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M. Sc. Economics

Labor Market Effects of the COVID-19 Pandemic on Individuals with Migratory Background in Germany

Master Thesis

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1 Introduction

The 5th of May 2023 marked the end of the three years and ninety-six days during which the COVID-19 pandemic was formally declared as a 'public health emergency of international concern' by the WHO. The public health measures taken to control the spread of the disease had major effects on labor markets. As this external shock phenomenon has effectively come to an end and data is increasingly both available and refined, the moment is favorable for an analysis that looks beyond the effects of the initial phase of the pandemic and includes periods of labor market recovery.

The virus and the accompanying infection prevention measures left no one untouched. However, as the initially unifying effects of the outbreak receded, an unequal distribution of burdens became apparent in both public perception and scientific literature. These studies were rarely revisited as the pandemic progressed. This thesis scrutinizes the effect that the COVID-19 pandemic and related policy responses had on labor market outcomes of a range of migrant groups in Germany, extending the time horizon from 2020 to 2021. Ultimately, the analysis aims to answer the question of whether there were significant, unexplained differences in the magnitude of employment shocks for different migrant groups compared to native Germans.

This thesis proceeds as follows. In the next section, the COVID-19 labor market shock and the ensuing adjustments are delineated. In Section 3, relevant literature around general and COVID-19-specific migrant-native gaps on the labor market is introduced. Section 4 describes the data and definitions used before developing and deploying an analytical strategy. The section closes by presenting the results. Section 5 discusses the analysis before Section 6 concludes.

2 Context

The aggregate economic impacts of COVID have by now been thoroughly examined and will not be discussed in detail here. However, to serve as a reference throughout the following sections, the main features of the labor market shock associated with the pandemic are outlined before briefly describing the relevant adjustment mechanisms, with a focus on short-time work. The section closes with a review of literature on the long-term effects of labor market inequalities.

2.1 Labor Market Developments

As a reference for the following paragraphs, Figure 1 provides a timeline on the number of individuals in unemployment and short-time work as well as the stringency of infection prevention policies. The timeline is restricted to 2020 and 2021 as this is the relevant timeframe as defined by (panel) data availability. The focus is on the main shock – the infection prevention measures – and the main outcomes of interest, the use of short-time work and unemployment.

Figure 1: Labor Market & Measure Timeline

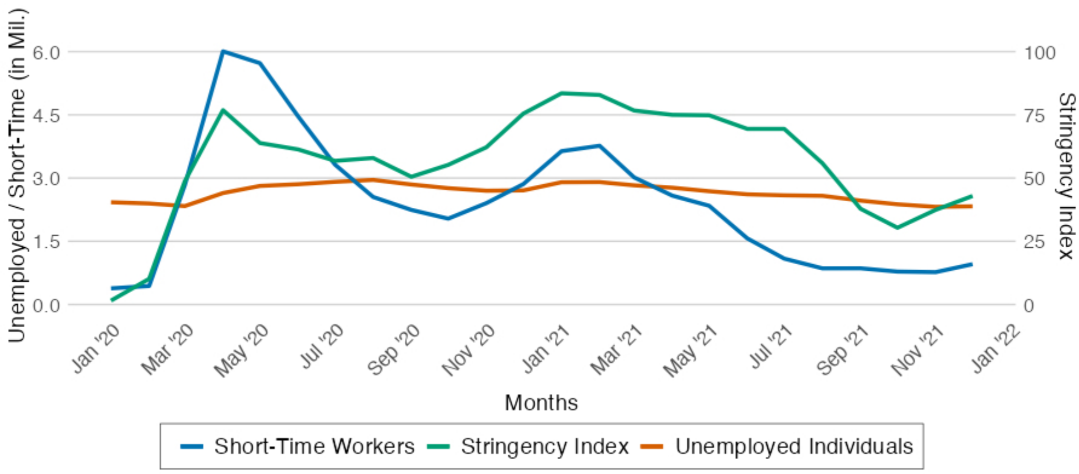


Figure 1 clearly shows the correlated trajectory for short-time work and stringency. The number of unemployed individuals is tracing the macro trends of the first two years of the pandemic in Germany: A sharp drop in output and employment during the first lockdown phase in the second quarter of 2020 followed by a mild recovery, and another rise in unemployment during the the second lockdown, which started in November 2020, before the figures started to return to pre-pandemic levels.

The stringency index for Germany is adopted from the Oxford COVID-19 Government Response Tracker that was initiated by Hale et al. (2021) and includes the following areas where infection prevention measures have been implemented: workplace, schools public events, public gatherings, public transport, domestic travels, international travels, home confinement and public information campaigns. The data on unemployed individuals and individuals on short-time work are taken from the labor market statistics of the Federal Employment Agency (via Federal Statistical Office of Germany, 2023b)

The direct effect of the government's closure of non-essential businesses explains only a fraction of the increase in unemployment. Auer (2022) suggests that it was the other measures' dampening effect on consumer demand that caused most of the negative shock. In this respect, the broad base of the stringency index is supporting its ability to capture the strain that containment measures put on the economy.

For in-depth information on broader economic impacts, the following literature is suitable. Auer (2022) gives a break-down of the industry-specific impacts in Germany. Adams-Prassl (2020) presents and contextualizes the German experience with that of the US and the UK. Mazza et al. (2022) break down the effects of the economic shocks on surrounding EU-countries.

2.2 Adjustment Mechanisms

Firms used different strategies to cope with the economic effects of the infection prevention measures. Using the classification that Burda and Hunt (2011) developed in their analysis of Germany's coping with the 2007–2008 financial crisis, they can be divided in two groups: External adjustment mechanisms encompass staff reduction, including both personnel downsizing and a restraint on new hires. Internal adjustments involve measures like implementing short-time work programs and reducing accrued overtime.

The sharp drop in aggregate volume of work in the second quarter of 2020 mainly consisted of internal adjustment: 86 % was due to the reduction in working hours per employee and 14 % was due to reductions in employment (Brücker et al., 2021).

The protection of employees in Germany has a long and idiosyncratic history and continues to be at a high level by international standards (Schmidt, 2012, p. 69). It roots back to its first application during a recession in the mining and fertilizer industry in 1910 (Adams-Prassl et al., 2020, p. 5). The policy gained international attention due to its effectiveness in the 2007–2008 financial crisis. The pandemic renewed that interest, cementing its status as the "gold standard" of job retention schemes (International Monetary Fund, 2020).

Adams-Prassl et al. (2020) argue that it was Germany's short-time work scheme that led to less severe outcomes in comparison with the the US and the UK. The policy aims to address the inefficiencies that arise when firms must first lay off and then rehire and retrain new workers, avoiding the loss of match-specific human capital and thereby contributing to a swift recovery of the economy in the aftermath of a shock (Hijzen & Venn, 2011).

Other countries also implemented similar policies. The United Kingdom introduced the Coronavirus Job Retention Scheme on March 20th, allowing for worker furloughing. However, this scheme lacked the flexibility of the German short-time work model, as it required furloughed workers to abstain from work entirely (Adams-Prassl et al., 2020).

On March 13th, Germany significantly reduced barriers to the implementation of short-time work arrangements. Notably, the government offered to reimburse firms for their social security contributions on behalf of employees receiving short-time work benefits, up to an equivalent of 20 % of employees' wages (Steffen, 2022). In the first lockdown of March and April 2020, when the short-time work registrations started to rise sharply, 10.1 million employees were registered. This can be considered a conservative figure, as it only covers initial registrations and significant portions of workers spent more than a month on short-time work. In 2009, when short-time work peaked in the context of the Great Recession, a mere 3.3 million people were registered for short-time work (Enzo Weber, 2020).

However, certain employees were disqualified from receiving short-time work allowance. This included individuals engaged in household-related services, marginal employees, temporary agricultural laborers with a short tenure (German Trade Union Confederation, 2022).

3 Literature Review

This section presents parts of the labor economics literature that are relevant to the analysis and discussion that follows. The general phenomenon of native-migrant gaps on the labor markets is treated before moving on to the COVID-specific literature on the subject.

3.1 Native-Migrant Gaps on Labor Markets

Research preceding the pandemic has repeatedly emphasized that migrant workers have worse labor market outcomes even after controlling for relevant factors. Country-specific studies include Bevelander (1999) regarding Sweden, Brekke & Mastekaasa (2008) regarding Norway, Demireva & Kesler (2011) regarding the UK, Neels (2000) regarding Belgium and Kogan (2011) and Lehmer & Ludsteck (2011) as well as Brenzel & Reichelt (2018) regarding Germany. Well-established cross-country studies covering European countries include Adsera & Chiswick (2007; 15 countries) and Kogan (2006; 14 countries).
This disadvantage is attributed to various factors, including lower levels of human capital originating from their home countries, the devaluation of their professional qualifications, the need to rebuild their social networks and language proficiency challenges. First-generation migrants often face difficulties catching up due to the complex and time-consuming bureaucratic processes involved in integrating into the host country's job market. Finally, there exists a large body of research documenting discrimination of minorities on the labor market (for meta-analyses, see Quillian et al., 2020; Lippens et al., 2023; Quillian & Midtbøen, 2021).

An uneven distribution of the burden during the pandemic would connect to a more general pattern. Previous recessions also appear to have contributed to existing inequalities. Hoynes et al. (2012) document differences in cyclicality in the US for Black and Hispanic workers for all labor market fluctuations since the 1980s. Couch et al. (2018) come to similar results in their analysis of the Great Recession in the US. In scrutinizing the Great Recession in Europe, a range of country-specific studies found a worsening of migrants' labor market standing

(Paggiaro (2013) regarding Italy, Cavounidis (2018) regarding males in Greece, Cebolla-Boado et al. (2015) regarding males in Spain).

3.2 COVID-Specific Native-Migrant Gaps

An unequal distribution of the burden of the pandemic has been documented around the world. In examining the first wave of protective measures and their impact on the labor markets of various countries, state institutions and researchers alike concluded that ethnic minorities and migrants were disproportionately affected.

Black and Hispanics have been disproportionally hit in the wake of the pandemic in the US (Borjas & Cassidy, 2020; Cho & Winters, 2020; Cortes & Forsythe, 2020; Couch et al., 2020; Elise Gould et al., 2020; Fairlie et al., 2020; Groshen, 2020; Hershbein & Holzer, 2021; Montenovo et al., 2020). This seems to be exacerbated when minority status and female gender intersect (Gezici & Ozay, 2020). Asian American employment has been affected particularly hard as well (Kim et al., 2021).

Jobs that were at a higher risk of being eliminated when the pandemic reached the EU tended to be occupied by migrants (Sanchez et al., 2020). Non-EU migrants are estimated to have been twice as as likely to have lost their job compared to natives (Fasani & Mazza, 2020).

Immigrant workers in the EU coming from East Europe faced greater risk to negative income-shocks related to the pandemic (Bossavie et al., 2021). Especially those born outside of the EU with little experience on the EU labor market were found to be disproportionally hit by job loss (Mazza et al., 2022).

In Germany, individuals with a migratory background and refugees in particular have been disproportionally affected by layoffs and short-time work when compared to employees with similar characteristics (Brücker et al., 2021). Similarly, individuals with migratory background in Germany were more likely to be fired (Auer, 2022).

While an increase in inequality to the demise of minorities has been documented, the underlying factors are less clear. It remains a topic of debate to which degree it is the result of discrimination and to which degree it is the result of the disproportional self-selection of minorities into industries and occupations with heightened vulnerability to labor market shocks.

Indeed, by controlling for factors like gender, age, experience, education, occupation, industry/sector, contract type and teleworkability, the differences in increases in unemployment or decreases in the number of working hours could partially be explained. For example, Borjas & Cassidy (2020) have attributed a third of the differential effect of the pandemic on migrants to their self-selection into jobs with less teleworkability.

However, explanatory gaps remain (e.g., Cortes & Forsythe, 2020; Fairlie et al., 2020; Fasani & Mazza, 2020; Gezici & Ozay, 2020). When the US economy recovered from the initial shock, Montenovo et al. (2020) found that Black workers re-entered into the job market at lower rates than other groups even after controlling for a variety of factors. On the other hand, when looking at the 2021 recovery period in the US, Borjas & Cassidy (2023) found that undocumented workers experienced a more rapid rebound into the job market. They attribute this to the lack of unemployment benefits they had faced while natives and legal migrants were eligible for generous benefits.

A considerable literature has formed around the gendered differences of the pandemic's labor market effects. As highlighted by Del Boca and colleagues (2020), the COVID-triggered economic downturn diverged from previous recessions, such as the 2008 financial crisis, by placing a heavier burden on women. This divergence can be attributed to two key factors. Firstly, industries with a higher proportion of female employees bore the brunt of the economic impact. Secondly, the closure of schools led to an increased demand for childcare, primarily shouldered by women, exacerbating their already disproportionate care responsibilities. While the primary dataset lacks the sufficient number of observations for simultaneous gender and migrant status disaggregation, gender is incorporated as a covariate in the subsequent regression analyses.

3.3 Lasting Effects of Labor Market Inequalities

Inequalities may only be exacerbated by the initial shock of the pandemic and and evened out again in the recovery phase (e.g., Borjas & Cassidy (2023) find this pattern). However, even temporary inequalities in labor market can have lasting effects that go beyond the loss of income. As mentioned in the section on short-time work, preserving the job matches avoids costs for firms and facilitates economic recovery. Similar dynamics hold true for employees.

Starting one's career with a high-status job can have a long-lasting, positive effects on immigrants' career trajectories (Kogan & Weißmann, 2013).Conversely, migrants that start out with negative experiences on the job market tend to have problems with future labor market integration (Åslund & Rooth, 2007). Rothstein (2023) even describes a "scarring effect" for college graduates in the US, where entering the labor market during a recession is found to have long-term negative effects on the individuals' careers.

For migrants, job loss may not only cause financial but also immediate legal insecurities. It is beyond the scope of this paper to discuss the connection between residence permission and employment status in Germany. German authorities relaxed their employment requirements for visa extension in the wake of the pandemic (Federal Government of Germany, 2021). Information on the topic however remained relatively vague and hard to find, most likely causing uncertainty among affected individuals.

Brücker et al. (2021) additionally point out that COVID-19 has disrupted programs aimed at integrating newly arrived migrants into the labor market. While this phenomenon is not part of the following analyses, negative effects on the professional trajectories of affected individuals are to be expected.

4 Analysis

In this section, panel data is used to scrutinize the potentially differential effect of the pandemic on the labor market outcomes of migrant groups. After discussing data sources and definitions, the first stage of analysis will be descriptive, comparing the transitions from regular employment to unemployment and short-time work. The second stage consists of regression analyses. The section closes by examining the results of the regressions.

4.1 Data

The main data source employed originates from the German Socio-Economic Panel (SOEP). Besides being the name of the longitudinal survey study used in this paper, SOEP is also the name of a research division based at German Institute for Economic Research (DIW Berlin).

Every year since 1984, the SOEP interviewed up to 30.000 individuals from 15.000 households, covering a wide range of questions on socioeconomic circumstances. This paper draws on the latest remote version (v38) which includes observations of the latest survey year, 2021. The data analysis was performed with the programming language R.

During the process of writing this thesis, the lack of detail in the methodological descriptions in the sources took a heavy toll in terms of time. Therefore, in the following, the example of Borjas & Cassidy (2023) is followed and procedures are described, if necessary, at the level of datasets and variables. The former are written in capital letters, as in the SOEP documentation. The latter are written in italics.

Dataset PPATHL is used to obtain demographical information (including information on personal migration history) and for merging operations between datasets. The especially harmonized ("generated") dataset PGEN is used to retrieve a variety of information on the occupation status and industry of respondents. Dataset PL provides details on the employment contract type of individuals. The harmonized, household-level dataset HGEN is levied to identify the federal state of the respondent.

Version 38 also includes the COV dataset, which draws on the SOEP-CoV project. From April to July 2020 as well as from January to February 2021, a subset of the SOEP-Core cohorts were asked COVID-specific questions (DIW Berlin, 2022). Unfortunately, the study could not be harnessed to the expected degree, as it is missing weights for the year 2021. Proper weight construction, as detailed by Siegers et al. (2021, p. 17 f.), is beyond the scope of this paper.

Using COV's 2020 cross-section is still challenging, as the underlying SOEP cohorts were not surveyed uniformly. The cohorts M3, M4 and M5 – almost the entirety of refugees that form part of the SOEP core sample – were questioned by the Institute for Employment Research (IAB). Comparing the questionnaires of the two fractions of the SOEP-CoV project, one finds differing question wordings and variable names for otherwise identical concepts (e.g., the employment status variables *perw_rn* of the IAB sample and its equivalent *plb0022_h* of the SOEP sample). For example, the key question of changes in individual's employment situations due to the pandemic come with differing response options depending on the cohort. For the refugee cohorts, the question was not posed to individuals that ended up not being employed.

As a result of all that, COV is only used to bridge a weakness of the SOEP-Core main datasets: In 2020, when short-time work was at peak usage and internationally celebrated, the SOEP main study did not include short-time work in the list of eligible employment statuses (nor was there any other question covering the central phenomenon). In the following, the subset of the SOEP-CoV respondents is used to calculate changes in short-time work status between 2019 and 2020.

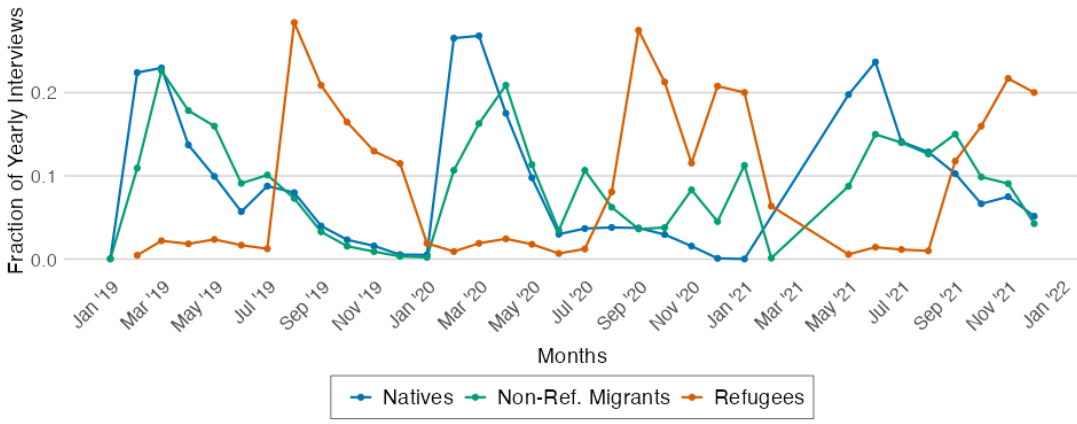
The SOEP's panel data on occupation and industry is enhanced by three external data sources, using the variable *pgkldb2010,* which provides the occupation code according to the Federal Employment Agency's (BA) classification *Klassifikation der Berufe 2010* (KldB 2010). Following Brücker et al. (2021), the tables of Burstedde et al. (2020) are used to determine whether an individual's job can be considered part of the critical infrastructure. Likewise, the classifications set up by Dingel & Neiman (2020), which identify the type of main task of an occupation, is adapted from Brücker et al. (2021). To integrate the ability of an individual to switch to remote work as a control variable, this paper uses Alexandra (2020). Other teleworkability classifications used in related literature were not directly available (e.g., Rohrbach-Schmidt & Hall, 2020).

Besides panel data, the SOEP-Core package comes with spell datasets. For each individual, a spell dataset usually comprises a succession of time periods during which the individual is in a particular state, such as being employed or unemployed. These time periods are called spells. There are two spell datasets of interest for this analysis. The dataset ARTKALEN provides spells until the end of 2020, as it is constructed from questionnaires that aim to construct monthly "calendars" of individuals' previous year. PBIOSPE on the other hand includes a fair amount of spell observations that reach into 2021, because it is the result of "biography" questionnaires that capture information up until the time of the interview. That makes the dataset an attractive candidate for a more full-cycle analysis of COVID-related effects. However, the fact that the dataset is structured year-wise and short-time work phases for many only lasted a few months creates problematically small observation numbers. There are only 53 out of 18863 individuals (for which PBIOSPE spells at least reach into 2020) that have a short-time spell reaching into 2020 or 2021.
The second spell dataset, ARTKALEN*,* is used to complement the descriptive evidence section of the analysis, giving a more fine-grained view on the distribution of months spent working in short-time work arrangements. Furthermore, it was used to identify and clear out ambiguities in the panel data (see below).

Observations that stem from interviews conducted in January and February 2020 were excluded to operationalize the survey year transition from 2019 to 2020 as the advent of the pandemic. As interview days are unavailable for a large fraction of observations, the cutoff point had to be placed at the end of a month. The choice is legitimized by the fact that policy responses only gathered steam with the very end of February, as seen in the Stringency Index regarding Germany (Figure 1). In my base specification, this step of filtering leads to the loss 18.75 % of the observation. Observations pertaining to survey year 2020 (variable *syear* equaling 2020) that stem from interviews in January or February of 2021 are not excluded.

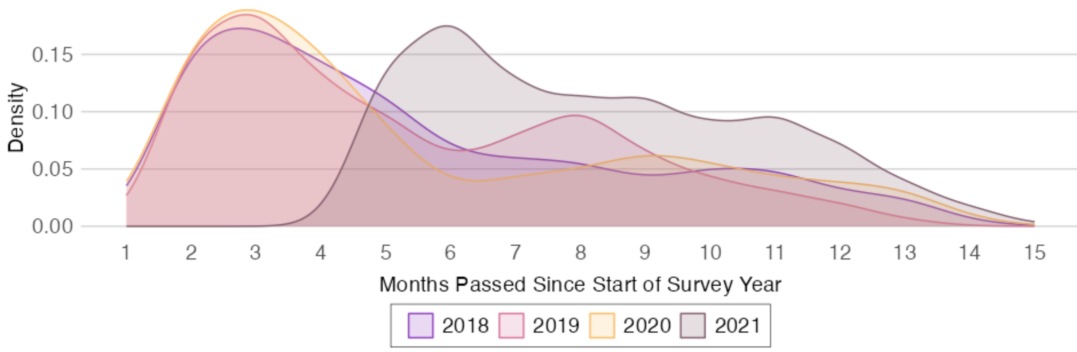
This leads us to a necessary clarification: the SOEP interviews have not been conducted in a narrow and consistent time frame every year, as one might expect from a yearly panel study. In fact, the distribution of interview months is spread across all calendar months and differing across relevant group characteristics. Furthermore, the distribution varies across years. Both is visualized in Figure 2, using the migrant grouping of Brücker et al. (2021).

Figure 2: Group Specific Distributions of Interview Months



As Figure 3 shows, there is also a significant fraction of interviews that has been conducted in the year following the survey year (*syear*) for which the information is mapped onto in the panel data. The fraction varies between 0.64 % and 5.74 %, depending on the survey year.

Figure 3: Interview Months



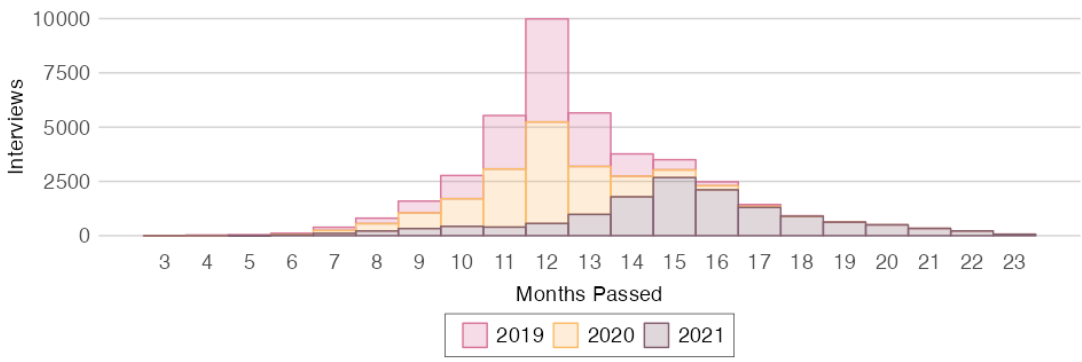
To the best of my knowledge, the SOEP documentation does not address the cases in which observations resulting from an interview in, e.g., February 2020

were mapped onto the preceding survey year (in this case, 2019). One option would be that these types of interviews are adjusted to produce information on the situation in the previous year. The other option is that, as an example, the employment status of an individual in February 2020 ends up in the row pertaining to survey year 2019. Scrutinizing individuals found both in the dataset ARTKALEN (spell data) and the dataset used for the regressions (panel data combined from PPATHL, PGEN, PL and HGEN), the second option is corroborated: Some individuals were documented to be working full-time in 2020 in the accurate spell data and categorized as not being employed in the panel data.
In fact, the SOEP's own documentation at times stumbles over the conceptional ambiguity. In a guide for merging spell and panel data, the interview month is used as join key while conflating the two temporal concepts of interview year and survey year (Hamjediers et al., 2018, p. 4, retraced in the provided STATA do-files). It can easily be shown that this approach leads to faulty outcomes.

In the following models, the default year variable *syear* is used despite its just-described imprecise nature. Thus, underneath the descriptions of inter-year changes there lies an overlapping of survey years. Two alternative approaches were tried to deal with this. First, observations in which the interview does not match the survey year were "cut off". This however leads to critical loss of observations for some groups. Secondly, the interview year was programmed to replace the survey year. To resolve the resulting loss of a unique (key) variable, another critical dropping of observations would have been necessary. Thus, both approaches were discarded.

Finally, the interval between interviews for a given individual is not strictly twelve months but varying throughout the years. The same holds when individuals are aggregated. Figure 4 visualizes the distribution of interview intervals between a given survey year and its previous year for all individuals that were successfully interviewed each year from 2018 to 2021. The figure resembles a stacked histogram where the years are aggregated vertically.

Figure 4: Interview Intervals



For all following calculations, the observations pertaining to the regular SOEP-Core study were weighted with variable *phrf*, as recommended when pooling all samples together during the analysis (DIW Berlin, personal communication, October 26, 2023). When working with data stemming from the SOEP-CoV study, the weighting variable *phrf20_core* was used for all samples but M3, M4 and M5 (samples of 2013-2016 refugees), for which *phrf20_ref* was used.

4.2 Definitions

Central to this analysis are two sets of concepts: labor market outcomes and migration-related statuses. For both, a great variety of categorizations are used in the literature.

Regarding labor market outcomes, four concepts were deployed: Being employed, unemployment, working under a short-time work scheme and being not employed. In accordance with the International Labour Organization's (ILO) definition of employment status used in the construction of the *pgemplst* variable, employment includes full-time and part-time work as well as marginal employment, vocational training or sheltered workshops for the disabled. Unemployment according to the ILO consists of four conditions: 1) to be aged between 15 and 74, 2) not to be employed, 3) to have looked for a job in the last four weeks and 4) to be available for work within two weeks. Although the dataset PL provides variables to check for the last two conditions (*plb0423* and *plb0424_v2* respectively), these variables have too many missing values to produce usable results. Unemployment has therefore been coded using the variable *pgstib*. This resembles a switch from the ILO's to the Federal Employment Agency's (BA) concept of unemployment. Here, unemployment mainly means being registered as unemployed with the BA. A person who is registered as unemployed with the BA, but who is marginally employed, would be classified as employed by the ILO and as unemployed by BA (Federal Employment Agency of Germany, 2023).

Working under a short-time work scheme is simply defined as continuing to be employed after being included in the official short-time work registration of one's firm.

Individuals who are neither employed nor unemployed are categorized as not employed. This group may consist of individuals aged 65 or older, currently undergoing vocational training or education, on parental leave, or actively participating in military or community service.

The choice of appropriate labor market concepts was complicated by the fact that the piece of literature that seems to be the closest to this thesis in terms of aim and scope, Brücker et al. (2021) is not rigorous in this respect. The concepts of unemployment and non-employment are used interchangeably for individuals who have ceased working since the previous year, with the strong assumption that they would still be willing to work. Furthermore, the willingness to work is then equated with availability for the labor market. The ILO condition of looking for a job was dropped (Institute for Employment Research, personal communication, October 12, 2023).

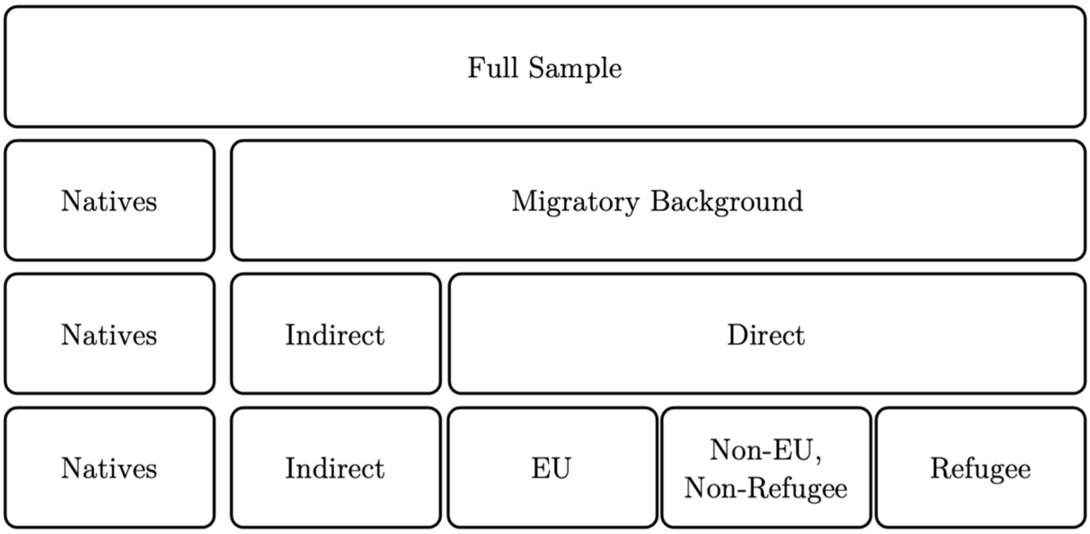
Similarly, there is a multitude of ways to categorize the main independent variable of the analysis, the migration-related status of a person.

When choosing the adequate categorization, there is a trade-off to be made between parsing apart individuals with impactful, unmeasured characteristics on the one hand and avoiding the problem of small cell sizes on the other hand. Using the SOEP's *migback* variable, individuals are defined as individuals without a migratory background if they and their parents were born in Germany. For the sake of simplicity, the term "native" is adopted from the US literature (e.g., Borjas & Cassidy, 2023) to describe these individuals.

Using natives as the base category, Auer (2022) settles for the complementary group of what he calls migrants and differentiates no further. Brücker et al. (2021)

adds the distinction between individuals with migratory background and the separate subgroup of refugees that migrated between 2013 and 2016 – a choice certainly motivated by data availability within the COV dataset (which is used extensively in the report). In their analysis at EU level, Mazza et al. (2022) distinguish between people in a respective country, those who were born in other EU countries and those born in non-EU countries. The US literature, e.g. Montenovo et al. (2021), usually differentiates by ethnicity (white, black, Hispanic). Borjas & Cassidy (2023) distinguish between natives, foreign-born and – with what they call "rough" imputation – undocumented immigrants.
Adopting Auer's (2022) categorization would mean lumping together individuals whose parents migrated from a neighboring country together with individuals who recently migrated from a far-away war zone. On the other hand, using the SOEP's information on migratory background to its fullest extend (by using individuals' countries of origin) would mean only having only one observation in certain categories. As a compromise, five categories are used to (sub)divide the sample, as shown in Figure 5.

Figure 5: Migrant Group Categories



In the following, the five groups at the bottom are labelled, from left to right, with the following abbreviations: Native, Indirect Migrant, EU Migrant, Non-EU Migrant and Refugee.

Table 1 shows the composition of the sample used for the following analyses in terms of migrant grouping (in percent). The figures are still reasonably close to the composition of the German population in 2020, which is marked with an asterisk in the table (Federal Office for Migration and Refugees, 2023; Federal Statistical Office of Germany, 2023a).

Table 1: Sample Composition over the Years

Year	Natives	Indirect Migrants	EU Migrants	Non-EU Migrants	Refugees
2019	76.8	6.9	6.3	8.2	1.9
2020	76.3	6.9	6.1	8.1	2.7
2020*	74.3	6,9	9.3	7.3	2.2
2021	74.7	7.3	7.3	8.2	2.6

4.3 Empirical Strategy

In order to compare the transitions from employment to unemployment or short-time work for the different migrant groupings, descriptive evidence is provided before a series of binary logistic regression analyses are conducted.

As a first step, year-specific samples were assembled from the different datasets mentioned in the data section. Individuals who were younger than 18, older than 64, self-employed, unemployed, or not employed were filtered out. Next, for each individual, the sample was enhanced by their observation in one of the two following years, enabling the tracing of their labor market trajectories. Individuals that did not participate in both years of such a year pair were filtered out.
The year pair 2018-2019 serves as comparison and is provided in the appendix. The year pair 2019-2020 aims to measures the initial impact of the pandemic. The year pair 2019-2021 aims to provide a mid-term view on the labor market effects and encompasses recovery phases. The year pair 2020-2021 provide an approximation on pandemic-related effects on initially unscathed individuals that continued to be employed until the time of their interview in 2020. Being a rather specific perspective, it is provided in the appendix only, due to the limitations of the thesis.

Ideally, the data would allow for a comparison of the groups within a narrow period, e.g., each year's July. However, as mentioned previously, the distribution of interview months is broad and varies from group to group. That also means that, for example, one group's transition-to-unemployment rate in this model may be based mostly on observations from spring while another group's rate may mostly draw on observations from fall. While this issue is not addressed in the descriptive evidence, the month dummies used in the regression analysis will remove some of the bias that this type of seasonality creates.

4.4 Descriptive Evidence

As a point of departure, the transitions from employment into unemployment or short-time work are presented in tables for the different migrant groupings. More precisely, the share of individuals who were regularly employed in the first year (of a year pair) that ended up being unemployed (Table 2) or in short-time work (Table 3) is depicted a a percentage of all previously employed individuals. Each cell additionally contains with the number of (unweighted) observations that make up the percentage in brackets.

Looking at the transition from employment to unemployment, we see that all groups except for EU Migrants saw an increase of transitions rates when comparing 2018-2019 with the year pair encompassing the onset of the pandemic, 2019-2020. The year pair 2020-2021, which approximates the transition rates of individuals who mostly remained "unscathed" during the first year of the pandemic, sees all groups but EU Migrants having lower than pre-pandemic transition rates. Finally, taking the mid-term view with the year pair 2019-2021, we see an even more divided picture: Indirect Migrants and Refugees had lower transition rates into unemployment than between 2018-2019, while the rest had higher transition rates. All in all, it is a rather mixed picture.

The inter-group comparison implies a clearer tendency: In all year pairs, Natives have lower transition-into-unemployment rates than groups with migratory background. As this includes the pre-pandemic year pair 2018-2019, the question arises as to whether this inequality has deepened and whether a potential deepening applies to all groups. Looking at relative changes in these differences in transition rates over the years and between the groups, no meaningful pattern emerges. One change that deserves attention is the fact that Indirect Migrants and Refugees had lower transition rates between 2019 and 2021 than between 2018 and 2019. At first sight, the labor market shock can cautiously be described as having an equalizing effect for these groups in terms of job loss.

Table 2: Descriptive Evidence, Employed to Unemployed

Year Pair	Natives	Indirect Migrants	EU Migrants	Non-EU Migrants	Refugees
2018-2019	1.59 (150)	3.13 (26)	4.85 (38)	2.86 (36)	5.5 (87)
2019-2020	1.96 (144)	3.82 (22)	2.78 (20)	3.28 (26)	8.16 (127)
2020-2021	1.3 (85)	2.26 (11)	3.97 (18)	4.18 (18)	2.76 (35)
2019-2021	2.21 (150)	2.45 (13)	6.55 (25)	5.27 (25)	3.61 (41)

Table 3 presents the descriptive evidence on short-time work. As it is mostly a crisis measure, comparison to pre-pandemic year pairs is not applicable. Comparing the pandemic year pairs, we see that short-time work played the largest role in the onset of the pandemic, with transition rates dropping as sharply as from 16.42 % between 2019 and 2020 to 0.77 % between 2020 and 2021 for Refugees.

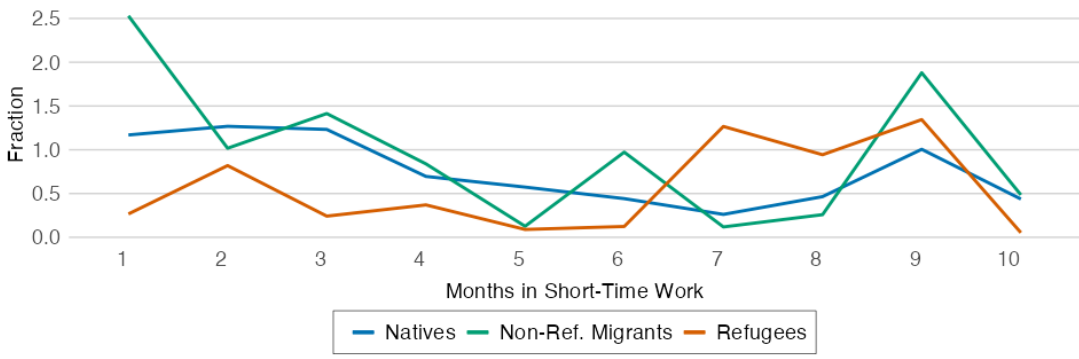
The comparison between groups suggests that all groups, except Indirect Migrants, moved into short-time work at a higher rate than Natives at the beginning of the pandemic (2019-2020). This tendency is reversed in for the 2020-2021 perspective on the initially "unscathed": All groups except Non-EU Migrants transitioned to short-time work at a lower rate than natives. In the mid-term view (2019-2021), the pattern of the 2019-2020 period dominates and all migrant groups but Refugees have higher transition rates than Natives.

Table 3: Descriptive Evidence, Employed to Short-Time Work

Year Pair	Natives	Indirect Migrants	EU Migrants	Non-EU Migrants	Refugees
2019-2020	5.06 (134)	2.21 (7)	6.11 (22)	12.89 (22)	16.42 (34)
2020-2021	1.11 (57)	0.26 (1)	1.74 (9)	0.26 (3)	0.77 (16)
2019-2021	1.09 (74)	1.15 (3)	1.54 (7)	1.28 (3)	1.06 (15)

As discussed in the data section, the spell data of the SOEP is only of limited use for this analysis. The spell dataset ARTKALLEN however gives additional insight regarding the usage short-time work in the first survey year of the pandemic: The groups are pooled together à la Brücker et al. (2021) for simplicity and the sample is filtered so that it only contains individuals who were employed in February 2020. Figure 6 shows the group-specific share of individuals who have spend a total of a certain number of months in short-time work in the remaining year of 2020. The strongest group-difference appears for the fraction of individuals who spent one month in short-time. The fraction of individuals with a migratory background (direct or indirect; refugees excluded) who worked one month in short-time was twice (ten times) as large as that of natives (refugees). The rest of the distribution appears to be relatively homogeneous, with differences in shares barely exceeding one percentage point.

Figure 6: Group-Specific Months in Short-Time Work in 2020



4.5 Regression Models

For this section, a series of binary logistic regression models was constructed across multiple dimensions. Three dependent variables were examined across four year pairs and two migrant groupings. The transition from employed to unemployed and the transition from employed to short-time work employment are the main dependent variables. Regressions using the transition to non-employment as the dependent variable are only presented in the appendix due to the limited scope of this thesis. Compared to unemployment, non-employment is a broader category with more possible determinants. Non-employed individuals may include disabled persons, stay-at-home parents, students or (early) retirees. Unemployment was prioritized because of its narrowness.

The year pairs 2019-2020 and 2019-2021 form the main time frames, as 2018-2019 serves only as a baseline and 2020-2021 covers a more niche phenomenon: the negative labor market market effects between the first and the second year of the pandemic on previously "unscathed" individuals. The regression tables of 2018-2019 and 2020-2021 are therefore only included in the appendix.

All regression tables presented in the following consist of two regression models. Model (1) resembles the fine-grained migrant grouping akin to Figure 5. The omitted reference category for the migrant group variables are natives.

In model (2), the sample was restricted to individuals with direct experience of migration to facilitate the integration of length of stay as a covariate, akin to Mazza et al. (2022). Individuals with direct experience of migrating from an EU 27 country other than Germany (including the UK for consistency) form the base category here.

The remaining covariates are all programmed as dummies and closely oriented at the regressions of Brücker et al. (2021). Their main function is to control for individuals' self-sorting into more vulnerable positions.

The covariates "Woman", "Child under 16 in Household" and the interaction term "Woman x child" connect to the findings on gendered differences in recessions as covered in the literature section.

The education covariates are constructed with the variable *pgisced11* which corresponds to the International Standard Classification of Education (ISCED 2011) of the OECD. The covariates "Mid-Level Education" (ISCED 3, 4, 5), "High-Level Education" (ISCED 6, 7, 8) hereby come with an omitted base category "Low-Level Education" (ISCED 1 and 2).

The covariates "Vocational Training" and "Marginal Employment" are contrasted with the omitted base category "Full or Part-Time Employment" and capture the fact that employees of in these categories have different levels of protection against dismissal and eligibility for short-time work.

The covariates "Employed by Employment Agency" and "Permanent Work Contract" capture contract-specific protection against job loss.

The dummy variable "Large Company" considers a company to be large if it employs more than one hundred individuals.

The covariates "1-3 Years with Firm" and "3+ Years with Firm" refer to the omitted base category of "Less than 1 Year with Firm".

In contrast to Brücker et al. (2021), the teleworkability covariates "Mid-level Teleworkability" and "High-level Teleworkability" (and the reference category

"Low-level Teleworkability") are programmed following Alexandra (2020) due to data unavailability. For the main occupational group (2-digit code of the KldB framework) of an individual, the three variables are indicating whether 0-30 % ("Low-level"), 30-70 %, ("Mid-level") or 70-100 % ("High-level") of employees had the option to work from home before the pandemic.

The variables on requirement level are also constructed using the KldB classification, as the number on which the 5-digit code ends indicates the requirement level of an occupation. The four official levels of requirement were collapsed into three: "Mid-tier Requirement Level" (skilled tasks), "High-tier Requirement Level" (complex or highly complex tasks) and the omitted base category "Low-tier Requirement Level" (unskilled/semiskilled tasks).

As mentioned in the data section, table A2 of Burstedde et al. (2020) was used to construct the covariate "Critical Relevance". Similarly, the classifications set up by Dengler et al. (2014) were used to program control variables for the type of main task of an occupation (covariates "Interactive Non-Routine Tasks", "Cognitive Routine Tasks", "Manual Routine Tasks", "Manual Non-Routine Tasks" and reference category "Analytical Non-Routine Tasks").

In addition to just listed covariates that appear in the tables, all regressions also contain a constant, indicator variables for the interview month, three indicator variables for the age of the respondents, nine indicator variables for the economic sector as well as three indicator variables for the unemployment rate at federal state level in February 2020.

The economic sector variables were constructed using the Statistical Classification of Economic Activities in the European Community (NACE Rev. 2) via the variable *pgnace2* and aggregated to the so-called A*10 categorization. Month dummies are included to account for seasonality, as long as there are observations falling into the month for a certain model specification, for a certain second year of a year pair.

As the remote version lacks fine-grained data on the respondent's place of residence, it was not possible to construct a county-level unemployment control as seen in other publications on the issue (e.g., Brücker et al.). Instead, the unemployment rate of the federal state of residence in February 2020 was used. The four categories underlying both the unemployment and age control variables were constructed using quartiles.

4.6 Results

For purposes of presentation and brevity, the coefficients resulting from the logistic regression were transformed into odds ratios and standard errors were omitted. The significance of covariates is symbolized by asterisks and calculated using the untransformed log-odds and their standard errors. Only statistically significant results concerning migrant status variables will be mentioned in the text due to the thesis' limitations. The results should not be interpreted causally. The following statements on estimated effects should be understood through the lens of the ceteris paribus assumption.

The effect associated with the onset of the pandemic will be scrutinized first by looking at the year pair 2019-2020. Table 4 shows that with regards to unemployment and in contrast to the descriptive data, no statistically significant effect associated with migrant group status was found.

Table 4: 2019 to 2020, Employed to Unemployed

	(1)	(2)
Indirect Migrant	1.67	
EU Migrant	0.82	
Non-EU Migrant	0.90	0.85
Refugees	1.28	2.05
Length of Stay		1.01
Woman	0.61	2.50
Child under 16 in Household	0.89	1.43
Woman x Child	2.09	1.13
Mid-tier Education	0.89	0.69
High-tier Education	0.93	0.18**
Vocational Training	0.55	0.29
Marginal Employment	0.61	0.80
Employed by Employment Agency	0.92	4.34*
Permanent Work Contract	0.39***	0.77
Large Company	0.42***	0.25**
1-3 Years with Firm	0.75	1.34
3+ Years with Firm	0.33***	0.56
Critical Relevance	0.87	1.22
Mid-level Teleworkability	0.78	0.94
High-level Teleworkability	1.10	0.31
Mid-tier Requirement Level	0.67	0.56
High-tier Requirement Level	0.45*	0.95
Interactive Non-Routine Tasks	3.40	0.23
Cognitive Routine Tasks	1.57	1.31
Manual Routine Tasks	2.21	0.92
Manual Non-Routine Tasks	3.67*	0.32
N	9,377	2,020
Log Likelihood	-853.33	-202.34

Notes: ***, **, * denote significance at the 1, 5 and 10 percent level.

Taking the wider perspective from 2019 to 2021 with the regressions of Table 5, migrant status covariates become significant regarding unemployment. EU migrants are estimated to be 2.42 times as likely to have become unemployed than natives (column 1). Similarly, non-EU migrants are estimated to have been 2.11 times as likely to have transitioned into unemployment than natives (column 1).

In the 2019-2021 time frame, length of stay plays a statistically significant role. For individuals with direct migration experience (column 2) the odds of becoming unemployed are estimated to decrease by 5 % with every year that the individual has stayed in their country of destination.

Table 5: 2019 to 2021, Employed to Unemployed

	(1)	(2)
Indirect Migrant	1.10	
EU Migrant	2.42*	
Non-EU Migrant	2.11*	1.11
Refugees	1.32	0.37
Length of Stay		0.95*
Woman	0.75	3.20
Child under 16 in Household	0.32***	0.75
Woman x Child	2.06	0.33
Mid-tier Education	1.11	0.52
High-tier Education	1.54	0.21**
Vocational Training	2.23	1.29
Marginal Employment	0.65	0.18
Employed by Employment Agency	2.77**	1.35
Permanent Work Contract	0.39***	0.39*
Large Company	0.76	0.23**
1-3 Years with Firm	0.79	1.14
3+ Years with Firm	0.60	1.13
Critical Relevance	1.03	2.14
Mid-level Teleworkability	1.06	2.00
High-level Teleworkability	0.78	3.23
Mid-tier Requirement Level	0.64	1.16
High-tier Requirement Level	0.20***	0.16*
Interactive Non-Routine Tasks	1.36	0.55
Cognitive Routine Tasks	1.24	1.35
Manual Routine Tasks	0.99	3.41
Manual Non-Routine Tasks	0.97	0.81
N	7,700	1,331
Log Likelihood	-803.13	-206.57

Notes: ***, **, * denote significance at the 1, 5 and 10 percent level.

Looking at the transition rates of individuals that reported being employed during the first year of the pandemic in Germany and ended up being unemployed in 2021 (Table A1 in the appendix), a similar but more pronounced pattern to 2019-2021 occurs: EU migrants were estimated to have been 3.57 times more likely to transition than natives, Non-EU Migrants 3.38 times more likely (column 1). Comparing the direct migrant groupings solely to each other (column 2), we find previously employed refugees to be more than five times less likely to transition to unemployment than EU migrants. The protective effect of length of stay appears to have increased in comparison with the 2019-2020 year pair.

The lack of statistically significant association of the migrant group status with transitions into unemployment at the beginning of the pandemic (2019-2020) and the just-mentioned increased likelihoods for EU and Non-EU migrants (2020-2021 and 2019-2021) needs to be contrasted with the pre-pandemic status quo. Table A2 (in the appendix) presents an analogous regression on the year pair 2018-2019. EU migrants are estimated to have been 3.89 times more likely than natives to have transitioned into unemployment, exceeding the number found for 2019-2021 (and of course the lack of findings in 2019-2020) (column 1). Non-EU migrants' likelihood for a transition into unemployment is not significantly different (statistically) in comparison to natives (column 1). In comparison to EU migrants (column 2), non-EU migrants are estimated to be nearly half (0.56) as likely to transition into unemployment.

However, for indirect migrants, the risk of becoming unemployed was estimated as 2.48 times higher than for natives in the 2018-2019 year pair – a relationship that was not reproduced for the following year pairs.

In summary, disproportionally high pre-pandemic transition rates for indirect and EU-migrants were equalized at the onset of the pandemic, before non-EU and EU migrants emerged as the new groups with disproportionally high transition rates.

As mentioned in the model section, analogous regressions on the transition from employment to non-employment were computed but only included in the appendix. As seen in Table B1, the 2018-2019 estimation implies that indirect migrants had a higher risk of transitioning into non-employment when compared to natives (column 1). This relation disappears for year pair 2019-2020 (Table B2), implying an equalizing effect of the pandemic in this narrow sense. Moving on to the year pair 2020-2021 (Table B3), those non-EU migrants who were employed in 2020 were found to be 2.14 times more likely than natives to move into non-employment in 2021 (again column 1). In the longer time frame of 2019-2021 however, this differential result again disappears from the estimation results (Table B4). In summary, similarly to the unemployment figures, we see disproportionally high rates for indirect migrants before the pandemic and a shift to non-EU migrants during the pandemic (at least for 2020-2021). Differing from the unemployment examination, the non-employment regression does not suggest higher transition rates for EU migrants at any point.

Regarding the comparison between direct migrants (column 2), we see refugees being less likely to move into non-employment than EU migrants between 2020 and 2021 as well as 2019 and 2021 (Tables B3 and B4).

The focus now shifts to the short-time work. Table 6 shows a significant association between migrant group status and labor market outcome at the onset of the pandemic: non-EU migrants are estimated to have had a 2.58 higher risk of transitioning from employment to short-time work than natives (column 1). In comparison to EU migrants, that odds ratio is reduced to 2.34, but still statistically significant (column 2).

The 2020-2021 estimation has limited validity as it suffers from low numbers of observations. Its results suggest no significant association between the transitions from employment to short-time work and migrant group status (Table C1).

Table 6: 2019 to 2020, Employed to Short-Time Work

	(1)	(2)
Indirect Migrant	1.32	
EU Migrant	1.54	
Non-EU Migrant	2.83***	2.34*
Refugees	1.08	0.57
Length of Stay		0.99
Woman	1.10	1.20
Child under 16 in Household	1.14	1.07
Woman x Child	1.09	0.91
Mid-tier Education	0.92	0.26**
High-tier Education	0.54*	0.11***
Vocational Training	0.37	0.01***
Marginal Employment	0.22***	0.28
Employed by Employment Agency	0.22**	0.31
Permanent Work Contract	1.09	0.93
Large Company	0.82	0.73
1-3 Years with Firm	1.81*	3.21*
3+ Years with Firm	1.30	1.42
Critical Relevance	0.59***	0.48
Mid-level Teleworkability	1.09	1.08
High-level Teleworkability	0.61	0.32
Mid-tier Requirement Level	1.00	0.96
High-tier Requirement Level	1.41	1.05
Interactive Non-Routine Tasks	1.33	0.98
Cognitive Routine Tasks	0.79	0.65
Manual Routine Tasks	0.65	0.05**
Manual Non-Routine Tasks	1.19	0.51
N	3,624	746
Log Likelihood	-1,366.89	-320.29

Notes: ***, **, * denote significance at the 1, 5 and 10 percent level.

Finally, the transition from employment to short-time work is looked at in the mid-term perspective of 2019-2021 (Table 7). No statistically significant connection between migrant status and outcome variables is found. The direct migrant model in column 2 however shows a significant effect of length of stay. For each year in the country of destination, direct migrants' odds of transitioning into short-time work are estimated to be reduced by 14 %.

As seen by the large log likelihood value and unrealistic coefficients for other covariates, the just-mentioned result is not sound (likely due to low numbers of observations).

Table 7: 2019 to 2021, Employed to Short-Time Work

	(1)	(2)
Indirect Migrant	0.73	
EU Migrant	1.23	
Non-EU Migrant	1.09	0.93
Refugees	0.45	2.80
Length of Stay		0.86***
Woman	0.58	17.92**
Child under 16 in Household	0.72	1.31
Woman x Child	0.86	0.002***
Mid-tier Education	1.25	1.64
High-tier Education	0.76	0.30
Vocational Training	0.41	0.0000***
Marginal Employment	0.03***	0.03
Employed by Employment Agency	3.29	112.94***
Permanent Work Contract	2.10	95.03**
Large Company	0.85	0.06***
1-3 Years with Firm	1.70	0.002**
3+ Years with Firm	2.40	1.60
Critical Relevance	0.34***	0.03**
Mid-level Teleworkability	1.01	6.34
High-level Teleworkability	0.74	10,089.02***
Mid-tier Requirement Level	0.86	1.17
High-tier Requirement Level	1.33	0.005***
Interactive Non-Routine Tasks	0.58	0.02
Cognitive Routine Tasks	0.29**	31.12
Manual Routine Tasks	0.43	11,514.85
Manual Non-Routine Tasks	0.96	200.82
N	7,723	1,341
Log Likelihood	-378.17	-8.31

Notes: ***, **, * denote significance at the 1, 5 and 10 percent level.

Comparing to a pre-COVID status quo is not adequate in case of short-time work, as short-time work is mostly a crisis measure. While it is used even between crises, the number of observations is too small to contribute to this analysis.

In summary, non-EU migrants are the only group that is estimated to have been pushed into short-time work at a disproportionally high rate, albeit only at the onset of the pandemic, when short-time work usage was peaking (2019-2020).

5 Discussion

The results of both the descriptive and the regression analysis are not lending themselves to be summarized. However, to inform the discussion of the results, a simplified overview is provided before insufficiencies of the analysis are addressed.

As a point of departure, the descriptive section showed that for all year pairs, natives had lower transition-to-unemployment rates than the other groups. The relative changes of transition rates between the groups over the years however resembled a mixed picture.

The regression analysis of the pre-COVID period 2018-2019 yielded higher transition-to-unemployment rates for indirect and EU migrants when compared to natives. This (or any) inter-group difference was not found in the 2019-2020 period. However, a differential pattern reemerges when looking at 2020-2021 and the wider picture of 2019-2021, with EU and non-EU migrants having higher transition-to-unemployment rates than natives according to the estimation. A statistically significant effect of length of stay for direct migrants was only found for 2020-2021 and 2019-2021.

Regarding short-time work, the only clear pattern emerging from the descriptive evidence is the measure's concentration on the first year of the pandemic. The relative changes of transition rates between the groups do not seem to follow a pattern.

The only association with migrant status emerging from the short-time work regressions was found for non-EU migrants in the 2019-2020 period. They were estimated to have been 2.83 times as likely to be send into short-time work compared to natives. A protective effect of length of stay for direct migrants is only implied by the 2019-2021 estimation.

The relatively mixed results suggest that unobserved factors dominate the impact that COVID's labor market shock had on the groups. The variability of inter-group differences also calls into question the external validity of the highly aggregated categories that existing literature is based on, e.g., the dualistic native-migrant categorization deployed by Auer (2022).

Besides the modestly insightful results, the research approach has serval other potential insufficiencies. For one, a holistic view on the labor market trajectories of individuals would need to entail not only transitions *into* unemployment, short-time work or non-employment but also transitions *out* of them, back into employment. Especially the aspect of job-finding is challenging to scrutinize with similarly detailed regression models. When dealing with persons that are lacking a job in the first period of a year-pair, many controls can not be applied. By ignoring this part of the labor market however, we are blind to scenarios in which a group is more likely to be fired but in which this is overcompensated by them finding jobs at a higher rate.

Another group of problems arise from the fact that the analysis uses yearly data. As Figure 6 is showing regarding short-time work, labor market outcomes do not necessarily change yearly. Two groups may have equal transition rates into unemployment (or any other outcome) from a year-perspective. However, individuals of one of the groups may transition more often on average to arrive at the annually captured employment status.

Furthermore, as mentioned earlier, the distribution of interview months is different from group to group. This allows for scenarios in which the datapoints that make up one group's transition-to-unemployment rate may mostly draw on observations from a lockdown while another group's rate may mostly draw on observations from a relatively relaxed labor market situation. Such seasonality may dominate the descriptive evidence presented. Month dummies account for some but possibly not all this seasonality in the regressions.

As literature on discrimination was inspiring this thesis, the following aspects are to be considered. Firstly, the diversity of results does not inspire causal analysis along the lines of discrimination against minorities. Regarding unemployment, the onset of the pandemic (2019-2020) seems to even have had a slightly equalizing effect according to the estimations. Even considering the often worse outcomes for non-EU migrants, it is questionable to proceed to a discussion of

discrimination when an arguably more marginalized group (refugees) did not fare worse than natives according to the regression results. Secondly, an analysis of (taste-based) discrimination would also have to entail an analysis of productivity along the lines of Auer (2022). This endeavor, if done properly, is beyond the scope of the paper. Furthermore, for an analysis of discrimination to take into consideration racial discrimination, more fine-grained data on the countries of origin would have to be utilized. This kind of approach is not feasible with the number of observations in the SOEP data.
Conversely, precluding discrimination because significant shares of labor market differences are explained by occupational sorting (e.g. Borjas & Cassidy; 2023) could be premature, especially when discrimination could be located more upstream, e.g., in the hiring process.

Throughout the paper, short-time work has been treated as an unpreferable labour market outcome. Although short-time work is indeed associated with income losses, it is still generally preferable to being fired. Hypothetically, a discriminatory act could consist of firing a migrant worker and sending their native coworker's department into short-time work only. A specification that considers the two main dependent variables jointly is necessary to explore this scenario.

Not captured by the data are undocumented or regular migrants working in undeclared work. Given that this group does not benefit from the extensive employee protection of German labor law, it seems plausible to assume that they were more likely to have lost their job. Borjas & Cassidy (2023) use an algorithm to synthetically assign undocumented status to parts of their sample and argue that undocumented workers were particularly hard hit.
Relatedly, the analysis is blind to emigration that happened in response to the labor market tightening and other factors. As Mazza et al. (2022) point out, this may hide the "true damage" that some migrant groups have taken as a consequence of the COVID-related shock.

6 Conclusion

The aim of this paper was to answer the question of whether there were significant, unexplained differences in the size of labor market shocks for different migrant groups compared to natives during the first two years of the COVID-19 pandemic in Germany.

For this purpose, the population was divided into five groups: natives, in-direct migrants, EU migrants, non-EU migrants and refugees. Logistic regressions with a variety of control variables related to occupational and personal characteristics were then performed to compute unexplained differences in labor market trajectories.

The analysis found that in no scenario it was natives who transitioned into unemployment, non-employment, or short-time work at a statistically significant, higher rate than any of the migrant groups. Complementary statements on relatively more unfavorable outcomes for migrants are harder to make. Inequalities shifted in magnitude as well as between years and groups. Indirect migrants saw a bettering of their relative transition-to-unemployment rates as the pandemic started. EU migrants faced disproportionally high transition rates regarding unemployment even before the pandemic. This pattern was equalized at the start of the pandemic before it reemerged in later periods. On this ground, it is not justified to assume that the employment shock disproportionally affected non-native groups per se. However, non-EU migrants consistently experienced higher transition rates to undesirable labour market outcomes that cannot be explained by differences in a range of occupational and personal characteristics. Further research could pick up this lead, incorporating remarks of the discussion section and tracking the group through the rest of the pandemic and its recovery phase.

The data used for this analysis turned out to inadequate. The sort of migrant groupings that capture crucial differences in individuals' biographies overburden the SOEP data. Due to the small cell sizes, some of the results lack robustness and validity. Furthermore, the SOEP's quasi-annual data was not fine-grained enough to properly retrace the unfolding of COVID's labor market shock.

The US-American labor economics literature on COVID-19 developed rapidly and extensively thanks to the U.S. Bureau of Labor Statistics' monthly Current Population Survey. German panel studies are at most annual and even the EU Labour Force Survey is carried out only quarterly. In closing, I want to underscore the need for more fine-grained labor market surveys to enable research on the subject that is on a par with the Americans.

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Appendix

Table A1: 2020 to 2021, Employed to Unemployed

	(1)	(2)
Indirect Migrant	0.81	
EU Migrant	3.57***	
Non-EU Migrant	3.38**	0.66
Refugees	1.94	0.17*
Length of Stay		0.89***
Woman	0.53	0.43
Child under 16 in Household	0.35**	0.03***
Woman x Child	6.47***	5.79
Mid-tier Education	0.73	0.21***
High-tier Education	0.78	0.04***
Vocational Training	1.11	0.64
Marginal Employment	1.34	0.20
Employed by Employment Agency	0.95	0.07**
Permanent Work Contract	0.22***	0.03***
Large Company	0.54**	0.21***
1-3 Years with Firm	0.50*	0.24**
3+ Years with Firm	0.27***	0.51
Critical Relevance	1.07	5.26***
Mid-level Teleworkability	3.50**	5.05**
High-level Teleworkability	1.99	60.57***
Mid-tier Requirement Level	0.96	0.58
High-tier Requirement Level	0.73	0.16*
Interactive Non-Routine Tasks	0.67	7.39*
Cognitive Routine Tasks	0.83	1.40
Manual Routine Tasks	0.88	5.55
Manual Non-Routine Tasks	0.50	0.44
N	6,069	1,448
Log Likelihood	-395.70	-87.23

Notes: ***, **, * denote significance at the 1, 5 and 10 percent level.

Table A2: 2018 to 2019, Employed to Unemployed

	(1)	(2)
Indirect Migrant	2.48**	
EU Migrant	3.89***	
Non-EU Migrant	1.98	0.56*
Refugees	1.15	0.71
Length of Stay		0.98
Woman	1.08	0.75
Child under 16 in Household	0.82	0.81
Woman x Child	1.03	1.49
Mid-tier Education	0.66	0.74
High-tier Education	0.32***	0.39
Vocational Training	1.31	0.79
Marginal Employment	0.55*	1.03
Employed by Employment Agency	2.56*	0.99
Permanent Work Contract	0.37***	1.94
Large Company	0.34***	0.45**
1-3 Years with Firm	0.39***	0.25***
3+ Years with Firm	0.19***	0.19***
Critical Relevance	1.68**	1.03
Mid-level Teleworkability	0.58*	0.80
High-level Teleworkability	0.46	1.16
Mid-tier Requirement Level	1.19	0.63
High-tier Requirement Level	2.78**	3.17**
Interactive Non-Routine Tasks	2.24	2.83
Cognitive Routine Tasks	1.46	3.26*
Manual Routine Tasks	3.97**	14.98***
Manual Non-Routine Tasks	1.09	2.36
N	11,080	2,177
Log Likelihood	-747.21	-204.44

Notes: ***, **, * denote significance at the 1, 5 and 10 percent level.

Table B1: 2018 to 2019, Employed to Not-Employed

	(1)	(2)
Indirect Migrant	1.52*	
EU Migrant	1.35	
Non-EU Migrant	1.10	0.83
Refugees	0.88	0.70
Length of Stay		0.99
Woman	1.12	1.45
Child under 16 in Household	0.94	0.78
Woman x Child	4.51***	1.96
Mid-tier Education	0.93	0.63
High-tier Education	0.65	0.45
Vocational Training	0.84	1.03
Marginal Employment	1.55**	0.73
Employed by Employment Agency	2.22**	1.67
Permanent Work Contract	0.59***	2.04*
Large Company	0.62**	0.65
1-3 Years with Firm	0.71*	0.23***
3+ Years with Firm	0.40***	0.18***
Critical Relevance	1.44**	1.95*
Mid-level Teleworkability	0.93	0.91
High-level Teleworkability	0.79	1.29
Mid-tier Requirement Level	0.93	0.89
High-tier Requirement Level	1.63	1.77
Interactive Non-Routine Tasks	0.86	2.97
Cognitive Routine Tasks	0.79	3.73**
Manual Routine Tasks	1.11	9.59***
Manual Non-Routine Tasks	0.85	2.53
N	11,090	2,183
Log Likelihood	-1,968.17	-369.07

Notes: ***, **, * denote significance at the 1, 5 and 10 percent level.

Table B2: 2019 to 2020, Employed to Not-Employed

	(1)	(2)
Indirect Migrant	1.19	
EU Migrant	0.85	
Non-EU Migrant	0.93	0.85
Refugees	1.25	1.20
Length of Stay		1.02
Woman	0.90	1.99
Child under 16 in Household	0.70	1.21
Woman x Child	4.19***	2.15
Mid-tier Education	0.99	0.70
High-tier Education	1.10	0.66
Vocational Training	0.65	0.57
Marginal Employment	3.51***	3.06**
Employed by Employment Agency	1.75	4.02**
Permanent Work Contract	0.42***	0.48**
Large Company	0.78*	0.49**
1-3 Years with Firm	0.88	1.59
3+ Years with Firm	0.60**	0.99
Critical Relevance	0.98	1.37
Mid-level Teleworkability	0.84	0.62
High-level Teleworkability	1.09	0.68
Mid-tier Requirement Level	0.86	0.60
High-tier Requirement Level	0.83	1.04
Interactive Non-Routine Tasks	1.29	1.15
Cognitive Routine Tasks	1.03	1.77
Manual Routine Tasks	1.44	1.62
Manual Non-Routine Tasks	1.76	0.51
N	9,393	2,027
Log Likelihood	-2,044.97	-428.29

Notes: ***, **, * denote significance at the 1, 5 and 10 percent level.

Table B3: 2020 to 2021, Employed to Not-Employed

	(1)	(2)
Indirect Migrant	1.18	
EU Migrant	1.37	
Non-EU Migrant	2.14**	1.51
Refugees	1.17	0.26**
Length of Stay		0.98
Woman	1.07	1.29
Child under 16 in Household	0.43**	0.13***
Woman x Child	9.10***	5.74*
Mid-tier Education	0.68	0.32**
High-tier Education	0.67	0.12***
Vocational Training	0.89	0.85
Marginal Employment	2.44***	1.69
Employed by Employment Agency	0.64	0.24*
Permanent Work Contract	0.28***	0.19***
Large Company	0.64**	0.90
1-3 Years with Firm	0.95	0.31**
3+ Years with Firm	0.80	0.66
Critical Relevance	1.02	1.92
Mid-level Teleworkability	1.62	2.63*
High-level Teleworkability	1.07	13.08**
Mid-tier Requirement Level	1.09	0.85
High-tier Requirement Level	1.22	0.12***
Interactive Non-Routine Tasks	0.61	1.01
Cognitive Routine Tasks	0.96	0.31*
Manual Routine Tasks	0.66	0.70
Manual Non-Routine Tasks	0.70	0.21*
N	6,087	1,459
Log Likelihood	-1,142.04	-241.50

Notes: ***, **, * denote significance at the 1, 5 and 10 percent level.

Table B4: 2019 to 2021, Employed to Not-Employed

	(1)	(2)
Indirect Migrant	1.14	
EU Migrant	1.05	
Non-EU Migrant	1.12	1.22
Refugees	0.70	0.27**
Length of Stay		0.99
Woman	1.18	2.34
Child under 16 in Household	0.57*	0.48
Woman x Child	4.47***	2.31
Mid-tier Education	1.03	0.49
High-tier Education	1.29	0.22**
Vocational Training	1.60	1.12
Marginal Employment	1.98***	1.47
Employed by Employment Agency	1.05	0.90
Permanent Work Contract	0.55***	0.44*
Large Company	0.80	0.52
1-3 Years with Firm	0.64*	0.76
3+ Years with Firm	0.63*	1.53
Critical Relevance	0.99	1.14
Mid-level Teleworkability	0.80	1.00
High-level Teleworkability	0.62	1.37
Mid-tier Requirement Level	0.81	1.57
High-tier Requirement Level	0.53**	0.25*
Interactive Non-Routine Tasks	0.97	0.28
Cognitive Routine Tasks	0.82	0.45
Manual Routine Tasks	0.55	0.39
Manual Non-Routine Tasks	0.85	0.26*
N	7,723	1,341
Log Likelihood	-1,938.89	-307.65

Notes: ***, **, * denote significance at the 1, 5 and 10 percent level.

Table C1: 2020 to 2021, Employed to Short-Time Work

	(1)	(2)
Indirect Migrant	0.23	
EU Migrant	1.69	
Non-EU Migrant	0.22	0.30
Refugees	0.74	2.41
Length of Stay		0.82
Woman	0.20***	128.62***
Child under 16 in Household	0.65	5.24*
Woman x Child	3.11	0.08
Mid-tier Education	1.76	2.16
High-tier Education	0.90	3.50
Vocational Training	0.0000***	0.00***
Marginal Employment	0.07***	0.03
Employed by Employment Agency	1.14	3.50
Permanent Work Contract	2.25	0.48
Large Company	0.50*	0.77
1-3 Years with Firm	1.33	3.51
3+ Years with Firm	1.25	91.24***
Critical Relevance	0.39*	0.39
Mid-level Teleworkability	1.18	1.04
High-level Teleworkability	2.80	9.09
Mid-tier Requirement Level	1.41	7.91*
High-tier Requirement Level	1.79	10.89
Interactive Non-Routine Tasks	0.77	11.60
Cognitive Routine Tasks	0.35*	5.16
Manual Routine Tasks	1.35	104.67
Manual Non-Routine Tasks	1.53	791.66
N	6,087	1,459
Log Likelihood	-261.01	-8.45

Notes: ***, **, * denote significance at the 1, 5 and 10 percent level.

Affidavit

I certify that the thesis at hand was made without unauthorized help and that I only used the tools denoted. All statements literally or logically taken from publications are marked as quotes. The submitted thesis has not been the subject of another examination procedure, neither in its entirety nor in significant parts. The electronic version of the submitted thesis is identical in content and formatting to the paper copy.

David Ammann, Berlin, 05.12.2023

D. Ammann