less likely for the treatment group. By contrast, results in column (8) show that the probability for downgrading into part-time employment (after initially holding a full-time job) is, on average, more likely among the treatment group, especially during the years 2021 and 2022.

[21] The occupational wage distribution is computed by using data from November of the years 2016 and 2019 for the control group and the treatment group, respectively. Results when either 2016 or 2019 is used to compute the distribution for treatment as well as control group can be found in columns (7) and (8) in Appendix Table B7.

[22] We choose the year 2022, as at this point the positive selection into employment among the treatment group, which is observed at the start of the pandemic, is no longer found, while, at the same time, there is a negative and statistically significant wage effect of the pandemic.

**Table 3.: Wage adjustments**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | | | Mean wage | | | | Downgrading | |
| | Occupational mobility | Occupation | Firm | Sector | AKM firm effect | Occupational rank | Marginal | Part-time |
| Average | | | | | | | | |
| Treatment period | 0.031*** | 0.008*** | -0.000 | -0.005*** | -0.003* | -1.178*** | -0.004*** | 0.009*** |
| | (0.003) | (0.002) | (0.004) | (0.002) | (0.002) | (0.165) | (0.001) | (0.003) |
| Treatment period ($\hat{\gamma}$) | 0.497*** | 0.011*** | 0.018*** | 0.005*** | -0.000 | 2.136*** | 0.051*** | -0.014*** |
| | (0.002) | (0.002) | (0.003) | (0.002) | (0.001) | (0.124) | (0.001) | (0.002) |
| Pre-treatment period | 0.001 | 0.000 | 0.001** | 0.001*** | 0.000 | -0.149** | 0.000 | -0.001** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.058) | (0.000) | (0.000) |
| Feb-May 2020 | 0.034*** | 0.008*** | 0.024*** | 0.010*** | 0.008*** | -0.864*** | -0.008*** | 0.005 |
| | (0.004) | (0.002) | (0.004) | (0.003) | (0.002) | (0.186) | (0.002) | (0.003) |
| Jun-Sep 2020 | 0.031*** | 0.008*** | 0.013*** | 0.005* | 0.003 | -1.017*** | -0.007*** | 0.005 |
| | (0.003) | (0.002) | (0.004) | (0.003) | (0.002) | (0.180) | (0.001) | (0.003) |
| Oct-Dec 2020 | 0.031*** | 0.008*** | 0.007* | -0.001 | -0.002 | -1.273*** | -0.008*** | 0.004 |
| | (0.003) | (0.002) | (0.004) | (0.003) | (0.002) | (0.179) | (0.001) | (0.003) |
| 2021 | 0.032*** | 0.009*** | -0.006 | -0.010*** | -0.007*** | -1.740*** | -0.006*** | 0.009*** |
| | (0.003) | (0.002) | (0.004) | (0.003) | (0.002) | (0.177) | (0.001) | (0.003) |
| 2022 | 0.029*** | 0.006** | -0.010** | -0.011*** | -0.006*** | -0.750*** | 0.002 | 0.012*** |
| | (0.003) | (0.002) | (0.004) | (0.003) | (0.002) | (0.186) | (0.001) | (0.003) |
| N | 6,747,890 | 6,715,320 | 6,177,056 | 6,655,717 | 5,754,885 | 6,715,320 | 6,747,890 | 6,747,890 |

Note: Table 3 shows the estimated coefficients $\hat{\beta}_p$ from Equation 1 with occupational mobility, occupational mean log wage, sector mean log wage, firm mean log wage, AKM firm fixed effects, rank in the occupational wage distribution, downgrading from regular into marginal employment as well as downgrading from full-time into part-time employment as dependent variables using data on individuals who became unemployed in February 2020 and 2017, respectively. All variables are conditional on employment. The estimation is weighted by the inverse propensity score. The table displays the averaged $\hat{\beta}_p$ for specific time periods, the (treatment) effect averaged over the whole period and the baseline estimate for the control group averaged over the whole period ($\hat{\gamma}$). Standard errors clustered at the individual level are in parentheses. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.
Source: IEB, BHP, own calculations.

To quantify the contribution of the different variables shown in Table 3 towards the wage loss among the treatment group, we estimate an extended baseline model, where we include these variables as additional control variables (see, e.g., Schmieder/von Wachter/Heining, 2023). We expect that if these variables are relevant for explaining the wage loss among treated individuals, their inclusion in the wage model should reduce the size of the estimated coefficients $\hat{\beta}_p$ in Equation 1. Compared to Schmieder/von Wachter/Heining (2023), we evaluate the relevance of each potential mechanism by conducting a decomposition based on Gelbach (2016) rather than estimating separate models for different combinations of additional control variables. Further details on the decomposition can be found in Section B.5 in the Appendix.

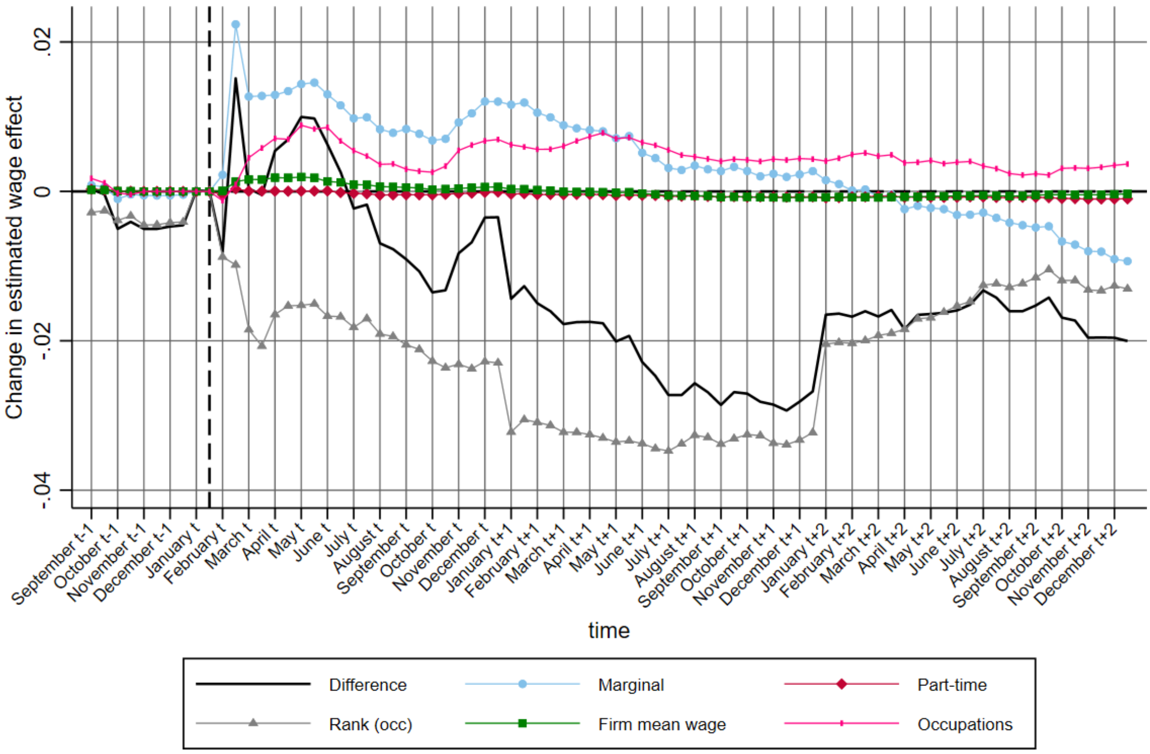
Figure 3 shows the results of the decomposition. The bold black line represents the difference between the estimated coefficients $\hat{\beta}_p$ from the baseline model and the model including the additional control variables. Since the estimated coefficients $\hat{\beta}_p$ from the model with the additional control variables are mainly close to zero, the difference between the estimates is negative. Figure 3 highlights the contribution of the different control variables to this difference. The main insight is that the excess wage loss of the treatment group can almost exclusively be attributed to taking up jobs that are further down the occupational wage distribution. Up to the end of 2021, the contribution of the occupational rank variable is more negative than the estimated difference between the baseline model and the extended baseline model, suggesting that the negative consequences of taking up lower-paying jobs within occupations are partially compensated by moving to occupations that pay more on average and a lower incidence of marginal employment. However, towards the end of 2022, marginal employment also contributes to explaining the estimated wage loss. These findings suggest that changes in jobs *between* occupations tended to contribute positively to the wages of treated individuals (relative to the control group), changes in jobs *within* occupations, captured by movements along the occupation-specific wage distributions, contributed negatively, which is consistent with the results shown in Table 3. Moreover, accounting for part-time work and firm-level mean wages explains relatively little of the wage losses among the treatment group.

## 5.3 Robustness

### 5.3.1. Pre-pandemic weakening of the labour market

The underlying assumption of our empirical DiD model is that the average outcomes for the treatment and the control group would have developed along the same path in the absence of the pandemic. One concern might be that aggregate labour market conditions already started worsening before the start of the Covid-19 pandemic and that therefore the

**Figure 3.: Decomposition of the wage effect**




Note: Figure 3 shows the change in the estimated wage effect when additional control variables are added to the baseline model of Equation 1 (solid line) using data on individuals who became unemployed in February 2020 and 2017, respectively. Moreover, it shows how much each additional control variable (or set of control variables) contributes to this change: downgrading into marginal employment (circles), downgrading into part-time employment (diamonds), rank in the occupational wage distribution (triangles), firm mean wage (squares) and occupation dummies (X). The estimation is weighted by the inverse propensity score. $t$ denotes the year in which the individuals in the sample became unemployed. This means that individuals in the treatment group are observed from September 2019 until December 2022, while the control group is observed from September 2016 until 2019.
Source: IEB, BHP, own calculations.

outcomes of the treatment group would have developed less favourably than those of the control group, even if the pandemic had not occurred. To address this concern, we conduct a linear extrapolation of the treatment effects that are based on a rolling set of different treatment and control groups (see, e.g., Biewen/Fitzenberger/Rümmele, 2022). To do this, we first estimate Equation 1 over a one-year window separately for three cohorts, $c$, of matched individuals who became unemployed during the first half of February 2017, 2018 and 2019 (treatment group) or 2016, 2017 and 2018 (control group). In a second step, we estimate an auxiliary model in which we regress the estimated coefficients for the different outcome variables for the different cohorts, $\hat{\beta}_p^c$, on a constant and a linear time trend. We then compute the linear extrapolation of the estimates $\hat{\beta}_p^c$ for the pandemic cohort 2020. This yields an estimate of the counterfactual scenario that captures the gradual worsening of labour market conditions. Finally, these values are compared to the coefficient estimates for one year only from estimating Equation 1 using the newly unemployed individuals from the year 2020 (treatment group) and 2019 (control group). A detailed description of the extrapolation can be found in Appendix Section B.6.

The results of the extrapolation are presented in Figure B8 in the Appendix. The main finding for earnings and employment is that although part of the negative effects can be ascribed to a worsening of the labour market that started before the onset of the pandemic, the extrapolation does not pick up the sharp drop that occurred shortly after the start of the pandemic. For wages, the estimated counterfactual is relatively close to the estimated effects of the pandemic. However, as can be seen in panel (c) of Figure 2, the wage effects are negative mainly during the years 2021 and 2022.

### 5.3.2. Short-time work

Short-time work schemes were heavily used in Germany to buffer the pandemic's adverse impact on employment (Giupponi/Landais, 2022). Since the incidence of short-time work was minimal in the years before the outbreak of the Covid-19 pandemic, one concern is that the estimated differences in the employment trajectories of the treatment and control group are related to the use of short-time work schemes. For example, the negative earnings and wage effects might reflect the fact that short-time work schemes typically do not provide a full compensation of the wage loss due to reduced working hours.

While we have no information about whether and to what extent an individual worker was placed on short-time work, we have information about whether establishments used short-time work at a monthly level. Consistent with our expectation that short-time work is far less relevant for the control group than the treatment group, we find that 28% of the individuals in the treatment group were employed at least once at a firm that used short-time work over the whole treatment period compared to only 2% of individuals in the control group.

To assess the impact of short-time work on our results, we estimate Equation 1 using only individuals who were never employed at an establishment that used short-time work. As shown in Figure B9 in the Appendix, we also find negative effects on all three outcomes – earnings, employment and wages – and qualitatively similar developments among those individuals who never worked at establishments that used short-time work schemes. This implies that the less favourable development of the employment trajectories among the individuals of the treatment group are not driven by the use of short-time work.

### 5.3.3. Other robustness checks

**Sample.** We also estimate the effects on earnings, employment and wages using samples that are based on different requirements concerning the prior duration of employment, the later transition into unemployment in the second half of February and a longer duration in unemployment. Figures B10, B11 and B12 in the Appendix show similar results as in the baseline specification.

**Weighting.** Appendix Figure B13 indicates that the effects on earnings, employment and wages are qualitatively similar but larger in magnitude when no weights are used. Moreover, the figure shows that when smaller sets of control variables are included in the propensity score estimation, the results fall between those obtained from models without weights and those from the baseline specification.

**Inflation adjustment.** The consumer price index that we use for the inflation adjustment of wages increased considerably in 2022 compared to the previous years. Moreover, no comparable increase applies to the control group. Applying the actual consumer price index leads to substantially larger negative effects on wages for the treatment group, but these are mainly due to a reduction in purchasing power. For this reason, we estimate the baseline model using the index from the year 2020 (2017) for all years in which the treatment (control) group is observed. We show the results on wages when the actual consumer price index is used in Figure B14 in the Appendix.

# 6 Occupation-specific effects of the pandemic

Some occupations were more affected by the Covid-19 pandemic than others. Low-LWA occupations, in particular, were less suited to be carried out under lockdown conditions and, therefore, provided fewer opportunities for unemployed individuals to find a new suitable job. To uncover effect heterogeneity by the LWA of a worker's previous occupation, we use LWA as a continuous treatment variable. To support the validity of this approach, we show that treated and control individuals are well-balanced along the occupational LWA distribution (see Table A5 in the Appendix). Moreover, we also estimate our baseline model (Equation 1) separately for workers who used to be employed in occupations with different degrees of LWA. The event-study plots can be found in Figure B15 in the Appendix. Crucially, the ordering of the effect sizes is consistent with the results that we obtain from estimating Equation 2.

## 6.1 Earnings, employment and wages

Table 4 summarises the results of estimating Equation 2 for the three main outcomes in two different ways. Odd-numbered columns show $\hat{\phi}_p$, i.e. the change in the effect of the pandemic that is associated with a reduction in the LWA of the occupation that a person was initially employed in by 0.1 units. To better illustrate its magnitude, even-numbered columns report the additional effect associated with a reduction in LWA by 0.1 units relative to the effect estimated for individuals who used to be employed in an occupation with a mean LWA ($\frac{\hat{\phi}_p}{\hat{\beta}_p}$).[23] The event-study plots showing $\hat{\phi}_p$ can be found in Figure B16 in the Appendix.

Individuals who used to be employed in occupations with a lower LWA experience a long-lasting and statistically significant additional earnings loss. According to column (1) of Table 4, a reduction in LWA by 0.1 units is predicted to increase earnings losses, on average, by €7.91 per half-month or by about €553.86 over the whole treatment period. Column (2) shows that this excess earnings loss amounts to about 11.8% compared to individuals who used to be employed in an occupation with mean LWA.

In contrast to long-lasting additional earnings losses over the whole period, the additional effect on employment is more pronounced at the onset of the pandemic and then diminishes in the longer run. In particular, column (3) of Table 4 shows that the additional employment loss associated with a reduction in LWA by 0.1 units is 0.08 days per half-month between February and May 2020. This increases to 0.14 days per half-month between June and September 2020, before decreasing in magnitude until the end of the treatment period. Over the whole treatment period the additional employment loss amounts to 6.45 days which translates to an excess loss of approximately 17.1% compared to individuals who used to work in an occupation with mean LWA. Similar, to the results for the whole sample, the additional employment reduction can mainly be explained by a shift to unemployment (see Table B9 in the Appendix).

Column (5) of Table 4 shows that individuals who used to work in occupations with a lower LWA also experience an additional wage loss. On average, a reduction in LWA by 0.1 units leads to an additional reduction of 0.5 percentage points per half-month among employed workers which corresponds to an increase of almost 60% compared to the pandemic effect on workers who used to be employed in occupations with a mean LWA (column (6)). The additional wage loss is statistically significant until the end of 2021. In contrast to the findings on the overall pandemic effect on wages (see Section 5.1), the additional effect is always negative.

[23] We provide descriptive statistics of the LWA variable in Table A3 in the Appendix which can be used to compute the size of the additional effect for other changes in the LWA of a worker's initial occupation, such as the standard deviation or the inter-quartile range.

**Table 4.: Effect heterogeneity by LWA: main outcomes**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Earnings | | Days in employment | | Log wages | | Hypothetical earnings | |
| | absolute | relative | absolute | relative | absolute | relative | absolute | relative |
| Average | | | | | | | | |
| Treatment period | -7.912*** | 0.118 | -0.092*** | 0.171 | -0.005*** | 0.593 | -5.881*** | 0.106 |
| | (1.795) | | (0.011) | | (0.002) | | (1.433) | |
| Pre-treatment | 0.923* | -0.207 | -0.001 | -0.039 | -0.000 | 0.007 | 0.239 | -0.108 |
| | (0.548) | | (0.002) | | (0.001) | | (0.204) | |
| Feb-May 2020 | -3.186*** | 0.033 | -0.078*** | 0.113 | -0.008*** | -1.027 | -2.656 | 0.029 |
| | (1.871) | | (0.011) | | (0.002) | | (1.751) | |
| Jun-Sep 2020 | -6.486*** | 0.043 | -0.136*** | 0.083 | -0.005*** | -2.353 | -5.549*** | 0.038 |
| | (2.073) | | (0.015) | | (0.002) | | (1.8609) | |
| Oct-Dec 2020 | -5.723*** | 0.047 | -0.109*** | 0.088 | -0.005*** | 0.727 | -4.460** | 0.039 |
| | (2.080) | | (0.015) | | (0.002) | | (1.828) | |
| 2021 | -7.500*** | 0.115 | -0.070*** | 0.141 | -0.006*** | 0.421 | -4.046** | 0.086 |
| | (1.982) | | (0.014) | | (0.002) | | (1.606) | |
| 2022 | -4.668** | 0.266 | -0.033** | -2.038 | -0.002 | 0.222 | -3.115* | 0.458 |
| | (2.064) | | (0.014) | | (0.002) | | (1.586) | |
| Cumulative | | | | | | | | |
| Treatment period | -553.858*** | 0.118 | -6.452*** | 0.171 | -0.316*** | 0.593 | -411.655*** | 0.106 |
| | (125.668) | | (0.792) | | (0.109) | | (100.288) | |
| N | 10,583,520 | | 10,583,520 | | 6,747,890 | | 10,583,520 | |

Note: Table 4 shows the estimated coefficients $\hat{\phi}_p$ from Equation 2 with earnings, days in employment, log wages and hypothetical earnings as dependent variables using data on individuals who became unemployed in February 2020 and 2017, respectively. The estimation is weighted by the inverse propensity score. The table displays the averaged $\hat{\phi}_p$ and the ratio $\frac{\hat{\phi}_p}{\hat{\beta}_p}$ for specific time periods and the (treatment) effect averaged over the whole period. Standard errors clustered at the individual level are in parentheses. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.
Source: IEB, BHP, own calculations.

Finally, column (7) of Table 4 shows the results for hypothetical earnings for which wages are held constant at the level that is observed in November of the year $t-1$. Over the whole treatment period, the additional effect on hypothetical earnings is €5.88 per half-month as opposed to €7.91 for actual earnings. This indicates that the major part of the additional earnings loss is explained by the additional loss in employment. However, employment does not exclusively drive the additional earnings loss and, in particular, the part that can be ascribed to an additional reduction in employment becomes smaller during 2021 and 2022 (both absolutely and proportionately).

Overall, these results provide further evidence that the impact of becoming unemployed on a worker's subsequent career depends on the available job opportunities. Low-LWA occupations experienced a greater decline in the vacancies to job seekers ratio, thereby limiting the prospects of finding a new job. As hypothesised, we find more detrimental effects on the labour market outcomes of workers from those occupations. As a final illustration, we refer to occupations in gastronomy which are among those most affected by the Covid-19 pandemic (with a LWA of approximately 0.3). Over the whole treatment period, individuals from those occupations, on average, suffered an additional earnings loss of €1,600, an additional employment reduction of about 20 days and an additional wage penalty of 1.5 percentage points compared to individuals from occupations with the mean LWA.

## 6.2 Wage mechanisms

To shed further light on the mechanisms behind the additional wage reduction, we replicate the analysis from Section 5.2 and assess the relevance of the same set of variables as in Table 3 for the excess wage effect. The results for the absolute and the relative effects are summarised in Table 5.

According to the results in column (1), a reduction in the LWA of an individual's initial occupation is associated with a higher probability of subsequently working in a different occupation. Specifically, we find that a reduction in LWA by 0.1 units increases the probability of working in a different occupation by a further 0.7 percentage points per half-month, on average. This represents a proportional increase of the pandemic's effect by about 26% compared to individuals who used to work in an occupation with the mean LWA. Panel (b) of Figure B19 in the Appendix shows that the pandemic additionally increases the probability of being employed in an occupation with a higher LWA as before for treated individuals from occupations with a 0.1 lower LWA as compared to treated individuals with a mean LWA.

Similar to the findings for the overall effect on occupational mean wages, the additional effect is also positive and mostly significant at the 10% level. However, treated individuals also experience a greater reduction in the rank of their new job in the occupation-specific wage distribution than individuals from high-LWA occupations. In particular, column (11) of Table 5 shows that a reduction in LWA by 0.1 units leads to an excess loss in the wage rank of 0.29 percentiles, on average, which represents an additional loss of 27% relative to the loss of the rank of treated individuals with a mean LWA.

Columns (5) and (6) show that individuals from low-LWA occupations tend to find new job in firms that pay significantly lower mean wages than individual from mean-LWA occupations. However, we do not find a statistically significant effect in terms of the estimated AKM firm effect (columns (9) and (10)) or sectoral mean wages (columns (7) and (8)). Columns (13) and (14) indicate that a reduction in the occupational LWA leads to a significant increase in the probability of being marginally employed. Differences in LWA are, in contrast, not related to differences in the probability of part-time employment, as evidenced by the absence of statistically significant effects in columns (15) and (16).

To assess the relevance of these above factors for the excess wage effect associated with having been employed in an occupation with lower LWA, we again apply the Gelbach-decomposition. To ensure a better comparability with the results from the pooled model, we conduct the decomposition separately for individuals who used to be employed in low-LWA (below the 25% quantile), medium-LWA (between the 25% and 75% quantile) and high-LWA (above the 75% quantile) occupations, respectively. For each of these groups, we first separately estimate Equation 1. Next, we extend these models by including the outcomes from Table 5 as additional control variables and then compute the contribution of each of these outcomes to the change in the estimated wage effects. The results are shown in Figure B18 in the Appendix.

For low-LWA occupations (panel (a)), we find that, until the end of 2021, the negative wage effects can be almost exclusively ascribed to finding a new job that is further down the occupational wage distribution. From the end of 2021, a higher probability of working in marginal employment and occupational mobility also contribute to the negative wage effects. The results for medium-LWA occupations (panel (b)) resemble the results from the decomposition across all individuals: while individuals tend to find jobs that a further down the occupational wage distribution, these effects are partly compensated by a lower probability of working in marginal employment and by moving to occupations that pay higher average wages. Finally, individuals who used to work in high-LWA occupations experience a more favourable development of wages than the corresponding control group. Panel (c) shows that this is due to a lower probability of being marginally employed, the observed pattern of occupational mobility and – in the first year of the pandemic – the taking up of new jobs further up the occupational wage distribution.

## 6.3 Robustness

### 6.3.1. Parallel trends assumption

In Section 4.2, we discussed that the estimation of Equation 2 requires stricter parallel trend assumptions. To assess the non-occurrence of parallel trends, first, the continuous treatment variable of Equation 2 is replaced by a dummy that divides all individuals into low- and high-LWA categories at certain thresholds of the LWA distribution (namely the 33%- and the 75%-quantile) as in Bauernschuster/Hener/Rainer (2015). Figure B20 in the Appendix provides evidence that for all three main outcomes – earnings, employment and wages – there are mostly no significant deviations from zero before the transition into unemployment in February $t$ independent of the threshold. Second, Equation 1 is estimated for the following different subsets of the LWA distribution: low-LWA, medium-LWA as well as high-LWA. Doing so provides insights into the behaviour of trends for those different quantiles. The results which are presented in Appendix Figure B15 confirm that there seems to be no diverging trends for all three groups.

**Table 5.: Effect heterogeneity by LWA: wage adjustments**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | Mean wage | | | | | | | | | | Downgrading | | | |
| | Occupational mobility | | Occupation | | Firm | | Sector | | AKM firm effect | | Occupational rank | | Marginal | | Part-time | |
| | Abs. | Rel. | Abs. | Rel. | Abs. | Rel. | Abs. | Rel. | Abs. | Rel. | Abs. | Rel. | Abs. | Rel. | Abs. | Rel. |
| Average | | | | | | | | | | | | | | | | |
| Treatment period | 0.007$^{***}$ | 0.264 | 0.001$^{*}$ | 0.161 | -0.002$^{**}$ | -2.055 | -0.001 | 0.111 | -0.001 | 0.258 | -0.287$^{***}$ | 0.272 | 0.001$^{***}$ | -0.238 | 0.001 | 0.070 |
| | (0.001) | | (0.001) | | (0.001) | | (0.001) | | (0.001) | | (0.054) | | (0.000) | | (0.001) | |
| Pre-treatment period | 0.000 | 0.071 | 0.000$^{**}$ | 0.646 | 0.000$^{**}$ | 0.390 | 0.000 | 0.152 | 0.000 | 0.846 | -0.006 | 0.043 | -0.000 | -0.107 | -0.000 | 0.108 |
| | (0.000) | | (0.000) | | (0.000) | | (0.000) | | (0.000) | | (0.019) | | (0.000) | | (0.000) | |
| Feb-May 2020 | 0.006$^{***}$ | 0.202 | 0.001$^{*}$ | 0.181 | -0.003$^{**}$ | -0.105 | 0.000 | 0.019 | -0.001 | -0.100 | -0.350$^{***}$ | 0.493 | 0.002$^{***}$ | -0.226 | 0.001 | 0.307 |
| | (0.001) | | (0.001) | | (0.001) | | (0.001) | | (0.001) | | (0.062) | | (0.001) | | (0.001) | |
| Jun-Sep 2020 | 0.007$^{***}$ | 0.264 | 0.001$^{*}$ | 0.164 | -0.002$^{*}$ | -0.172 | -0.000 | -0.063 | -0.001$^{*}$ | -0.341 | -0.292$^{***}$ | 0.321 | 0.001$^{**}$ | -0.130 | 0.001 | 0.104 |
| | (0.001) | | (0.001) | | (0.001) | | (0.001) | | (0.001) | | (0.060) | | (0.000) | | (0.001) | |
| Oct-Dec 2020 | 0.008$^{***}$ | 0.291 | 0.001 | 0.135 | -0.002 | -0.217 | -0.000 | 0.202 | -0.001$^{*}$ | 0.698 | -0.280$^{***}$ | 0.240 | 0.001 | -0.076 | 0.000 | 0.046 |
| | (0.001) | | (0.001) | | (0.001) | | (0.001) | | (0.001) | | (0.060) | | (0.000) | | (0.001) | |
| 2021 | 0.008$^{***}$ | 0.262 | 0.001$^{*}$ | 0.134 | -0.003$^{**}$ | 0.661 | -0.001 | 0.120 | -0.001 | 0.145 | -0.359$^{***}$ | 0.227 | 0.001$^{**}$ | -0.182 | 0.000 | 0.056 |
| | (0.001) | | (0.001) | | (0.001) | | (0.001) | | (0.001) | | (0.059) | | (0.000) | | (0.001) | |
| 2022 | 0.006$^{***}$ | 0.250 | 0.001 | 0.140 | -0.003$^{**}$ | 0.424 | -0.001 | 0.082 | -0.001 | 0.112 | -0.173$^{***}$ | 0.258 | 0.001$^{**}$ | 0.833 | 0.000 | 0.036 |
| | (0.001) | | (0.001) | | (0.001) | | (0.001) | | (0.001) | | (0.063) | | (0.000) | | (0.001) | |
| N | 6,715,320 | | 6,715,320 | | 6,177,056 | | 6,655,717 | | 5,754,885 | | 6,715,320 | | 6,747,890 | | 6,747,890 | |

Note: Table 5 shows the estimated coefficients $\hat{\phi}_p$ from Equation 2 with occupational mobility, occupational mean log wage, sector mean log wage, firm mean log wage, AKM firm fixed effects, rank in the occupational wage distribution, downgrading from regular into marginal employment as well as downgrading from full-time into part-time employment as dependent variables using data on individuals who became unemployed in February 2020 and 2017, respectively. All variables are conditional on employment. The estimation is weighted by the inverse propensity score. The table displays the averaged $\hat{\phi}_p$ and the ratio $\frac{\hat{\phi}_p}{\hat{\beta}_p}$ for specific time periods and the (treatment) effect averaged over the whole period. Standard errors clustered at the individual level are in parentheses. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.
Source: IEB, BHP, own calculations.

### 6.3.2. Confounding variables

There might be the concern that the documented differences in effect size along the LWA distribution are actually due to heterogeneities in other variables. Following this thought, Table A5 in the Appendix shows that there are differences between individuals who were previously employed in occupations with different degrees of LWA. In particular, individuals from low-LWA occupations are more likely to be male, low-skilled and to have previously worked in smaller firms. In order to investigate whether the documented heterogeneous effects of LWA are due to differences in these variables, interaction terms of gender, skill level and firm size (measured at the matching point in November $t-1$) with the treatment dummy, the event time and a combination of both are additionally included in Equation 2. Figure B21 in the Appendix shows that our results hold even with these additional control variables. Thus, we conclude that the results in Section 6.1 represent genuine heterogeneity across occupations.

# 7 Conclusion

This paper examines the impact of an exogenous shock to job opportunities on worker careers. To identify these effects, we use administrative social security data from Germany and compare the employment trajectories of individuals who became unemployed shortly before the start of the Covid-19 pandemic to a control group of individuals who entered unemployment three years earlier. Our identification strategy is based on the assumption that the emergence of the pandemic (and the subsequent containment measures) constituted an unforeseen event, which triggered a sudden worsening of the prospects of finding a new suitable job, as evidenced by a pronounced drop in the ratio of vacancies to job seekers.

Consistent with being exposed to worse labour market conditions, individuals in the treatment group experience significantly more adverse effects on their labour market outcomes. Our difference-in-differences analysis shows that between February 2020 and December 2022 treated individuals realise a cumulated earnings loss that is about 15% larger (approximately €4,900) than for the control group. In the short run, earnings losses are driven by a reduction in employment along the extensive margin, while, in the longer run, lower wages earned upon reemployment explain an increasing part of the earnings loss. We analyse the mechanisms underlying the wage losses and find that while individuals in the treatment group are more likely to switch to occupations that pay a higher mean wage, they also more often take up jobs that are further down the occupation-specific wage distribution. Falling down the within-occupation job ladder therefore appears to be the main reason for the excess wage penalty experienced by the treatment group.

The nature of the Covid-19 pandemic was such that it reduced the job prospects more in some occupations than in others depending on their suitability to be carried out under lockdown conditions. We use the variation in exposure between occupations to further substantiate the finding that the development of employment trajectories after becoming unemployed depend on the available job opportunities. Individuals who used to work in occupations that either did not allow for working from home or were not systemically relevant experienced a significantly greater adverse effect on their labour market outcomes. We also show that similar mechanisms, especially finding new jobs that are further down the occupation-specific wage distribution, explain the greater wage losses experienced by individuals who used to be employed in occupations with a lower lockdown work ability.

This paper concentrates on individuals who became unemployed shortly before the pandemic and who were thus exposed to a sudden worsening of their job opportunities. It appears reasonable, however, that our findings are also relevant for understanding the effects on all individuals who searched for a new job at the start of the pandemic as these individuals will also have been exposed to an environment in which the prospects of finding a new job had worsened especially for individuals from low-LWA occupations. This paper therefore provides insights that potentially contribute to a better understanding of the shock that the Covid-19 pandemic constituted for workers' careers: which outcomes were affected, through which channels and how the size of these effects is depended on a worker's occupation.

A large literature has shown that exposure to (temporary) shocks can have long-lasting adverse effects on individuals' labour market outcomes. The Covid-19 pandemic came as a sudden and severe shock and therefore provides an ideal setting for analysing the consequences of the associated deterioration of labor market conditions using methods that are, for example, also used in the displacement literature. Hence, the results of this paper are not only relevant for understanding the consequences of the Covid-19 pandemic. As we argue in the paper, the pandemic exogenously reduced the prospects of finding a new job, but the extent to which job finding prospects changed depended on a worker's previous occupation. The pandemic thus provides an environment in which the importance of job opportunities for the severity of an adverse economic shock can be assessed. Therefore, our findings are potentially also relevant for understanding the consequences of economic shocks more generally.

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# Appendix for Online Publication

The material contained in this document represents an Appendix to the paper "The impact of the Covid-19 pandemic on worker careers: do different job opportunities matter?". It provides supplementary information related to the data and to empirical results.

# A Data appendix

## A.1 Data preparation

This section provides further details about how the sample in this paper is constructed. The empirical analysis uses administrative microdata based on the Integrated Employment Biographies (IEB), which are provided by the Institute of Employment Research (IAB), the research institute of the German Federal Employment Agency. The IEB cover the universe of labour market participants in Germany (with the exception of the self-employed, civil servants and soldiers). In the following, we describe how a panel data set with half-month observations is created.

Two challenges arose during the data preparation: First, the challenge of parallel spells and, second, the challenge of missing spells. The challenge of parallel spells refers to the fact that at any point in time a person can have more than one record in the IEB data. For example, individuals can have more than one job at the same time or they may receive transfer payments during unemployment, which creates two spells for the same time period. To keep only one observation per period for each individual, several decision rules have been developed. In doing so, this paper applies (most of) the decision rules suggested by Dauth/Eppelsheimer (2020). In particular, this means that, in a first step, all parallel spells which do not include information on employment or unemployment (such as participating in a labour market programme or receiving financial transfers) are excluded. The cases in which there are parallel unemployment and/or employment spells are more difficult. Here the paper proceeds as follows: first, all spells with information that do not contain the main (regular and marginal) employment or the main unemployment information ("unemployed and searching for work") were dropped. Second, spells containing more information on other observable characteristics, e.g. vocational degree, establishment or occupation, (meaning less missings) were kept. Third, spells with a longer duration were included. However, there are two exceptions: first, if there is an unemployment spell parallel to a marginal employment spell, the unemployment spell is kept and, second, if there is a transition of an employment period to an unemployment period, where both spells are overlapping at some time of the transition, the overlapping employment spell is dropped. Regarding the case of two parallel employment spells with the same duration, the spell with lower daily wages is excluded. In the end, if all of the described rules cannot be applied, one of the parallel spells is randomly chosen.

In contrast to the parallel spells, the challenge of missing spells means that individuals might not have an observed spell for some periods. This happens, for instance, if the individual has left the labour market, becomes self-employed or retired. Those missing spells are filled with “artificial” spells which contain no information but ensure that every individual has one observation for each time period.

After these data preparation steps, the treatment and control group are defined. For being in either group, certain criteria had to be fulfilled: first, individuals had to be registered unemployed in February 2017 or in February 2020. Registered unemployed means that individuals had to be registered as unemployed and are searching for a job. Individuals who have been registered as unemployed in 2017 as well as in 2020 are only considered in the control group. The same rule is applied for individuals who became unemployed in the first as well as in the second half of February: they are only counted in the first half. Moreover, there is no restriction on the duration of the unemployment spell, which indicates that individuals who find a new job after one day in unemployment are still part of the sample. Second, individuals in the sample had to be employed at least until the 31$^{st}$ of January before becoming unemployed. This means that all individuals whose employment spell ends before the 31$^{st}$ of January were excluded, whereas all individuals whose employment spell ends on some day in February are in the sample. Third, individuals in the sample have to be employed on every day at least since November of the previous year. Before that date, they are allowed to have any possible labour market status. Fourth, individuals in the sample had to be employed in the same establishment and same occupation during the employment period from November to February. Thus, individuals who switch either their establishment or their occupation or both during that time period were excluded from the sample.

Applying these restrictions yields a sample of 172,631 individuals in total: 132,797 in the first half (treatment and control group) and 39,834 in the second half of February (treatment and control group). Due to weighting procedure, some individuals do not receive a weight, which reduces the sample to 132,294 in the first half and 33,308 in the second half of February.

## A.2 Descriptive statistics of the unemployed of the second half of February

The descriptive statistics of the individuals who became unemployed in the second half of February are displayed in Table A1. In contrast to individuals who became unemployed in the first half of February (see Table 1 in the paper), they are, on average, slightly younger, more often low skilled, earn less, more often have foreign nationality and are more likely to have been unemployed before.

**Table A1.: Descriptive statistics: second half of February**

| | (1) Treatment | (2) Control (weighted) | (3) Control (unweighted) | (4) Standard. diff. |
|---|---|---|---|---|
| **Socio-demographic characteristics (at the time of matching)** | | | | |
| Age | 37.460 | 37.294 | 37.413 | 0.014 |
| | (12.001) | (11.677) | (11.773) | |
| Male (fraction) | 0.638 | 0.639 | 0.620 | -0.002 |
| | (0.481) | (0.480) | (0.485) | |
| Foreign (fraction) | 0.302 | 0.306 | 0.215 | -0.009 |
| | (0.459) | (0.461) | (0.411) | |
| Low skilled (no completed apprenticeship, fraction) | 0.196 | 0.195 | 0.171 | 0.002 |
| | (0.397) | (0.397) | (0.376) | |
| Middle skilled (completed apprenticeship, fraction) | 0.589 | 0.584 | 0.679 | 0.012 |
| | (0.492) | (0.493) | (0.467) | |
| High skilled (tertiary education, completed) | 0.108 | 0.108 | 0.097 | -0.000 |
| | (0.310) | (0.310) | (0.297) | |
| **Current employment (at the time of matching)** | | | | |
| Current wage | 65.006 | 60.636 | 60.902 | 0.119 |
| | (38.245) | (35.245) | (34.085) | |
| Current earnings | 975.089 | 909.538 | 913.524 | 0.119 |
| | (573.670) | (528.681) | (511.271) | |
| In regular employment (fraction) | 0.891 | 0.887 | 0.888 | 0.012 |
| | (0.312) | (0.317) | (0.316) | |
| In full-time employment (fraction) | 0.647 | 0.638 | 0.660 | 0.019 |
| | (0.478) | (0.481) | (0.474) | |
| Very small establishment (less than 10, fraction) | 0.205 | 0.207 | 0.211 | -0.004 |
| | (0.404) | (0.405) | (0.408) | |
| Small establishment (10-49, fraction) | 0.322 | 0.327 | 0.292 | -0.011 |
| | (0.467) | (0.469) | (0.455) | |
| Medium-sized establishment (50-249, fraction) | 0.304 | 0.298 | 0.292 | 0.013 |
| | (0.460) | (0.457) | (0.455) | |
| Large establishment (more than 250, fraction) | 0.167 | 0.166 | 0.173 | 0.002 |
| | (0.373) | (0.372) | (0.378) | |
| Estimated AKM firm effect | -0.222 | -0.244 | -0.240 | 0.088 |
| | (0.247) | (0.247) | (0.244) | |
| **Employment biography** | | | | |
| Work experience | 9.688 | 9.455 | 10.139 | 0.027 |
| | (8.715) | (8.395) | (8.312) | |
| Tenure in current establishment | 1.751 | 1.663 | 1.805 | 0.028 |
| | (3.307) | (3.046) | (3.279) | |
| Tenure in current occupation | 4.249 | 4.186 | 4.416 | 0.011 |
| | (5.815) | (5.733) | (5.853) | |
| Number of job changes | 3.290 | 2.988 | 3.174 | 0.080 |
| | (3.922) | (3.619) | (3.657) | |
| Being unemployed before (fraction) | 0.818 | 0.806 | 0.836 | 0.031 |
| | (0.386) | (0.396) | (0.370) | |
| Employed in manufacturing sector (fraction) | 0.128 | 0.121 | 0.113 | 0.021 |
| | (0.334) | (0.326) | (0.317) | |
| Employed in service sector (fraction) | 0.426 | 0.437 | 0.443 | -0.021 |
| | (0.495) | (0.496) | (0.497) | |
| Estimated AKM worker effect | 4.269 | 4.281 | 4.288 | -0.041 |
| | (0.306) | (0.298) | (0.288) | |
| N | 18,331 | 14,977 | 14,977 | |

Note: Table A1 reports descriptive statistics of the treatment and the control group. Columns (1) to (3) show the mean value and standard deviation (in parentheses) of individual characteristics that are measured at the first half of November $t-1$ (the point for the weighting). Column (4) reports the standardised difference between columns (1) and (2), which is defined as $\Delta_X = \left(\bar{X}_1 - \bar{X}_0\right) / \left((S_1^2 + S_0^2)/2\right)^{0.5}$, where $\bar{X}_w$ is the sample mean of the treated ($w = 1$) or (weighted) control ($w = 0$) individuals and $S_w^2$ are the respective sample variances. Note that the observations for the AKM worker and firm fixed effects are smaller than the reported number of observations. Not all shown characteristics, such as current wages, establishment size or AKM firm effects, are used in propensity score weighting. For the full list of propensity score weighting variables see Table A2.
Source: IEB, own calculations.

## A.3 Inverse propensity score weighting

The inverse propensity score weighting (IPW) approach aims at making the treatment group comparable to the control group in terms of observable characteristics (see, e.g., Wooldridge, 2007). Comparability is achieved by placing lower weights on outcomes of control individuals that are over-represented and by applying higher to the outcomes of those that are under-represented in terms of observable characteristics in either group. The weights are determined by the propensity score, or the probability of belonging to the treatment group ($D = 1$), given observed covariates $x$: $p(x) = P(D = 1|X = x)$. While treated observations receive a weight of one, weights for the control group are given by $\frac{\hat{p}(x_i)}{1-\hat{p}(x_i)}$, where $\hat{p}(x_i)$ is the predicted probability of belonging to the treatment group conditional on observed characteristics $x_i$.

The individual probability of belonging to the treatment group is estimated by means of a logit model, given a detailed set of observed individual, job and establishment characteristics. These variables are measured during the first half of November, so that their levels are not affected by future treatment. In particular, the following matching variables are chosen: male (dummy), skill (dummy for three qualification levels), age (dummies for quartiles), foreign (dummy), wage growth between the years 2016 and 2017 for the control group and between 2019 and 2020 for the treatment group (dummy for deciles), type of current employment (dummies for marginal or regular employment as well as part-time or full-time employment), establishment size (dummies for four categories), experience (dummies for quartiles), tenure in current occupation (dummy for quartiles), duration in previous unemployment (dummy for quartiles), establishment change before the matching point (dummy) and sector (dummies for 3 sector classification). The estimation of the propensity score is conducted separately for each 2-digit occupation.

In order to test for balance, we compare the differences in means after weighting between individuals of the treatment and the control group. The balancing tests for the baseline specification can be found in Table A2. The table shows the mean values of various characteristics that were used for the weighting for the treatment group (column (1)), the unweighted control group (column (2)) and the weighted control group (column (3)). In addition, the p-value of a standard t-test (column (4)) as well as the standardised difference between the treatment and the (weighted) control group are displayed. The standardised differences in covariate means ($\Delta_X$) between treated and weighted control observations can be interpreted as a scale-free measure of balancing (see e.g., Austin, 2011).[24] Since there is no universally agreed criterion for how small the standardised difference must be to

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

provide balance, we apply the rule of thumb of $\Delta_X < |0.1|$ as suggested by Austin (2011). Without weighting, the difference between treatment and control group are already relatively small. After applying weights, the differences are even smaller and statistically insignificant in each case (in terms of both p-values and standardised differences). However, differences in two quartiles of unemployment experience are still significant at conventional significance levels, but the standardised difference is smaller than 0.1 which does not indicate an economically significant difference between the treatment and control group. Overall, the sample appears to be balanced.

**Table A2.: Balancing table**

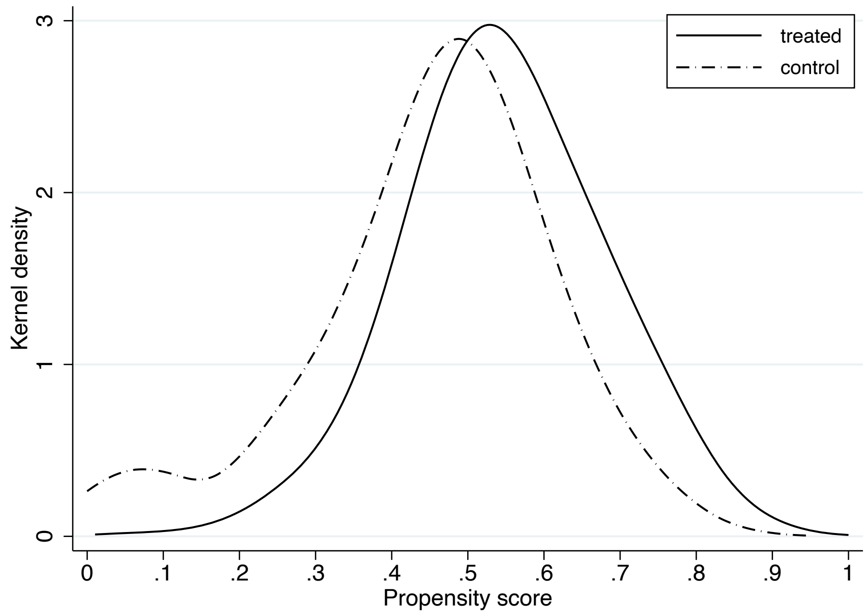
| | Treatment | Control | | Difference | |
|---|---|---|---|---|---|
| | | Unweighted | Weighted | P-value | Standardised |
| | (1) | (2) | (3) | (4) | (5) |
| **Worker variables (contemporaneous)** | | | | | |
| Male | 0.612 | 0.592 | 0.613 | 0.718 | -0.002 |
| Low skilled | 0.153 | 0.132 | 0.154 | 0.624 | -0.003 |
| Middle skilled | 0.594 | 0.673 | 0.592 | 0.385 | 0.005 |
| High skilled | 0.172 | 0.148 | 0.171 | 0.793 | 0.001 |
| Missing skill | 0.082 | 0.046 | 0.084 | 0.205 | -0.007 |
| Age (1st quartile) | 0.242 | 0.234 | 0.242 | 0.805 | 0.001 |
| Age (2nd quartile) | 0.257 | 0.244 | 0.258 | 0.595 | -0.003 |
| Age (3rd quartile) | 0.245 | 0.257 | 0.245 | 0.891 | -0.001 |
| Age (4th quartile) | 0.256 | 0.265 | 0.255 | 0.671 | 0.002 |
| Foreign nationality | 0.244 | 0.180 | 0.246 | 0.412 | -0.005 |
| Missing nationality | 0.001 | 0.001 | 0.001 | 0.528 | -0.003 |
| **Worker variables (employment biography)** | | | | | |
| 2019(16)/2020(17) wage growth (1st decile) | 0.104 | 0.096 | 0.104 | 0.830 | 0.001 |
| 2019(16)/2020(17) wage growth (2nd decile) | 0.097 | 0.103 | 0.097 | 0.855 | 0.001 |
| 2019(16)/2020(17) wage growth (3rd decile) | 0.096 | 0.104 | 0.095 | 0.460 | 0.004 |
| 2019(16)/2020(17) wage growth (4th decile) | 0.097 | 0.103 | 0.097 | 0.743 | 0.002 |
| 2019(16)/2020(17) wage growth (5th decile) | 0.060 | 0.140 | 0.061 | 0.769 | -0.002 |
| 2019(16)/2020(17) wage growth (6th decile) | 0.131 | 0.068 | 0.133 | 0.198 | -0.007 |
| 2019(16)/2020(17) wage growth (7th decile) | 0.106 | 0.095 | 0.105 | 0.919 | 0.001 |
| 2019(16)/2020(17) wage growth (8th decile) | 0.103 | 0.097 | 0.103 | 0.764 | -0.002 |
| 2019(16)/2020(17) wage growth (9th decile) | 0.104 | 0.096 | 0.104 | 0.811 | -0.001 |
| Marginal employment | 0.067 | 0.072 | 0.070 | 0.025 | -0.012 |
| Regular employment | 0.933 | 0.928 | 0.930 | 0.027 | 0.012 |
| Full-time employment | 0.654 | 0.657 | 0.652 | 0.564 | 0.003 |
| Part-time employment | 0.346 | 0.343 | 0.348 | 0.564 | -0.003 |
| Very small establishment (less than 10, fraction) | 0.205 | 0.216 | 0.202 | 0.162 | 0.008 |
| Small establishment (10-49, fraction) | 0.303 | 0.287 | 0.302 | 0.909 | 0.001 |
| Medium-sized establishment (50-249, fraction) | 0.285 | 0.261 | 0.287 | 0.508 | -0.004 |
| Large establishment (more than 250, fraction) | 0.201 | 0.169 | 0.203 | 0.362 | -0.005 |
| Missing establishment size | 0.006 | 0.067 | 0.006 | 0.522 | 0.004 |
| Experience (1st quartile) | 0.271 | 0.229 | 0.272 | 0.643 | -0.003 |
| Experience (2nd quartile) | 0.248 | 0.252 | 0.249 | 0.854 | -0.001 |
| Experience (3rd quartile) | 0.237 | 0.263 | 0.237 | 0.865 | 0.001 |
| Experience (4th quartile) | 0.244 | 0.256 | 0.243 | 0.619 | 0.003 |
| Tenure (last occupation) (1st quartile) | 0.257 | 0.242 | 0.259 | 0.455 | -0.004 |
| Tenure (last occupation) (2nd quartile) | 0.257 | 0.243 | 0.256 | 0.893 | 0.001 |
| Tenure (last occupation) (3rd quartile) | 0.235 | 0.266 | 0.235 | 0.871 | 0.001 |
| Tenure (last occupation) (4th quartile) | 0.251 | 0.249 | 0.250 | 0.645 | 0.003 |

| | | | | | |
|---|---|---|---|---|---|
| Duration in unemployment (1st quartile) | 0.259 | 0.236 | 0.265 | 0.020 | -0.013 |
| Duration in unemployment (2nd quartile) | 0.261 | 0.241 | 0.259 | 0.382 | 0.005 |
| Duration in unemployment (3rd quartile) | 0.244 | 0.257 | 0.240 | 0.076 | 0.010 |
| Duration in unemployment (4th quartile) | 0.236 | 0.266 | 0.236 | 0.782 | -0.002 |
| Establishment switch before matching | 0.019 | 0.017 | 0.019 | 0.631 | 0.003 |
| **Occupations** | | | | | |
| Agriculture, forestry, farming | 0.008 | 0.011 | 0.008 | 1.000 | -0.000 |
| Gardening, floristry | 0.011 | 0.013 | 0.011 | 1.000 | 0.000 |
| Production, processing of raw materials | 0.004 | 0.006 | 0.004 | 1.000 | 0.000 |
| Plastic-making, -processing, wood-working, -processing | 0.020 | 0.019 | 0.020 | 1.000 | 0.000 |
| Paper-making, -processing, printing, technical media design | 0.011 | 0.011 | 0.011 | 1.000 | 0.000 |
| Metal-making, -working, metal construction | 0.049 | 0.039 | 0.049 | 1.000 | -0.000 |
| Technical machine-building, automotive industry | 0.048 | 0.038 | 0.048 | 1.000 | 0.000 |
| Mechatronics, energy electronics, electrical engineering | 0.022 | 0.021 | 0.022 | 1.000 | 0.000 |
| Technical research, development, construction, production planning, | 0.021 | 0.016 | 0.021 | 1.000 | 0.000 |
| Textile-, leather-making, -processing | 0.005 | 0.004 | 0.005 | 1.000 | 0.000 |
| Food-production, -processing | 0.047 | 0.053 | 0.047 | 1.000 | 0.000 |
| Construction scheduling, architecture, surveying | 0.004 | 0.005 | 0.004 | 1.000 | -0.000 |
| Building construction | 0.023 | 0.032 | 0.023 | 1.000 | -0.000 |
| Interior construction | 0.023 | 0.034 | 0.023 | 1.000 | -0.000 |
| Building services engineering, technical building services | 0.019 | 0.022 | 0.019 | 1.000 | 0.000 |
| Mathematics, biology, chemistry, physics | 0.009 | 0.007 | 0.009 | 1.000 | -0.000 |
| Geology, geography, environmental protection | 0.001 | 0.001 | 0.001 | 1.000 | -0.000 |
| Computer science, information, communication technology | 0.016 | 0.012 | 0.016 | 1.000 | -0.000 |
| Traffic, logistics | 0.104 | 0.091 | 0.104 | 1.000 | 0.000 |
| Drivers and operators of vehicles and transport equipment | 0.047 | 0.048 | 0.047 | 1.000 | 0.000 |
| Safety and health protection, security, surveillance | 0.015 | 0.021 | 0.015 | 1.000 | -0.000 |
| Cleaning services | 0.053 | 0.051 | 0.053 | 1.000 | 0.000 |
| Purchasing, sales, trading | 0.029 | 0.026 | 0.029 | 1.000 | -0.000 |
| Retail trade | 0.086 | 0.093 | 0.086 | 1.000 | 0.000 |
| Tourism, hotels, restaurants | 0.055 | 0.054 | 0.055 | 1.000 | -0.000 |
| Business management, organisation | 0.100 | 0.104 | 0.100 | 1.000 | -0.000 |
| Financial services, accounting, tax consultancy | 0.017 | 0.018 | 0.017 | 1.000 | -0.000 |
| Law and public administration | 0.011 | 0.012 | 0.011 | 1.000 | 0.000 |
| Medical and health care | 0.035 | 0.032 | 0.035 | 1.000 | 0.000 |
| Non-medical healthcare, body care, wellness, medical technicians | 0.021 | 0.022 | 0.021 | 1.000 | 0.000 |
| Education, social work, housekeeping, theology | 0.032 | 0.031 | 0.032 | 1.000 | -0.000 |
| Teaching, training | 0.020 | 0.018 | 0.020 | 1.000 | 0.000 |
| Philology, literature, humanities, social sciences, economics | 0.003 | 0.002 | 0.003 | 1.000 | 0.000 |
| Advertising, marketing, commercial, editorial media design | 0.023 | 0.022 | 0.023 | 1.000 | 0.000 |
| Product design, artisan craftwork, fine arts, making of musical inst | 0.002 | 0.002 | 0.002 | 1.000 | 0.000 |
| Performing arts, entertainment | 0.004 | 0.005 | 0.004 | 1.000 | 0.000 |
| **Sectors** | | | | | |
| Agriculture | 0.008 | 0.012 | 0.008 | 0.972 | -0.000 |
| Manufacturing | 0.394 | 0.404 | 0.395 | 0.781 | -0.002 |
| Service | 0.598 | 0.584 | 0.597 | 0.775 | 0.002 |
| N | 66,070 | 66,199 | 66,199 | | |

Note: Table A2 reports descriptive statistics that refer to the time before the onset of unemployment (November 2019 for the treatment group and November 2016 for the control group): mean in the treatment group (column (1)), mean in the control group (column (2)), weighted mean in the control group (column (3)), p-value for the null hypothesis of equality between the mean in the treatment group and the weighted mean in the control group (column (4)), standardised difference between the mean in the treatment group and the weighted mean in the control group (column (5)).
Source: IEB, BHP, own calculations.

The overlap assumption requires some randomness in the treatment assignment, meaning that we need to observe persons with identical characteristics in the treatment and control group. To check whether the overlap assumption holds, we compare the distribution of the estimated propensity scores for both groups. Figure A1 shows the distribution of the estimated propensity score for the treatment (solid line) and the control group (dashed line). Although the distribution of the treated individuals is slightly shifted to the right, the majority of both distributions is nearly identical, which supports the overlap assumption.

**Figure A1.: Overlap after inverse propensity score weighting**



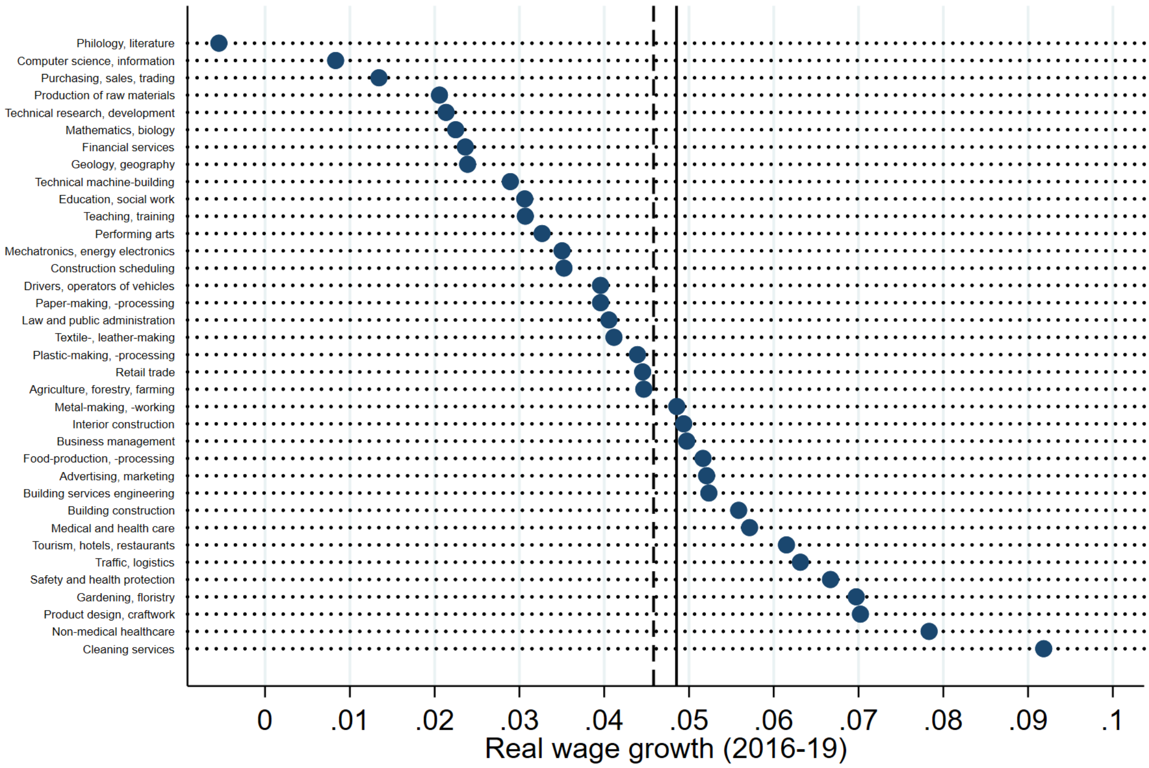

Note: Figure A1 shows the estimated propensity score for the treatment and the control group.
Source: IEB, own calculations

### A.3.1. Wage differences between treatment and control group

Table 1 in the paper shows that individuals in the treatment group, on average, earn a higher daily wage in November $t-1$ compared to the control group, even when matching weights are used (though the standardised difference is small than 0.1) . We argue that this difference reflects real wage growth that took place between the years 2016 (the year before which individuals in the control group became unemployed) and 2019 (the corresponding year for individuals in the treatment group) rather than any difference in the composition of the two groups.

To assess this hypothesis, we compute the change in the mean real wage for each occupation between 2016 and 2019 based on the universe of employees. Figure A2 shows that almost all occupations experienced an increase in mean real wages over this period. This increase was especially pronounced among a number of lower-wage occupations, such as *cleaning services* or *non-medical healthcare occupations*, which likely reflects binding increases in the minimum wage during the period. The employment-weighted average across all occupations is 4.6%. Using the occupational employment shares of the treatment group in 2019 as weights, the average real wage growth amounts to 4.9%. This values is very close to the difference in the mean real wage between the treatment and the control group that is shown in Table 1 in the paper.

**Figure A2.: Change in real wage by occupation**




Note: Figure A2 shows the change in mean real wages between 2016 and 2019 for each 2-digit occupation (occupational titles are shortened due to space constraints). The average change in the mean real wage is represented by the dashed line, while the solid line represents the weighted average based on the occupational employment shares in the treatment group.
Source: IEB, own calculations.

# A.4 Lockdown work ability index (LWA)

## A.4.1. Construction

The LWA index consists of three components: the possibility to work from home (H), whether occupations are essential (E) or had to close (C) during the lockdown.[25] All

[25] Similar to Palomino/Rodríguez/Sebastian (2020), the working-from-home indicator is based on Dingel/Neiman (2020), which is derived by the composition of tasks with working-from-home possibilities for each occupation via O*Net. Values for essential or closed occupations at the 2-digit ISCO-08 level are based on the decision by the Spanish and Italian government (though Palomino/Rodríguez/Sebastian

indicators rage from 0 to 1, where 0 indicates that an occupation is not essential, not closed or can not be carried out from home and 1 indicates that an occupation is essential, closed or suitable for working from home. The LWA index is generated by the following formula similar to Palomino/Rodríguez/Sebastian (2020):

$$LWA_o = \begin{cases} E_o + (1 - E_o)H_o & E_o = e \\ (1 - C_o)H_o & C_o = c \\ H_o & E_o \neq 0 \wedge C_o \neq 0 \end{cases} \tag{A1}$$

$o$ is the occupation at the 2-digit KldB level, $e \in (0;1]$ denotes the extent to which an occupation is essential and $c \in (0;1]$ denotes whether an occupation was closed during the pandemic. Thus, the LWA index captures the ability to work during the pandemic based on the extent to which tasks can be done from home. Additionally, if the occupation is essential (or partly essential), then it is able to operate, regardless of its working from home potential. However, if the occupation is closed, then only the part of the occupation which is not closed is able to operate to the extent of the working from home potential. The LWA index ranges from $0$ (low LWA) to $1$ (high LWA). Table A3 displays the corresponding mean, the standard deviation and the distribution of LWA across occupations.

**Table A3.: Descriptive statistics of LWA**

| | LWA |
|---|---|
| Mean | 0.396 |
| Standard deviation | 0.299 |
| Percentile 10 | 0.060 |
| Percentile 25 | 0.127 |
| Percentile 75 | 0.680 |
| Percentile 90 | 0.824 |

Note: Table A3 reports the statistical properties of the lockdown work ability (LWA) index.
Source: Dingel/Neiman (2020); Palomino/Rodríguez/Sebastian (2020), own calculations.

(2020) use these values also for Germany) and are transformed to the 2-digit KldB (36 different occupations) used in this paper. The values for essential and closed occupations were transformed from 2-digit ISCO-08 into 5-digit KldB and then aggregated into 2-digit KldB by weighting the relative employment size of the 5-digit occupations. After the aggregation, some occupations have a value greater than zero in the essential as well as the closed index. This is, by definition of the index, not possible. To adjust this, we set every index to zero if it is smaller than a threshold of 0.1 (which is arguably close to zero). After applying this rule, three occupations remained with this conflict: occupations in food-production and -processing, in non-medical healthcare, body care, wellness and medical technicians and in education and social work, housekeeping, and theology. By comparing them to similar occupations, we manually set either the essential or the closed index to zero.

Table A4 shows occupations and their corresponding LWA index ranked from occupations with a high LWA to occupations with a low LWA. Occupations with a high LWA include occupations in computer science, information or communication technology or medical and health care occupations, while occupations with a low LWA include occupations in the field of construction or occupations in tourism, hotels and restaurants.

**Table A4.: Occupations ranked by LWA**

| Occupation | LWA |
|---|---|
| Computer science, information and communication technology | 0.98 |
| Medical and health care occupations | 0.95 |
| Gardening and floristry | 0.88 |
| Teaching and training | 0.82 |
| Agriculture, forestry and farming | 0.82 |
| Business management and organisation | 0.72 |
| Philology, literature, humanities, social sciences, and economics | 0.71 |
| Non-medical healthcare, body care, wellness and medical technicians | 0.70 |
| Financial services, accounting and tax consultancy | 0.68 |
| Law and public administration | 0.68 |
| Safety and health protection, security and surveillance | 0.65 |
| Advertising and marketing, in commercial and editorial media design | 0.52 |
| Purchasing, sales and trading | 0.50 |
| Construction scheduling, architecture and surveying | 0.48 |
| Technical research and development, construction, and production planning and scheduling | 0.47 |
| Geology, geography and environmental protection | 0.45 |
| Traffic and logistics (without vehicle driving) | 0.40 |
| Product design, artisan craftwork, fine arts and the making of musical instruments | 0.39 |
| Mathematics, biology, chemistry and physics | 0.33 |
| Performing arts and entertainment | 0.26 |
| Education and social work, housekeeping, and theology | 0.22 |
| Papermaking and -processing, printing, and in technical media design | 0.20 |
| Cleaning services | 0.20 |
| Textile- and leather-making and -processing | 0.20 |
| Plastic-making and -processing, and wood-working and -processing | 0.17 |
| Food-production and -processing | 0.15 |
| Sales occupations in retail trade | 0.13 |
| Mechatronics, energy electronics and electrical engineering | 0.13 |
| Drivers and operators of vehicles and transport equipment | 0.10 |
| Building construction above and below ground | 0.10 |
| Technical occupations in machine-building and automotive industry | 0.10 |
| Tourism, hotels and restaurants | 0.07 |
| Production and processing of raw materials, glass- and ceramic-making and -processing | 0.06 |
| Metal-making and -working, and in metal construction | 0.02 |
| Building services engineering and technical building services | 0.02 |
| Interior construction | 0.00 |

Note: Table A4 shows the lockdown work ability (LWA) index by 2-digit occupation.
Source: Dingel/Neiman (2020); Palomino/Rodríguez/Sebastian (2020), own calculations.

## A.5 Descriptive statistics by quantile of the LWA distribution

**Table A5.: Descriptive statistics: individuals from low-, medium- and high-LWA occupations**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | Low LWA (25%-Quantile) | | | Medium LWA (25%-75%-Quantile) | | | High LWA (75%-Quantile) | | |
| | Treatment | Control | Standard. diff. | Treatment | Control | Standard. diff. | Treatment | Control | Standard. diff. |
| **Socio-demographic characteristics (at the time of matching)** | | | | | | | | | |
| Age | 39.047 | 39.104 | -0.005 | 38.754 | 38.776 | -0.002 | 40.281 | 40.221 | 0.005 |
| | (12.672) | (12.522) | | (12.408) | (12.230) | | (12.066) | (11.919) | |
| Male (fraction) | 0.836 | 0.835 | 0.003 | 0.565 | 0.566 | -0.003 | 0.428 | 0.430 | -0.005 |
| | (0.371) | (0.372) | | (0.496) | (0.496) | | (0.495) | (0.495) | |
| Foreign (fraction) | 0.275 | 0.278 | -0.007 | 0.278 | 0.278 | -0.001 | 0.134 | 0.138 | -0.011 |
| | (0.447) | (0.448) | | (0.448) | (0.448) | | (0.341) | (0.345) | |
| Low skilled (no completed apprenticeship, fraction) | 0.164 | 0.166 | -0.006 | 0.187 | 0.188 | -0.002 | 0.068 | 0.068 | -0.000 |
| | (0.370) | (0.372) | | (0.390) | (0.391) | | (0.252) | (0.252) | |
| Middle skilled (completed apprenticeship, fraction) | 0.688 | 0.684 | 0.010 | 0.563 | 0.561 | 0.004 | 0.539 | 0.539 | -0.001 |
| | (0.463) | (0.465) | | (0.496) | (0.496) | | (0.499) | (0.498) | |
| High skilled (tertiary education, completed) | 0.057 | 0.057 | -0.000 | 0.147 | 0.148 | -0.001 | 0.365 | 0.362 | 0.007 |
| | (0.232) | (0.232) | | (0.354) | (0.355) | | (0.481) | (0.480) | |
| **Current employment (at the time of matching)** | | | | | | | | | |
| Current wage | 74.846 | 70.857 | 0.110 | 70.486 | 67.377 | 0.069 | 95.910 | 90.467 | 0.09 |
| | (36.869) | (35.520) | | (45.600) | (44.705) | | (56.336) | (55.295) | |
| Current earnings | 1,122.694 | 1,062.858 | 0.110 | 1,057.286 | 1,010.656 | 0.069 | 1,438.654 | 1,357.007 | 0.098 |
| | (553.037) | (532.797) | | (684.003) | (670.575) | | (845.040) | (829.427) | |
| In regular employment (fraction) | 0.935 | 0.932 | 0.010 | 0.917 | 0.914 | 0.013 | 0.963 | 0.960 | 0.015 |
| | (0.247) | (0.251) | | (0.275) | (0.281) | | (0.189) | (0.196) | |
| In full-time employment (fraction) | 0.775 | 0.774 | 0.002 | 0.593 | 0.591 | 0.004 | 0.627 | 0.626 | 0.002 |
| | (0.418) | (0.418) | | (0.491) | (0.492) | | (0.484) | (0.484) | |
| Very small establishment (less than 10, fraction) | 0.235 | 0.232 | 0.008 | 0.166 | 0.164 | 0.007 | 0.248 | 0.244 | 0.009 |
| | (0.424) | (0.422) | | (0.372) | (0.370) | | (0.432) | (0.430) | |
| Small establishment (10-49, fraction) | 0.344 | 0.343 | 0.000 | 0.284 | 0.283 | 0.002 | 0.289 | 0.290 | -0.002 |
| | (0.475) | (0.475) | | (0.451) | (0.451) | | (0.453) | (0.454) | |
| Medium-sized establishment (50-249, fraction) | 0.262 | 0.261 | 0.002 | 0.318 | 0.321 | -0.006 | 0.247 | 0.250 | -0.007 |
| | (0.440) | (0.439) | | (0.466) | (0.467) | | (0.431) | (0.433) | |

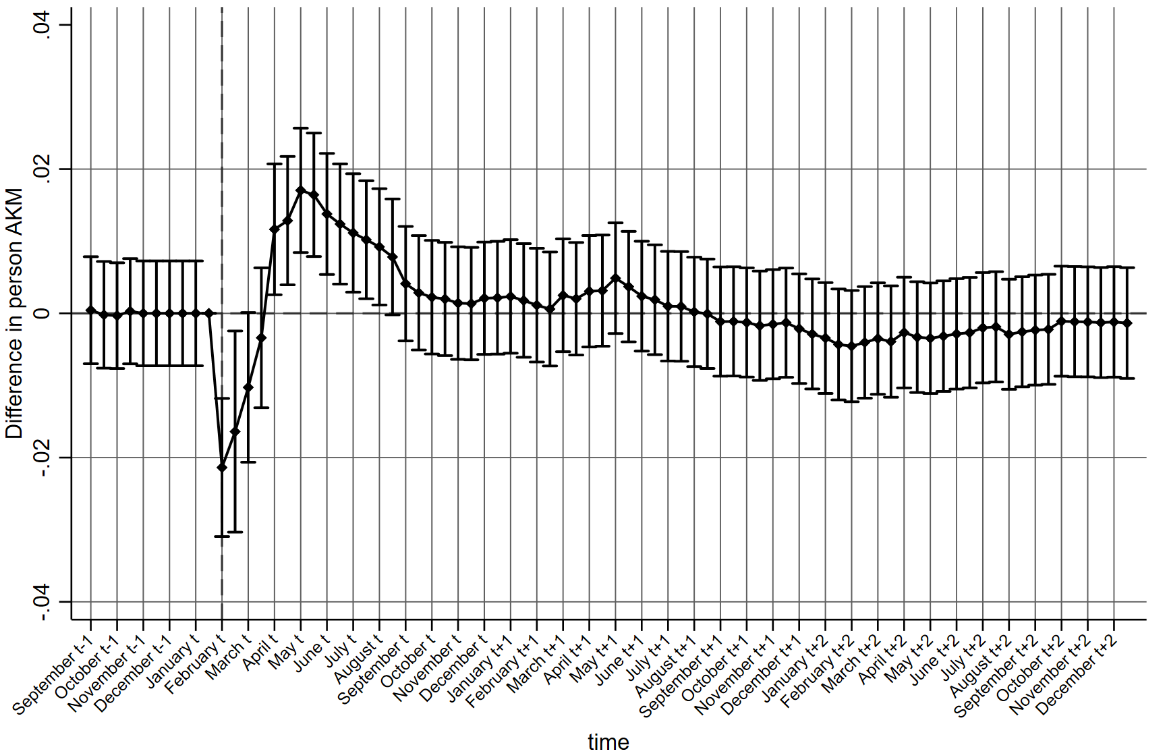
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | Low LWA (25%-Quantile) | | | Medium LWA (25%-75%-Quantile) | | | High LWA (75%-Quantile) | | |
| | Treatment | Control | Standard. diff. | Treatment | Control | Standard. diff. | Treatment | Control | Standard. diff. |
| Large establishment (more than 250, fraction) | 0.152 | 0.157 | -0.014 | 0.226 | 0.227 | -0.002 | 0.209 | 0.209 | -0.001 |
| | (0.359) | (0.363) | | (0.418) | (0.419) | | (0.407) | (0.407) | |
| Estimated AKM firm effect | -0.181 | -0.200 | 0.069 | -0.171 | -0.186 | 0.057 | -0.097 | -0.110 | 0.052 |
| | (0.270) | (0.271) | | (0.257) | (0.256) | | (0.262) | (0.260) | |
| **Employment biography** | | | | | | | | | |
| Work experience | 12.100 | 11.949 | 0.015 | 11.066 | 10.882 | 0.019 | 13.627 | 13.315 | 0.031 |
| | (10.165) | (9.824) | | (9.901) | (9.516) | | (10.195) | (9.811) | |
| Tenure in current establishment | 3.164 | 3.387 | -0.042 | 2.687 | 2.678 | 0.002 | 3.510 | 3.489 | 0.004 |
| | (5.241) | (5.482) | | (4.895) | (4.596) | | (5.596) | (5.516) | |
| Tenure in current occupation | 5.831 | 5.983 | -0.021 | 4.955 | 5.008 | -0.008 | 7.206 | 7.205 | 0.000 |
| | (7.221) | (7.411) | | (6.488) | (6.482) | | (8.112) | (8.060) | |
| Number of job changes | 3.315 | 3.015 | 0.081 | 3.167 | 2.957 | 0.055 | 3.379 | 3.102 | 0.082 |
| | (3.821) | (3.564) | | (3.673) | (3.893) | | (3.492) | (3.288) | |
| Being unemployed before (fraction) | 0.780 | 0.769 | 0.027 | 0.775 | 0.768 | 0.015 | 0.698 | 0.696 | 0.006 |
| | (0.414) | (0.422) | | (0.418) | (0.422) | (0.409) | (0.459) | (0.460) | |
| Employed in manufacturing sector (fraction) | 0.436 | 0.440 | -0.007 | 0.170 | 0.162 | 0.020 | 0.121 | 0.120 | 0.003 |
| | (0.496) | (0.496) | | (0.376) | (0.369) | | (0.326) | (0.325) | |
| Employed in service sector (fraction) | 0.561 | 0.558 | 0.007 | 0.829 | 0.836 | -0.020 | 0.851 | 0.852 | -0.002 |
| | (0.496) | (0.497) | | (0.377) | (0.370) | | (0.356) | (0.355) | |
| Estimated AKM worker effect | 4.303 | 4.314 | -0.041 | 4.314 | 4.325 | -0.027 | 4.551 | 4.551 | -0.001 |
| | (0.270) | (0.262) | | (0.374) | (0.370) | | (0.442) | (0.449) | |
| N | 19,247 | 19,590 | | 31,568 | 31,223 | | 15,368 | 15,390 | |

Note: Table A5 reports descriptive statistics for three different subsets of the LWA distribution: low, medium and high. For each group the first two columns show the mean value and standard deviation (in parentheses) of individual characteristics that are measured at the first half of November $t-1$ (the point for the weighting). The third column reports the standardised difference between the first two columns, which is defined as $\Delta_X = \left(\bar{X}_1 - \bar{X}_0\right) / \left((S_1^2 + S_0^2)/2\right)^{0.5}$, where $\bar{X}_w$ is the sample mean of the treated $(w=1)$ or (weighted) control $(w=0)$ individuals and $S_w^2$ are the respective sample variances. The subsets are defined by the quantiles of the LWA distribution: low LWA below the 25% quantile, medium LWA between the 25% and the 75% quantile as well as high LWA above the 75% quantile. Note, that the observations for the AKM worker and firm fixed effects are smaller than the reported number of observations.
Source: IEB, BHP, own calculations.

# B Results appendix

## B.1 AKM worker effect

**Figure B3.: The effect of the Covid-19 pandemic on AKM worker effects**

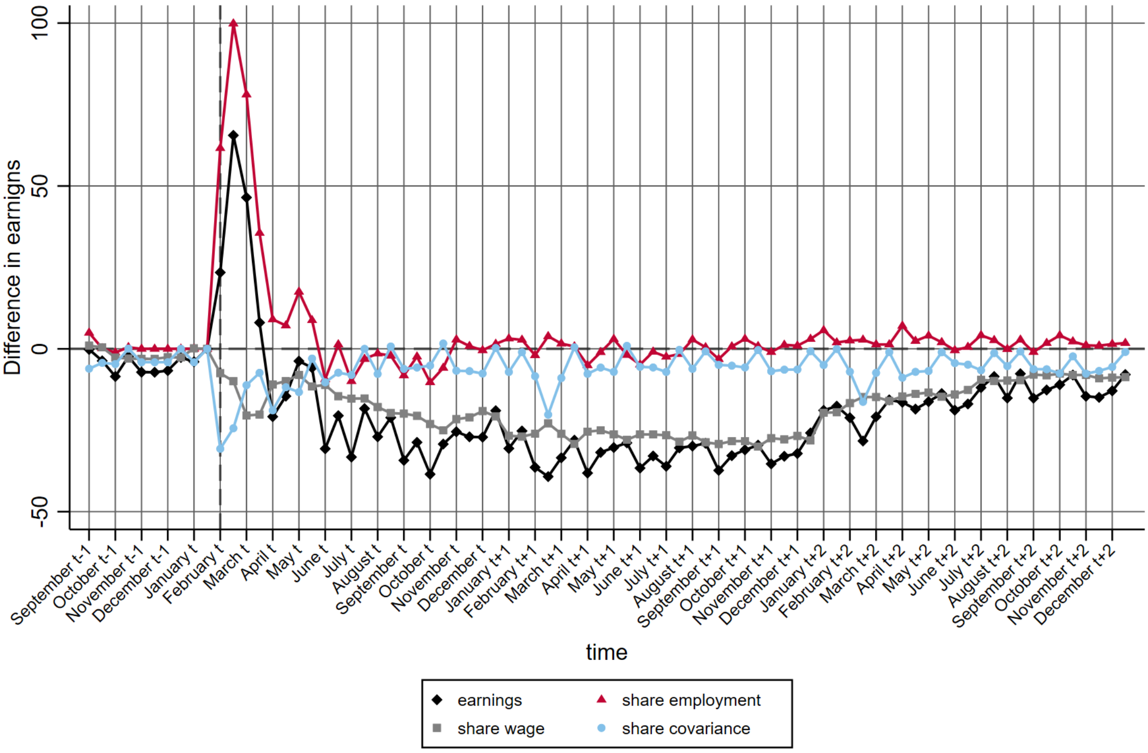



Note: Figure B3 shows the estimated coefficients $\hat{\beta}_p$ from Equation 1 with the AKM worker effect as dependent variable using data on individuals who became unemployed in February 2020 and 2017, respectively. The variable is conditional on employment. The estimation is weighted by the inverse propensity score. $t$ denotes the year in which the individuals in the sample became unemployed. This means that individuals in the treatment group are observed from September 2019 until December 2022, while the control group is observed from September 2016 until 2019. The vertical bars represent the 95% confidence interval which are based on standard errors that are clustered at the individual level.

Source: IEB, BHP, own calculations.

## B.2 Earnings decomposition

**Figure B4.: Earnings decomposition**

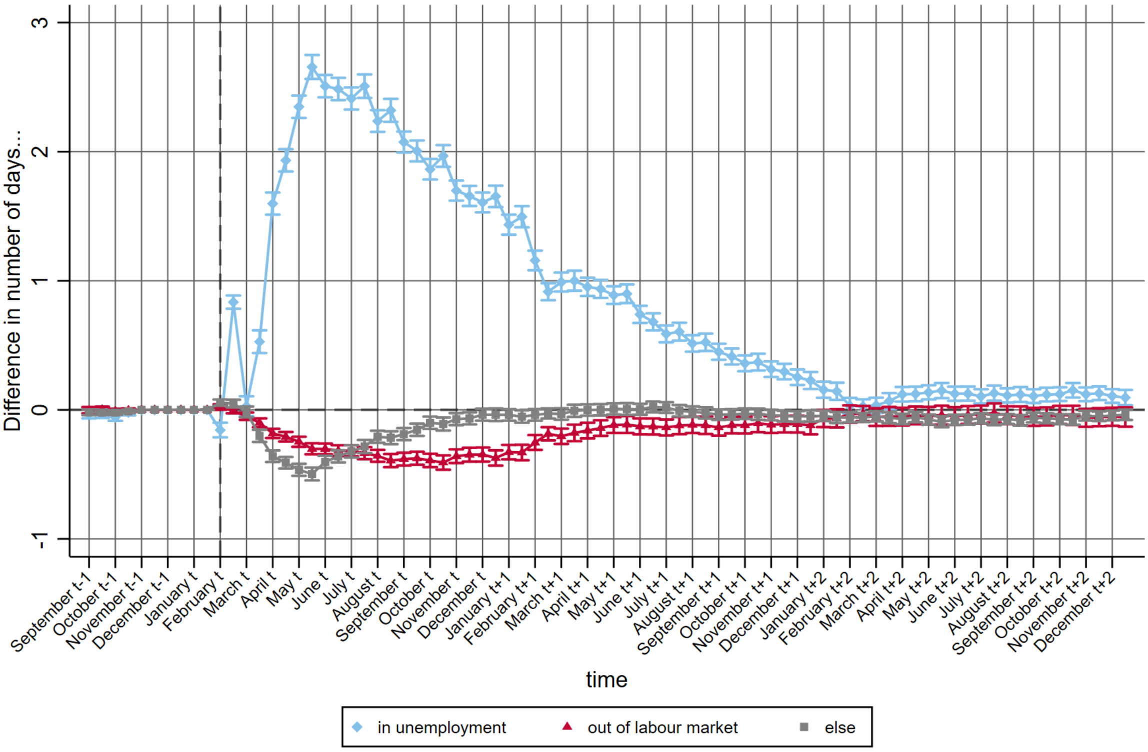



Note: Figure B4 shows the estimated coefficients $\hat{\beta}_p$ from Equation 1 with earnings conditional on employment (black) as dependent variable as well as the decomposition following (Schmieder/von Wachter/Heining, 2023) into the shares of the explaining variables employment (red), wage (grey) and their corresponding covariance (blue). The estimation is weighted by the inverse propensity score. $t$ denotes the year in which the individuals in the sample became unemployed. This means that individuals in the treatment group are observed from September 2019 until December 2021, while the control group is observed from September 2017 until 2019. The vertical bars represent the 95% confidence interval which are based on standard errors that are clustered at the individual level.

Source: IEB, BHP, own calculations.

## B.3 Employment mechanisms

**Figure B5.: The effect of the Covid-19 pandemic on other labour market states**




Note: Figure B5 shows the estimated coefficients $\hat{\beta}_p$ from Equation 1 with days in unemployment, days out of the labour market as well as the remaining labour market status (which includes days in a measure, days receiving transfer payments and days registered in the unemployment data but not being unemployed) as dependent variables using data on individuals who became unemployed in February 2020 and 2017, respectively. The estimation is weighted by the inverse propensity score. $t$ denotes the year in which the individuals in the sample became unemployed. This means that individuals in the treatment group are observed from September 2019 until December 2022, while the control group is observed from September 2016 until 2019. The vertical bars represent the 95% confidence interval which are based on standard errors that are clustered at the individual level. Note that individuals can be in several labour market states at the same time, but the figure shows only one state per individual per period.

Source: IEB, BHP, own calculations.

**Table B6.: Employment adjustments**

| | Days in unemployment | Days out of the labour market | Days in other states |
|---|---|---|---|
| Average | | | |
| Treatment period | 0.835*** | -0.156*** | -0.092*** |
| | (0.021) | (0.023) | (0.012) |
| Treatment period ($\hat{\gamma}$) | 3.280*** | 1.978*** | 0.965*** |
| | (0.015) | (0.018) | (0.010) |
| Pre-treatment period | -0.013** | -0.001 | -0.007** |
| | (0.005) | (0.003) | (0.003) |
| Feb-May 2020 | 1.221*** | -0.134*** | -0.231*** |
| | (0.031) | (0.014) | (0.016) |
| Jun-Sep 2020 | 2.319*** | -0.347*** | -0.267*** |
| | (0.039) | (0.021) | (0.020) |
| Oct-Dec 2020 | 1.741*** | -0.371*** | -0.071*** |
| | (0.038) | (0.026) | (0.022) |
| 2021 | 0.709*** | -0.154*** | -0.021 |
| | (0.027) | (0.028) | (0.017) |
| 2022 | 0.111*** | -0.047 | -0.064*** |
| | (0.022) | (0.033) | (0.016) |
| Cumulative | | | |
| Treatment period | 58.438*** | -10.916*** | -6.442*** |
| | (1.464) | (1.591) | (0.864) |
| N | 10,583,520 | 10,583,520 | 10,583,520 |

Note: Table B6 shows the estimated coefficients of $\hat{\beta}_p$ from Equation 1 with days in unemployment, out of the labour market as well as the remaining labour market status (which includes days in a measure, days receiving transfer payments and days registered in the unemployment data but not being unemployed) as dependent variables using data on individuals who became unemployed in February 2020 and 2017, respectively. The estimation is weighted by the inverse propensity score. The table displays the averaged $\hat{\beta}_p$ for specific time periods, the (treatment) effect averaged over the whole period and the baseline estimate for the control group averaged over the whole period ($\hat{\gamma}$). Standard errors clustered at the individual level are in parentheses. Note that it is possible that individuals are in several labour market states at the same time, but Table B6 shows only one labour market per individual per period. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: IEB, BHP, own calculations

## B.4 Wage mechanisms

### B.4.1. Reference year of wage distributions

The mean wages of occupations, sectors and firms as well as the wage distribution within occupations are calculated on the basis of two separate years for the treatment (2019) and the control group (2016). Table B7 presents the results when either data from a single year – either 2016 or 2019 – are used to compute mean wages.

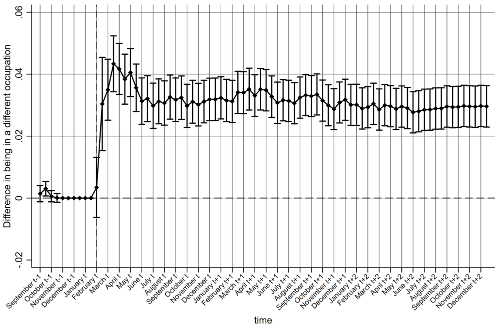
**Table B7.: Wage adjustments: constant year of wage distributions**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Mean wage | | | | | | Occupational rank | |
| | Occupation | | Firm | | Sector | | | |
| | 2016 | 2019 | 2016 | 2019 | 2016 | 2019 | 2016 | 2019 |
| Average | | | | | | | | |
| Treatment period | 0.008*** | 0.008*** | -0.024*** | 0.014*** | -0.006** | -0.006** | -1.095*** | -1.111*** |
| | (0.002) | (0.002) | (0.005) | (0.005) | (0.003) | (0.002) | (0.165) | (0.163) |
| Treatment period ($\hat{\gamma}$) | 0.011*** | 0.011*** | 0.018*** | 0.003 | 0.005*** | 0.005*** | 2.136*** | 2.069*** |
| | (0.002) | (0.002) | (0.003) | (0.005) | (0.002) | (0.002) | (0.124) | (0.122) |
| Pre-treatment period | 0.000 | 0.000 | -0.000 | 0.002*** | 0.001*** | 0.001*** | -0.012 | -0.028 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.058) | (0.058) |
| Feb-May 2020 | 0.008*** | 0.008*** | 0.011** | 0.021*** | 0.009*** | 0.009*** | -0.759*** | -0.812*** |
| | (0.002) | (0.002) | (0.005) | (0.005) | (0.003) | (0.003) | (0.187) | (0.184) |
| Jun-Sep 2020 | 0.008*** | 0.008*** | -0.007 | 0.021*** | 0.004 | 0.004 | -0.832*** | -0.881*** |
| | (0.002) | (0.002) | (0.005) | (0.005) | (0.003) | (0.003) | (0.181) | (0.178) |
| Oct-Dec 2020 | 0.008*** | 0.008*** | -0.018*** | 0.018*** | -0.002 | -0.002 | -1.095*** | -1.148*** |
| | (0.002) | (0.002) | (0.005) | (0.005) | (0.003) | (0.003) | (0.181) | (0.177) |
| 2021 | 0.009*** | 0.009*** | -0.033*** | 0.011** | -0.010*** | -0.010*** | -1.622*** | -1.656*** |
| | (0.002) | (0.002) | (0.005) | (0.005) | (0.003) | (0.003) | (0.178) | (0.175) |
| 2022 | 0.007*** | 0.006*** | -0.034*** | 0.011** | -0.010*** | -0.010*** | -0.767*** | -0.733*** |
| | (0.003) | (0.002) | (0.005) | (0.006) | (0.003) | (0.003) | (0.187) | (0.185) |
| N | 6,715,320 | 6,715,320 | 4,225,042 | 4,106,833 | 6,655,597 | 6,655,554 | 6,715,320 | 6,715,320 |

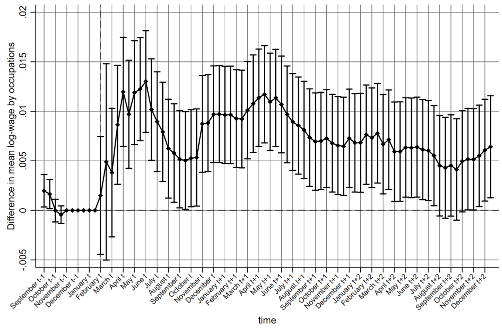
Note: Table B7 shows the estimated coefficients $\hat{\beta}_p$ from Equation 1 with occupational mean log wage, sector mean log wage, firm mean log wage and rank in the occupational wage distribution as dependent variables, where the mean log wages and the rank in the occupational wage distribution are calculated on the basis of all employed individuals in Germany for November 2016 and 2019. All variables are conditional on employment. The estimation is weighted by the inverse propensity score. The table displays the averaged $\hat{\beta}_p$ for specific time periods, the (treatment) effect averaged over the whole period and the baseline estimate for the control group averaged over the whole period ($\hat{\gamma}$). Standard errors clustered at the individual level are in parentheses. $^{*}\, p < 0.1$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.
Source: IEB, BHP, own calculations.

## B.4.2. Event-study plots

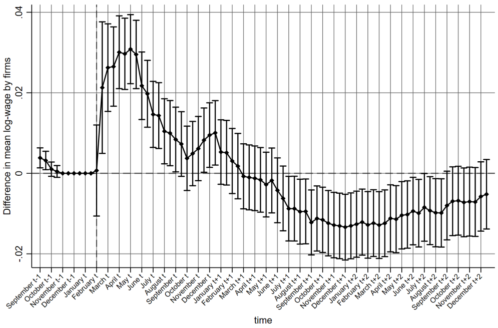
### Figure B6.: Wage mechanisms: event-study plots



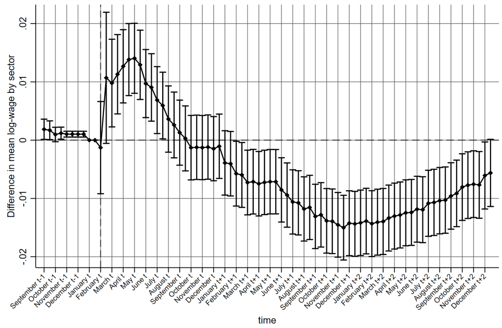

**(a) Occupational mobility**



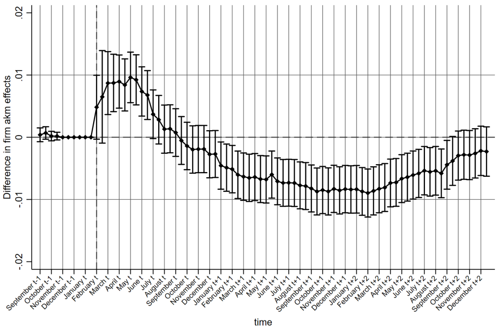

**(b) Mean wage (occupation)**



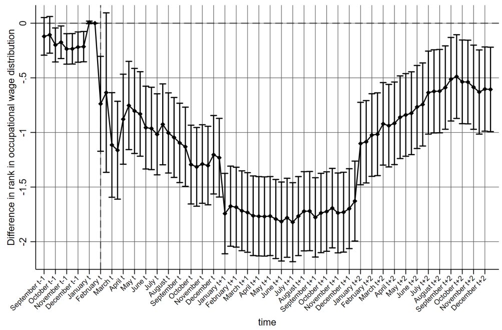

**(c) Mean wage (firm)**



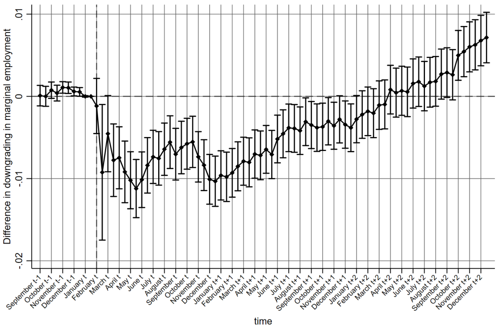

**(d) Mean wage (sector)**



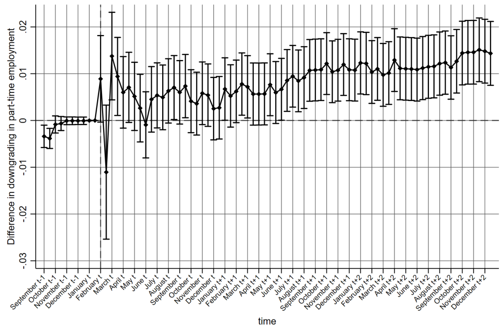

**(e) AKM firm effect**




**(f) Occupational rank**




**(g) Downgrading (marginal)**




**(h) Downgrading (part-time)**

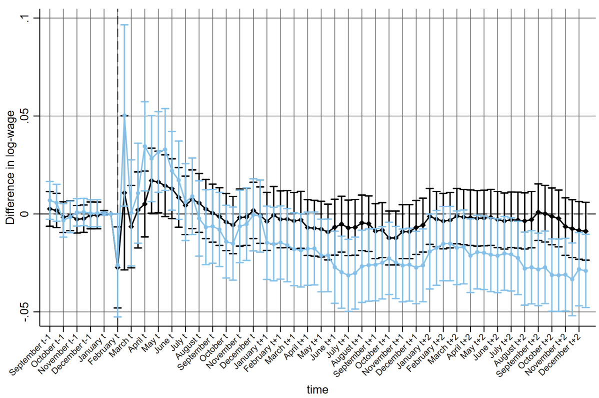
Note: Figure B6 shows the estimated coefficients $\hat{\beta}_p$ from Equation 1 with occupational mobility (panel (a)), occupational mean log wage (panel (b)), firm mean log wage (panel(c)), sector mean log wage (panel (d)), AKM firm fixed effects (panel (e)), rank in the occupational wage distribution (panel (f)), downgrading from regular into marginal employment (panel (g)) as well as downgrading from full-time into part-time employment (panel (h)) as dependent variables using data on individuals who became unemployed in February 2020 and 2017, respectively. The estimation is weighted by the inverse propensity score. $t$ denotes the year in which the individuals in the sample became unemployed. This means that individuals in the treatment group are observed from September 2019 until December 2022, while the control group is observed from September 2016 until 2019. The vertical bars represent the 95% confidence intervals which are based on standard errors that are clustered at the individual level.

Source: IEB, BHP, own calculations.

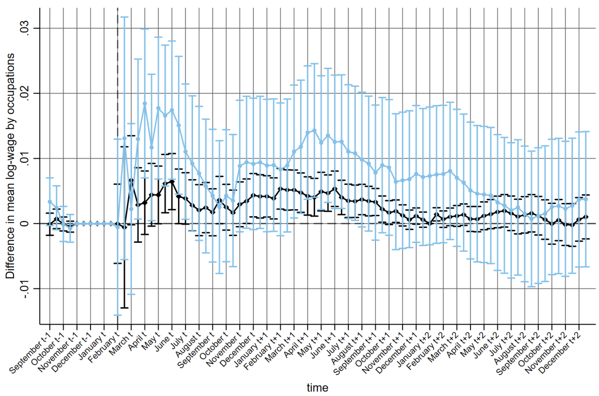
### B.4.3. Occupational mobility

To investigate whether wage loss can be attributed to individuals who switch their occupation, the sample is split into “movers” who are employed on January $t + 2$ in a different occupation and “stayers” who are employed on January $t + 2$ in the same occupation as before their transition into unemployment. January $t + 2$ is chosen as the reference date, because we no longer find a difference in terms of employment between the treatment and the control group at that time, suggesting that differences in selection into employment, which are visible right after the start of the pandemic, are no longer present. Note that it is possible that before and after this reference date individuals are allowed to switch their occupations. Weights via inverse propensity score weighting are then computed separately for the groups of movers and stayers. Figure B7 shows the estimated coefficients $\beta_p$ from Equation 1 for log wages, the mean occupational log wage and the rank of the occupational wage distribution for movers (blue line) and stayers (black line).

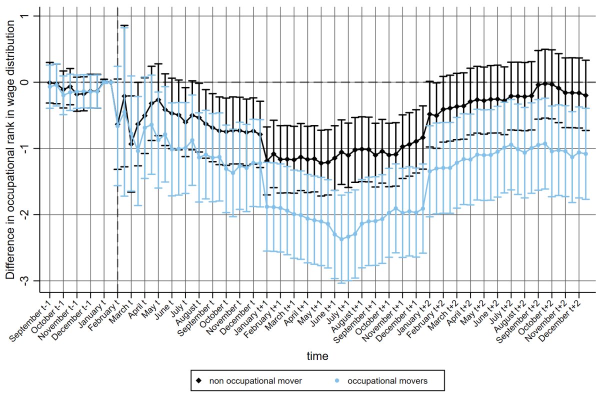
**Figure B7.: The effect of the Covid-19 pandemic on earnings, employment and wages – Occupational mobility**




**(a) Log wage**




**(b) Occupational mean wage**




**(c) Occupational wage rank**

Note: Figure B7 shows the estimated coefficients $\hat{\beta}_p$ from Equation 1 with log wages (panel (a)), occupational mean log wage (panel (b)) and rank in the occupational wage distribution (panel (c)) as dependent variables using data on individuals who became unemployed in February 2020 and 2017, respectively. Estimates are shown for individuals who are employed in the same occupation in January $t$ as in November $t-1$ ("stayers", black line) and individuals who are employed in a different occupation in January $t$ as in November $t-1$ ("movers", blue line). The estimation is weighted by the inverse propensity score. $t$ denotes the year in which the individuals in the sample became unemployed. This means that individuals in the treatment group are observed from September 2019 until December 2022, while the control group is observed from September 2016 until 2019. The vertical bars represent the 95% confidence interval which are based on standard errors that are clustered at the individual level.

Source: IEB, BHP, own calculations.

## B.5 Gelbach decomposition

For the decomposition, we first estimate Equation 1 using the log daily wage as dependent variable and store the estimates of the average excess wage loss among individuals in the treatment group for each event time, $\hat{\beta}_p$.[26] We then estimate the extended model and store the corresponding estimates, $\hat{\beta}_p^*$. In the extended model, we additionally control for the (time-invariant) mean wage of the firm in which individual $i$ is employed at time $p$ (corresponding to column (3) in Table 3), indicators for marginal and part-time employment (columns (7) and (8)), the rank of an individual's wage in the occupational wage distribution (column (6)) as well as occupation dummies to capture the effects of changes in the occupational mean wage as well as of occupational mobility (columns (1) and (2)). Figure 3 in the paper shows that the size of the estimated wage loss among treated individuals (compared to individuals in the control group) decreases in magnitude in the extended model compared to the baseline model. For most points in time, we no longer find significant differences between the wages of individuals in the treatment and the control group once the additional control variables are included. We interpret this finding as evidence that the included variables are associated with the wage loss among treated individuals that we identify in the baseline model without control variables.

We proceed to compute the difference in the estimated coefficients from the two models, $\hat{d}_p = \hat{\beta}_p - \hat{\beta}_p^*$, and compute which part of this difference can be attributed to each of the additional control variables in the extended model. Figure 3 in the paper shows the difference between the coefficient estimates of the two models (in black) as well as the part of this difference that can be assigned to each of the additional control variables.

## B.6 Counterfactual employment trajectories in the absence of the Covid-19 pandemic

According to the results in Section 5 in the paper, individuals who became unemployed shortly before the onset of the Covid-19 pandemic in Germany subsequently experienced a more adverse development of their employment trajectories than observationally identical individuals from the control group. One concern is that these adverse effects are not the result of the pandemic, but rather reflect a general worsening of labour market opportunities. Figure 1 in the paper provides some support for this hypothesis as it shows that the number of vacancies as well as the ratio of vacancies to job seekers were already decreasing before the start of the pandemic.

[26] The estimated coefficients are identical to those shown in panel (c) of Figure 2 in the paper.

To assess this hypothesis empirically, we construct a counterfactual development of the main labour market outcomes (earnings, employment and wages) that is based on a linear extrapolation from pre-pandemic years. Specifically, we estimate Equation 1 separately for three cohorts of individuals who became unemployment during the first half of February in the years 2017, 2018 and 2019, respectively (the corresponding control groups entered unemployment during the first half of February 2016, 2017 and 2018) as well as for the cohort 2020 (control group: 2019). For each cohort, we employ the weighting procedure that is described in Section 3.3 in the paper to ensure comparability of treatment and control group in terms of observable characteristics. Table B8 shows that both groups are balanced for each cohort.

Compared to the analysis in the paper where we observe individuals up to year $t+2$ after becoming unemployed, individuals are only followed until the end of the year in which they entered unemployment for the extrapolation analysis. We do this for two reasons. First, the largest effects can be found during the first year, so that focusing on this year appears to be most relevant. Second, a longer period of observation would have required us to use earlier cohorts to ensure that the period of observation for these cohorts does not contain the Covid-19 pandemic. This would have increased the risk of differences in the composition of the unemployed between cohorts as earlier cohorts are subject to other labour market shocks.

We start by estimating separate models for each cohort $c$ using earnings, days in employment and log wages as dependent variables:

$$y_{i,p}^c = \alpha_i^c + \sum_{\tau \neq -1} \gamma_\tau^c I(\tau = p) + \sum_{\tau \neq -1} \beta_\tau^c I(\tau = p) I(D_i = 1) + \varepsilon_{i,p}^c \tag{B2}$$

After storing the coefficient estimates, $\hat{\beta}_p^c$, for the three pre-pandemic cohorts (2017, 2018, 2019), we estimate an auxiliary model in which we regress the estimated coefficients on a constant and a linear time trend:

$$\hat{\beta}_p^c = \alpha^c + \beta^c p + \epsilon_p^c \tag{B3}$$

Based on the estimated coefficients from Equation B3, we then compute the linear extrapolation of the estimates $\hat{\beta}_p^c$ for the pandemic cohort 2020. Based on the assumption that the employment trajectories of newly unemployed individuals in 2020 would have followed the (linear) path of the three preceding cohorts, these predicted values present the counterfactual employment trajectories in the absence of the Covid-19 pandemic. Finally, we compare the predicted path of the different labour market outcomes with the corresponding coefficient estimates that are obtained when estimating the model for the 2020 cohort.

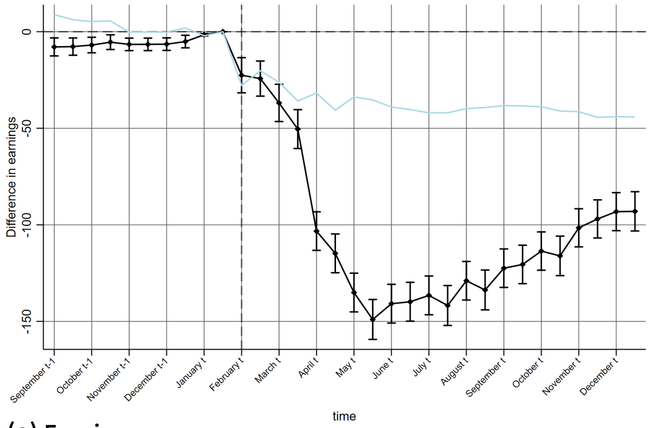
**Table B8.: Descriptive statistics: different cohorts**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | 2017 | | | 2018 | | | 2019 | | | 2020 | |
| | Treatment | Control | Standard. diff. | Treatment | Control | Standard. diff. | Treatment | Control | Standard. diff. | Treatment | Control | Standard. diff. |
| **Socio-demographic characteristics (at the time of matching)** | | | | | | | | | | | | |
| Age | 39.594 | 39.561 | 0.003 | 39.423 | 39.339 | 0.007 | 39.331 | 39.293 | 0.003 | 39.223 | 39.291 | -0.005 |
| | (12.266) | (12.272) | | (12.349) | (12.273) | | (12.365) | (12.301) | | (12.379) | (12.328) | |
| Male (fraction) | 0.581 | 0.580 | 0.001 | 0.589 | 0.589 | 0.002 | 0.604 | 0.602 | 0.004 | 0.596 | 0.597 | -0.001 |
| | (0.493) | (0.494) | | (0.492) | (0.492) | | (0.489) | (0.490) | | (0.491) | (0.491) | |
| Foreign (fraction) | 0.181 | 0.182 | -0.002 | 0.209 | 0.209 | -0.001 | 0.227 | 0.228 | -0.001 | 0.246 | 0.245 | 0.001 |
| | (0.385) | (0.386) | | (0.407) | (0.407) | | (0.419) | (0.419) | | (0.431) | (0.430) | |
| Low skilled (no completed apprenticeship, fraction) | 0.133 | 0.134 | -0.003 | 0.144 | 0.144 | -0.001 | 0.145 | 0.146 | -0.002 | 0.154 | 0.153 | 0.001 |
| | (0.339) | (0.340) | | (0.351) | (0.351) | | (0.352) | (0.353) | | (0.361) | (0.360) | |
| Middle skilled (completed apprenticeship, fraction) | 0.664 | 0.663 | 0.002 | 0.634 | 0.634 | 0.001 | 0.612 | 0.612 | 0.000 | 0.585 | 0.585 | -0.001 |
| | (0.472) | (0.473) | | (0.482) | (0.482) | | (0.487) | (0.487) | | (0.493) | (0.493) | |
| High skilled (tertiary education, completed) | 0.155 | 0.154 | 0.003 | 0.163 | 0.163 | 0.001 | 0.174 | 0.173 | 0.003 | 0.177 | 0.178 | -0.001 |
| | (0.362) | (0.361) | | (0.370) | (0.369) | | (0.379) | (0.378) | | (0.382) | (0.382) | |
| **Current employment (at the time of matching)** | | | | | | | | | | | | |
| Current wage | 67.673 | 66.411 | 0.030 | 68.699 | 67.002 | 0.040 | 71.615 | 69.897 | 0.039 | 72.853 | 71.552 | 0.029 |
| | (42.003) | (42.256) | | (41.568) | (42.660) | | (41.147) | (44.079) | | (45.017) | (44.939) | |
| Current earnings | 1,015.096 | 996.163 | 0.030 | 1,030.486 | 1,005.034 | 0.040 | 1,074.232 | 1,048.459 | 0.039 | 1,092.794 | 1,073.279 | 0.029 |
| | (630.051) | (633.841) | | (639.896) | (617.209) | | (661.182) | (647.903) | | (675.257) | (674.078) | |
| In regular employment (fraction) | 0.925 | 0.925 | 0.002 | 0.927 | 0.927 | 0.001 | 0.928 | 0.928 | 0.003 | 0.931 | 0.930 | 0.005 |
| | (0.263) | (0.264) | | (0.260) | (0.260) | | (0.258) | (0.259) | | (0.254) | (0.256) | |
| In full-time employment (fraction) | 0.646 | 0.642 | 0.010 | 0.644 | 0.642 | 0.003 | 0.653 | 0.652 | 0.002 | 0.641 | 0.642 | -0.001 |
| | (0.478) | (0.479) | | (0.479) | (0.479) | | (0.476) | (0.476) | | (0.480) | (0.479) | |
| Very small establishment (less than 10, fraction) | 0.248 | 0.242 | 0.015 | 0.230 | 0.230 | 0.001 | 0.222 | 0.222 | -0.000 | 0.206 | 0.204 | 0.005 |
| | (0.432) | (0.428) | | (0.421) | (0.421) | | (0.416) | (0.416) | | (0.405) | (0.403) | |
| Small establishment (10-49, fraction) | 0.306 | 0.304 | 0.004 | 0.301 | 0.301 | -0.001 | 0.297 | 0.298 | -0.002 | 0.306 | 0.306 | -0.001 |
| | (0.461) | (0.460) | | (0.459) | (0.459) | | (0.457) | (0.457) | | (0.461) | (0.461) | |
| Medium-sized establishment (50-249, fraction) | 0.267 | 0.272 | -0.011 | 0.280 | 0.280 | 0.000 | 0.281 | 0.281 | 0.001 | 0.285 | 0.286 | -0.003 |
| | (0.443) | (0.445) | | (0.449) | (0.449) | | (0.450) | (0.449) | | (0.452) | (0.452) | |
| Large establishment (more than 250, fraction) | 0.172 | 0.176 | -0.011 | 0.183 | 0.182 | 0.001 | 0.194 | 0.193 | 0.002 | 0.197 | 0.197 | -0.001 |
| | (0.378) | (0.381) | | (0.386) | (0.386) | | (0.395) | (0.395) | | (0.398) | (0.398) | |

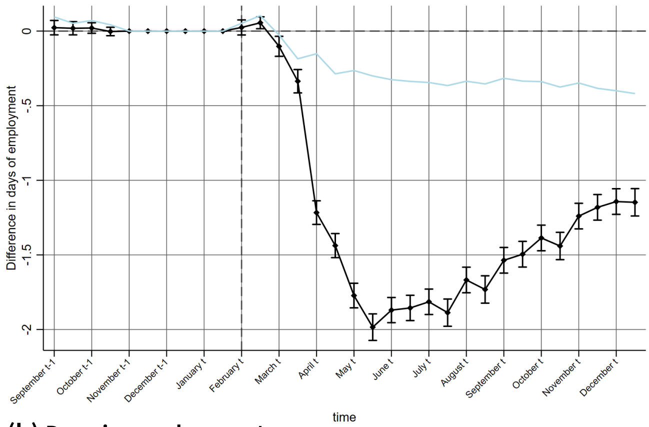
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | 2017 | | | 2018 | | | 2019 | | | 2020 | |
| | Treatment | Control | Standard. diff. | Treatment | Control | Standard. diff. | Treatment | Control | Standard. diff. | Treatment | Control | Standard. diff. |
| Estimated AKM firm effect | -0.186 | -0.183 | -0.010 | -0.184 | -0.187 | 0.014 | -0.169 | -0.174 | 0.021 | -0.161 | -0.171 | 0.039 |
| | (0.264) | (0.264) | | (0.266) | (0.263) | | (0.264) | (0.263) | | (0.265) | (0.267) | |
| **Employment biography** | | | | | | | | | | | | |
| Work experience | 12.269 | 12.176 | 0.010 | 11.916 | 11.839 | 0.008 | 11.946 | 11.823 | 0.013 | 11.893 | 11.817 | 0.008 |
| | (9.573) | (9.525) | | (9.710) | (9.530) | | (9.921) | (9.758) | | (10.056) | (9.963) | |
| Tenure in current establishment | 3.177 | 3.202 | -0.005 | 2.969 | 3.040 | -0.014 | 2.951 | 3.024 | -0.014 | 2.987 | 2.967 | 0.004 |
| | (5.104) | (5.052) | | (4.953) | (4.996) | | (4.954) | (5.085) | | (5.139) | (4.988) | |
| Tenure in current occupation | 5.911 | 5.808 | 0.015 | 5.674 | 5.606 | 0.010 | 5.680 | 5.748 | -0.010 | 5.734 | 5.785 | -0.007 |
| | (7.141) | (7.022) | | (7.014) | (6.960) | | (7.095) | (7.159) | | (7.169) | (7.173) | |
| Number of job changes | 2.728 | 2.658 | 0.022 | 2.731 | 2.673 | 0.018 | 2.790 | 2.699 | 0.028 | 2.885 | 2.795 | 0.027 |
| | (3.188) | (3.102) | | (3.223) | (3.186) | | (3.273) | (3.238) | | (3.383) | (3.311) | |
| Being unemployed before (fraction) | 0.767 | 0.763 | 0.010 | 0.768 | 0.768 | -0.000 | 0.766 | 0.767 | -0.003 | 0.758 | 0.757 | 0.002 |
| | (0.423) | (0.425) | | (0.422) | (0.422) | | (0.423) | (0.423) | | (0.429) | (0.429) | |
| Employed in manufacturing sector (fraction) | 0.396 | 0.395 | 0.001 | 0.379 | 0.379 | 0.001 | 0.395 | 0.394 | 0.000 | 0.379 | 0.380 | -0.001 |
| | (0.489) | (0.489) | | (0.485) | (0.485) | | (0.489) | (0.489) | | (0.485) | (0.485) | |
| Employed in service sector (fraction) | 0.593 | 0.594 | -0.002 | 0.611 | 0.612 | -0.001 | 0.596 | 0.596 | -0.000 | 0.613 | 0.613 | 0.001 |
| | (0.491) | (0.491) | | (0.487) | (0.487) | | (0.491) | (0.491) | | (0.487) | (0.487) | |
| Estimated AKM worker effect | 4.365 | 4.367 | -0.006 | 4.363 | 4.358 | 0.012 | 4.365 | 4.368 | -0.009 | 4.364 | 4.365 | -0.003 |
| | (0.360) | (0.357) | | (0.368) | (0.354) | | (0.374) | (0.371) | | (0.382) | (0.386) | |
| N | 60,808 | 61,001 | | 59,690 | 65,548 | | 58,113 | 58,210 | | 62,393 | 61,347 | |

Note: For each treatment year - 2017, 2018, 2019 and 2020 - the first two columns show the mean value and standard deviation (in parentheses) of individual characteristics that are measured at the first half of November $t-1$ (the point for the weighting). The third column reports the standardised difference between the first two columns, which is defined as $\Delta_X = (\bar{X}_1 - \bar{X}_0) \,/\, ((S_1^2 + S_0^2)/2)^{0.5}$, where $\bar{X}_w$ is the sample mean of the treated ($w = 1$) or (weighted) control ($w = 0$) individuals and $S_w^2$ are the respective sample variances. The corresponding control year for the treatment year $t$ is $t-1$. Note that the observations for the AKM worker and firm fixed effects are smaller than the reported number of observations.
Source: IEB, BHP, own calculations.

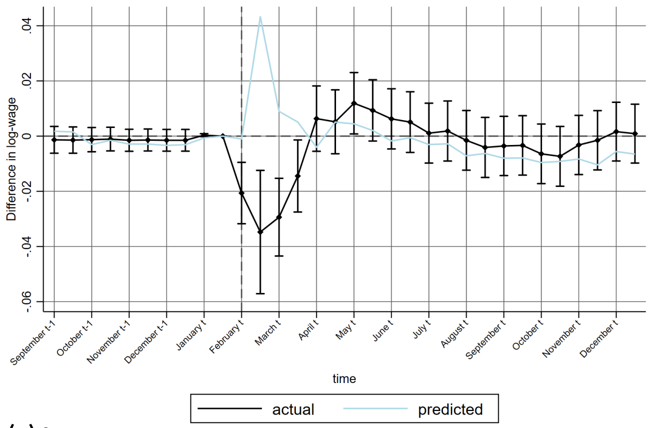
**Figure B8.: The effect of the Covid-19 pandemic on earnings, employment and wages – Counterfactual scenarios**




**(a) Earnings**




**(b) Days in employment**



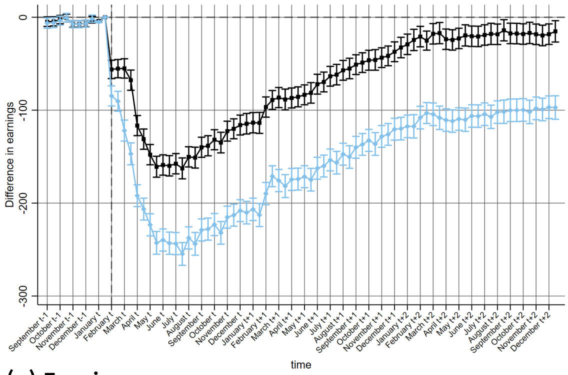

**(c) Log wages**

Note: Figure B8 shows the estimated coefficients $\hat{\beta}_p$ from Equation 1 (black line) and the predicted $\hat{\beta}_p^c$ from Equation B3 (blue line) with earnings (panel (a)), days in employment (panel (b)) and log wages (panel (c)) as dependent variables. The estimation is weighted by the inverse propensity score. $t$ denotes the year in which the individuals in the sample became unemployed. For treated individuals $t$ refers to the years 2017, 2018, 2019 (blue line) and 2020 (black line) and for control individuals to the years 2016, 2017, 2018 (blue line) and 2019 (black line). The vertical bars represent the 95% confidence interval which are based on standard errors that are clustered at the individual level.

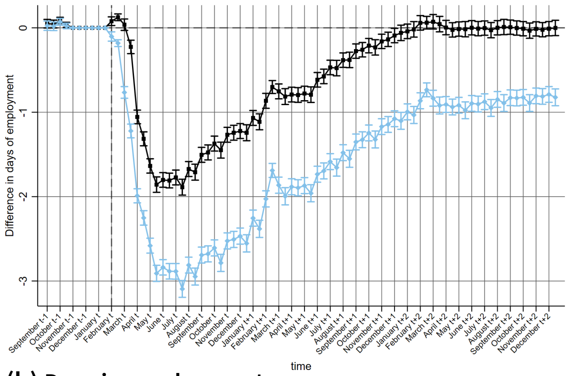
Source: IEB, BHP, own calculations.

# B.7 Short-time work

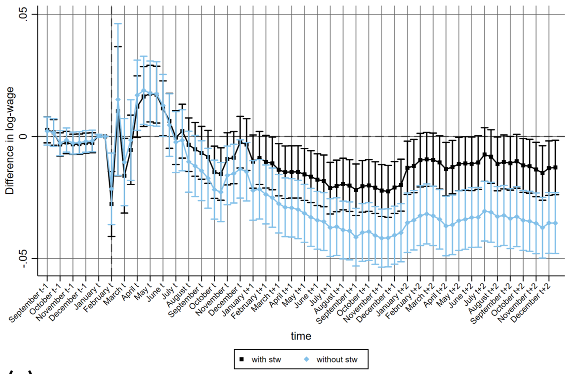
**Figure B9.: The effect of the Covid-19 pandemic on earnings, employment and wages – Role of short-time work**




**(a) Earnings**




**(b) Days in employment**




**(c) Log wages**

Note: Figure B9 shows the estimated coefficients $\hat{\beta}_p$ from Equation 1 with earnings (panel (a)), days in employment (panel (b)) and log wages (panel (c)) as dependent variables using data on individuals who became unemployed in February 2020 and 2017, respectively. The black line indicates the baseline estimates and the blue line indicates the estimates for a sample restricted to individuals who never had been employed in a firm that used short-time work. The estimation is weighted by the inverse propensity score.$t$ denotes the year in which the individuals in the sample became unemployed. This means that individuals in the treatment group are observed from September 2019 until December 2022, while the control group is observed from September 2016 until 2019. The vertical bars represent the 95% confidence interval which are based on standard errors that are clustered at the individual level.

Source: IEB, BHP, own calculations.

## B.8 Sensitivity to changes in sample restrictions

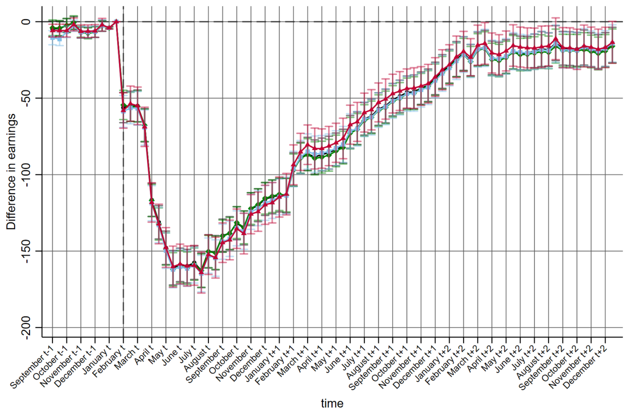
In this section, robustness checks for different sample restrictions are presented.

The first sample restriction implies that individuals have to be employed from at least November in the same establishment and occupation. Figure B10 shows the results for shorter (December, grey line) and longer (October, blue line; August, red line) durations in employment as well as for the baseline duration (November, black line). Overall, the estimated effects are similar, indicating that longer or shorter lengths of employment preceding the transition into unemployment do not substantially change results.

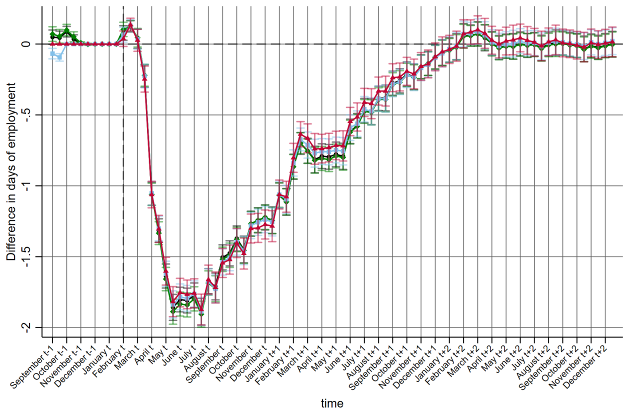
The baseline sample is restricted to individuals who enter unemployment in the first half of February. Figure B11 displays results if individuals who enter unemployment during the second half of February (for the treatment as well as the control group) are chosen. As can be seen, for the main outcomes - earnings, employment, wages - the negative effect size is stronger for individuals entering unemployment in the second half and remains visible for longer, at least for earnings and employment. However, the differences are small and the overall pattern is similar compared to the baseline specification.

At the same time, the baseline restriction includes all individuals who were unemployed for at least one day in (the first half of) February. This sample potentially includes job-to-job transitions. In order the exclude the latter, we run a separate analysis for individuals who were unemployed for at least one month. Figure B12 shows that the results do not change considerably by imposing this restriction (except log wages in the longer run).

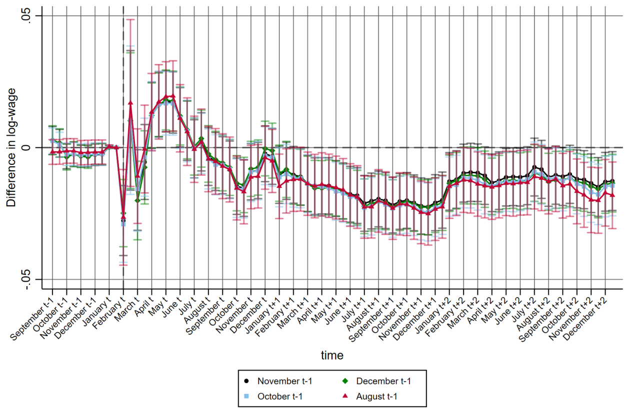
Figure B10.: The effect of the Covid-19 pandemic on earnings, employment and wages – Different sample restrictions I




(a) Earnings




(b) Days in employment

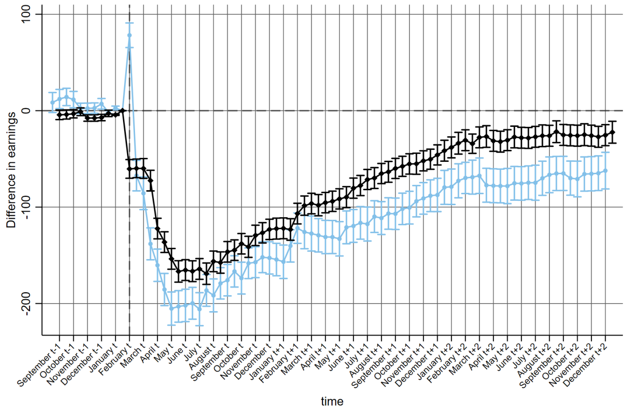



(c) Log wages

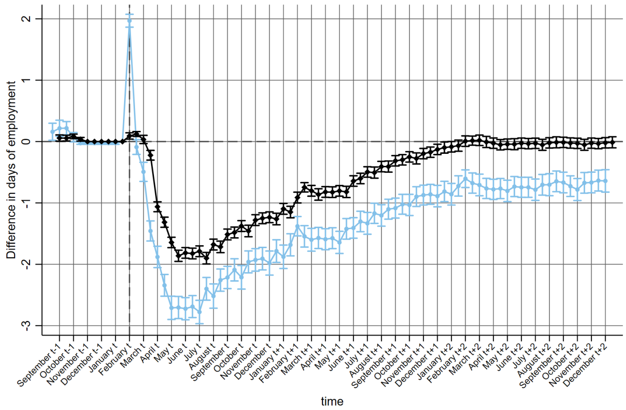
Note: Figure B10 shows the estimated coefficients $\hat{\beta}_p$ from Equation 1 with earnings (panel (a)), days in employment (panel (b)) and log wages (panel (c)) as dependent variables. Estimates are shown for different sample restrictions: individuals that were employed since December $t-1$ (green line), November $t-1$ (black line), October $t-1$ (blue line) and August $t-1$ (red line). The estimation is weighted by the inverse propensity score. $t$ denotes the year in which the individuals in the sample became unemployed. This means that individuals in the treatment group are observed from September 2019 until December 2022, while the control group is observed from September 2016 until 2019. The vertical bars represent the 95% confidence interval which are based on standard errors that are clustered at the individual level.

Source: IEB, BHP, own calculations.

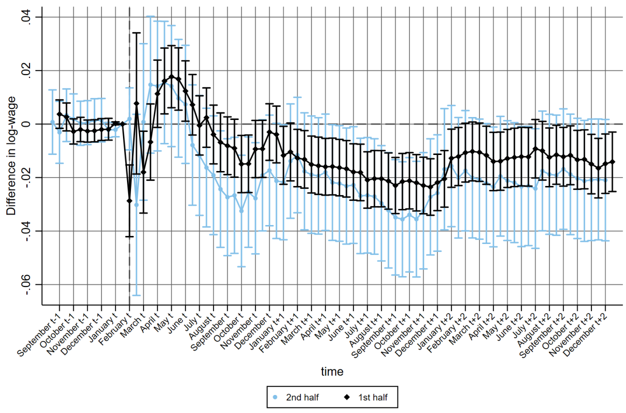
**Figure B11.: The effect of the Covid-19 pandemic on earnings, employment and wages – Different sample restrictions II**




(a) Earnings




(b) Days in employment

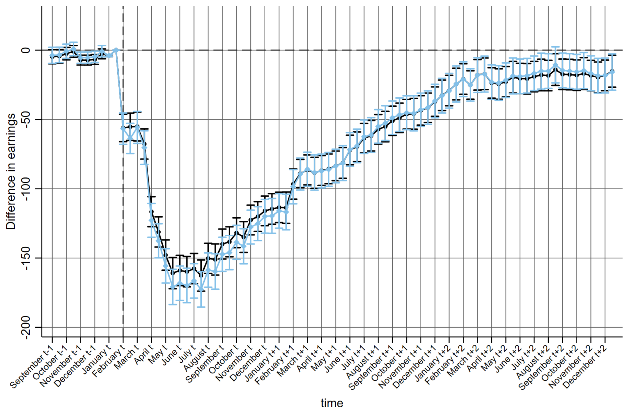



(c) Log wages

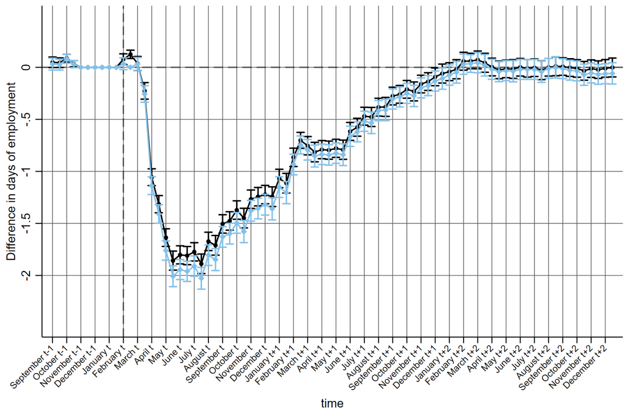
Note: Figure B11 shows the estimated coefficients $\hat{\beta}_p$ from Equation 1 with earnings (panel (a)), days in employment (panel (b)) and log wages (panel (c)) as dependent variables. Estimates are shown for different samples: individuals who became unemployed in the first half of February $t$ (black line) and in the second half (blue line). The estimation is weighted by the inverse propensity score. $t$ denotes the year in which the individuals in the sample became unemployed. This means that individuals in the treatment group are observed from September 2019 until December 2022, while the control group is observed from September 2016 until 2019. The vertical bars represent the 95% confidence interval which are based on standard errors that are clustered at the individual level.

Source: IEB, BHP, own calculations.

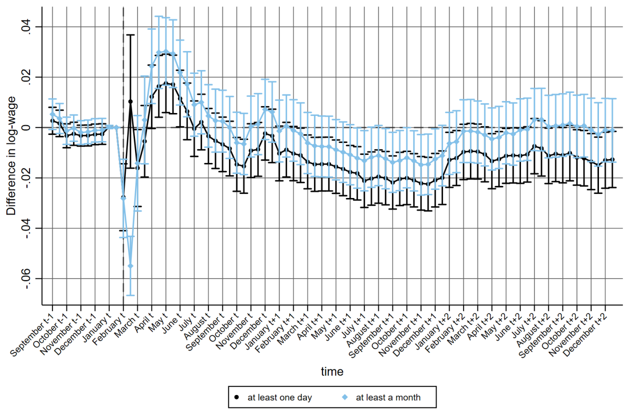
Figure B12.: The effect of the Covid-19 pandemic on earnings, employment and wages – Different sample restrictions III




(a) Earnings




(b) Days in employment




(c) Log wages

Note: Figure B12 shows the estimated coefficients $\hat{\beta}_p$ from Equation 1 with earnings (panel (a)), days in employment (panel (b)) and log wages (panel (c)) as dependent variables. Estimates are shown for different sample restrictions: individuals that were unemployed in February $t$ at least one day (black line) or at least one month (blue line). The estimation is weighted by the inverse propensity score. $t$ denotes the year in which the individuals in the sample became unemployed. This means that individuals in the treatment group are observed from September 2019 until December 2022, while the control group is observed from September 2016 until 2019. The vertical bars represent the 95% confidence interval which are based on standard errors that are clustered at the individual level.

Source: IEB, BHP, own calculations.

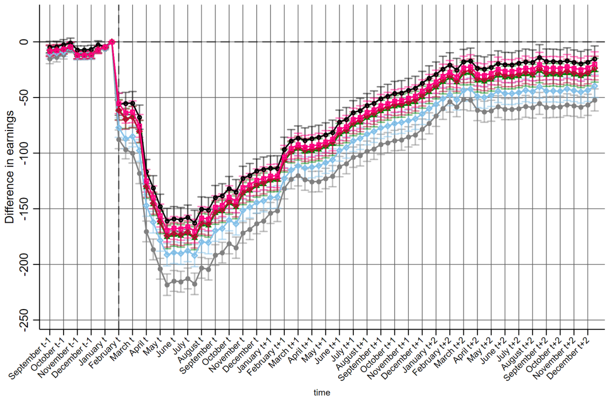
## B.9 Sensitivity to changes in the set of weighting covariates

Figure B13 presents the results of estimating Equation 1 for earnings, days in employment and log wages using different sets of IPW covariates. The grey line shows the development of outcomes for the model without any weighting and the coloured lines show results when increasing sets of variables are used in the weighting procedure: socio-demographic (blue), firm characteristics (green), current employment characteristics (red) and employment biography (pink). The black line adds the wage-growth variable and, as such, corresponds to the final set of weighting variables of our approach.

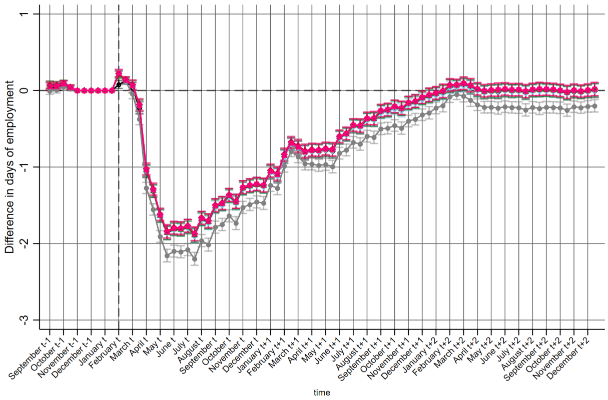
Overall, it can be seen that the estimation results are not substantially affected by the choice of IPW variables. The same holds for the other outcomes that are investigated in the paper.[27]

[27] Results are available upon request.

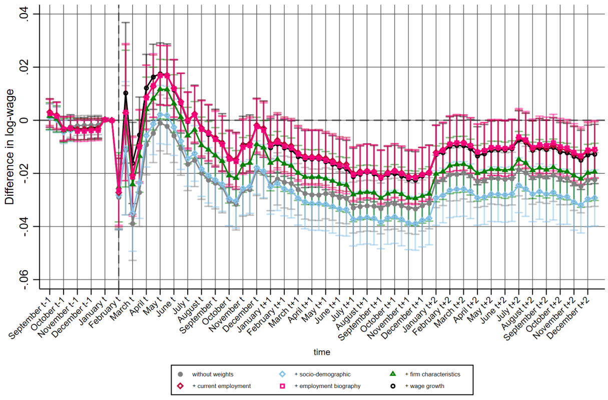
**Figure B13.: The effect of the Covid-19 pandemic on earnings, employment and wages – Different weighting variables**




**(a) Earnings**




**(b) Days in employment**



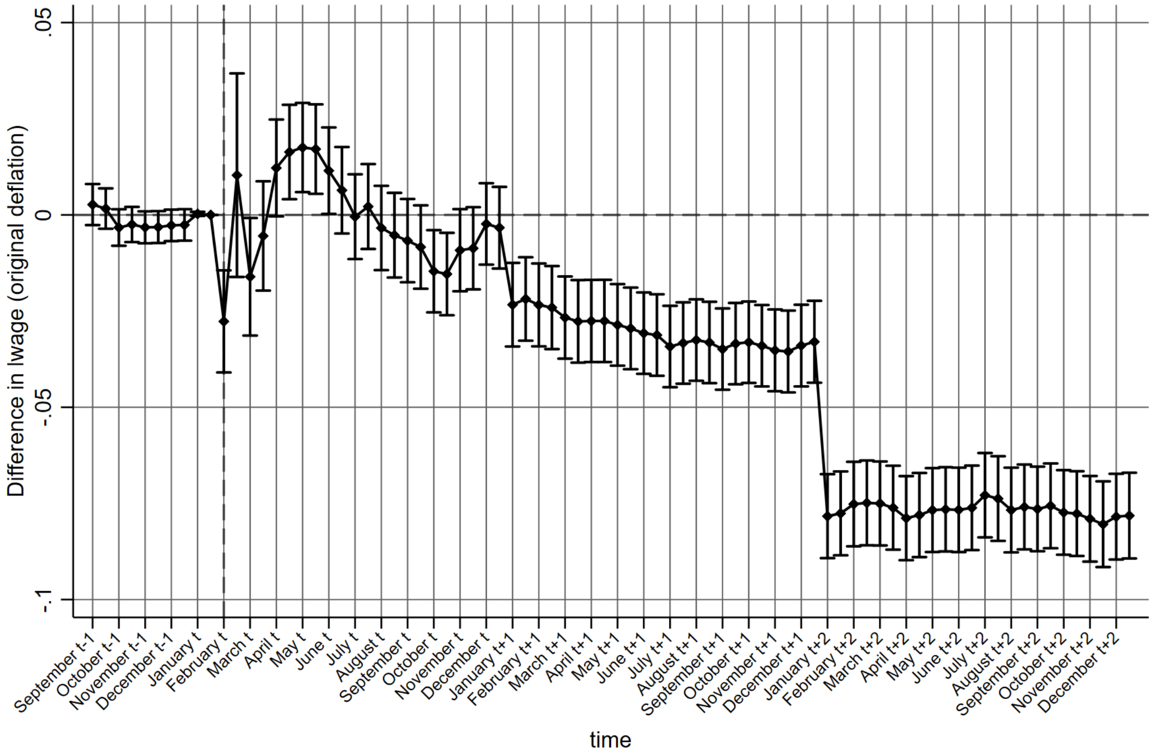

**(c) Log wages**

Note: Figure B13 shows the estimated coefficients $\hat{\beta}_p$ from Equation 1 with earnings (panel (a)), days in employment (panel (b)) and log wages (panel (c)) as dependent variables using data on individuals who became unemployed in February 2020 and 2017, respectively. The estimation is weighted by the inverse propensity score with a growing set of variables: without weights (grey line), with socio-demographics (blue line), firm characteristics (green line), current employment characteristics (red line), employment biography (pink line) and the full set of variables (black line). $t$ denotes the year in which the individuals in the sample became unemployed. This means that individuals in the treatment group are observed from September 2019 until December 2022, while the control group is observed from September 2016 until 2019. The vertical bars represent the 95% confidence interval which are based on standard errors that are clustered at the individual level.

Source: IEB, BHP, own calculations.

## B.10 Wage variable

**Figure B14.: The effect of the Covid-19 pandemic on log wages – Original deflation**



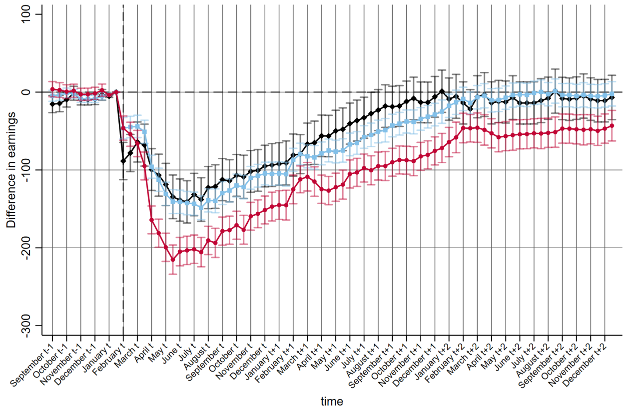

Note: Figure B14 shows the estimated coefficients $\hat{\beta}_p$ from Equation 1 with log wages with the original deflation as dependent variable using data on individuals who became unemployed in February 2020 and 2017, respectively. The estimation is weighted by the inverse propensity score. $t$ denotes the year in which the individuals in the sample became unemployed. This means that individuals in the treatment group are observed from September 2019 until December 2022, while the control group is observed from September 2016 until 2019. The vertical bars represent the 95% confidence interval which are based on standard errors that are clustered at the individual level.

Source: IEB, BHP, own calculations.

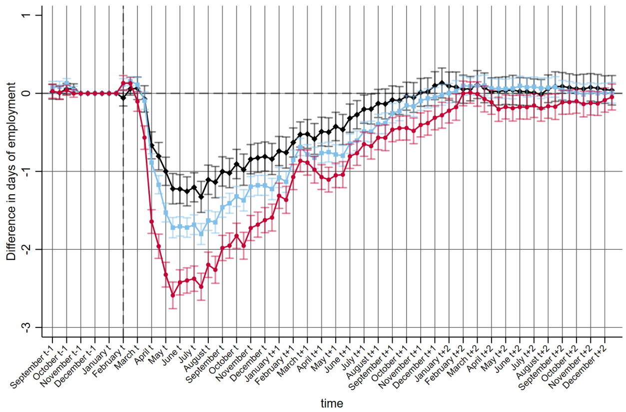
## B.11 Sensitivity to LWA distribution

Equation 1 is estimated for the main outcomes - earnings, employment and log wages - for different subsets of the LWA distribution. These subsets are defined based on different thresholds of the LWA distribution. We define individuals from low-LWA occupations (below the 25% quantile, grey line), from medium-LWA occupations (between the 25% and the 75% quantile, blue line) and from high-LWA occupations (above the 75% quantile, black line). The group of individuals from medium-LWA occupations are pooled in order to get a better understanding of less and high affected occupations.

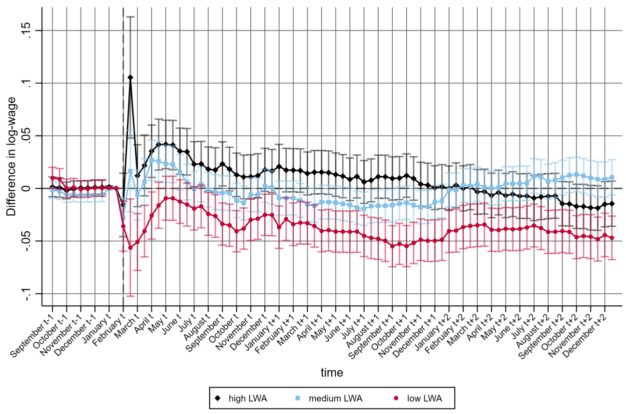
## Figure B15.: The effect of the Covid-19 pandemic on earnings, employment and wages – Different subsets of LWA distribution




**(a) Earnings**




**(b) Days in employment**



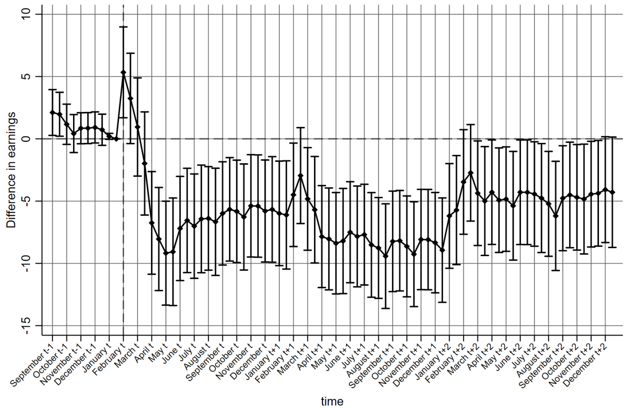

**(c) Log wages**

Note: Figure B15 shows the estimated coefficients $\hat{\beta}_p$ from Equation 1 with earnings (panel (a)), days in employment (panel (b)) and log wages (panel (c)) as dependent variables using data on individuals who became unemployed in February 2020 and 2017, respectively. Estimates are shown for different subsets of the LWA distribution: low LWA (below the 25% quantile), medium LWA (between the 25% and the 75% quantile) as well as high LWA (above the 75% quantile). The estimation is weighted by the inverse propensity score. $t$ denotes the year in which the individuals in the sample became unemployed. This means that individuals in the treatment group are observed from September 2019 until December 2022, while the control group is observed from September 2016 until 2019. The vertical bars represent the 95% confidence interval which are based on standard errors that are clustered at the individual level.

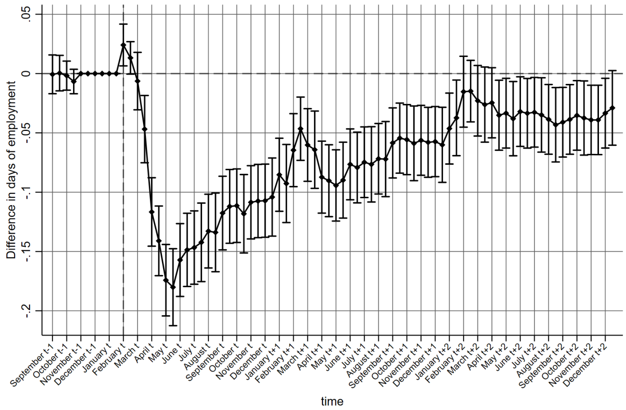
Source: IEB, BHP, own calculations.

# B.12 Occupational heterogeneity: event-study plots

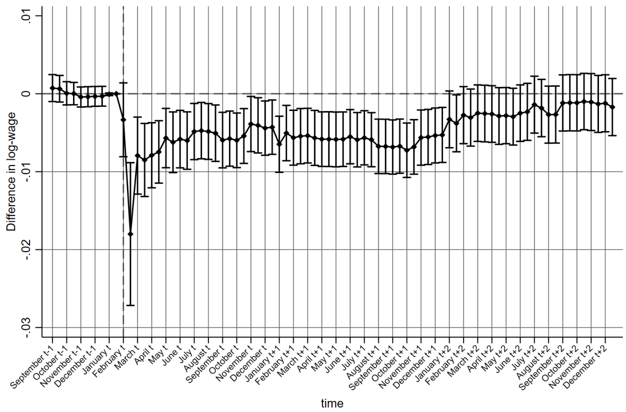
**Figure B16.: The heterogeneous effect of the Covid-19 pandemic by LWA**




**(a) Earnings**




**(b) Days in employment**




**(c) Log wages**

Note: Figure B16 shows the estimated coefficients $\hat{\phi}_p$ from Equation 2 with earnings (panel (a)), days in employment (panel (b)) and log wages (panel (c)) as dependent variables using data on individuals who became unemployed in February 2020 and 2017, respectively. The estimation is weighted by the inverse propensity score. $t$ denotes the year in which the individuals in the sample became unemployed. This means that individuals in the treatment group are observed from September 2019 until December 2022, while the control group is observed from September 2016 until 2019. The vertical bars represent the 95% confidence interval which are based on standard errors that are clustered at the worker level.

Source: IEB, BHP, own calculations.

## B.13 Occupational heterogeneity: employment mechanisms

**Table B9.: Effect heterogeneity by LWA: employment adjustments**

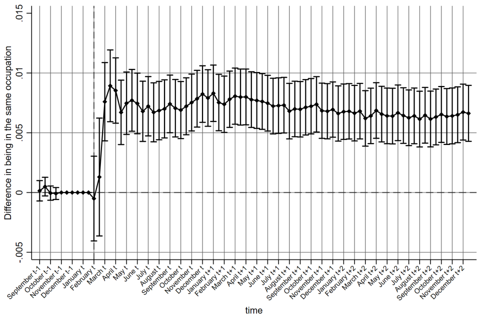
| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Days in unemployment | | Days out of the labour market | | Days in other states | |
| | Abs. | Rel. | Abs. | Rel. | Abs. | Rel. |
| Average | | | | | | |
| Treatment period | 0.080*** | 0.098 | 0.007 | -0.042 | 0.006 | -0.058 |
| | (0.007) | | (0.008) | | (0.004) | |
| Pre-treatment | 0.002 | -0.119 | -0.001 | 1.500 | 0.000 | -0.044 |
| | (0.002) | | (0.001) | | (0.001) | |
| Feb-May 2020 | 0.067*** | 0.057 | 0.007 | -0.051 | 0.004 | -0.018 |
| | (0.011) | | (0.005) | | (0.005) | |
| Jun-Sep 2020 | 0.104*** | 0.046 | 0.015* | -0.041 | 0.018** | -0.064 |
| | (0.014) | | (0.008) | | (0.007) | |
| Oct-Dec 2020 | 0.069*** | 0.041 | 0.020** | -0.052 | 0.020*** | -0.253 |
| | (0.013) | | (0.009) | | (0.007) | |
| 2021 | 0.048*** | 0.070 | 0.011 | -0.069 | 0.011** | -0.425 |
| | (0.009) | | (0.010) | | (0.005) | |
| 2022 | 0.015** | 0.142 | 0.010 | -0.200 | 0.008 | -0.122 |
| | (0.007) | | (0.012) | | (0.005) | |
| Cumulative | | | | | | |
| Treatment period | 5.583*** | 0.098 | 0.475 | -0.042 | 0.394 | -0.058 |
| | (0.483) | | (0.564) | | (0.284) | |
| N | 10,583,520 | | 10,583,520 | | 10,583,520 | |

Note: Table B9 shows the estimated coefficients $\hat{\phi}_p$ from Equation 2 with days in unemployment, out of the labour market as well as the remaining labour market status (which includes days in a measure, days receiving transfer payments and days registered in the unemployment data but not being unemployed) as dependent variables using data on individuals who became unemployed in February 2020 and 2017, respectively. The estimation is weighted by the inverse propensity score. The table displays the averaged $\hat{\phi}_p$ and the ratio $\frac{\hat{\phi}_p}{\hat{\beta}_p}$ for specific time periods and the (treatment) effect averaged over the whole period. Standard errors clustered at the individual level are in parentheses. Note that it is possible that individuals are in several labour market states at the same time, but Table B9 shows only one labour market per individual per period.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: IEB, BHP, own calculations.

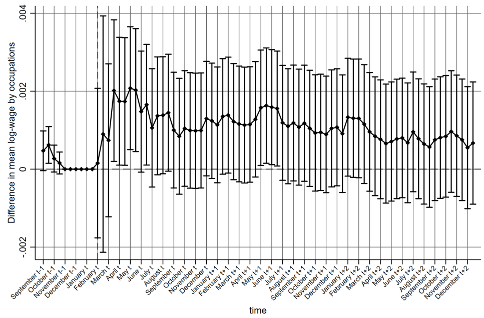
## B.14 Occupational heterogeneity: wage mechanisms

### B.14.1. Event-study plots by LWA

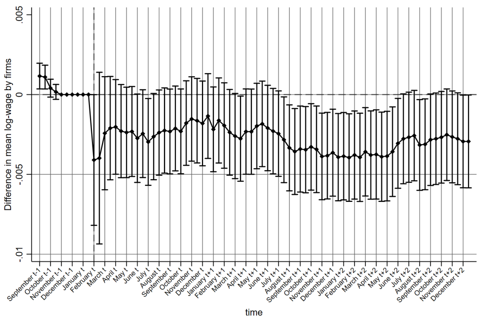
## Figure B17.: Wage mechanisms by LWA: event-study plots



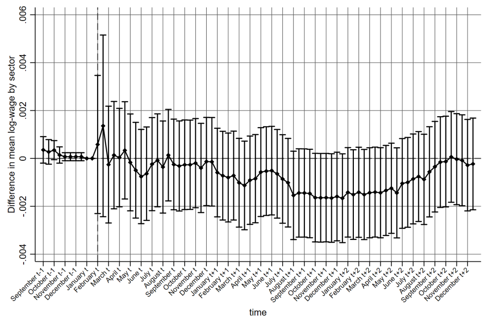

**(a) Occupational mobility**



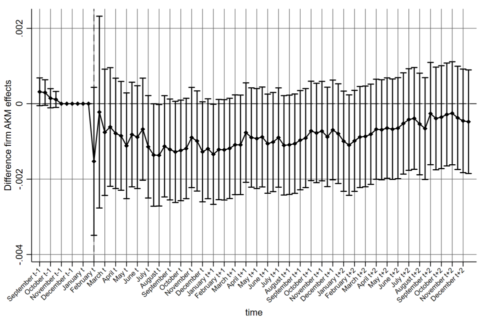

**(b) Mean wage (occupation)**



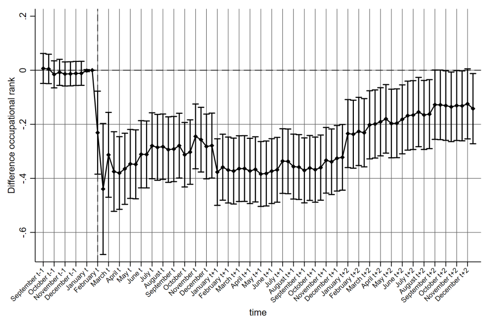

**(c) Mean wage (firm)**



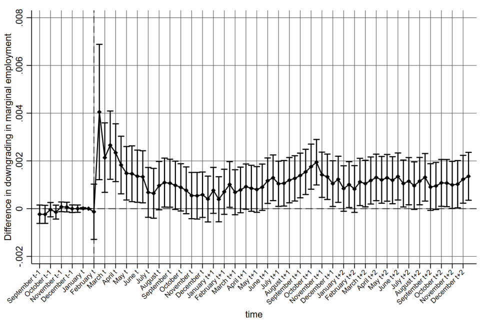

**(d) Mean wage (sector)**



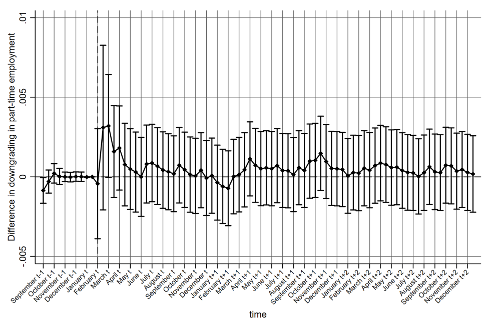

**(e) AKM firm effect**




**(f) Occupational rank**




**(g) Downgrading (marginal)**



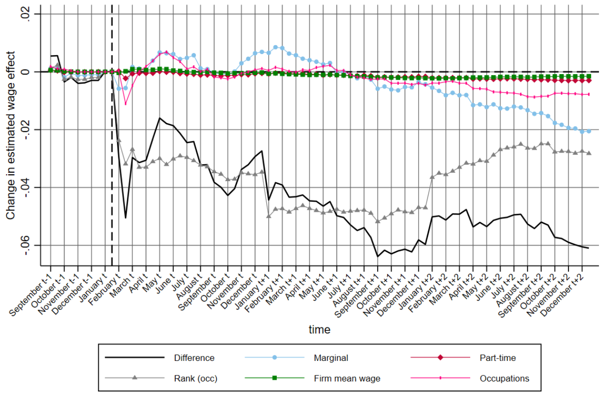

**(h) Downgrading (part-time)**

Note: Figure B17 shows the estimated coefficients $\hat{\phi}_p$ from Equation 2 with occupational mobility (panel (a)), occupational mean log wage (panel (b)), firm mean log wage (panel(c)), sector mean log wage (panel (d)), AKM firm fixed effects (panel (e)), rank in the occupational wage distribution (panel (f)), downgrading from regular into marginal employment (panel (g)) as well as downgrading from full-time into part-time employment (panel (h)) as dependent variables using data on individuals who became unemployed in February 2020 and 2017, respectively. The estimation is weighted by the inverse propensity score. $t$ denotes the year in which the individuals in the sample became unemployed. This means that individuals in the treatment group are observed from September 2019 until December 2022, while the control group is observed from September 2016 until 2019. The vertical bars represent the 95% confidence intervals which are based on standard errors that are clustered at the individual level.

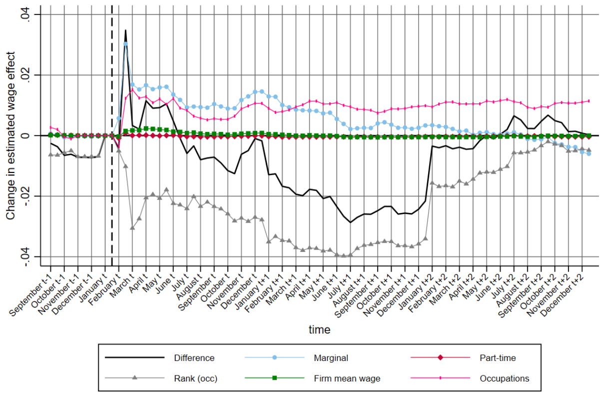
Source: IEB, BHP, own calculations.

## B.14.2. Gelbach decomposition by LWA

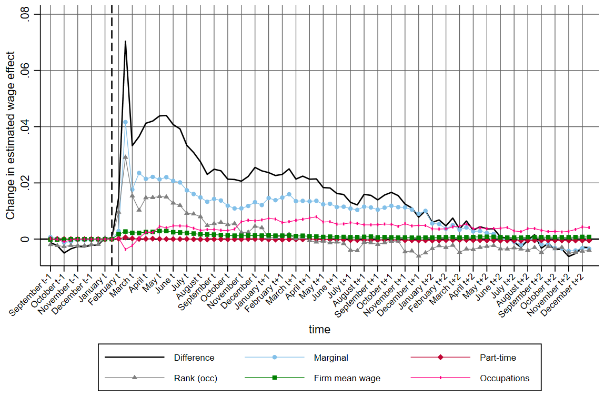
**Figure B18.: Decomposition of the wage effect by LWA**




**(a) Low LWA**




**(b) Medium LWA**



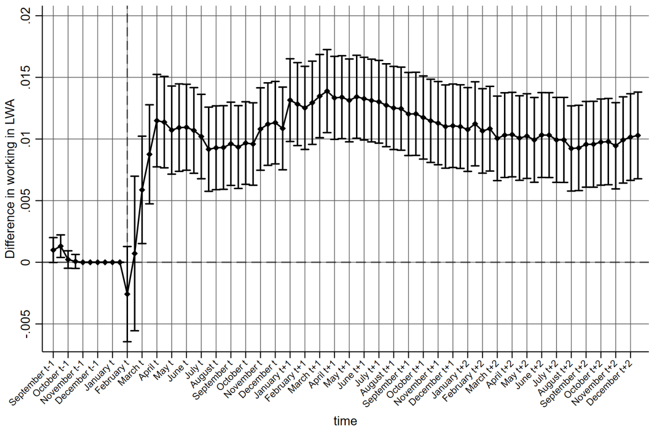

**(c) High LWA**

Note: Figure B18 shows the change in the estimated wage effect when additional control variables are added to the baseline model of Equation 1 (solid line) for different subsets of the LWA distribution: low LWA (below the 25% quantile), medium LWA (between the 25% and the 75% quantile) as well as high LWA (above the 75% quantile). Moreover, it shows how much each additional control variable (or set of control variables) contributes to this change: downgrading into marginal employment (circles), downgrading into part-time employment (diamonds), rank in the occupational wage distribution (triangles), firm mean wage (squares) and occupation dummies (X) using data on individuals who became unemployed in February 2020 and 2017, respectively. The estimation is weighted by the inverse propensity score. $t$ denotes the year in which the individuals in the sample became unemployed. This means that individuals in the treatment group are observed from September 2019 until December 2022, while the control group is observed from September 2016 until 2019.

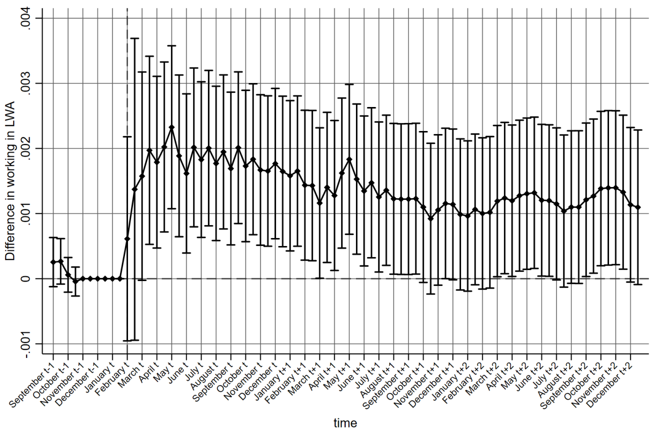
Source: IEB, BHP, own calculations.

## B.14.3. LWA as dependent variable

**Figure B19.: The effect of the Covid-19 pandemic on LWA**




**(a) Baseline model**



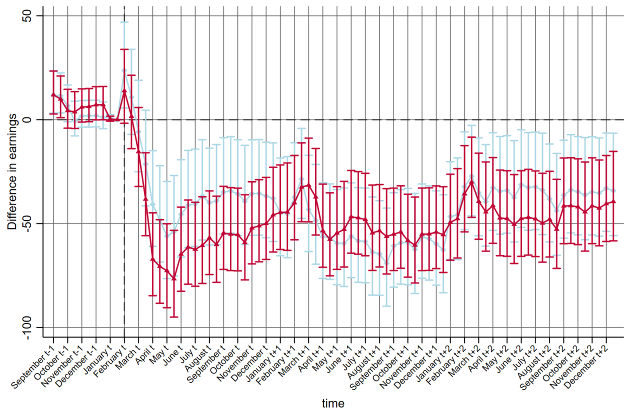

**(b) Heterogeneous model**

Note: Figure B19 shows the estimated coefficients $\hat{\beta}_p$ from Equation 1 (panel (a)) and the estimated coefficients $\hat{\phi}_p$ from Equation 2 (panel (b)) with LWA as dependent variable using data on individuals who became unemployed in February 2020 and 2017, respectively. The estimation is weighted by the inverse propensity score. $t$ denotes the year in which the individuals in the sample became unemployed. This means that individuals in the treatment group are observed from September 2019 until December 2022, while the control group is observed from September 2016 until 2019. The vertical bars represent the 95% confidence interval which are based on standard errors that are clustered at the individual level.

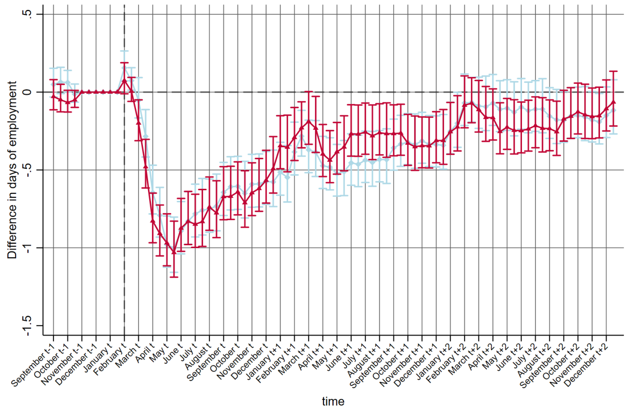
Source: IEB, BHP, own calculations.

# B.15 Occupational heterogeneity: parallel trends assumption

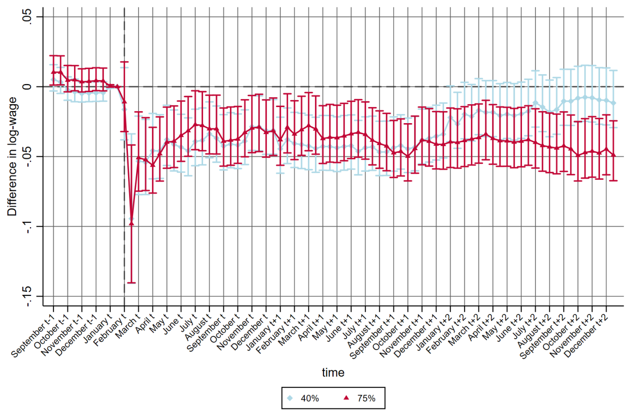
**Figure B20.: The effect of the Covid-19 pandemic on earnings, employment and wages – Assessing parallel trends**




**(a) Earnings**




**(b) Days in employment**



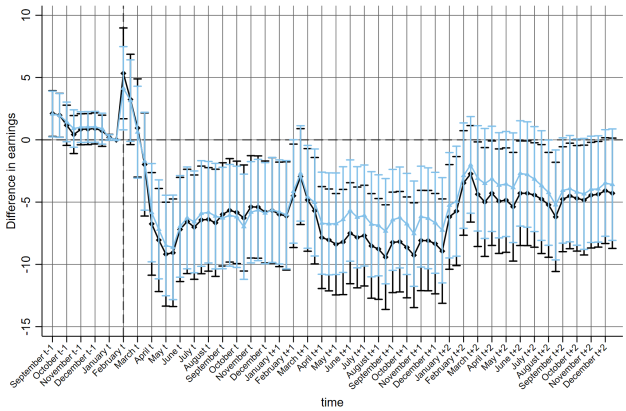

**(c) Log wages**

Note: Figure B20 shows the estimated coefficients $\hat{\phi}_p$ from an adjusted Equation 2 in which instead of the continuous (inverse) LWA index a dummy for low- (below the 33% quantile of the LWA distribution) and high-LWA occupations (above the 75%-quantile of the LWA distribution) is applied, using data on individuals who became unemployed in February 2020 and 2017, respectively. The dependent variables are earnings (panel (a)), days in employment (panel (b)) and log wages (panel (c)). The estimation is weighted by the inverse propensity score. $t$ denotes the year in which the individuals in the sample became unemployed. This means that individuals in the treatment group are observed from September 2019 until December 2022, while the control group is observed from September 2016 until 2019. The vertical bars represent the 95% confidence interval which are based on standard errors that are clustered at the individual level.

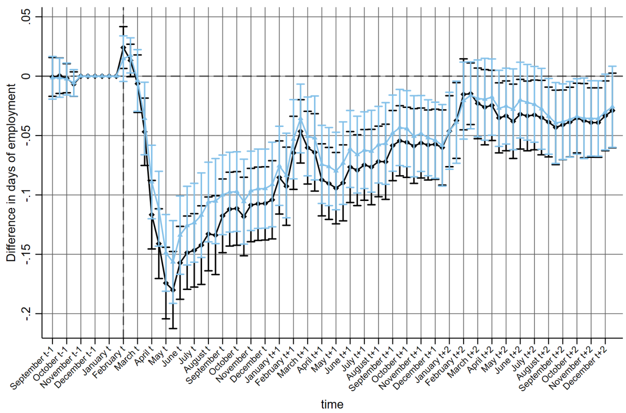
Source: IEB, BHP, own calculations.

# B.16 Occupational heterogeneity: accounting for confounding variables

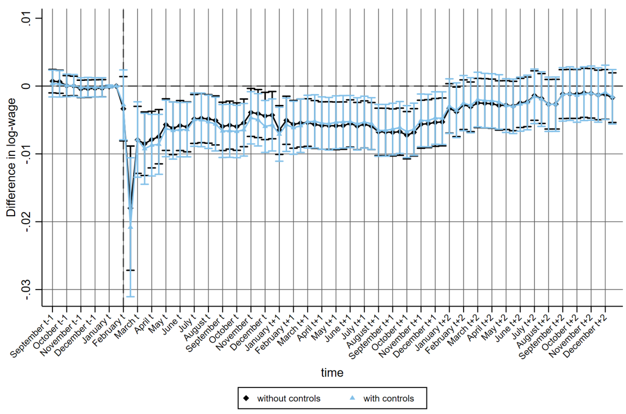
**Figure B21.: The effect of the Covid-19 pandemic on earnings, employment and wages - Additional control variables**




**(a) Earnings**




**(b) Employment**




**(c) Log wage**

Note: Figure B21 shows the estimated coefficients $\hat{\phi}_p$ from Equation 2 (blue line) as well as the coefficients from Equation 2 including interaction terms of gender, skill level and firm size (measured at the matching point in November $t-1$) with the treatment dummy, the event time and a combination of both (black line) using data on individuals who became unemployed in February 2020 and 2017, respectively. The dependent variables are earnings (panel (a)), days in employment (panel (b)) and log wages (panel (c)). The estimation is weighted by the inverse propensity score. $t$ denotes the year in which the individuals in the sample became unemployed. This means that individuals in the treatment group are observed from September 2019 until December 2022, while the control group is observed from September 2016 until 2019. The vertical bars represent the 95% confidence intervals which are based on standard errors that are clustered at the individual level.

Source: IEB, BHP, own calculations.

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