Minimum Wages

On the way to livelihood-securing work

Dissertation
zur Erlangung des Doktorgrades
der Wirtschafts- und Sozialwissenschaftlichen Fakultät
der Eberhard Karls Universität Tübingen

vorgelegt von

René Kalweit

Tübingen

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Contents

1. Introduction

In a nutshell, this dissertation makes three key contributions. Firstly, it adds to the political economy dimension of minimum wage policies. Specifically, it examines whether minimum wages are an effective means of regaining the electorate's confidence. However, it does not suggest that parties will be punished for failing to fulfil election pledges, nor that gaining votes with new pledges is ineffective. Second, the dissertation explores the unintended effects of minimum wage laws. Using the introduction of the German minimum wage in 2015 as an exogenous policy shock, the study evaluates whether wage increases for low-wage migrant workers influenced their intentions to return to their home countries. Although theoretical predictions offer ambiguous expectations, the empirical results reveal no statistically significant link between wage increases and revised migration plans. Thirdly, the dissertation makes a methodological contribution by applying Natural Language Processing (NLP) techniques to qualitative interview data on the evaluation of minimum wages. The study assesses whether machine learning tools can support, or even partially automate, the coding process in qualitative policy research. The findings suggest that, while current models are not yet fully reliable for independent coding, they can improve reproducibility and complement human interpretation.

General debate about minimum wages

The introduction of minimum wages has become a common labour market intervention across industrialized economies. As of 2022, 30 out of 38 Organisation for Economic Co-operation and Development (OECD) countries had a statutory minimum wage in place. The remaining eight use sector- or occupation-wide minimum wages to establish an effective wage floor (OECD, 2022). Within the European Union, Directive 2022/2041 requires member states with collective bargaining coverage rates below 80 per cent to implement adequate minimum wage frameworks (Council of the European Union, 2022).

As the largest economy in the European Union, Germany introduced a nationwide minimum wage of EUR 8.50 per working hour on 1 January 2015. This was one of the most significant labour market reforms since the 'Hartz I-IV' reforms to the German labour market.

Policymakers intend to address several shortcomings in the market outcome by introducing a minimum wage. While some of these objectives are based on predictions from theoretical models, others contradict the latter. In general, the aims of a minimum wage can be categorised into two broad areas.

The first relates to the protection of workers. The objective here is to enhance the remuneration of low-skilled workers to ensure they can make a living through employment, thereby reducing income poverty and labour income inequality (see, for example, Bosch, 2007; Kalina & Weinkopf, 2014). By establishing a minimum wage, it becomes easier to ensure that workers earn a

sufficiently high income to meet their basic needs and demands. Specifically, minimum wages help to prevent individual poverty and relieve the state's burden of providing social benefits to people in gainful employment. Furthermore, in many countries, social benefits paid to individuals outside the labour market, such as retirees (pension system), the unemployed (unemployment insurance) and those incapable of working due to health reasons (disability insurance), depend on recent or lifetime earnings, among other things. Therefore, minimum wages help to protect people from poverty in old age, guaranteeing them an adequate standard of living. In times of economic crisis, minimum wages prevent wage cuts below the subsistence level. Finally, they can also promote economic and social equality for people who, based on social categories such as gender or ethnic background, are more likely than others to find themselves in precarious low-wage employment.

The second, broader group of reasons is connected to overall economic performance. Raising the wage level paid to a proportion of the workforce stimulates aggregate demand in the domestic economy. This is particularly significant given that poor people have been seen to have a higher propensity to consume than those with greater financial means. Increasing their disposable income therefore benefits the economy as a whole and might also have positive effects in sectors not directly affected by the minimum wage. In addition, raising the minimum wage increases the opportunity costs of unemployment, thus implicitly incentivising jobseekers to find employment. This only applies, of course, as long as the state-guaranteed subsistence minimum does not increase, or increases at a slower rate than the minimum wage. Ultimately, minimum wages help foster competition. Through paying their employees excessively low wages, firms gain a competitive advantage over those who pay more acceptable salaries to their staff. Firms that are unproductive to the extent that they should not be able to remain in the market, yet do so by engaging in wage dumping practices, can be eliminated via the mechanism of a minimum wage.

However, critics of minimum wages argue that a statutory minimum wage significantly interferes with price formation in labour markets and could therefore lead to losses in economic efficiency, particularly reflected in lower employment and higher unemployment (see Knabe et al., 2014).

This debate sheds light on the most relevant outcomes in the context of the minimum wage as an intervention in the labour market.

Theoretical opponents

Two chief theoretical frameworks are often used to study the effects of minimum wages on equilibrium wages and employment levels in labour markets: the neoclassical demand-and-supply model and the Keynesian labour market model.

The neoclassical framework provides the theoretical foundation for a narrative that has become dominant in the ex ante minimum wage debate, namely that minimum wages have negative

employment effects (Boeri & van Ours, 2013; Cahuc et al., 2014a). Assuming perfect competition and the absence of market frictions, the neoclassical demand-and-supply model posits an efficient labour market equilibrium that would only be disturbed by the introduction of a minimum wage. Consequently, employment would decrease in the economy.

However, the assumption of perfect competition in the labour market is rarely realistic. In specialised or spatially restricted labour markets, for example, the number of firms hiring a certain 'kind' of labour can be substantially lower than assumed under perfect competition. In this type of monopsonistic labour market, minimum wages can help to offset the welfare losses associated with the firm's market power (Manning, 2003; Manning, 2021). As such, minimum wages increase employment and bring the labour market back to its competitive equilibrium, as long as they are not set at too high a level. Additionally, introducing a minimum wage within a job search model could offset negative employment effects due to increased labour costs if it incentivises those not in employment to seek and accept work (Cahuc et al., 2014, p. 793 ff.).

In a Keynesian labour market, it is aggregate demand rather than wages that determines the overall level of employment. From this perspective, the effects of a minimum wage on employment are indeterminate. As Herr et al. (2009) explain, the implications of introducing a minimum wage in a Keynesian model can be considered in terms of three factors: the price level, distribution and employment. In the case of homogeneous labour, only the nominal wage is affected, while real wages and, consequently, aggregate demand remain unchanged due to price increases (see Herr et al., 2009). Accordingly, minimum wages are an unsuitable means of addressing unemployment.

In contrast to homogeneous labour, the employment effects of a minimum wage are more complex with heterogeneous labour. Two channels must be taken into account here: changes in income distribution and changes in relative prices. In the most likely scenario, it is low-wage workers who will benefit most from the introduction or increase of the minimum wage (wage structure compression). Therefore, aggregate demand is likely to increase. While the economy is operating below full capacity, an increase in aggregate demand will be met by an increase in aggregate supply, i.e. production/output will increase. Changes in the relative prices of goods and services, on the other hand, affect the demand for the products as well as the demand for the inputs needed to produce them. Typically, low-wage sectors of the economy tend to be labour-intensive, whereas high-wage sectors are more capital-intensive. Due to a minimum wage, labour, particularly in labour-intensive sectors, becomes more expensive. This incentivises firms to switch to a more capital-intensive production method, e.g. buying new machinery or investing in more productive processes and technologies.

Consequently, demand for low-wage workers will decline immediately. Apart from that, those firms will try increase their prices to offset their increased wage costs. The additional machinery that

these firms will acquire is usually manufactured in capital-intensive, high-wage sectors of the economy. This stimulates demand for high-skilled workers in those industries, and employment growth is expected. However, the different market sectors are further interdependent, which creates additional ways in which wage variation in one sector affects employment in another. Many low-wage sectors of the economy produce auxiliary or pre-products that are then used in high-wage sectors. Therefore, capital-intensive sectors will also experience changes in the composition of inputs that reflect the price increases of goods and services associated with low-wage labour market segments, which may include substituting more or less labour in the production process. Overall, this makes it almost impossible to predict the actual employment effect of this market intervention (Herr et al., 2009: 13). As with the neoclassical framework, the Keynesian paradigm is unable to provide a clear prediction of how employment will adjust in response to the introduction of a minimum wage.

In summary, theoretical predictions in labour market economics concerning the effect of minimum wages on important factors such as employment, unemployment and working hours yield ambiguous results. The outcomes depend on the level of the minimum wage compared to the level of wages that would prevail in its absence, as well as on the specific supply and demand conditions in the relevant labour markets.

Empirics

The overall welfare and employment effects of theoretical frameworks remain unclear due to diverging and debatable assumptions. Furthermore, theoretical models necessarily simplify the world in order to be able to provide answers. They fail to show the unintended effects that occur outside of the model. Consequently, the focus shifts towards empirical studies on this topic to shed more light on the effects of the minimum wage. A large body of empirical literature exists on the impact of minimum wages on employment, earnings and the wage distribution, as well as various other dimensions.

Methodologically, empirical minimum wage studies employ a variety of broader approaches. Firstly, those that utilise state-year panel data. Among the first to adopt this approach were Neumark and Wascher (1992), who estimated a fixed effects model with employment as the dependent variable and minimum wage level as the independent variable. Another approach that emerged was the use of quasi-experiments, most notably by Card and Krueger (1994), who incorporated the fact that New Jersey increased its minimum wage while Pennsylvania did not, into a difference-in-difference framework, with New Jersey serving as the treatment group and Pennsylvania as the control group. As methodology continually improves, concerns about the identification of earlier studies have come to light. Consequently, newer minimum wage studies, while staying in line with the methodology popularised by the aforementioned studies, feature more elaborate identification strategies. In particular, regression analyses based on state-year or state-month longitudinal data sources, such as difference-in-difference models, are used, with the addition of clustered standard errors, more

comprehensive control variables, and so-called 'bite' measures, such as the Kaitz index, which proxy the degree to which a state or region is affected by a minimum wage. Other studies also rely on repeated cross-sectional data at the individual level rather than panel data. Finally, some empirical literature utilizes simulation techniques which typically rely on previously estimated minimum wage effects and incorporate them into a simulation framework.

These methods are used to identify the various effects of the minimum wage. The largest strand of minimum wage literature focuses on the employment effects of minimum wages. Starting with time-series analyses in the 1970s — such as those by Kaitz (1970) — the academic consensus has favoured the existence of negative employment effects. See Brown (1999) for an overview of the relevant literature. However, studies such as those by Card (1992), Katz and Krueger (1992) and Card and Krueger (1994) increasingly challenged this view with the emergence of panel data and quasi-experiments in the early 1990s. However, most of these studies had a rather narrow scope of analysis, identifying only Local Average Treatment Effects (LATE), as they, with a few exceptions, investigated teenage employment in the US.

Newer minimum wage research largely indicates mixed results with respect to potential employment effects (see, for example, Belman and Wolfson, 2014, or Manning, 2019, for extensive literature reviews). Whilst longitudinal data sources remained the most commonly utilised basis for analysis, the general methodology continuously improved. Data has also become more accurate, and other new approaches have been adopted, such as incorporating labour market search models (Flinn, 2006), accounting for regional heterogeneity (Dube, Lester & Reich, 2010) and modelling long-term consequences (Neumark & Nizalova, 2007). Similarly, revised estimation techniques have been employed to address identification issues that past studies were unable to address. The scope of the analysis has also expanded with respect to other demographic groups (see, for example, Sabia (2008) on single mothers, Mastracci and Persky (2008) on low-wage workers in general and Orrenius and Zavodny (2008) on immigrants), as well as to other countries where minimum wages have been introduced (see, for example, Yuen (2003) and Brochu and Green (2013) on Canada, Cardoso (2006) on Portugal and Caliendo et al. , 2018; Dustmann et al., 2022 for Germany), and other dimensions of employment (Dube, Lester & Reich, 2016 for employment flows; Gopalan et al., 2021 for firm hiring behaviour; Jardim et al., 2022 for an analysis of extensive and intensive margins).

Another area of interest is the effect of minimum wages on overall earnings and their distributional implications. Quite trivially, workers who earned below the new minimum wage level prior to the change in legislation should experience higher average wages as a result of an increase in wages. Indeed, this is what the majority of studies find (see Belman and Wolfson, 2014, for an overview). However, when accounting for potential job losses or reductions in working hours, the

impact on overall earnings becomes less clear-cut. The more ambiguous effects of minimum wages have been studied in a wide range of papers (Meyer & Wise, 1983; Currie & Fallick, 1996; Neumark & Wascher, 2011).

Other strands of related literature have investigated the effects of minimum wages on wage spillovers (Biewen et al., 2022; Dickens & Manning, 2004; Gopalan et al., 2021; Gregory & Zierahn, 2022), the wage distribution (Machin & Manning, 1994; DiNardo, Fortin & Lemieux, 1996; Lee, 1999; Autor, Manning & Smith, 2016) and its link to poverty (Sabia & Nielsen, 2013).

Overall, the empirical literature reaches a consensus on the positive effects on wages for low-wage workers earning either below or slightly above the new minimum wage levels. Further up the wage distribution, however, the results are more equivocal. Consequently, many studies suggest that minimum wages significantly reduce wage inequality by compressing the wage distribution. These findings support the argument for the distributional implications of minimum wages. For the economy as a whole, minimum wages tend to be beneficial if the minimum wage level is below a certain threshold. However, identifying this threshold is difficult, and there is still no clear empirical answer (Chen & Teulings, 2022; Lee & Saez, 2012; Boeri, 2012). The EU directive demanding a minimum wage equivalent of 60 % of the median wage can be seen as a bold move. Literature fails to draw a clear picture whether minimum wages are an effective tool to address poverty in general. As circumstances between nations vary, so do the effects of minimum wages. Minimum wage regimes need to take into account the whole economic and labour market structure among many other factors.

While the major effects of the minimum wage have been discussed so far, many more remain to be explored. For instance, further studies have examined the impact of minimum wages on factors such as well-being (Bossler & Broszeit, 2017; Kuroki, 2018; Gülal & Ayaita, 2020), firm value (Bell & Machin, 2016), firm productivity (Owens & Kagel, 2010; Riley & Rosazza Bondibene, 2017), and human capital (Pacheco & Cruickshank, 2007; Smith, 2021).

Minimum Wages and Political Economy

Both theoretical and empirical examinations have revealed that minimum wages may have significant distributional implications. This reveals the conflicting nature of labour market policies such as the minimum wage, as some demographic or socio-economic groups may benefit from them, while others benefit less or not at all, especially if the outcome is not Pareto optimal. However, it is unclear to what extent this can be observed among the population. Waltman (2000) found that popular support for minimum wages in the US had remained at around three-quarters since the 1930s. Similar levels of support can be found in other countries.

So why are minimum wages introduced and raised so reluctantly by policymakers and legislation? Waltman (2000) suggests that one possible explanation is that, although most people support minimum wage legislation, few are passionate about this issue. Neumark and Wascher (2008)

therefore propose that minimum wage legislation is largely driven by individual interest groups who push policymakers in either direction — i.e. to introduce/raise minimum wages or abstain from doing so. While trade unions (seeking to support workers) and employers in high-wage sectors (perhaps hoping to increase purchasing power) may favour minimum wages, there may be a simultaneous backlash and opposition from other constituent groups who exert more influence and pressure on policymakers. A key implementation challenge therefore particularly pertains to employers in low-wage sectors of the labour market. Introducing or raising a minimum wage, particularly in low-wage sectors and industries, immediately increases labour costs for firms. If these costs cannot be passed on to consumers in the form of higher prices for goods and services, profit margins will decrease. This explains why higher minimum wages are sometimes met with outright refusal by individual employers and entire employers' associations. Ultimately, the respective distribution of political power within those constituent groups determines what force possesses the upper hand. Some studies have examined this question empirically in an attempt to shed light on the linkages between voting behaviour and the interests of different pressure groups (Silberman and Durden, 1976; Poole and Rosenthal, 1991; Seltzer, 1995).

This path is followed in Chapter 2 of the dissertation. Together with Natalie Herdegen, we investigated policy feedback in response to minimum wage laws and whether these increased support for parties associated with introducing the minimum wage.

Limits of empirical approaches

Based on the extensive literature on minimum wages, one conclusion that can be drawn is that past empirical research has failed to provide a clear and concise picture of the consequences of introducing and increasing minimum wages. This may be due to limitations in the identification strategies employed, a lack of precise data, or the fact that minimum wages simply have different implications at different times and in different spaces. Quantitative methods may struggle to identify effects if affected and unaffected workers experience wage increases, if people are paid for undeclared work or if employers do not comply with the law by merely decreasing reported working hours to maintain monthly gross wages.

Therefore, alternative approaches could help to improve the precision and rigour of minimum wage research. Where empirical investigations fail to produce unambiguous results, qualitative approaches may be able to bridge the gap between theory and reality. For example, if no clear consensus emerges from empirical or theoretical literature regarding the effect of minimum wages on poverty, qualitative research (e.g. interviewing or surveying people directly affected by minimum wage laws) may be more fruitful than analysing vast, impersonal datasets using purely quantitative or microeconomic methods. After all, the data may not reflect the actual circumstances of a person near or below the poverty line, as these may differ from their subjective experience. The same goes for the

employment effects of minimum wages, where analyses of administrative employment data could be complemented by qualitative conclusions obtained from surveys or interviews with firms (in the form of their owners or people involved in human resources), thus ultimately providing a better picture of how firms adapt their behaviour regarding personnel issues.

Such approaches have slowly begun to emerge not just in minimum wage research, but in economic research in general. However, a more pronounced focus might yield results that could inform both quantitative research and policymaking favourably. Nevertheless, time constraints and the difficulty of reproducing results from such approaches remain barriers to their application. This dissertation discusses in chapter 3 whether and how Natural Language Processing (NLP) can be integrated into conventional qualitative research workflows to support researchers. This work was undertaken in collaboration with my colleagues Philipp Kugler, Yvette Bodry, Andreas Koch, Marcel Reiner and Tobias Scheu. Our focus is on transcripts of interviews conducted with stakeholders during the introduction of the minimum wage. Typically, these interviews are manually coded using predefined categories before they can be analysed. We explore the possibility of carrying out the coding step using NLP instead of assigning codes manually. We conclude that the representation of language, particularly specialised language, in the currently available language models is not yet sufficiently refined to be applicable with any reliability in qualitative research. Nevertheless, off-the-shelf language models can currently be used to validate results obtained by manual coding. This could make qualitative research more traceable and fully reproducible. Furthermore, it provides insight into how large language models work and how deeply they understand the given data.

(Un)intended Effects of the MW

Apart from the effect of the minimum wage on the overall economy, its introduction is an exogenous event from an individual's perspective. They are certainly not able to influence its introduction or level. This makes it very appealing to use the introduction of the minimum wage as an event to study other related economic subjects. Chapter 4 observes the link between wages and return migration intentions, using the introduction of the minimum wage as an exogenous event.

This dissertation consists of three self-contained chapters dealing with the effects of the minimum wage on voter outcomes, the use of the minimum wage as a quasi-experimental setting to study return migration intentions, and a new methodological approach to improving qualitative research using artificial intelligence.

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2. The minimum wage: An effective means to regain the electorate's confidence?

Abstract

We analyse the policy feedback on the minimum wage introduction in Germany, which was the core promise of the Social Democrats in the 2013 elections. Overall, there is no positive policy effect on the electorate's preference for the Social Democrats. We find even the contrary: workers earning below the minimum wage tend to give a negative policy feedback in Eastern Germany. Our main estimation strategy exploits the variation of median wages across regions using the Kaitz index. The Difference-in-Differences estimations use data from a large household panel. We conclude that the Social Democrats were unsuccessful in regaining the confidence of the electorate through the introduction of the minimum wage.

Introduction

The introduction of the general statutory minimum wage in Germany in 2015 was antedated by a controversial debate both in the public sphere and among scientists (see Caliendo et al., 2022; Paloyo et al., 2013). Knabe et al. (2014) for example predicted severe disemployment effects and the German Council of Economic Experts also expressed a pessimistic view in its annual report (Sachverständigenrat, 2014). Among the population, on the one hand, the support for the minimum wage introduction was high at 82 per cent. On the other hand, 40 per cent were afraid of substantial disemplyoment effects.[1] According to Bossler (2017), employers affected by the minimum wage expressed increased employment uncertainty in 2014 and expected lower growth and increasing wage costs in the following year. Regarding politicians' motives, Dostal (2012) hypothesises that the Social Democrats favoured the introduction of a statutory minimum wage in order to restore credibility lost during deregulation reforms of the early 2000s and to ease the dissatisfaction among voters caused by liberalisation of the labour market. In retrospect, Caliendo et al. (2018) and Dustmann et al. (2022) show that many individuals have indeed experienced an increase in their hourly wage due to the introduction of the minimum wage.

Against this background, we ask the question whether the Social Democrats actually succeeded in winning back lost credibility via the introduction of a statutory minimum wage or whether concerns were large enough to translate into decreasing support. Therefore, in this paper we analyse whether the introduction of the minimum wage in Germany changed voters' preferences for the Social Democrats and was thus successful in regaining electoral support after a period of deregulating labour market reforms. We also examine whether the introduction of the statutory minimum wage had heterogeneous effects on different groups of individuals.

We use survey data and apply Difference-in-Differences estimations. We identify the effect by exploiting the regional variation in the bite of the minimum wage. Our results do not show any positive effect of the minimum wage introduction on the preference for the Social Democrats. We run a variety of model specifications. Individuals earning below the minimum wage tended to turn away from the Social Democrats in regions with higher bites in Eastern Germany.

The introduction of a minimum wage in Germany happened quite late compared to other countries. Thus, evidence for Germany has been scarce until about a decade ago. Since then, the literature has grown substantially and shows results in line with international evidence (e.g. Cengiz et al., 2019). By now, positive effects of the minimum wage introduction on hourly wages in Germany are well documented, for example by Bossler and Gerner (2020), Dustmann et al. (2022), Caliendo et al.

[1] See results of a poll by Forschungsgruppe Wahlen in March 2014: https://www.forschungsgruppe.de/Umfragen/Politbarometer/Archiv/Politbarometer_2014/Maerz_II_2014/ (accessed 28th Nov. 2023).

(2023) or Biewen et al. (2022). Burauel et al. (2020) study heterogeneous effects of the minimum wage and show the largest growth of hourly wages among the marginally employed. Bossler et al. (2024) show remarkable effects both on hourly and on monthly wages for the substantial increase in the minimum wage of 2022. Both Bossler and Schank (2023) and Biewen et al. (2022) show that wage inequality decreased due to the minimum wage introduction. While forecasts on employment losses caused by a minimum wage of 8.50 Euro varied been between 140,000 and one million jobs (see Schulten and Weinkopf, 2015), ex-post evaluations hardly find any employment effects as Caliendo et al. (2018) and Dustmann et al. (2022) show.

The scope of minimum wage research is however broader than studying labour market effects. It includes, for example, the analysis of the population's attitude towards the minimum wage and the impact of the minimum wage on life satisfaction (Gülal and Ayaita, 2020; Kuroki, 2018; Fedorets and Schröder, 2019). Bossler and Broszeit (2017) analyse the effects of the minimum wage introduction in Germany on job satisfaction and pay satisfaction of adversely affected employees (i.e. employees who report that they experienced a reduction in bonus payments, an increased workload, or did not receive an expected pay increase). The results indicate that the minimum wage might affect this group negatively.[2] With the question we ask, we contribute to this strand of literature by broadening the scope to the sphere of political economy.

Political backlash to policy reforms has already been studied in other contexts, albeit with inconclusive results. Blattman et al. (2018) do not find increased support for the government after the introduction of cash transfers in Uganda which is in contrast to the results by Manacorda et al. (2011) for Uruguay. Fetzer (2019) finds that in the UK support for the Conservative Party decreased after it had introduced cuts in welfare spending, while the UK Independence Party (UKIP) substantially gained electoral support. In a related strand of literature, Markovich and White (2021) estimate the effect of minimum wage increases on voter turnout in the U.S. and find a significantly positive policy feedback. In addition to that, we also contribute to the strand of literature analysing economic voting (see Killian et al., 2008; Lewis-Beck and Stegmaier, 2013).

The rest of this paper is organised as follows: The next section summarizes the institutional details of the minimum wage introduction in Germany. Thereafter, we describe the database and the estimation strategy. Afterwards, we present our multivariate analysis, including several robustness analyses. We discuss potential pitfalls before concluding.

[2] Adverse effects of the minimum wage are also documented by Gregory and Zierahn (2022). The authors show that high-skilled workers experience negative net employment effects as the cost shock induced by the minimum wage reduces demand for labour and low-skilled workers are substituted by medium- not by high-skilled workers.

The introduction of the minimum wage in Germany

Germany has only introduced a statutory minimum wage on January 1st, 2015 – quite late in comparison to e.g. the U.S. (1938) or the UK (1998).[3] A coalition of Social Democrats (SPD) and Conservatives (CDU and CSU) passed the law as central election promise of the Social Democrats.[4]

Sector-specific wage floors had however already been in place since the 1990s. In several sectors such as meat processing, hairdressing, agriculture, temporary agency work, textiles, clothing and industrial laundries and newspaper delivery, sector-specific wage-floors were below the statutory minimum wage in 2015. These lower wages were admissible during a transition period which lasted until the end of 2017. In several other sectors, such as construction, commercial cleaning and caring, a wage floor above the national minimum wage is still in place.

The statutory minimum wage (MW) initially amounted to 8.50 Euro per hour. It was increased biannually until 2019. From then on increases gathered pace, as table 2.1 shows. The minimum wage amounted to 48 per cent of the median wage for full-time workers in 2015.[5] In the same year, the UK minimum wage reached about the same level, while its level was more than ten percentage points lower in the U.S. Over the following years, the share increased to 51 per cent in Germany (and even more in the UK), while it decreased in the U.S.

Table 2.1 *Minimum wages in place 2015-2022*

In place from	Amount in €	Median wage
Jan 14		14.5
Jan 15	8.50	14.9
Jan 17	8.84	16.2
Jan 19	9.19	17.3
Jan 20	9.35	
Jan 21	9.50	
Jul 21	9.60	
Jan 22	9.82	
Jul 22	10.35	
Okt 22	12.00	

Source: Bundesregierung (2022), Beschluss der Mindestlohnkommission nach § 9 MiLoG (2016), SOEP v36.

The German statutory minimum wage exempts only few groups of employees. These exemptions apply to underage employees, apprentices, certain categories of trainees and interns and former long-term unemployed persons during the first six months following unemployment. Exemptions also apply to non-profit and voluntary work.

[3] For a detailed overview, see Bruttel et al. (2018).

[4] Mabbett (2016) summarizes the opinion of the two ruling parties regarding the minimum wage and its development in the course of the introduction.

[5] The figures are taken from OECD (2022).

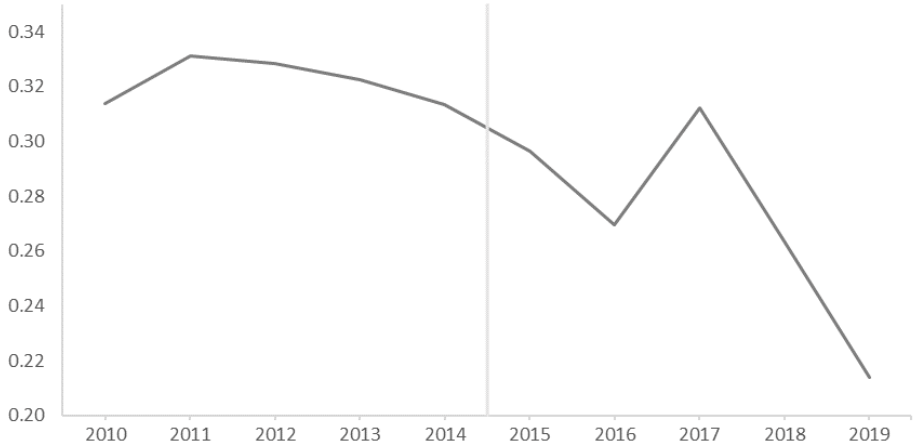
Data and sample construction

For our analysis, we need panel data containing voters' responses over time and information on the extent to which the minimum wage affects individuals. The German Socio-Economic Panel (SOEP) contains information on socio-economic characteristics such as employment and wages as well as on voting behaviour and preferences for a specific party (Liebig et al., 2021).

The SOEP is a representative household survey that has been conducted annually in Germany since 1984. Up to 30,000 people are interviewed each year. The SOEP uses a stratified random sample to select households in which all members aged 16 and over are interviewed on a voluntary basis. We use the SOEP v36 remote version, which contains information up to the year 2019. Our sample includes all individuals who are eligible to vote, whom we follow for at least two years and who do not move across labour market regions. Our analysis covers the years 2010 to 2019.

With the SOEP we can approach our research question as the survey contains several questions on the interviewee's interest in politics, party preferences and voting behaviour. The interviewee is asked whether they lean towards a particular party in general and to which specific party they lean towards. The interviewee can choose from a list of all major parties, and it is possible to choose more than one party. We construct dichotomous outcome variables, e.g. if we take leaning towards the Social Democrats as the outcome variable, each person who has answered that they lean towards this party is assigned a 1, otherwise a 0. As can be seen from figure 2.1, the share of respondents indicating that they lean towards the Social Democrats declines from 31 per cent in 2010 to 21 per cent in 2019.[6]

Fig. 2.1 Share of respondents leaning towards the Social Democrats



Source: SOEP v.36, population-weighted results.

[6] The share of respondents who say that they lean towards a party but to not indicate to which one, amounts to between 1 and 3 per cent throughout the observation period.

The question asking which party an individual leans towards is conditional on the previous question asking whether the interviewee leans towards any party at all. We therefore reduce our sample for the main analysis to those who answer the latter question in the affirmative. The composition of our sample is stable over time (see table 2.2).

Table 2.2 *Composition of analysis sample (individuals leaning towards a particular party, share)*

	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019
Share in %										
Female	47.38	46.55	46.13	47.43	48.52	47.70	47.11	47.39	46.91	47.88
Male	52.62	53.45	53.87	52.57	51.48	52.30	52.89	52.61	53.09	52.12
Age										
24 and younger	6.60	6.63	5.79	5.68	6.01	4.56	4.75	5.16	5.75	5.09
25 - 54	46.06	40.98	40.63	39.40	43.46	42.29	43.52	41.68	41.97	41.05
55 and older	47.34	52.39	53.58	54.92	50.52	53.15	51.73	53.16	52.28	53.86
Place of residence										
East	15.80	14.70	14.36	14.55	15.00	14.79	14.19	13.85	13.48	14.10
West	84.20	85.30	85.64	85.45	85.00	85.21	85.81	86.15	86.52	85.90
Citizenship										
Foreign	4.09	3.79	4.16	4.55	4.69	4.35	4.86	5.98	5.69	5.63
German	95.91	96.21	95.84	95.45	95.31	95.65	95.14	94.02	94.31	94.37
Type of employment										
Fulltime	83.92	84.97	85.62	85.12	83.08	83.91	83.90	82.01	81.64	82.04
Part-time	16.08	15.03	14.38	14.88	16.92	16.09	16.10	17.99	18.36	17.96
Marginal	7.46	7.72	8.20	7.87	9.15	7.90	8.17	7.54	7.61	6.54
Household characteristics										
Married	58.80	56.63	58.45	58.58	58.04	57.35	56.87	57.01	56.97	57.77
Living with children age 16 and younger	17.24	11.20	11.47	11.75	17.11	16.53	16.87	16.66	16.81	17.60
Education										
No vocational education	14.41	14.79	14.16	15.13	15.22	13.49	12.85	14.46	14.09	12.94
Vocational Education	58.75	57.31	57.59	56.37	55.75	55.81	55.21	54.06	53.83	53.86
University degree	25.25	26.33	27.03	27.46	27.68	29.33	30.42	30.15	29.91	30.83
Economic sector										
Production	27.80	28.86	28.26	26.82	27.13	25.63	25.95	24.68	23.21	23.65
Trade, Transportation, Logistics	15.51	15.10	15.63	14.27	14.57	13.26	13.90	15.24	15.40	13.70
Services	17.07	16.07	17.37	18.28	17.90	18.15	18.03	17.88	19.66	18.31
Public Administration	31.33	30.66	30.58	32.11	31.96	33.07	32.88	32.98	34.26	35.55
Other	8.30	9.32	8.16	8.52	8.45	9.88	9.24	9.22	7.47	8.78
No. Of observations	10434	9096	9141	9412	11172	10226	9882	9942	10327	9729

Source: SOEP v36, population-weighted results, persons who have moved and minors excluded. Additionally, persons excluded who have missing values in the variable indicating the party the individual leans toward.

In comparison, without the restriction to individuals with a party preference, the share of women, of the youngest age group, of individuals living in Eastern Germany, of marginally employed, of individuals without a vocational degree or with vocational education and of individuals working in production or trade is slightly higher (see table A.2.3 in the appendix). In contrast, the unrestricted

sample contains a slightly smaller share of married individuals and individuals working in the services sector or in public administration. Larger differences between the two samples can be found for individuals aged 24 to 54, foreigners and individuals living with children. Their share is notably higher in the unrestricted sample (albeit the gap narrows over the period observed for individuals living with children). The share of individuals with a university degree is notably smaller in the unrestricted sample.

If the share of persons indicating that they lean towards a specific party changed substantially over the observation period (and if the changes differed between labour market regions), this would be a major threat to our analysis. However, we cannot detect a change neither in the share of individuals not leaning towards a specific party nor in the share of individuals interested in politics, as table A.2.4 in the appendix shows.

In the literature, party identification is seen as one of the most important predictors for individual voting behaviour (Johnston, 2006; Lewis-Beck et al. 2008). Arzheimer (2017) analyses the 2013 election in Germany (and previous ones) and finds that party identification is still a very strong predictor of voting intentions. The outcome variable "leaning towards a certain party" has a long tradition in opinion polls. In Germany, it has been asked regularly together with the "Sonntagsfrage"[7] from the 1970s on.

Estimation approach

Basic approach

First, we analyse whether the electorate's preferences for Social Democrats (SPD) have changed as a result of the introduction of the minimum wage. We expect that voters earning above the minimum wage may also change their party preferences. Hence, we cannot apply a classical Difference-in-Difference (DiD) setup with separation of treated and untreated individuals along the minimum wage threshold. This would crucially violate the SUTVA assumption, as spillover effects arise. Therefore, we identify the estimand by exploiting the regional variation in the bite of the minimum wage with a continuous DiD approach. The mechanism behind this idea is straightforward: The higher the minimum wage is in a given region compared to the median wage, the higher should be the effect of its introduction, i.e. the more are residents in that particular region exposed to the minimum wage.

To assess the regional variation in the bite of the minimum wage, we apply the Kaitz index which sets the minimum wage in relation to the median wage. We use the level of the minimum wage

[7] This question asks: "Which party would you vote for, if general elections took place next Sunday?".

at its introduction, which was 8.50 Euro. We use median wages in 2014 to obtain regional wage levels before the minimum wage intervention.[8]

$$Kaitz_r = \frac{minimum\ wage}{median\ wage_r}$$

In this way the Kaitz index is a continuous treatment variable measured separately for all 258 German labour market regions (*r*), which are designed to comprise regions with high shares of commuters but have low spatial dependency among each other (Kosfeld and Werner, 2012).[9]

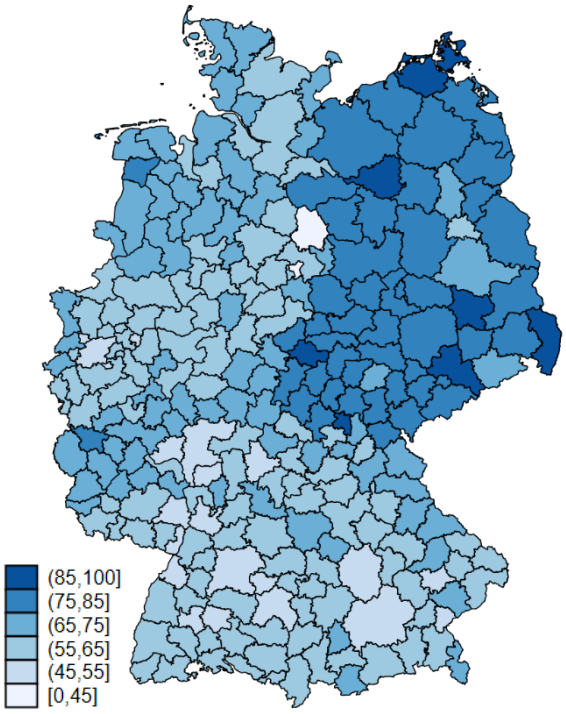
According to Caliendo et al. (2018), the Kaitz index is the most widely used indicator in the literature (see also Caliendo et al., 2018; Card, 1992; Dolton et al., 2015; United States Bureau of Labor Statistics, 1970). Moreover, the EU directive on adequate minimum wages uses the Kaitz index as a target value and recommends setting minimum wages at 60 per cent of the median wage (EU 2020/0310, 2020). Typically, the Kaitz index relates the minimum wage to the regional mean wage. However, the mean would be more sensitive to individual wage changes induced by the minimum wage itself (i.e. wage changes at the lower end of the wage distribution). Thus, a Kaitz index with the mean wage in the denominator would be more prone to bias from anticipation effects and spillover effects. Another possibility would be to use the share of employees earning less than the minimum wage. However, this indicator neglects the density below the minimum wage as it does not take into account the distance to this threshold. Hence, we use a Kaitz index that relates the minimum wage to the median wage in our main specification. Later in the robustness checks, we show results for alternative bite measures.

Figure 2.2 shows the variation of the Kaitz index among labour market regions. The minimum wage varies widely between 45 per cent and 88 per cent of the median wage with a standard deviation of 9.1. The highest bite is found in the (north) Eastern Germany, while the regions with the lowest bite are located in the southern regions. The former intra German border is still visible in the interregional wage distribution, i.e. median wages in Eastern German are lower. Therefore, we run all our analyses separately for Eastern and Western Germany.

[8] As the number of observations within one labour market region is too small in the SOEP, we use the structure of earnings survey by the German Federal Statistical Office to calculate the bite, see RDC of the Federal Statistical Office and Statistical Offices of the Länder (2020). The data is available for 2014 and 2018.

[9] Labour market regions (commuting zones) merge administrative districts with high shares of commuters. For a methodological explanation see Binder and Schwengler (2006).

Fig. 2.2 *Kaitz index (minimum wage/median wage) by labour market regions in 2014*



Source: Survey on the earnings structure 2014, population-weighted results.

In our analysis sample, the average number of survey respondents per local labour market region amounts to 39, with a minimum of 0 (which does not occur in more than 3 out of the 258 labour market regions per year) and a maximum of 489.

We exploit the regional variation in the minimum wage bite and apply the following DiD regression equation to estimate the effect of the minimum wage on the preference for the Social Democrats:

(1)
$$Y_{it} = \alpha_i + \beta_t + \sum_{t=2011, t\neq 2013}^{2019} \delta_t \cdot (t \cdot Kaitz_r) + \mu \cdot \boldsymbol{X}_{it} + \varepsilon_{it}$$

The effect of the minimum wage on individual *i's* preference for the SPD (*Y*) in year *t* is explained by the bite of the minimum wage (*Kaitz*) in region *r* (i.e. where individual *i* lives) interacted with a set of year dummies (reference year is 2013 to account for anticipation effects). The year dummies report the common development of the preference for the SPD on average over time, while the individual fixed effects (α_i) capture time invariant characteristics. The interaction term of Kaitz and time is the coefficient of interest. It measures whether individuals give a stronger policy feedback when their region is more affected by the minimum wage or anything else that might have changed party preferences over the observed time and differs regionally in terms of median income.

A set of time-varying control variables (***X***) enter the regression equation, such as age, having children aged under 16, a change in the achieved level of vocational education attained or job characteristics such as firm size, sector, occupation and whether the contract is temporary. As these job characteristics may themselves be influenced by the minimum wage introduction, we construct a control variable which captures the change in these characteristics. Standard errors are clustered at the level of labour market regions as treatment varies at this level (Bertrand et al., 2004). To test whether the assumption on common trends holds, we include placebo tests in our analysis and show raw time trends by different levels of the Kaitz index. We use survey weights in all our descriptions and regressions.

Identifying assumptions

For the identification of the estimand, we have to assume that the wage level in a labour market region and the level of the federal minimum wage are exogenously given for an individual, i.e. there is no endogenous change of the individual with respect to the treatment. Therefore, we exclude individuals from the sample who move to a different – e.g. less treated – region; otherwise the SUTVA assumption could be violated if individuals move due to the minimum wage introduction. The SUTVA assumption also requires that the division of the intervention and comparison groups (along the Kaitz index) is stable over time and that there are no spillover effects. For this reason, we decided to use labour market regions rather than counties to define the bite. Furthermore, parallel trends in the groups are necessary to identify the estimation coefficient. The individual fixed effects model also assumes that any unobserved individual heterogeneity correlated with the right-hand side of the equation is time-invariant. In addition, the standard OLS assumptions apply.

Finally, we have to assume that the government responsibility for the introduction of the minimum wage was salient to the electorate and that a large effect on the economy was expected. This is based on what is known from the literature on economic voting. According to Lewis-Beck and Stegmaier (2013), economic voting is conditioned by the clarity of the government's responsibility for economic policy. Moreover, unemployment and growth have the strongest effects on electoral outcomes. As the introduction of the minimum wage was the main election promise of the Social Democrats, we should be able to take the salience and clarity of government responsibility as given.[10] Before the minimum wage was introduced, there was an intensive debate about how this policy might harm the economy as a whole and lead to an increase in unemployment.[11] After its introduction, it

[10] Bruttel (2019) calls it the largest labour market reform since the early 2000s.

[11] The majority of the German Council of Economic Experts (Sachverständigenrat) expressed its scepticism about the introduction of the statutory minimum wage in its annual report 2014/2015, see Sachverständigenrat, (2014). Likewise, several research institutes, which jointly publish a biannual report and forecast of the economic situation in Germany, expressed their concerns about job losses due to the minimum wage in their spring report 2014, see Projektgruppe Gemeinschaftsdiagnose (2014).

turned out that many individuals did actually benefit, as they received higher hourly wages but at the same time reduced their total hours worked, leaving their monthly wages more or less unchanged (see Caliendo et al., 2022). Taken together, we expect the introduction of the minimum wage to have an effect on voters' preferences. However, this effect is not necessarily positive, especially for those whose expectations have been disappointed.

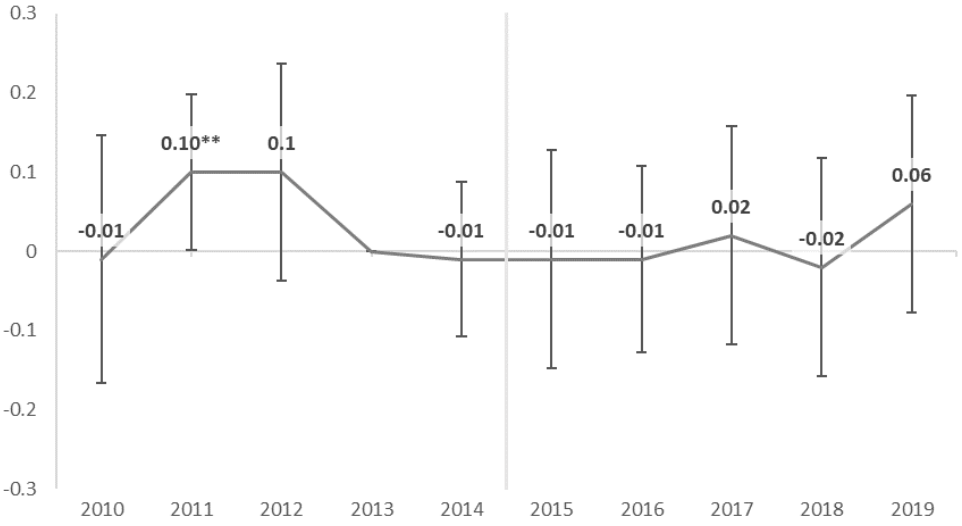
The lively debate about consequences of a minimum wage introductions was already running by the time the 2014 SOEP interviews were conducted (which is usually in the first two quarters of each year). Between the end of 2013 and mid-2014, all major economic research institutes in Germany published studies on the effects of a minimum wage (Arni et al. 2014, Bosch and Weinkopf 2014, Kluve 2013, Brenke and Müller 2013, Falck et al. 2013). The German Council of Economic Experts covered the topic extensively in its annual reports published in November 2013 and 2014 respectively (Sachverständigenrat 2013 and 2014). As results for 2014 may already be influenced by debate preceding the actual introduction of the minimum wage, we chose 2013 as the base year.

Results

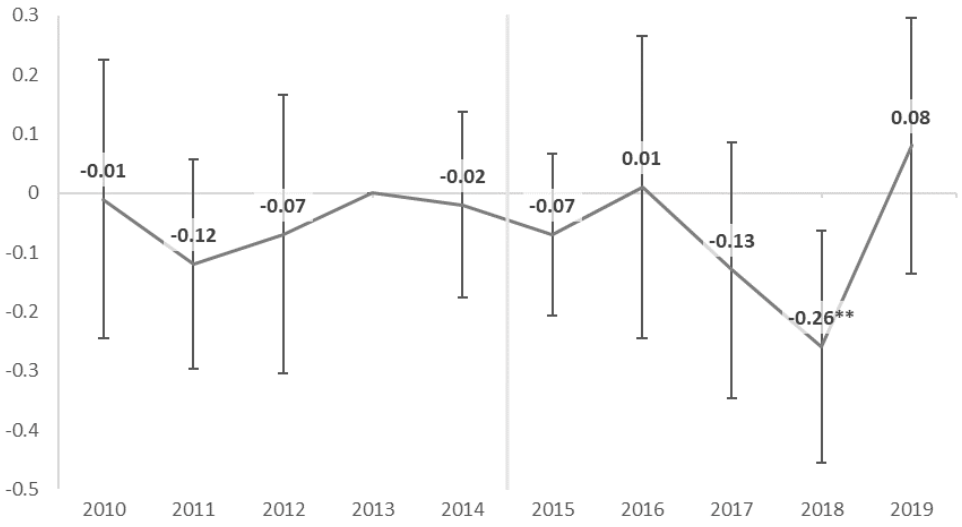
In this section, we apply the estimation strategy described above to examine whether the introduction of the minimum wage has affected preferences for the Social Democrats.

Our results show that the introduction of the minimum wage did not have a significant effect on the preference for the Social Democrats in more affected labour market regions – neither in Western Germany nor in Eastern Germany (see figure 2.3). This result does not vary significantly using control variables (see table A.2.5 a) and b) in the appendix).

Fig. 2.3 *Basic regression results – preference for Social Democrats*



(a) Western Germany



(b) Eastern Germany

*Source: SOEP.v36. – Notes: The dependent variable is the preference for Social Democrats. Individual fixed effects and weights are used. Base year is 2013. Robust standard errors clustered at the level of labour market regions, with * p<0.1, ** p<0.05, *** p<0.01.*

Our analysis relates party preferences to a policy implemented at the federal level. However, local state governments could also influence voters' preferences. In column (4) of table A.2.5a and table A.2.5b we add state-year level dummy variables for a particular party in power and for election years. The results are not significantly different from those without these dummies. Thus, the actions of state governments do not bias the change in party preferences.

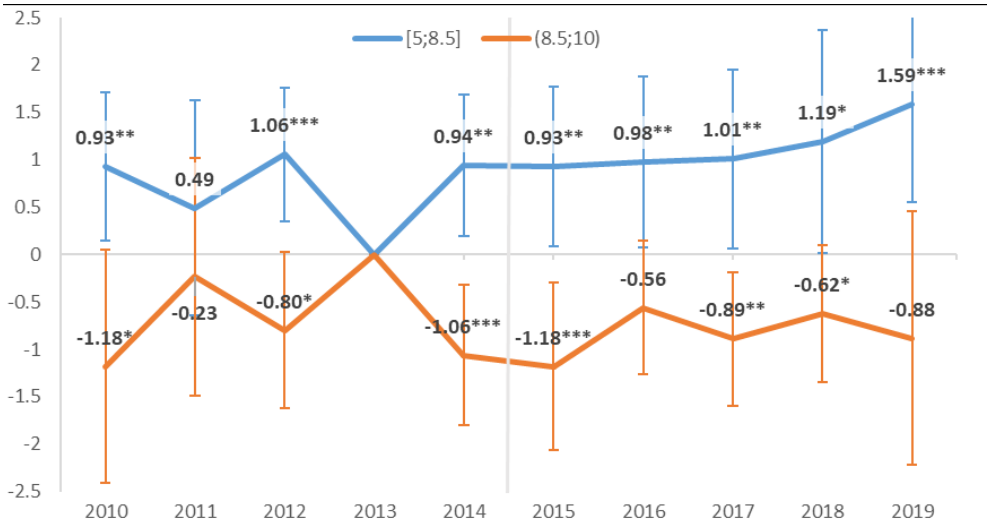
The years before the introduction of the minimum wage show no significance in the coefficients of the interaction term for Eastern Germany indicating that the pre trend assumption holds (see figure 2.3b). For Western Germany, the coefficients show no significance in two years before the treatment takes place. This can be seen as a simple placebo test and it is an additional indicator that

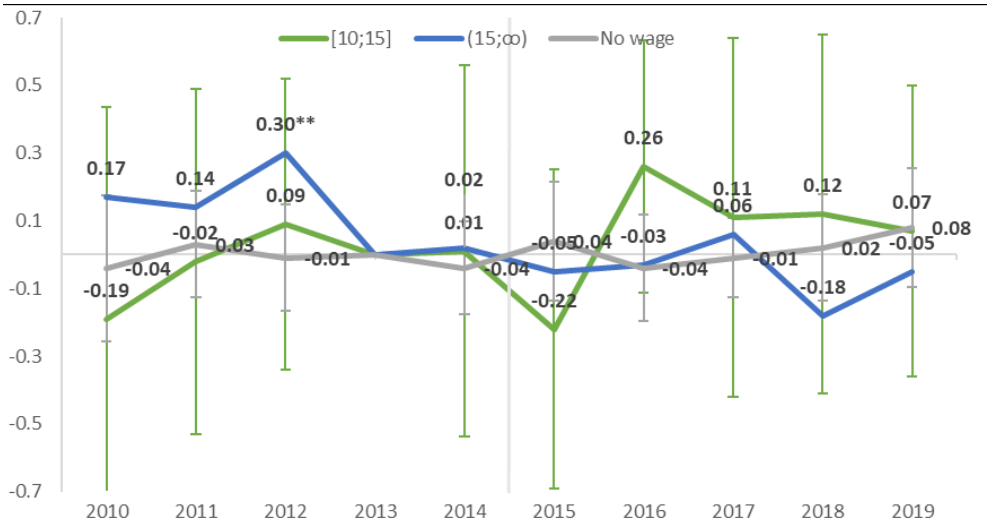
the common trend assumption holds. Furthermore, the implementation of the minimum wage act did not lead to an anticipation effect, i.e. the public debate about the minimum wage had no significant impact on party preferences for the Social Democrats.

The introduction of the minimum wage was the core electoral promise of the Social Democrats. Therefore, it may seem rather obvious to analyse the effect of this policy on preferences for the Social Democrats. But the minimum wage was actually introduced by a coalition of the Social Democrats and the conservative party (CDU/CSU). Moreover, if the consequences of the reform disappointed workers, we might also see changes in preferences for non-centrist parties (Pirates, left-wing and right-wing parties). So we test whether other parties may have benefitted instead. However, our analysis shows no effects of the minimum wage on the support for the conservative party, for non-centrist parties or for right-wing parties, neither in Western nor in Eastern Germany (see table A.2.6 in the appendix).

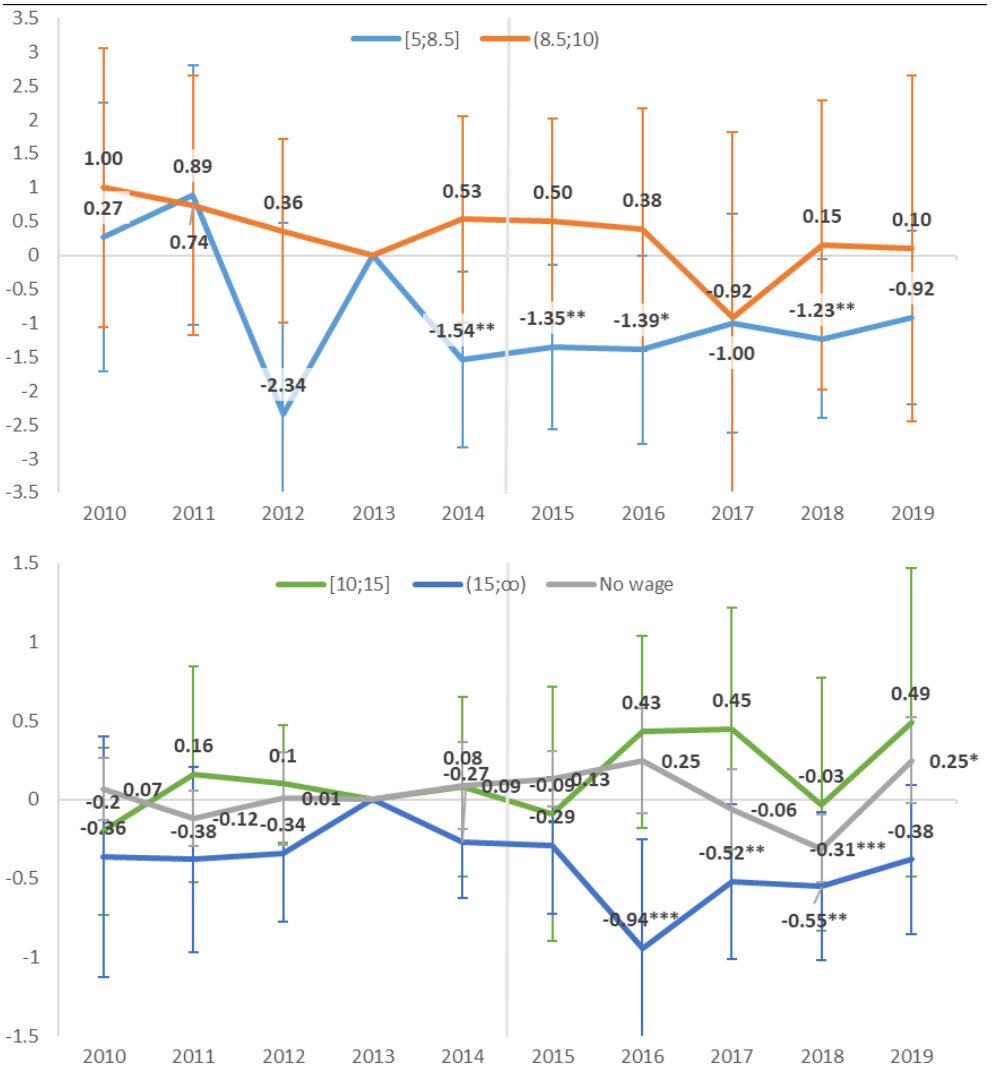
The possibly negative employment effects of a minimum wage introduction were lively debated in the first half of 2014 (i.e. when the SOEP interviews were conducted). Thus, individuals with more general political interest might have followed this debate more closely and in consequence might have reacted differently than individuals with less general political interest. We test this by dividing our sample into two different groups. One group contains all individuals who indicated that they are very interested or moderately interested in politics. The other group contains individuals who are not so interested or completely disinterested in politics. As table A.2.7 in the appendix shows, neither of the two groups exhibits a significant change in their preference for the Social Democrats. This holds for both Eastern and Western Germany.

Fig. 2.4 *Preference for Social Democrats by different wage groups*





(a) Western Germany



(b) Eastern Germany

Source: SOEP v36. – see figure 2.3 for notes.

The results so far do not show a positive policy feedback in regions with a higher bite, i.e. where the population should benefit most from the minimum wage. To analyse heterogeneous effects of the

minimum wage introduction on the preferences for the Social Democrats, we have run the regression on sub-sampled population by wage groups (see figure 2.4 and tables A.2.8 a) and b) in the appendix). We split the sample by hourly wages in 2014 and follow these individuals in subsequent years. We do not allow individuals to change wage groups afterwards, as the evolution of the wage could be an effect of the treatment itself. Thus, we only include individuals who are already present in the survey in 2014 and keep them in their 2014 wage group throughout the observation period. We use five wage groups: the population slightly below and above the minimum wage of 8.50 Euro, two further wage groups (far) above the minimum wage (10 to 15 Euro and more than 15 Euro) and one group representing the non-working population.

For Western Germany, we see increases in the support for the Social Democrats among individuals earning slightly below the minimum wage in 2014 (<=8.50 Euro/hour). Individuals earning slightly below the minimum wage in 2014 (>8.50 Euro/hour) are more likely to turn their back on the ruling parties in regions with higher bites (column 2 of table A.2.8a). However, as the coefficients are significant in several pre-reform years as well, we cannot attribute these effects to the introduction of the minimum wage. The pattern is different for Eastern Germany: We only see significant effects for individuals earning slightly below the minimum wage in 2014. Among them, the Social Democrats lost support in 2014, 2015 and 2016. On average, among individuals earning slightly below the minimum wage in 2014 in labour market regions with a bite that amounts a one standard deviation higher Kaitz index in Eastern Germany, preferences for the Social Democrats decreased by 12.2 per cent in 2015.[12] Among this group, the fear of future job loss might have been stronger than a (slight) wage increase.

Robustness Analysis

We now show that our main results hold for a variety of robustness checks. First, we test alternative bite measures. We then use general elections in 2018 as an alternative outcome. Next, we extend our sample to individuals without any party preference. Finally, we consider important events that also occurred in 2015 and could have affected our results.

Alternative definitions of the bite measure

The results may differ depending on the choice of the bite measure applied. A Kaitz index constructed on the basis of mean wages instead of median wages could also be used as well as the gap measure or the fraction of workers affected by the minimum wage, i.e. those earning less than 8.5 Euro.

The gap measure was introduced by Card and Krueger (1993) and recently used in Dustmann et al. (2020). It indicates the relative wage increase required in a region to bring workers' wages up to

[12] We derive this figure by multiplying the actual effect size for 2015 (1.35) by the standard deviation of the Kaitz index (9.1).

the minimum wage level. We apply the definition of the gap measure in Dustmann et al. (2022, p.309 ff.), but stick to labour market regions instead of NUTS-3 districts in order to have sufficient observations in each region. The gap measure, averaged across labour market regions equals 0.031 with a standard deviation of 0.021. Based on wages in 2014, one would expect an average wage increase of 3.1 per cent due to the minimum wage. The results of the regression using the gap measure show a significant decrease in the preferences for the Social Democrats in Western Germany in 2015, whereas we cannot find an effect in Eastern Germany. (see table A.2.9, column 1).

When using the Kaitz index with mean wages, or when using the share of people earning less than the minimum wage before its introduction, we do not find any significant effects (see table A.2.9).

Alternative outcomes

Every year following general elections, the SOEP asks participants which party they voted for. Germany held federal elections in 2013 and 2017. We do not find a significant difference in vote shares for the Social Democrats in regions that are more exposed to the minimum wage, neither in Western nor in Eastern Germany (see table A.2.10 in the appendix). Actual voting behaviour does not show any policy feedback after the introduction of the minimum wage.

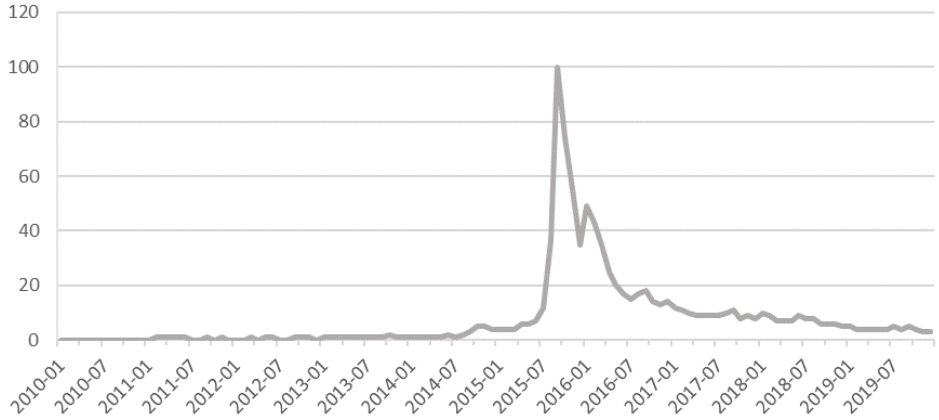
Inclusion of individuals without a party preference

In our main analysis, we excluded all individuals who do not lean towards any party at all. Another way to deal with these individuals is to count them as not leaning towards the party in question (e.g. the Social Democrats in the main analysis) and thus explicitly include them in the regressions. The inclusion of these individuals does not alter our main results, as can be seen from table A.2.11 in the appendix. We do not see any change in the preference for the Social Democrats in the year of the minimum wage introduction, neither in Eastern nor in Western Germany.

Other events potentially affecting the results

One important threat to our analysis is that the results are driven by any other policy measure, which we have disregarded. Introducing the minimum wage in 2015 was by far the largest and most visible reform enacted at that time (see Bruttel 2019). Other reforms or changes to contribution rates and benefits that took place in the same year are unlikely to have had such a regionally differentiated impact as the minimum wage did.

Fig. 2.5 *Search interest for "Flüchtlinge" (refugees) 2010-2019*



Source: Google, own figure.

One of the most influential events in Germany in 2015 was however an extraordinarily high inflow of refugees. Regions in the south-east of the country, close to the Czech border, were much more affected than other regions. However, the inflow of refugees took place mainly in the second half of 2015.[13]

An analysis of Google searches for the German term for refugees (i.e. “Flüchtlinge”) shows that the awareness of this issue has only increased in the second half of 2015 and was more or less stable before (figure 2.5). This might be taken as an indication that the massive increase in the inflow of refugees was not anticipated by the German population.[14] By the second half of 2015, most of the interviews for the SOEP had already been conducted. To rule out the influence of this event, we conduct the baseline regression excluding individuals interviewed in the second to fourth quarters of 2015. Table A.2.12 in the appendix shows that the basic result does not change – we do not find any significant effects.

However, voters may have shifted towards right-wing parties before the refugee crisis, especially in low-income regions, although there is no specific reason why this trend should have started in 2015. However, one indicator could be the xenophobic demonstrations in Leipzig and Dresden that started in the winter of 2014/15. Separating this shift in voting behaviour from the introduction of the minimum wage is far from easy. We approach this challenge by dividing the population according to their prejudices against foreigners. If our results are biased by the upcoming trend towards right-wing parties, we should see stronger results in the population with stronger prejudices against foreigners. The SOEP does not address prejudices against foreigners directly, but it surveys personality traits. Ekehammar and Akrami (2012) show a correlation between agreeableness

[13] Gallegos Torres (2022) shows that the number of arrivals massively increased after May 2015 and peaked in November of the same year.

[14] Furthermore, Sola (2018) shows that SOEP respondents interviewed after June exhibit an increase in concerns over migration.

and prejudices against foreigners. We use this result to test the following hypothesis: If less agreeable people are more likely to be prejudiced against foreigners, and if these sentiments are indeed driving the change in preferences for the Social Democrats in regions with a high minimum wage bite, our effect should be more pronounced among less agreeable individuals.

We use the Big Five questions contained in the SOEP to characterise the personality traits of individuals and repeat our estimation for two separate groups. In one group, the score for agreeableness is at least one standard deviation lower than for the average person, while the other group contains all the remaining people (see table A.2.13 in the appendix). Less agreeable persons (or those with more prejudices) do not show a significant response in their party preferences in regions with a higher bite in 2015 or later. This indicates that the xenophobic demonstrations or a general shift to the right does not affect our results, otherwise we should also see a negative effect within the less agreeable group.

Discussion and conclusion

We analyse the policy feedback associated with the introduction of the minimum wage in Germany. Our main result is that electoral preferences for Social Democrats do not show significantly positive effects using exogenous variation in the extent to which regions are affected by the minimum wage. This result is in line with what Huet-Vaughn (2023) recently showed for the U.S. We can only detect a tendency to give negative policy feedback in the year following the introduction of the minimum wage among workers who earn less than the minimum wage in Eastern Germany. Our robustness checks suggest that an emerging right-wing extremism associated with refugee migration in 2015 does not drive our results. Our analysis is based on the precondition that the policy feedback on the minimum wage differs according to the regional median income level (bite). If the policy feedback were homogenous across Germany, our estimation strategy could not detect any effects. However, based on previous literature, this scenario is rather implausible.

In 2022, the minimum wage was increased substantially to 12 Euro. This increase was also proposed by the Social Democrats. Having in mind the results presented for the introduction of the minimum wage, the question arises whether the Social Democrats were able to gain support this time. The minimum wage increase to 12 Euro might have saved Olaf Scholz's election victory in 2021, as this was his major pledge. However, data to analyse this hypothesis only became available recently. For now, this interesting question must remain subject to further analysis. An analysis of the 2021 federal elections and the minimum wage increase in 2022 would also permit to focus on turnout rather than on party preferences – which was not possible in our analysis.

Despite these limitations, we conclude that the intention of the Social Democrats to regain lost credibility with the electorate has not been fulfilled. A policy such as the introduction of a minimum

wage is likely to be an appropriate means of generating positive policy feedback only if the extent to which the electorate benefits is large enough. Furthermore, gaining increased support through election pledges before an election might be a totally different story than being rewarded for enacting the policy in question once having governmental responsibility.

Table A.2.3 *Composition of unrestricted sample (share)*

	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019
Share in %										
Female	51.49	51.64	51.52	51.55	51.22	51.17	51.00	50.95	50.90	51.39
Male	48.51	48.36	48.48	48.45	48.78	48.83	49.00	49.05	49.10	48.61
Age										
24 and younger	9.59	9.08	8.94	8.42	8.64	8.32	8.79	8.73	8.63	7.41
25 - 54	49.87	49.85	49.56	48.99	48.78	48.52	48.80	48.17	47.19	47.51
55 and older	40.54	41.08	41.50	42.59	42.56	43.13	42.41	43.10	44.18	45.08
Place of residence										
East	18.27	18.57	17.95	18.31	17.86	17.88	17.70	17.43	17.23	17.11
West	81.73	81.43	82.05	81.69	82.14	82.12	82.30	82.57	82.77	82.89
Citizenship										
Foreign	8.41	7.84	8.17	7.77	9.12	9.15	11.08	11.86	12.20	12.71
German	91.59	92.16	91.83	92.23	90.86	90.82	88.92	88.14	87.80	87.29
Type of employment										
Fulltime	84.28	84.81	84.89	84.87	83.57	83.49	84.02	82.78	82.40	82.83
Part-time	15.72	15.19	15.11	15.13	16.43	16.51	15.98	17.22	17.60	17.17
Marginal	8.49	8.37	8.75	8.99	9.41	8.43	8.92	8.24	8.05	7.33
Household characteristics										
Married	54.89	54.71	55.12	54.51	54.02	52.98	52.77	52.55	52.92	53.87
Living with children age 16 and younger	20.03	20.73	20.41	19.72	20.11	20.00	20.34	20.18	20.21	20.80
Education										
No vocational education	18.11	18.37	18.51	18.69	19.16	18.80	19.42	19.95	19.19	18.31
Vocational Education	60.39	59.75	59.40	58.92	57.37	56.99	56.07	55.15	54.82	55.03
University degree	19.18	19.56	19.97	20.46	21.32	21.85	22.25	22.64	22.98	23.45
Economic sector										
Production	29.85	30.43	30.05	29.06	28.63	28.06	27.83	26.81	26.80	25.59
Trade, Transportation, Logistics	18.05	17.70	17.89	17.22	17.02	17.02	17.31	17.46	17.21	16.76
Services	14.41	14.21	14.79	15.45	15.57	15.50	15.39	16.11	17.11	17.06
Public Administration	28.76	27.97	28.14	28.66	29.25	29.41	30.10	29.94	30.70	31.80
Other	8.93	9.69	9.13	9.61	9.54	10.02	9.38	9.68	8.18	8.79
No. Of observations	25511	26070	27055	27042	27756	27889	28313	27856	28092	26903

Source: SOEP v36, population-weighted results, persons who have moved and minors excluded. With persons who have missing values in the variable indicating the party they lean toward.

Table A.2.4 *Changes in share of individuals not leaning towards a given party or changes in interest for politics*

	1	2	3	4
	West		East	
	Not leaning towards a party	Interested in politics	Not leaning towards a	Interested in politics
Constant	0.56***	0.36***	0.67***	0.31***
	(0.02)	(0.01)	(0.02)	(0.01)
Year dummies				
2010	-0.14**	0.00	0.07	0.02
	(0.06)	(0.05)	(0.12)	(0.10)
2011	-0.07	0.05	0.07	0.04
	(0.05)	(0.05)	(0.12)	(0.10)
2012	-0.01	-0.02	0.01	0.02
	(0.06)	(0.03)	(0.13)	(0.11)
2014	-0.07*	0.01	-0.01	0.15**
	(0.04)	(0.04)	(0.09)	(0.06)
2015	0.00	-0.02	0.04	0.14*
	(0.05)	(0.03)	(0.10)	(0.08)
2016	0.00	0.03	0.20**	0.21**
	(0.04)	(0.04)	(0.09)	(0.09)
2017	0.04	0.05	0.12*	0.19**
	(0.05)	(0.04)	(0.07)	(0.08)
2018	0.03	0.00	0.15	0.22*
	(0.05)	(0.03)	(0.10)	(0.13)
2019	-0.02	0.03	0.11	0.25**
	(0.07)	(0.04)	(0.11)	(0.10)
Year dummies x Kaitz				
2010	0.18*	0.05	-0.15	0.04
	(0.10)	(0.09)	(0.15)	(0.13)
2011	0.13	-0.04	-0.07	0.01
	(0.09)	(0.08)	(0.15)	(0.13)
2012	0.04	0.06	0.01	0.01
	(0.10)	(0.05)	(0.16)	(0.14)
2014	0.06	0.06	-0.04	-0.14*
	(0.07)	(0.06)	(0.12)	(0.08)
2015	0.02	0.05	-0.06	-0.14
	(0.08)	(0.05)	(0.13)	(0.10)
2016	0.18*	0.02	-0.25**	-0.21*
	(0.07)	(0.06)	(0.12)	(0.12)
2017	-0.04	-0.01	-0.16*	-0.17
	(0.08)	(0.06)	(0.09)	(0.10)
2018	-0.05	0.07	-0.21*	-0.23
	(0.08)	(0.06)	(0.12)	(0.16)
2019	0.01	0.02	-0.16	-0.25*
	(0.12)	(0.06)	(0.14)	(0.13)
Socio-demographics	Yes	Yes	Yes	Yes
Change in job	Yes	Yes	Yes	Yes
Politics on state level	Yes	Yes	Yes	Yes
Individuals	816	12629	172	2689
Observations	4451	62076	923	13605
Within R^2	0.02	0.01	0.06	0.01

*Source: SOEP.v36. – Notes: Individual fixed effects and weights used. Dependent variable: preference for Social Democrats. Base year 2013. Robust standard errors clustered at the level of labour market regions in parentheses, with * p<0.1, ** p<0.05, *** p<0.01.*

Table A.2.5 *for figure 2.3: Basic regression analysis – preference for the Social Democrats*

a) Western Germany

	1	2	3	4
Constant	0.31***	0.33***	0.33***	0.31***
	(0.00)	(0.01)	(0.01)	(0.01)
Year dummies				
2010	0.01	0.01	0.01	0.01
	(0.06)	(0.05)	(0.05)	(0.05)
2011	-0.05*	-0.06*	-0.06*	-0.06**
	(0.03)	(0.03)	(0.03)	(0.03)
2012	-0.05	-0.05	-0.05	-0.05
	(0.05)	(0.05)	(0.05)	(0.05)
2014	0.01	0.01	0.01	0.01
	(0.03)	(0.03)	(0.03)	(0.03)
2015	0.02	0.01	0.01	0.02
	(0.04)	(0.04)	(0.04)	(0.04)
2016	-0.01	-0.02	-0.02	0.00
	(0.03)	(0.04)	(0.04)	(0.04)
2017	-0.01	-0.02	-0.02	0.00
	(0.04)	(0.04)	(0.04)	(0.04)
2018	-0.01	-0.02	-0.02	0.00
	(0.04)	(0.04)	(0.04)	(0.04)
2019	-0.09**	-0.10**	-0.10**	-0.08**
	(0.04)	(0.04)	(0.04)	(0.04)
Year dummies x Kaitz				
2010	-0.02	-0.01	-0.01	-0.01
	(0.09)	(0.09)	(0.09)	(0.08)
2011	0.08	0.09*	0.09*	0.10**
	(0.05)	(0.05)	(0.05)	(0.05)
2012	0.08	0.09	0.09	0.10
	(0.08)	(0.08)	(0.08)	(0.07)
2014	0.00	-0.01	-0.01	-0.01
	(0.05)	(0.05)	(0.05)	(0.05)
2015	-0.01	-0.01	-0.01	-0.01
	(0.06)	(0.07)	(0.07)	(0.07)
2016	0.01	0.01	0.01	-0.01
	(0.06)	(0.06)	(0.06)	(0.06)
2017	0.03	0.05	0.05	0.02
	(0.07)	(0.07)	(0.07)	(0.07)
2018	0.00	0.01	0.01	-0.02
	(0.07)	(0.07)	(0.07)	(0.07)
2019	0.07	0.09	0.09	0.06
	(0.07)	(0.07)	(0.07)	(0.07)
Socio-demographics	No	Yes	Yes	Yes
Change in job	No	No	Yes	Yes
Politics on state level	No	No	No	Yes
Individuals	14452	14404	14404	14404
Observations	68887	68650	68650	68650
Within R^2	0.01	0.01	0.01	0.01

*Source: SOEP.v36. – See table A.2.4 for notes. Robust standard errors clustered at the level of labour market regions in parentheses, with * p<0.1, ** p<0.05, *** p<0.01.*

b) Eastern Germany

	1	2	3	4
Constant	0.24***	0.21***	0.21***	0.19***
	(0.00)	(0.03)	(0.03)	(0.03)
Year dummies				
2010	-0.04	-0.02	-0.01	-0.01
	(0.09)	(0.09)	(0.09)	(0.09)
2011	0.08	0.09	0.09	0.08
	(0.07)	(0.07)	(0.07)	(0.07)
2012	0.04	0.06	0.06	0.06
	(0.08)	(0.09)	(0.09)	(0.09)
2014	-0.03	-0.02	-0.02	0.00
	(0.06)	(0.06)	(0.06)	(0.06)
2015	0.02	0.01	0.01	0.03
	(0.05)	(0.05)	(0.05)	(0.05)
2016	-0.07	-0.07	-0.07	-0.05
	(0.09)	(0.09)	(0.09)	(0.10)
2017	0.07	0.06	0.06	0.10
	(0.07)	(0.07)	(0.07)	(0.09)
2018	0.14**	0.13**	0.13**	0.16**
	(0.06)	(0.06)	(0.06)	(0.07)
2019	-0.15**	-0.16**	-0.16**	-0.13
	(0.07)	(0.07)	(0.07)	(0.08)
Year dummies x Kaitz index				
2010	0.03	-0.01	-0.01	-0.01
	(0.12)	(0.12)	(0.12)	(0.12)
2011	-0.12	-0.12	-0.12	-0.12
	(0.09)	(0.09)	(0.09)	(0.09)
2012	-0.04	-0.07	-0.07	-0.07
	(0.11)	(0.11)	(0.12)	(0.12)
2014	0.02	0.01	0.01	-0.02
	(0.07)	(0.07)	(0.07)	(0.08)
2015	-0.04	-0.04	-0.04	-0.07
	(0.07)	(0.07)	(0.07)	(0.07)
2016	0.04	0.05	0.05	0.01
	(0.12)	(0.12)	(0.12)	(0.13)
2017	-0.09	-0.08	-0.08	-0.13
	(0.10)	(0.10)	(0.10)	(0.11)
2018	-0.22**	-0.22**	-0.21**	-0.26**
	(0.08)	(0.08)	(0.08)	(0.10)
2019	0.13	0.13	0.13	0.08
	(0.10)	(0.10)	(0.10)	(0.11)
Socio-demographics	No	Yes	Yes	Yes
Change in job characteristics	No	No	Yes	Yes
Politics on state level	No	No	No	Yes
Individuals	3070	3059	3059	3059
Observations	15011	14963	14963	14963
Within R^2	0.01	0.01	0.01	0.01

*Source: SOEP.v36. – See table A.2.4 for notes. Robust standard errors clustered at the level of labour market regions in parentheses, with * p<0.1, ** p<0.05, *** p<0.01.*

Table A.2.6 *Basic regression analysis – preference for other parties*

	1	2	3	4	5	6
		West			East	
	CDU	Non-centrist	Right-wing	CDU	Non-centrist	Right-wing
Constant	0.39***	0.04***	0.01	0.41***	0.30***	0.02
	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)	(0.02)
Year dummies						
2010	-0.05	-0.01	0.01	-0.04	0.20*	0.03
	(0.05)	(0.03)	(0.01)	(0.08)	(0.11)	(0.06)
2011	-0.02	-0.02	0.03**	-0.13***	0.12**	0.06
	(0.04)	(0.02)	(0.01)	(0.04)	(0.05)	(0.06)
2012	0.01	0.03	0.03***	0.00	0.06	0.04
	(0.03)	(0.02)	(0.01)	(0.06)	(0.06)	(0.04)
2014	-0.04	0.03	0.01	-0.07	0.19*	0.03
	(0.03)	(0.02)	(0.02)	(0.07)	(0.09)	(0.04)
2015	0.05	0.02	0.01	0.00	0.13*	0.05
	(0.04)	(0.02)	(0.02)	(0.06)	(0.07)	(0.11)
2016	0.01	0.04	0.04**	0.04	0.21**	-0.01
	(0.03)	(0.03)	(0.02)	(0.08)	(0.09)	(0.10)
2017	0.00	0.04	0.04	0.05	0.11	0.09
	(0.04)	(0.03)	(0.02)	(0.09)	(0.09)	(0.08)
2018	-0.01	0.07**	0.05*	-0.04	0.01	-0.03
	(0.05)	(0.03)	(0.03)	(0.10)	(0.10)	(0.11)
2019	-0.06	0.03	0.03	-0.01	0.14	-0.02
	(0.05)	(0.03)	(0.03)	(0.09)	(0.11)	(0.10)
Year dummies x Kaitz index						
2010	0.05	0.04	-0.02	0.03	-0.23	-0.04
	(0.08)	(0.05)	(0.03)	(0.10)	(0.14)	(0.08)
2011	0.01	0.03	-0.05**	0.16***	-0.14**	-0.06
	(0.07)	(0.04)	(0.02)	(0.05)	(0.07)	(0.07)
2012	-0.03	-0.02	-0.05***	0.00	-0.07	-0.05
	(0.04)	(0.04)	(0.02)	(0.08)	(0.08)	(0.05)
2014	0.06	-0.02	-0.01	0.11	-0.23*	-0.04
	(0.05)	(0.04)	(0.03)	(0.09)	(0.12)	(0.06)
2015	-0.09	0.01	0.00	0.02	-0.12	-0.04
	(0.07)	(0.04)	(0.03)	(0.08)	(0.09)	(0.14)
2016	-0.02	-0.02	-0.03	-0.04	-0.21*	0.07
	(0.05)	(0.05)	(0.04)	(0.10)	(0.12)	(0.13)
2017	0.00	-0.03	-0.03	-0.07	-0.11	-0.07
	(0.07)	(0.05)	(0.04)	(0.11)	(0.11)	(0.11)
2018	-0.01	-0.07	-0.04	0.04	0.05	0.10
	(0.08)	(0.06)	(0.04)	(0.13)	(0.13)	(0.14)
2019	0.04	0.00	0.00	-0.01	-0.12	0.08
	(0.08)	(0.06)	(0.04)	(0.12)	(0.15)	(0.13)
Socio-demographics	Yes	Yes	Yes	Yes	Yes	Yes
Change in job characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Politics on state level	Yes	Yes	Yes	Yes	Yes	Yes
Individuals	14404	14404	14404	3059	3059	3059
Observations	68650	68650	68650	14963	14963	14963
Within R^2	0.01	0.01	0.01	0.01	0.01	0.03

*Source: SOEP.v36. – see table A.2.4 for notes. Robust standard errors clustered at the level of labour market regions in parentheses, with * p<0.1, ** p<0.05, *** p<0.01.*

Table A.2.7 *Basic regression analysis – differentiated by strong and weak interest for politics*

	1	2	3	4
	West		East	
	Interest in politics			
	Strong	Weak	Strong	Weak
Constant	0.30***	0.32***	0.23***	0.16***
	(0.02)	(0.02)	(0.05)	(0.03)
Year dummies				
2010	0.04	-0.02	-0.03	-0.01
	(0.06)	(0.07)	(0.11)	(0.14)
2011	-0.04	-0.08*	0.11	(0.07)
	(0.04)	(0.05)	(0.10)	(0.12)
2012	-0.04	-0.07	0.04	0.01
	(0.06)	(0.06)	(0.09)	(0.21)
2014	0.03	-0.04	-0.01	-0.06
	(0.03)	(0.06)	(0.10)	(0.11)
2015	0.02	-0.01	0.09	(0.14)
	(0.04)	(0.07)	(0.06)	(0.12)
2016	0.00	(0.02)	(0.11)	0.02
	(0.04)	(0.06)	(0.13)	(0.16)
2017	0.00	0.03	0.08	0.04
	(0.05)	(0.07)	(0.09)	(0.22)
2018	0.00	0.01	0.14	0.14
	(0.05)	(0.09)	(0.09)	(0.14)
2019	-0.07	-0.11	-0.13*	-0.23
	(0.05)	(0.08)	(0.07)	(0.19)
Year dummies x Kaitz index				
2010	-0.05	0.03	0.02	-0.01
	(0.10)	(0.12)	(0.15)	(0.18)
2011	0.07	0.12*	-0.16	0.07
	(0.06)	(0.07)	(0.14)	(0.16)
2012	0.08	0.11	-0.05	0.00
	(0.09)	(0.09)	(0.13)	(0.27)
2014	-0.03	0.08	0.00	0.05
	(0.06)	(0.10)	(0.14)	(0.14)
2015	-0.01	0.03	-0.14	0.14
	(0.07)	(0.11)	(0.09)	(0.16)
2016	-0.05	0.03	0.10	-0.09
	(0.07)	(0.10)	(0.17)	(0.21)
2017	0.03	-0.04	-0.12	-0.07
	(0.09)	(0.12)	(0.12)	(0.28)
2018	-0.01	-0.06	-0.23*	-0.23
	(0.08)	(0.15)	(0.12)	(0.18)
2019	0.03	0.11	0.10	0.19
	(0.09)	(0.13)	(0.10)	(0.25)
Socio-demographics	Yes	Yes	Yes	Yes
Change in job characteristics	Yes	Yes	Yes	Yes
Politics on state level	Yes	Yes	Yes	Yes
Individuals	8567	6628	172	2689
Observations	39156	25459	923	13605
Within R^2	0.01	0.01	0.06	0.01

*Source: SOEP.v36. – Notes: See table A.2.4 for notes. Robust standard errors clustered at the level of labour market regions in parentheses, with * p<0.1, ** p<0.05, *** p<0.01.*

Table A.2.8 *Preference for Social Democrats by different wage groups*

a) Western Germany

	1 [5;8.5]	2 (8.5;10)	3 [10;15]	4 (15;∞)	5 No wage
Constant	0.35***	0.25**	0.27***	0.33***	0.29***
	(0.07)	(0.10)	(0.04)	(0.03)	(0.02)
Year dummies					
2010	-0.55**	0.82**	0.1	-0.09	0.02
	(0.25)	(0.37)	(0.20)	(0.10)	(0.07)
2011	-0.31	0.19	0.01	-0.08	-0.02
	(0.35)	(0.39)	(0.16)	(0.07)	(0.05)
2012	-0.66***	0.54**	-0.06	-0.17**	0.01
	(0.23)	(0.27)	(0.14)	(0.08)	(0.05)
2014	-0.55**	0.70***	0.00	0.01	0.03
	(0.23)	(0.24)	(0.17)	(0.07)	(0.04)
2015	-0.53**	0.77***	0.14	0.05	-0.02
	(0.27)	(0.28)	(0.15)	(0.08)	(0.06)
2016	-0.60**	0.38*	-0.16	0.02	0.01
	(0.29)	(0.23)	(0.12)	(0.08)	(0.05)
2017	-0.59**	0.66***	-0.07	-0.01	0.01
	(0.30)	(0.23)	(0.17)	(0.08)	(0.04)
2018	-0.70*	0.41*	-0.10	0.10	-0.03
	(0.38)	(0.23)	(0.16)	(0.08)	(0.05)
2019	-1.03***	0.55	-0.11	-0.02	-0.09*
	(0.33)	(0.42)	(0.13)	(0.08)	(0.05)
Year dummies x Kaitz index					
2010	0.93**	-1.18*	-0.19	0.17	-0.04
	(0.40)	(0.63)	(0.32)	(0.17)	(0.11)
2011	0.49	-0.23	-0.02	0.14	0.03
	(0.58)	(0.64)	(0.26)	(0.11)	(0.08)
2012	1.06***	-0.80*	0.09	0.30**	-0.01
	(0.36)	(0.42)	(0.22)	(0.13)	(0.08)
2014	0.94**	-1.06***	0.01	0.02	-0.04
	(0.38)	(0.38)	(0.28)	(0.11)	(0.07)
2015	0.93**	-1.18***	-0.22	-0.05	0.04
	(0.43)	(0.45)	(0.24)	(0.13)	(0.09)
2016	0.98**	-0.56	0.26	-0.03	-0.04
	(0.46)	(0.36)	(0.19)	(0.13)	(0.08)
2017	1.01**	-0.89**	0.11	0.06	-0.01
	(0.48)	(0.36)	(0.27)	(0.14)	(0.06)
2018	1.19*	-0.62*	0.12	-0.18	0.02
	(0.60)	(0.37)	(0.27)	(0.14)	(0.08)
2019	1.59***	-0.88	0.07	-0.05	0.08
	(0.53)	(0.68)	(0.22)	(0.13)	(0.09)
Socio-demographics	Yes	Yes	Yes	Yes	Yes
Change in job characteristics	Yes	Yes	Yes	Yes	Yes
Politics on state level	Yes	Yes	Yes	Yes	Yes
Individuals	368	246	1056	3136	5751
Observations	1683	1031	5091	17562	33034
Within R^2	0.05	0.06	0.02	0.02	0.01

Source: SOEP.v36. – see table A.2.4 for notes. Wage groups defined in 2014. No wage: individuals not working in 2014 or with missing wages.

b) Eastern Germany

	1 [5;8.5]	2 (8.5;10)	3 [10;15]	4 (15;∞)	5 No wage
Constant	0.02	0.39***	0.27***	0.19***	0.16***
	(0.07)	(0.09)	(0.05)	(0.03)	(0.04)
Year dummies					
2010	-0.20	-0.86	0.14	0.25	-0.07
	(0.80)	(0.80)	(0.21)	(0.29)	(0.08)
2011	-0.67	-0.74	-0.15	0.28	0.09
	(0.74)	(0.76)	(0.27)	(0.22)	(0.07)
2012	1.89	-0.35	-0.07	0.26	0.00
	(1.12)	(0.53)	(0.14)	(0.16)	(0.12)
2014	1.17**	-0.49	-0.06	0.19	-0.08
	(0.51)	(0.60)	(0.22)	(0.13)	(0.11)
2015	0.97**	-0.45	0.08	0.20	-0.12*
	(0.47)	(0.59)	(0.31)	(0.16)	(0.06)
2016	1.02*	-0.37	-0.35	0.67**	-0.23*
	(0.56)	(0.71)	(0.24)	(0.25)	(0.13)
2017	0.75	0.69	-0.34	0.38*	0.04
	(0.64)	(1.09)	(0.30)	(0.19)	(0.10)
2018	0.85*	-0.18	0.00	0.36**	0.22**
	(0.48)	(0.84)	(0.32)	(0.18)	(0.09)
2019	0.62	-0.14	-0.44	0.20	-0.26**
	(0.53)	(0.98)	(0.39)	(0.18)	(0.10)
Year dummies x Kaitz index					
2010	0.27	1.00	-0.20	-0.36	0.07
	(1.01)	(1.05)	(0.27)	(0.39)	(0.10)
2011	0.89	0.74	0.16	-0.38	-0.12
	(0.98)	(0.98)	(0.35)	(0.30)	(0.09)
2012	-2.34	0.36	0.10	-0.34	0.01
	(1.44)	(0.69)	(0.19)	(0.22)	(0.15)
2014	-1.54**	0.53	0.08	-0.27	0.09
	(0.66)	(0.78)	(0.29)	(0.18)	(0.14)
2015	-1.35**	0.50	-0.09	-0.29	0.13
	(0.62)	(0.77)	(0.41)	(0.22)	(0.09)
2016	-1.39*	0.38	0.43	-0.94***	0.25
	(0.71)	(0.91)	(0.31)	(0.35)	(0.17)
2017	-1.00	-0.92	0.45	-0.52**	-0.06
	(0.82)	(1.40)	(0.39)	(0.25)	(0.13)
2018	-1.23**	0.15	-0.03	-0.55**	-0.31***
	(0.60)	(1.09)	(0.41)	(0.24)	(0.11)
2019	-0.92	0.10	0.49	-0.38	0.25*
	(0.65)	(1.30)	(0.50)	(0.24)	(0.14)
Socio-demographics	Yes	Yes	Yes	Yes	Yes
Change in job characteristics	Yes	Yes	Yes	Yes	Yes
Politics on state level	Yes	Yes	Yes	Yes	Yes
Individuals	118	104	274	499	1336
Observations	575	493	1410	2847	7754
Within R^2	0.11	0.10	0.03	0.03	0.02

Source: SOEP.v36. – see table A.2.4 for notes. Wage groups defined in 2014. No wage: individuals not working in 2014 or with missing wages.

Table A.2.9 *Robustness: Different bite measures*

	1	2	3	4	5	6
		West			East	
	Rel. wage gap to MW	Kaitz mean wage	Share affected by MW	Rel. wage gap to MW	Kaitz mean wage	Share affected by MW
Constant	0.31***	0.31***	0.31***	0.19***	0.19***	0.19***
	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)	(0.03)
Year dummies						
2010	0.01*	0.00	0.00	-0.03**	-0.03	-0.03
	(0.01)	(0.04)	(0.02)	(0.01)	(0.06)	(0.03)
2011	0.00	-0.05***	-0.02**	(0.02)	0.02	0.00
	(0.01)	(0.02)	(0.01)	(0.01)	(0.06)	(0.04)
2012	0.01	-0.04	-0.03*	(0.02)	0.04	0.00
	(0.01)	(0.03)	(0.01)	(0.02)	(0.07)	(0.05)
2014	0.02***	0.02	0.01	-0.03**	(0.02)	(0.04)
	(0.01)	(0.03)	(0.01)	(0.01)	(0.05)	(0.04)
2015	0.02***	0.02	0.02	-0.03**	(0.03)	(0.02)
	(0.01)	(0.03)	(0.01)	(0.01)	(0.05)	(0.03)
2016	0.00	0.01	0.01	-0.06***	(0.11)	-0.06
	(0.01)	(0.03)	(0.01)	(0.02)	(0.10)	(0.05)
2017	0.02***	0.01	0.02	(0.03)	0.05	0.01
	(0.01)	(0.03)	(0.01)	(0.02)	(0.08)	(0.05)
2018	0.00	0.01	-0.01	-0.02	0.09	0.03
	(0.01)	(0.03)	(0.01)	(0.02)	(0.07)	(0.05)
2019	-0.04***	-0.07**	-0.06***	-0.06***	-0.11*	-0.09**
	(0.01)	(0.03)	(0.02)	(0.01)	(0.05)	(0.04)
Year dummies x Bite						
2010	-0.50**	0.00	0.01	0.14	0.01	0.04
	(0.24)	(0.07)	(0.15)	(0.20)	(0.09)	(0.12)
2011	-0.26	0.10**	0.19*	0.24	-0.05	-0.05
	(0.22)	(0.04)	(0.10)	(0.26)	(0.09)	(0.15)
2012	-0.18	0.08	0.29**	0.42	-0.05	0.02
	(0.30)	(0.07)	(0.13)	(0.32)	(0.11)	(0.19)
2014	-0.30	-0.01	-0.01	0.17	0.01	0.08
	(0.22)	(0.05)	(0.10)	(0.19)	(0.08)	(0.15)
2015	-0.57**	-0.03	-0.06	0.07	0.00	-0.03
	(0.25)	(0.06)	(0.11)	(0.21)	(0.08)	(0.11)
2016	-0.31	-0.03	-0.14	0.37	0.10	0.10
	(0.33)	(0.05)	(0.11)	(0.34)	(0.14)	(0.21)
2017	-0.45	0.01	-0.03	0.47	-0.08	-0.08
	(0.32)	(0.06)	(0.12)	(0.31)	(0.12)	(0.19)
2018	-0.64***	-0.04	-0.05	-0.38	-0.19*	-0.26
	(0.23)	(0.06)	(0.14)	(0.32)	(0.11)	(0.19)
2019	-0.34	0.05	0.09	-0.18	0.06	0.10
	(0.30)	(0.06)	(0.15)	(0.28)	(0.09)	(0.16)
Socio-demographics	Yes	Yes	Yes	Yes	Yes	Yes
Change in job characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Politics on state level	Yes	Yes	Yes	Yes	Yes	Yes
Individuals	14404	14404	14404	3059	3059	3059
Observations	68650	68650	68650	14963	14963	14963
Within R^2	0.01	0.01	0.01	0.01	0.01	0.01

*Source: SOEP.v36. – Notes: Different regional bite measures used: (1) Average wage increase necessary to lift workers' wages in a given labour market region to the MW level in 2014. (2) Kaitz index using the mean wage instead of the median wage. (3) Share of individuals earning less than the MW in a labour market region in 2014. See table A.2.4 for more notes. Robust standard errors in parentheses, with * p<0.1, ** p<0.05, *** p<0.01.*

Table A.2.10 *Robustness: Voting between 2014 and 2018*

	1 **West** **SPD**	**2** **East** **SPD**
Constant	0.21***	0.17***
	(0.05)	(0.06)
Year dummies		
2018	-0.06	0.27
	(0.06)	(0.17)
Year dummies x Bite		
2018	0.03	-0.33
	(0.10)	(0.20)
Socio-demographics	Yes	Yes
Change in job characteristics	Yes	Yes
Politics on state level	Yes	Yes
Individuals	9624	2985
Observations	19248	5970
Within R^2	0.01	0.01

*Source: SOEP.v36. – Notes: See table A.2.4 for notes. Robust standard errors clustered at the level of labour market regions in parentheses, with * p<0.1, ** p<0.05, *** p<0.01.*

Table A.2.11 *Robustness: Inclusion of individuals without party preference*

	1 West SPD	2 East SPD
Constant	0.14***	0.08***
	(0.01)	(0.01)
Year dummies		
2010	0.05	-0.04
	(0.04)	(0.05)
2011	0.03	0.01
	(0.03)	(0.08)
2012	0.00	-0.01
	(0.03)	(0.07)
2014	0.03	-0.07
	(0.03)	(0.05)
2015	0.01	-0.03
	(0.03)	(0.03)
2016	0.01	-0.14**
	(0.03)	(0.06)
2017	-0.02	-0.06
	(0.03)	(0.07)
2018	-0.02	-0.08
	(0.03)	(0.05)
2019	-0.02	-0.10
	(0.03)	(0.07)
Year dummies x Kaitz index		
2010	-0.07	0.06
	(0.06)	(0.06)
2011	-0.06	-0.03
	(0.05)	(0.10)
2012	-0.01	0.01
	(0.05)	(0.09)
2014	-0.03	0.10
	(0.05)	(0.06)
2015	-0.03	0.02
	(0.05)	(0.05)
2016	-0.05	0.15**
	(0.05)	(0.07)
2017	0.03	0.07
	(0.06)	(0.08)
2018	0.01	0.08
	(0.05)	(0.07)
2019	-0.01	0.10
	(0.05)	(0.09)
Socio-demographics	Yes	Yes
Change in job characteristics	Yes	Yes
Politics on state level	Yes	Yes
Individuals	30854	7924
Observations	164839	46600
Within R^2	0.00	0.00

*Source: SOEP.v36. – Notes: See table A.2.4 for notes. Robust standard errors clustered at the level of labour market regions in parentheses, with * p<0.1, ** p<0.05, *** p<0.01.*

Table A.2.12 *Robustness: Influence of refugee migration*

	1	2	3	4
	West		East	
	SPD	Right-Wing	SPD	Right-wing
Constant	0.31***	0.01	0.18***	0.02
	(0.01)	(0.01)	(0.03)	(0.03)
Year dummies				
2010	0.01	0.01	0.00	0.04
	(0.05)	(0.01)	(0.09)	(0.06)
2011	-0.06**	0.03**	0.09	0.05
	(0.03)	(0.01)	(0.07)	(0.06)
2012	-0.05	0.03***	0.06	0.04
	(0.05)	(0.01)	(0.09)	(0.04)
2014	0.01	0.02	-0.01	0.02
	(0.03)	(0.02)	(0.06)	(0.05)
2015	0.01	0.00	0.02	0.08
	(0.04)	(0.02)	(0.05)	(0.09)
2016	0.00	0.05**	(0.04)	0.01
	(0.04)	(0.02)	(0.10)	(0.10)
2017	0.00	0.04	0.08	0.10
	(0.04)	(0.02)	(0.08)	(0.08)
2018	-0.01	0.05**	0.17**	-0.03
	(0.04)	(0.03)	(0.07)	(0.11)
2019	-0.08**	0.03	-0.13	-0.01
	(0.04)	(0.03)	(0.08)	(0.10)
Year dummies x Kaitz index				
2010	-0.01	-0.02	-0.03	-0.05
	(0.09)	(0.02)	(0.12)	(0.08)
2011	0.10**	-0.05**	-0.12	-0.06
	(0.05)	(0.02)	(0.09)	(0.07)
2012	0.09	-0.04**	-0.07	-0.05
	(0.07)	(0.02)	(0.11)	(0.05)
2014	0.00	-0.01	-0.01	-0.03
	(0.05)	(0.03)	(0.08)	(0.06)
2015	0.01	0.04	-0.06	-0.08
	(0.07)	(0.03)	(0.07)	(0.12)
2016	-0.01	-0.04	0.01	0.05
	(0.06)	(0.04)	(0.13)	(0.13)
2017	0.02	-0.03	-0.11	-0.09
	(0.07)	(0.04)	(0.11)	(0.11)
2018	-0.01	-0.04	-0.27***	0.10
	(0.07)	(0.04)	(0.10)	(0.15)
2019	0.06	-0.01	0.08	0.07
	(0.07)	(0.04)	(0.11)	(0.13)
Socio-demographics	Yes	Yes	Yes	Yes
Change in job characteristics	Yes	Yes	Yes	Yes
Politics on state level	Yes	Yes	Yes	Yes
Individuals	14039	14039	3003	3003
Observations	64654	64654	14250	14250
Within R^2	0.01	0.02	0.01	0.04

*Source: SOEP.v36. Notes: Excluding interviews from 04/2015 until 12/2015. See table A.2.4 for more notes. Robust standard errors in parentheses, with * p<0.1, ** p<0.05, *** p<0.01.*

Table A.2.13 *Robustness: Influence of hidden racism*

	1	2	3	4
	West		East	
	Agreeableness			
	Low	High	Low	High
Constant	0.36***	0.31***	0.13*	0.20***
	(0.05)	(0.01)	(0.07)	(0.03)
Year dummies				
2010	-0.16	0.02	-0.63**	0.04
	(0.18)	(0.06)	(0.27)	(0.10)
2011	-0.11	-0.04	-0.31**	0.13
	(0.12)	(0.04)	(0.13)	(0.09)
2012	-0.06	-0.06	(0.25)	0.12
	(0.09)	(0.05)	(0.30)	(0.08)
2014	0.05	0.01	-0.60**	0.07
	(0.11)	(0.03)	(0.23)	(0.07)
2015	0.09	0.01	(0.43)	0.08*
	(0.10)	(0.04)	(0.39)	(0.05)
2016	0.08	(0.01)	(0.10)	(0.03)
	(0.11)	(0.04)	(0.44)	(0.08)
2017	-0.05	0.01	0.00	0.13
	(0.14)	(0.04)	(0.42)	(0.08)
2018	-0.25	0.01	-0.14	0.21***
	(0.18)	(0.04)	(0.46)	(0.07)
2019	-0.1	-0.08*	-0.66*	-0.07
	(0.14)	(0.04)	(0.35)	(0.08)
Year dummies x Kaitz index				
2010	0.28	-0.02	0.77**	-0.08
	(0.30)	(0.09)	(0.34)	(0.13)
2011	0.19	0.06	0.33**	-0.18
	(0.20)	(0.06)	(0.15)	(0.12)
2012	0.12	0.10	0.28	-0.14
	(0.15)	(0.08)	(0.39)	(0.11)
2014	-0.07	-0.01	0.73**	-0.11
	(0.18)	(0.05)	(0.31)	(0.09)
2015	-0.12	-0.01	0.53	-0.13**
	(0.17)	(0.07)	(0.54)	(0.06)
2016	0.28	0.00	0.09	-0.02
	(0.19)	(0.06)	(0.58)	(0.11)
2017	0.10	0.01	0.03	-0.17
	(0.23)	(0.07)	(0.54)	(0.10)
2018	0.39	-0.04	0.18	-0.32***
	(0.30)	(0.07)	(0.57)	(0.09)
2019	0.07	0.06	0.74	0.00
	(0.24)	(0.07)	(0.45)	(0.11)
Socio-demographics	Yes	Yes	Yes	Yes
Change in job characteristics	Yes	Yes	Yes	Yes
Politics on state level	Yes	Yes	Yes	Yes
Individuals	816	12629	172	2689
Observations	4451	62076	923	13605
Within R^2	0.02	0.01	0.06	0.01

*Source: SOEP.v36. Notes: Columns (1) and (3) contain persons with a value for agreeableness which is by one standard deviation lower than the value for the average population defined by Big5-Identity. Column (2) and (4) contain the remaining population. See table A.2.4 for more notes. Robust standard errors in parentheses, with * p<0.1, ** p<0.05, *** p<0.01.*

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3. Content Analysis with Language Models – Exploring a Zero-Shot Learning Approach

Abstract

In this paper, we discuss whether and how Natural Language Processing (NLP) can be integrated into the workflows of conventional qualitative research to support the researcher. We focus on textual data which the researcher wants to analyse using predefined categories. We explore the possibility to carry out the coding step using NLP instead of assigning codes manually. Integrating such models into the qualitative research process makes the research more accessible and comprehensible for third parties and can contribute to moving research more into the direction of open science. We conclude that the representation of (specialized) language in the currently available language models is not yet refined sufficiently to be reliably applicable in qualitative research. Nevertheless, in their current state, off-the shelf language models can be used to validate the results obtained by manual coding. This could make qualitative research more traceable and fully reproducible.

Introduction

In recent years, methods in Natural Language Processing (NLP), a field of artificial intelligence developing statistical methods that enable computers to process and analyse vast amounts of text data, have significantly improved. In this context, Chat-GPT, a chatbot created by the company OpenAI, is the most prominent example. These advances raise the question to what extent such methods can support qualitative research.

Generally, integrating such methods into qualitative research is not straightforward, since qualitative methods consist of a wide range of different concepts and types of data, ranging from text to image and video data (Hammersley 2013). In this paper, we focus on data that is available in written form (especially interview data, but also written documents in general), that the researcher wants to analyse using predefined categories (also known as deductive coding). One popular approach in this context is content analysis (Mayring 2015, Silverman 2015), which is an important element of the mixed methods designs in order to analyse research topics in a comprehensive and differentiated manner (e.g., Groenewoud et al. 2022, Lindsäter et al. 2023, Valzadeh et al. 2020).

In this paper, we pursue two objectives. First, we explore a workflow in which NLP methods can be integrated into conventional content analysis by taking over isolated tasks that can be monitored and controlled by the researcher. Second, we discuss whether the approach is viable in practise and provide some directions for future research to improve it and make it more reliable.

Previous research on integrating NLP methods into qualitative research has focused on statistical models such as Topic Modelling (e.g., Baumer et al. 2017, Leeson et al. 2019) or Bag-of-Words models (e.g., Isoaho et al. 2021) to extract features from textual data. Topic Modelling summarises large amounts of texts into frequently occurring themes that are clusters of expressions or words. Using this technique, the researcher cannot influence the topic delimitations as the procedure is *unsupervised*. Moreover, since only the prevalence of words is considered, information about the context and semantic meaning of words is lost. The Bag-of-Words model counts the words of a predefined vocabulary in a document, but still faces identical shortcomings. Mikolov et al. (2013) developed the neural network Word2Vec which allows to estimate semantically meaningful representation of individual words by incorporating their surrounding context. To this end, Word2Vec represents each word as a vector of real numbers, where geometric proximity corresponds to semantic similarity. Thus, compared to previous models, Word2Vec captures a meaningful representation of language as similar words are close to each other in a vector space. Besides, the vectorisation of words ensures efficient machine processing and allows the application of linear algebra to examine the relationship between words. The idea of vector representations of words has been developed into more advanced and powerful language representation models (Vaswani et al. 2017). As one of the most influential

downstream models upon them, BERT (Bidirectional Encoder Representation from Transformers) performs complex tasks, such as identifying multiple connotations of language.

Our paper is most closely related to Leeson et al. (2019), which is a proof of concept paper to evaluate the potential of NLP to analyze qualitative data. They conclude that there are two potential ways to incorporate NLP methods: either, after coding to check the accuracy of codes, or before coding to guide the creation of the codebook. In this paper, we go one step further. We analyse whether we can carry out the coding step using NLP instead of assigning codes manually. To this end, our analysis is based on Sentence BERT (SBERT), a model that represents entire sentences in a vector space. Compared to fully unsupervised methods such as Topic Modelling, the procedure incorporates all the researcher's knowledge about the content of the text. By imposing constraints and defining topics, a researcher narrows down the search space (Yu et al. 2011).

Similar to Leeson et al. (2019), our approach seeks to improve two aspects of qualitative research. First, conventional qualitative research is time-consuming and labour-intensive (Winter 2000). We argue that the approach is less time intensive and can help to analyse contextualized, unstructured data at lower costs, especially when the sample size becomes larger (Abram et al. 2020, Clifford & Marcus 1986). Second, to evaluate the quality of qualitative research, there exist various criteria of goodness concerning the collection and analysis of data as well as reporting of the empirical findings. In qualitative research, criteria of goodness are not universal, but a matter of discourse related to epistemological schools of thought and derived assumptions (Kuckartz 2016). In general, many of these criteria are desirable but at the same time difficult to realise (O'Connor & Joffe 2020, Strübing et al. 2018). NLP methods, such as BERT, can facilitate the justification of criteria in an improved manner. In particular, the intercoder reliability (O'Connor & Joffe 2020, MacPhail et al. 2016, Feng 2015, Campbell et al. 2013) and intracoder reliability (O'Connor & Joffe 2020) are two core criteria of goodness that are often considered problematic. This may raise concerns about the reproducibility and consistency of the qualitative approach (Abram et al. 2020, Yu et al. 2011). In our approach, the coding process is precisely documented, making the results more traceable and fully reproducible. Hence, it is easier to credibly comply with the criteria of goodness that are directly connected with coding data, namely the intercoder and intracoder reliability.

In order to assess the viability and quality of the automated encodings generated by SBERT, we use empirical material from a recent study assessing the reactions and strategies of firms and employees to the introduction and the subsequent increase of the statutory minimum wage in Germany. Our analysis draws on one topic discussed in these interviews, namely how the introduction and subsequent increases of the minimum wage affects the wage structure of the firms. This question was recently analysed by Koch et al. (2020) using manual coding. Therefore, we are able to compare (1)

how the manual codes deviates from the codes created by SBERT and (2) how the final results derived by manual coding differs from the results derived by SBERT.

Based on our experience, we believe that outsourcing the coding step has numerous benefits and considerable potential. Although the researcher has to invest some time to get used to the handling of language models, the coding step with NLP can be less time consuming, especially for a considerable number of observations and vast amounts of text. More importantly, the coding step is well documented and easy to replicate, which moves qualitative research more into the direction of open science (e.g., Branney et al. 2023, Class et al. 2021, Humphreys et al. 2021). However, future research has to improve and adjust the language models to the specific topic of the data. Our analysis indicates that currently, the model struggles to distinguish between related topics as clearly as humans do. Moreover, humans understand and differently classify passages which only implicitly mention a topic of interest. We find that SBERT often is not able to identify such passages. Both *weaknesses* undoubtedly directly affect the quality of the coding. Whether the interpretation and conclusions differ depends on the exact texts and research question. In our specific case, the results derived by manual coding and those derived by SBERT do not substantially differ. In our opinion, this is impressive given that we use a general version of SBERT off-the-shelf without fine tuning it to the specifics of the data, which is usually done (e.g., Lee et al. 2020; Sun et al. 2019).

The paper is structured as follows. The next section describes the procedure. Afterwards, we conduct a case study to analyse whether automated encodings using SBERT are viable in practise. The final section discusses the results and outlines some perspectives for future research.

Analysing text data with known topics

The goal of this paper is to discuss whether and how NLP methods can be incorporated into qualitative content analysis when the researcher has knowledge about the topics that are prevalent in the text and intends to encode them using a predefined code system. We propose to apply a procedure that directly fits into conventional qualitative workstreams. Thereby, the coding step is carried out with NLP methods.

First, we divide the text into small parts, for example into sentences. Second, we encode each sentence of the text as well as the predefined topics of the code system into a numerical vector to ensure machine readability. Third, we assign a predefined topic to each sentence exploiting the properties of machine-readable encoding. Finally, we can proceed as usual with analysing the encoded sentences. In the following, we explain how the sentences are encoded and how codes are assigned to these sentences. Finally, we summarise the procedure.

Representing text with BERT

Generally, computers or statistical models are not able to read text in the same sense humans do. Therefore, it is imperative in NLP to explore ways for presenting text in a machine-readable format. Recently developed models are built on the idea of the distributional hypothesis from linguistics (Jurafsky & Martin 2021). The hypothesis states that words which share similar contexts tend to have similar meanings. For example, synonyms such as “car” and “automobile” are likely to have common surrounding word environments. Therefore, state of the art models represent text as numerical vectors which encode the contextual surrounding of the word. Such vectors are called embeddings. For example, the popular neural network model Word2Vec (Mikolov et al. 2013) represents a single word as a vector of probabilities. The model predicts the likelihood for every word in a defined vocabulary to occur in the surrounding context of the word of interest. Comparing word embeddings with each other cannot only provide information on the similarity or dissimilarity of the words’ meaning, but the difference of word embeddings is itself a vector which can have semantic meaning. The resulting geometric space of adding the embeddings of the words “London” and “Germany” and subtracting “UK” is close to the representation of “Berlin”.

For an embedding to be meaningful, the model has to take into account that words can have diverse semantic meanings. For example, in the sentences “I **left** the event at the river **bank**” and “I turned **left** into the **bank** to get cash”, the meanings of the words “left” and “bank” depend on their context. Whereas humans can easily identify the meaning of the words, a statistical model needs to follow a defined pattern. Early models were only able to *read* the sentence from one direction. However, if the words were mapped to their embeddings from left to right, the encoding model would not know which semantic meaning of the word “bank”, i.e., “bank/waterside” or “bank/cash-machine”, would be correctly meant. The language representation model BERT (Bidirectional Encoder Representations from Transformers) is able to encode a word by jointly depending on the left and right context (Devlin et al. 2018). Its improved performance compared to other models on a variety of different NLP tasks, such as answering questions and translation, has stirred up the machine learning community.

When analysing textual data, we are often not interested in the meaning of a single word, but in a set of connected words such as a sentence or a series of sentences. A naive approach would be to represent each sentence by pooling (for example averaging) the word embedding of all words in a sentence. However, this does not work well in practice (Reimers and Gurevych 2019). Therefore, we rely on Sentence-BERT (SBERT, Reimers and Gurevych 2019), a model that fine-tunes the pooled

embeddings obtained from BERT to increase semantic richness.[15] Further, SBERT is computationally rapid and cheap.

Encoding text without labels

After the embeddings of each sentence are computed by SBERT, we want to assign each sentence a predefined code. We follow recent work that shows that language models can handle tasks for which they were not explicitly trained for (see for example Radford et al. 2019). The approach can be considered as a zero-shot learning approach, i.e., an approach that is able to recognize new concepts by simply describing them (Romera-Paredes and Torr 2015). In our case, this means that we want to predict the code of a sentence only based on the description of that predefined codes. Since the description of the codes consist of pure text, we can proceed in an identical way as for the sentences and embed both sentences and codes into the same vector space using SBERT. Therefore, in this step, the predefined codes need to be operationalised by describing the topic they are referring to. Afterwards, SBERT is used to compute the embedding of the description. We assign a code to a sentence when both are semantically close to each other and therefore likely to concern the same topic. As described above, comparing the embeddings with each other provides information on the similarity or dissimilarity of the sentences' meaning. Therefore, we compute the cosine similarity between the embedding of the code and the embedding of the individual sentences. The cosine similarity is a measure of the similarity of two vectors that depends on the angle between them. The cosine defines a range between -1, indicating the opposite meaning and 1, indicating the same meaning of the two sentences. A predefined code is assigned to a sentence if they are similar in terms of the cosine similarity. Algorithm 3.1 summarizes this procedure.

***Algorithm 3.1**: Composition of unrestricted sample (share)*

Algorithm	
1:	Partition the texts into small parts. It is convenient to use sentences, since the input of SBERT is limited to 512 tokens, which roughly correspond to words.
2:	Compute the embedding of each part.
3:	Operationalise the topics, i.e., the conventional codes the text is about and compute the embedding of each topic/code.
4:	For each topic/code, compute the cosine similarity for each sentence. Assign the code to those sentences that are similar given a chosen minimum threshold similarity.
5:	Analyse the coded text in the conventional manner.

[15] To be precise, we used Cross English & German RoBERTa fine-tuned from Philip May and open-sourced by T-Systems-onsite: https://huggingface.co/T-Systems-onsite/cross-en-de-roberta-sentence-transformer.

Practical issues

The procedure raises some issues that the researcher has to address. First, the method heavily relies on the operationalisation of codes. If the quality of the operationalisation is poor, the assignment process will also be poor. If operationalization is too vague, then assigned codes will be too imprecise. Moreover, the procedure is difficult to apply when codes are semantically similar. There are no clear rules and specification on how topics must be defined and how semantically different topics should be framed in order to be distinguished. To obtain a well-defined operationalisation, the researcher has to become familiar with the ways language models practically work. We therefore recommend experimenting with the model before starting the analysis. Second, the procedure analyses each sentence independently. However, the context, i.e., the surrounding sentences, is usually important to completely understand the sentence. Thus, in our application, we do not only assign codes to a single sentence, but also to the five sentences preceding and following the encoded sentence. Third, the similarity between code and sentence is represented by the cosine similarity, i.e., a continuous number. The lower the number, the greater the dissimilarity between the code and the text partition. However, a binary decision is needed to code the sentence (similar or not similar). The decision, which numerical value still indicates similarity is an individual decision, the researcher has to make since there is no general cut-off threshold. Rather, it depends on the specific topic and text.

Are automated encodings using SBERT viable in practise?

In general, the performance of our procedure is difficult to measure since the process of coding is always subjective and there are no objective encodings.[16] Nevertheless, we need to obtain a better understanding of how the zero-shot learning approach described in the last section works and whether it is viable in practice. In order to assess the integrity and quality of the automated encodings generated by SBERT, we compare the results derived from this approach with those derived from a conventional approach (i.e., manual coding). To this end, we use a selection of our material to analyse (1) how the manual encodings differ from the encodings made by SBERT, and (2) how the final results derived by manual encoding differ from the results derived by SBERT encodings.

More precisely, we use empirical material from a recent study assessing the patterns of reaction and strategies of firms and employees in the context of the introduction and the subsequent increase of the statutory minimum wage in Germany (Koch et al. 2020). The following analysis is based on 60 literally transcribed guided interviews with firms' management or staff departments. They have been conducted by telephone or personally between May and September 2019. On average, the interviews

[16] In purely qualitative research, the intercoder reliability serves as a measure or process to ensure comparability of results between different individuals coding the same text (Campbell et al., 2013; MacPhail et al., 2016; O'Connor & Joffe, 2020).

took 44 minutes and contain 8,400 spoken words. The interviews are based on a comprehensive interview guideline comprising various topics concerning the reactions and the behaviour of firms to the minimum wage as well as firms' strategies to cope with the minimum wage. In order to identify and clarify the commonalities and the differences between the results of the (already existing) manual coding and the automated coding by SBERT, we focus on one specific topic of the interviews – namely the effects of the minimum wage on the internal wage structure of firms.

The minimum wage and internal wage structures

From the perspective of an affected firm[17], both the introduction as well as every subsequent increase of the minimum wage causes at least an increase of the wages of the employees directly affected by the minimum wage. Moreover, the overall wage structure of a firm employing both affected and non-affected workers changes. If the firm does nothing else than complying to the legal requirements, the wage differences between affected workers (rising wages) and non-affected workers (unaltered wages) diminish. Furthermore, all employees who previously earned wages below the new minimum wage level will now earn at least the minimum wage. As a result, any former wage differentiations between these employees may cease. To analyse how firms cope with these changes in the overall wage structure, Koch et al. (2020) develop a mainly deductive coding scheme complemented by selective inductive elements (as it is usually done when conducting a content analysis). Independently from these coding schemes, we operationalise each reference in the guideline as an input to SBERT. Table 1 summarises the relation between the guideline, coding scheme by Koch et al. (2020) and the defined topics for SBERT. While topics in SBERT almost entirely consist of buzzwords, the coding scheme used by Koch et al. (2020) is more detailed, contains further explanations and includes more context information. These differences in coding schemes make the differences in their operation very clear. SBERT solely relies on a measure of semantic similarity. In the course of conducting the study, we found that further information and clarification introduce disturbance in the embeddings. In contrast, further information and clarification is beneficial, or even necessary for humans to decide whether and how text passages should be encoded.

Table 3.1: *Operationalization of the coding scheme*

Topic in Guideline	**Topic in SBERT**	**Coding scheme in Koch et al. (2020)**
Development of the wage sum and affected wage groups	Minimum wage adjustment, wage increases, wage or salary of employees	Immediate change in wage costs or wages due to the introduction or increase of the minimum wage (so no wage reactions here), amount of wage difference (bite) (hourly wage before/after increase/introduction of minimum wage), number (or proportion of the total workforce) of

[17] We call firms „affected by the minimum wage" if they have at least one employee with a wage below the minimum wage at the time of its introduction or the subsequent increases.

	above the minimum wage	employees directly affected by the introduction or increase in the minimum wage
Changes of employees not directly affected by the minimum wage, overall internal wage structure, gaps between wage groups	Wage hierarchies and inner-company wage structure, distances between wage groups	Pay gaps between individual departments, functional and job levels, gender, (age) cohorts, etc., entire company wage structure, wage cuts for employees with hourly wages above the minimum wage, measures to restore wage gaps
Changes regarding the documentation and recording of working hours Effort for the implementation of the documentation and recording of working hours	Duty of documentation and recording of working time, electronic cash register systems, payroll accounting	Additional effort due to the documentation requirement (hours per week / month / year) coping with the additional effort (more staff, external consultants, digitization of the recording, etc.)

Comparison with SBERT

Comparing the encodings

First, we compare the SBERT encodings with the human encodings by Koch et al. (2020). The results of this comparison are presented in Figure 3.1. Each row depicts one interview. Coded areas are marked by colours and the colour shows the origin of the encoding. Yellow bars indicate human encodings. Depending on the similarity measures, encodings from SBERT are either blue, red or purple. Blue areas show SBERT encodings which are highly similar to the operationalization (cosine similarity of ≥ 0.6) and the red colour indicates encodings that are quite similar to the operationalization (cosine similarity between 0.55 and 0.59). Areas encoded in purple indicate that texts are more distant to the operationalization, but still meaningful (cosine similarity between 0.49 and 0.54). Hatched areas mark an overlapping between a SBERT and human encoding.

The results show some overlaps between SBERT and human encodings (hatched areas). However, quite often text passages do not overlap at all (coloured areas). The number of SBERT encodings not corresponding to a human encoding depends on the exact threshold. Mostly, these are passages that are rather distant to the operationalization (i.e., purple), e.g.: *"In the end, the employee certainly benefits from the fact that he has more in his hands with the rising minimum wage, be it twelve euros*

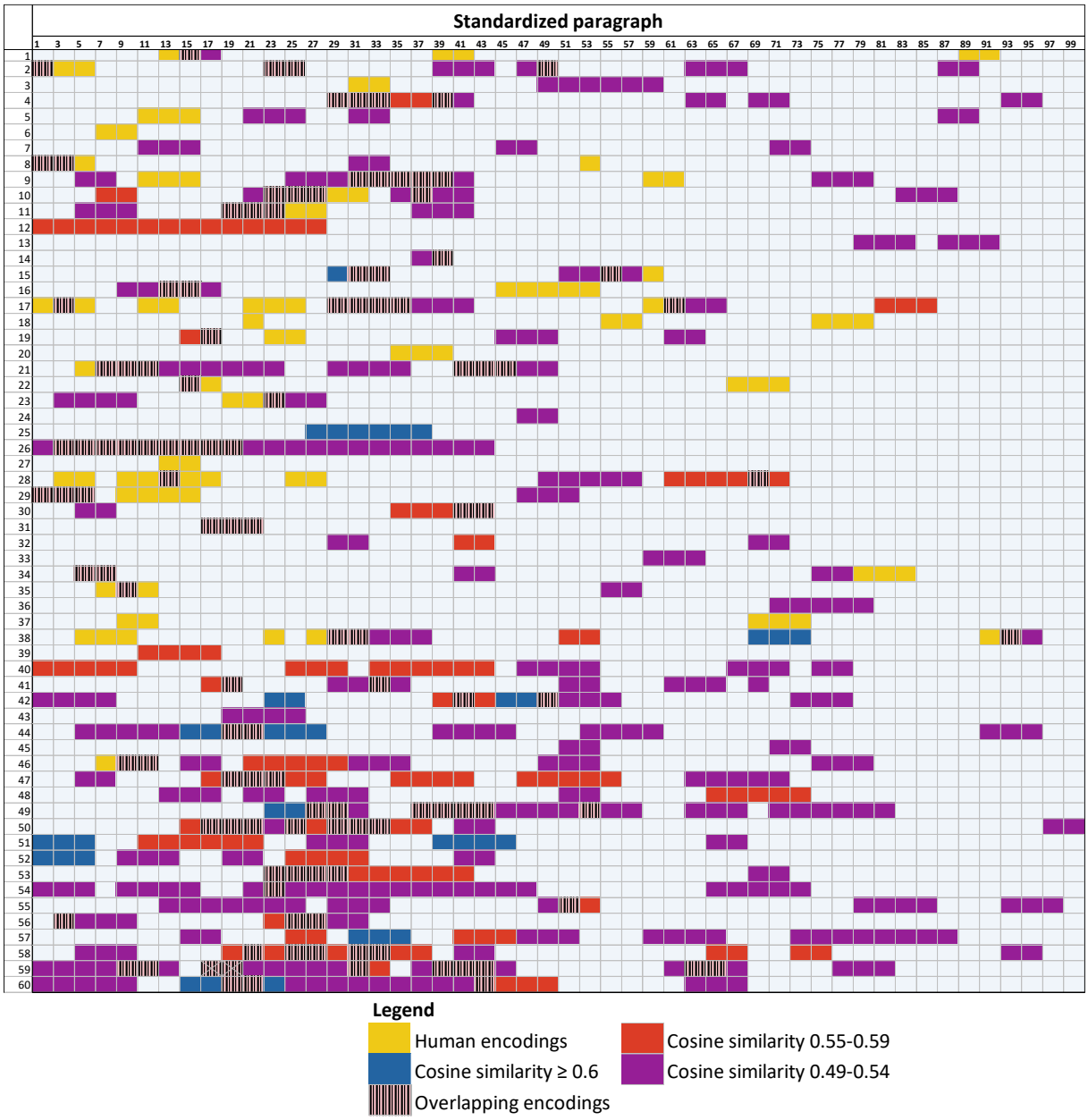
or whatever. [...] In the end, the whole thing is financed by the worker himself, in that he has higher expenses for his daily life, so that everyone else around him can also pay the increased wages, which have to be maintained through hierarchies and distances. In principle, we are only turning the inflation screw. Nothing more" (interview 4). In some cases, encodings are very close to the operationalization in terms of the cosine distance. It turns out that these text passages discuss topics such as work organization, working time or even general assessments of the minimum wage. For example, SBERT assigns a code with a high proximity to the operationalisation, even though there is no specific connection to a wage hierarchy in terms of content: *"Let's say, helpers or assistants who help me a lot before Christmas are for the most part from my circle of friends and acquaintances, of course not so much changes, because the wage is secondary, yes? And from that point of view, of course it's a matter of whether I've done an hour more or an hour less at Christmas, it doesn't really matter*" (Interview 51).

There is no doubt that these topics (or words used when someone talks about these topics) have some semantic relation with the topic of adjusting wage structures. However, in terms of content, these topics crucially differ from each other. For example, SBERT coded the following quote from a small farm in the context of wage hierarchies, even though it is about holiday entitlements of temporary workers as a consequence of the minimum wage law: *„Well, they get the 10 euros for the hour and of course they get all these holiday entitlements. Of course, this is calculated much more accurately than before. Before, of course, someone who, I know, had only been a student for two months or half a year, who had helped out somewhere, had no holiday entitlement calculated. [...] Of course, you have to calculate exactly how much holiday entitlement he would have had during that time."* (Interview 58).

Moreover, there are human encodings without any overlapping SBERT encoding. It is noticeable that SBERT has not assigned an encoding to passages in which wage structures are described more implicitly, without relying on vocabulary that specifically points to such a topic. Thus, the following passage from a call center was coded by a human, but not by SBERT: *"What we had to do when we introduced the minimum wage, we had to make much more of a one-size-fits-all. [...] What we had to do was to distribute it differently, so that employees who were not so good and more or less took it easy earned more than before and others who had earned a lot before due to their good performance had to cut their bonuses"* (Interview 20). This quote illustrates changes in the wage structure without explicitly naming them as such and therefore couldn't be detected by SBERT.

In summary, the results show that zero-shot learning is able to detect similar encodings compared to humans. However, off-the shelf language models like, non-fine-tuned SBERT, have considerable weaknesses. First, they are not able to distinguish between crucially different topics that rely on words that are semantically quite close. Second, the language model is not able to detect text passages where the topics are discussed more implicitly.

Figure 3.1: *Comparison between manual and automated encodings*



Comparing the findings

In this section, we compare the final results obtained by manual coding described and analysed in Koch et al. (2020) with those obtained by encoding via SBERT.

As wage differences between employees reflect differences between the employees' qualifications, occupations, tasks, competencies and personal characteristics, a modification of the firm's internal wage structures (i.e., the distances between different wage groups) might be undesirable and disadvantageous in the view of the firm. In this sense, Koch et al. (2020) find that firms which adjust wages exclusively for workers affected by the minimum wage, report that it has become more difficult

(i.e., more expensive) to reflect the performance and the qualifications of employees in their compensation. Other firms with a similar behaviour report declining job satisfaction, especially of higher qualified employees, a decline in team spirit and, ultimately, a deterioration in the working atmosphere. Such firms fear an increasing exit of skilled workers that do not benefit from the minimum wage. Other firms state that issues also emerged with regard to differentiations between marginally employed workers and workers subject to social insurance contributions, as the former usually do neither pay any taxes nor contributions to social insurance and thus frequently have significantly higher net wage gains than the other group (cf. Koch et al. 2020, p. 56f).

If, on the other hand, firms try to keep wage structures unaltered, they either face increasing personnel costs due to the wage adjustments in the higher wage groups, or they have to take organisational measures or precautions in order to be more efficient and to thereby avoid or compensate the rising wage costs. For instance, firms report that poorly qualified workers with low wages even have been dismissed due to the huge lack of the quality of their work in comparison to other (low wage) workers. Some firms adjusted the wages of employees, not directly affected by the minimum wage, gradually or with a delay. Other firms report that they changed their whole pay scheme, e.g., by strengthening performance-related compensation (cf. Koch et al. 2020, p. 59ff).

Although encodings differ between SBERT and those made by Koch et al. (2020), the analysis leads to the same conclusions. In our case, there is a clear intersection between the findings of both, SBERT and manual encoding. This includes especially the core findings by Koch et al. (2020) concerning diminishing illustration of qualification levels in earnings, negative effects on general working atmosphere in the firms and declining job satisfaction of workers that earn more than the minimum wage. Therefore, we can conclude that SBERT identifies the same core results as Koch et al. (2020).

However, there are some results that deviate. Using SBERT, we do not find results that are only marginally related to the change in wage structures (e.g., the dismissal of workers according to their productivity). Moreover, as indicated above, SBERT relates some findings to wage structures that deviate from the core understanding of the specific topic. We also obtain findings that relate to the general increase of wages or special payments.

In summary, the comparison identifies two issues. First, language models cannot distinguish between related topics as clearly as humans. This is because language models are only based on the semantic distance, which does not necessarily intersect with a human's core understanding and definition of a topic. Second, humans, in contrast to language models like SBERT, understand and classify passages which only implicitly mention some topic. These *weaknesses* undoubtedly directly affect the quality of the coding. Whether the interpretation and conclusions differ depends on the exact text and research question. In general, only the latter issue leads to an incomplete picture, since some relevant passages

might not be encoded. The first issue only leads to an excess amount of encoded passages which are not relevant for interpretation and have to be ignored or "decoded" by the researcher. The second issue might lead to wrong conclusions because of *sloppy* coding. In our application, the differences in SBERT encoding and manual encoding are rather large, whereas the first issue is particularly prevalent. However, through "decoding", the interpretation and conclusions reached from the relevant encodings do not crucially differ from the results elaborated by Koch et al. (2020).

Discussion and perspectives

This paper explores and discusses if and how statistical language models can be used for coding text data. The method directly fits into the conventional procedures of content analysis since it replaces the coding step usually executed manually by a researcher with a language model. Applying the procedure enables researchers to analyse large amounts of text data without losing the in-depth view, which distinguishes qualitative research from other disciplines. Moreover, the coding step can be fully understood and replicated by a third party, which is a crucial criterion of goodness (i.e., inter- and intracoder reliability). Therefore, we think that integrating language models into the qualitative research process makes the research more accessible and comprehensible for third parties and can help to move research more into the direction of open science.

Our study indicates that the procedure is potentially able to identify the core results. However, the results also show that there are weaknesses that strongly depend on the exact text and research question. We find that off-the-shelf language models cannot differ between related topics as clearly as humans. This is because language models are only based on the semantic distance, which does not necessarily intersect with a human's core understanding and definition of a topic. Moreover, humans understand and classify passages which only implicitly mention some topic. We find that SBERT often is not able to identify such passages. This weakness poses a great risk of overlooking relevant text passages.

In general, these weaknesses can be partly addressed by choosing a well-considered operationalisation of topics. However, our study indicates that this might not be enough. One important step to reliably apply language models in qualitative research consists of improving the models. In this paper, we only use an off-the-shelf version of SBERT which was trained on many different texts – given that, the results are impressive in our opinion. When analysing specific data, one might account for the nature of it. In our case, we might account for slang, incomplete sentences, idioms and, most importantly, the special vocabulary that often occurs in the context of labour market research. For example, BioBert (Lee et al. 2020) was developed to analyse biomedical text data. Training such a model goes beyond the scope of this explorative paper, but should be added to the future research agenda on integrating NLP methods into qualitative research. Nonetheless, we conclude, that in their current state off-the-shelf language

models can be utilized to validate the results obtained by manual coding. This could make qualitative research more traceable and fully reproducible.

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4. Stay or Leave? The link between wages and return migration plans. Evidence from Germany.

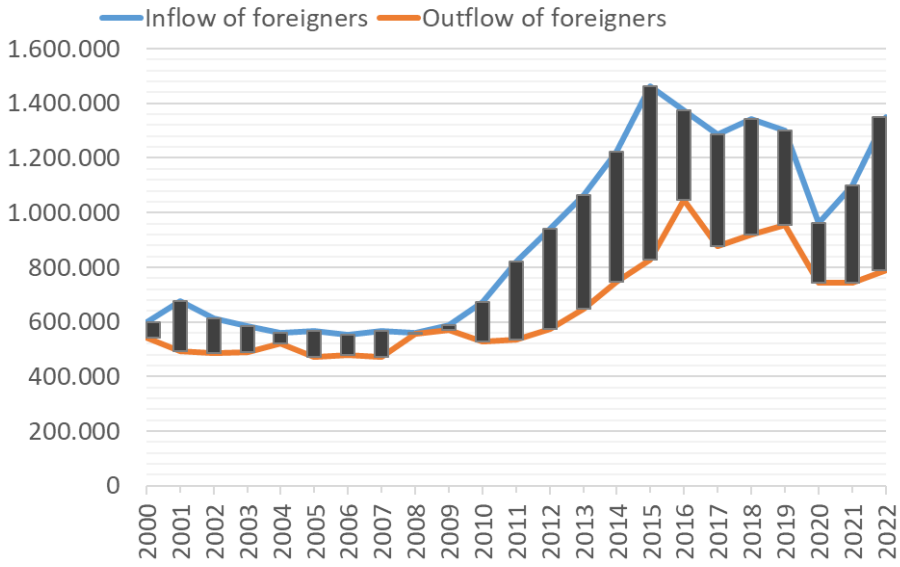
Abstract

This paper investigates the relationship between changes in marginal wages in the host country and the intention to return to the home country. Using panel data from the GSOEP between 1984 and 2019, it empirically examines the effect of individual wages on migrants' intentions to return. Local linear regression reveals a bell-shaped relationship between wages and the intention to remain. This suggests that increasing wages in the low-wage corridor provide a positive incentive to stay longer, while at a certain wage the relationship turns negative. This result lends support to the life-cycle explanation within the low-wage corridor. To further test this relationship, the paper exploits the introduction of the minimum wage using a Two-Way Mundlak approach, with the intended durations of stay in Germany as the dependent variable. However, the results do not support our preliminary findings and instead suggest that there is no significant link between marginal wage changes and return migration plans. Neither the life-cycle explanation nor the target-earner motive can be supported by the results, which call into question the relevance of wages on expected durations of stay.

Introduction

Return migration is not a new area of research. Nevertheless, it has remained a marginal topic due to poor data, the focus on the migration process itself, and the persistent belief that migration is either successful or a failure. However, migration is typically non-permanent, and the optimal duration might be a utility maximisation problem (Dustmann and Görlach, 2016). For governments, it is increasingly relevant to encourage immigrants to stay permanently, as populations in many developed countries are ageing and some economies are facing severe labour shortages (see, for example, Kalweit and Baumgärtner 2022 for Germany). The expected duration of immigrants is rather short, so high gross immigration flows alone do not solve this problem. Reasons to leave can be manifold (De Haas et al., 2015; Vanderkamp, 1971; Gibson & McKenzie, 2011). In other words, emigration flows are strongly correlated with (lagged) immigration flows, resulting in relatively low net migration (Gmelch, 1980). In Germany, for instance, between 2000 and 2020, 20 million foreign immigrants entered Germany, while 14 million emigrated (see Figure 4.1). Net immigration is much lower than overall immigration flows. The grey bars in the figure below illustrate the difference between immigration inflow and outflow.

Fig. 4.1 Flows of Migration in Germany



Note: Conflict countries excluded acc. to Uppsala Conflict Database Project.
Source: Migration statistics of the German Federal Statistical Office, UCDP, own figure.

The relatively high outflow of immigrants and upcoming labour shortages in Germany raise the economic question of whether wages influence the decision to stay or leave a country. Neoclassical approaches to classifying the causes of return migration address this issue using a model of rational choices and the accumulation of human capital and utility maximisation (Bailey and Law, 2013; Dustmann and Glitz, 2011; Hamdouch and Wahba, 2015; Kirdar, 2012). In short, they argue that return migration is the result of an individual's optimal plan for utility maximisation throughout their life cycle.

Two competing ideas of utility maximisation are predominant: the life-cycle explanation (consumption preferences in the home country) versus target-earners' motivation (saving for investment in the home country). Another common approach is to explain migration in terms of push and pull factors (see, for instance, Chort et al., 2016, or Gmelch, 1983). In the context of the effect of wages on return migration, the push factor is the realised wages in the host country, while the pull factor is the potential wage in the country of return, in a very simplified model.

Empirical evidence, particularly causal evidence, regarding what determines the intended duration of stay is limited (Abarcar, 2017; Yang, 2006; Kirdar, 2009, with GSOEP; Bijwaard et al., 2014; Zaiceva & Zimmermann, 2016). Abarcar (2017) and Yang (2006) exploit exchange rate shocks, which shift the relative wage differential between the home and host countries. Kirdar (2009) uses shifting purchasing power parity, while Bijwaard et al. (2014) examine administrative spell data from the Netherlands, finding causal evidence that longer periods of unemployment are linked to a higher probability of leaving the Netherlands. Most authors find support for the life-cycle explanation paradigm, i.e. that a marginal increase in wages in the host country leads to a reduction in the probability of leaving. However, evidence for Europe or developed countries is scarce. Therefore, this paper contributes to this field of study by focusing on how individual wage changes (especially those of low-paid workers) affect return migration plans.

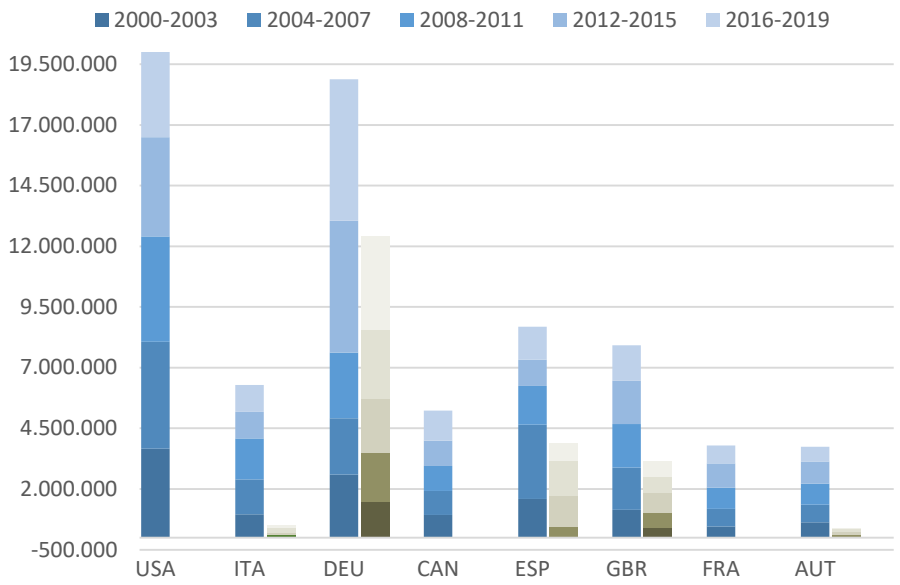
Apart from wages, many other factors may also influence return intentions. These will not be the focus of this paper. However, a broader overview of possible determinants supported by literature is provided using Survival Analysis. According to the literature, return migration is predominantly driven by a combination of economic and non-economic forces (Constant and Massey, 2003; Currle, 2006; Gibson and McKenzie, 2011; Gmelch, 1983; Gundel and Peters, 2008; Esser, 2001; Han, 2016). Recognition of vocational qualifications (Gaulé, 2014; Gibson and McKenzie, 2011), social integration (Constant and Massey, 2003; Constant and Zimmermann, 2012; Esser, 2001; Diehl and Preisendörfer, 2007; Levitt and Jaworsky, 2007; Toma and Castagnone, 2015; Vega and Brazil, 2015; Warrach, 2020), as well as further professional qualifications and education (Constant and Zimmermann, 2011; Dustmann and Görlach, 2016; Dustmann and Glitz, 2011; Nyborg, 2006; Yildirim, 2021), are just some of the many factors that influence return migration. Wahba (2022) provides a good overview to explore return migration in a broader sense.

Data

This paper focuses its analysis on Germany, as it is one of the major economies and one of the largest immigrations states among OECD countries (see Figure 4.2). Furthermore, Germany provides accurate migration statistics and a long-running, high-quality panel in the context of the German Socio-Economic Panel (GSOEP). As Bilecen (2022) states, 'It is well known that international migration

statistics lack absolute correctness and coherence'. However, the GSOEP allows us to delve deeper into the topic than other data would. Even though we may focus on one destination country.

Fig. 4.2 Gross & Net-migration flows in OECD Countries



Note: Blue = Gross Inflow, Green = Net-Inflow, Outflow missing for USA, CAN, FRA.

Source: OECD; own figure.

With more than 20,000 interviews per year, the German Socio-Economic Panel (GSOEP) provides a sound basis for studying return intentions (Glemser et al., 2020). Established in 1984, it covers a wide range of issues, including migration. The GSOEP has two salient features that are particularly relevant for migration research: First, it oversamples people with a migrant background. Secondly, the GSOEP has included questions on migration and return intentions since 1996, surveying over 1,000 immigrants annually. This enables specific migration research questions to be addressed, particularly with regard to the development of concrete return intentions.

This study uses the GSOEP to analyse the effect of wages on migrants' intended length of stay and return intentions. We do not merely focus on actual returns, as there are too few in the sample. It is important to note that the GSOEP only observes medium- and long-term migration intentions due to common left censoring in surveys, i.e. most immigrants are not interviewed upon arrival. Migrants join the panel later. Beforehand, they remain unobserved and at risk. This causes selection bias, meaning that very short migration periods remain unobserved in general. Short-term stayers (e.g. exchange students, rejected asylum seekers and participants in exchange programmes) are poorly represented or not represented at all. The weakness of the GSOEP can be inferred from the weaker return migration rates in the first years after immigration, compared to the figures in public migration statistics. However, as this study focuses on medium- and long-term labour migration, using the GSOEP is still reasonable.

Another weakness of the GSOEP is that accurate observation of actual departure (i.e. emigration to another country or return to the country of origin) is not possible. Individuals are not interviewed abroad because contact usually breaks off when they leave Germany (see Kirdar, 2009, for an extensive discussion of this issue). However, information on return migration plans is not biased by (un)intended departures.

Sample Construction

This study focuses on individuals who immigrated after 1984 and have autonomously decided to immigrate. Hence, we focus on foreign-born individuals who migrated to Germany as adults (15+ years of age).

Table 4.1 *Description of sample*

	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019
Composition of Immigrants by year (in %)										
Female	55.5	55.6	54.4	52.5	51.9	51.7	50.2	51.7	52.0	51.3
Male	44.5	44.4	45.6	47.5	48.1	48.3	49.8	48.3	48.0	48.7
ISCED: Enrolled in School	0.1	0.1	0.1	0.2	0.2	0.2	0.4	0.3	0.2	0.1
ISCED: None	6.1	6.3	5.4	7.0	6.5	6.3	7.8	10.4	10.4	11.3
ISCED: Primary	24.2	21.2	20.4	20.6	20.9	19.6	18.6	18.4	17.2	15.7
ISCED: Vocational	31.4	33.0	34.2	32.4	32.1	31.6	30.5	28.8	29.8	28.7
ISCED: Vocational + Secondary	10.9	12.5	12.6	13.5	13.0	14.9	14.9	13.9	13.3	13.8
ISCED: Tertiary	5.4	4.4	4.0	2.6	3.2	2.8	2.7	2.8	2.5	2.5
Age at Immigration: 15-29	65.6	64.6	64.5	62.7	64.2	63.0	62.4	63.1	61.9	64.0
Age at Immigration: 30-49	28.7	30.1	31.2	32.9	31.3	32.9	33.9	33.8	34.6	32.6
Age at Immigration: 50-65	5.7	5.4	4.2	4.4	4.5	4.1	3.7	3.2	3.5	3.4
Age 15-29 Years	5.1	4.9	5.9	7.6	6.7	7.8	11.1	10.5	9.6	8.6
Age 30-49 Years	41.7	42.6	42.2	44.9	46.0	45.9	42.8	42.6	42.1	41.5
Age 50-64 Years	32.4	31.9	32.6	30.4	29.8	28.2	28.2	27.2	27.6	29.8
Age 65+ Years	20.9	20.6	19.4	17.0	17.5	18.1	17.9	19.6	20.8	20.1
Immigrated before 2000	51.8	52.2	48.9	48.1	47.1	42.9	40.7	40.0	39.1	39.2
Immigrated 2000-2009	16.8	18.9	22.4	26.5	25.2	23.8	20.5	19.8	19.5	19.4
Immigrated 2010-2019	0.0	0.6	2.4	7.3	10.0	16.7	23.9	25.8	27.6	27.4
Asylum seeker	9.7	8.8	9.1	10.7	11.1	11.7	18.9	21.3	19.6	19.2
Intention to return within the next 2 years	2.0	0.7	.	2.0	.	1.7	1.7	1.7	0.8	1.0
Plans to stay for ever	53.6	55.1	0.0	75.4	0.0	78.0	8.0	79.6	1.3	0.5
Planned Duration of Stay	10.7	11.0	.	9.7	.	11.2	7.6	10.7	5.1	.
Observations	1,901	2,152	1,961	4,674	3,687	4,614	8,443	9,215	7,656	6,535

Source: GSOEP, weighted means, own calculations.

The sample contains more women than men. The share of immigrants with tertiary education living in Germany is decreasing over time. The majority of immigrants have been migrating at a relatively young age. In 2019, 64% of immigrants were between 15 and 29 years old at the time of immigration. While the population of younger immigrants is continuously changing due to the inflow and outflow of migrants, around 39% of all migrants in the sample arrived before 2000. The sample is left truncated and right censored, as immigrants are not observed upon entry or after leaving. Despite the large inflow of asylum seekers in 2015, the proportion of asylum seekers within the immigrant sample is relatively stable, peaking in 2017 at 21% refugees. The number of observations increased in 2013, as the GSOEP stocked up their panel and oversampled migrants once more. This led to a notable increase in the number of observations in the sample.

For the purposes of this study, return intentions are of particular interest. In most years of the survey, immigrants are asked whether they want to stay in Germany permanently, with the option to answer 'Yes' or 'No', and a follow-up question: 'How long would you like to stay in Germany?', with the options 'One Year Maximum' or 'Several Years, specifically...'. This question was not asked in 2012, 2014 or 2019. Generally, more than half of immigrants plan to stay permanently. If they do not intend to stay permanently, their average planned duration is about 10 years. Each year, between 20 and 90 individuals in the sample develop return intentions, i.e. they plan to return within the next two years.

Later on we will address this issue in more detail. Beforehand, however, the empirical strategy needs to be defined.

Estimation approach

Local Linear Regression

To get an initial idea of the connection between wages and the likelihood of staying permanently, we will use Local Linear Regression. This is a non-parametric estimation method. It estimates a conditional mean without making any assumptions about the functional form.

$$y_i = g(x_i) + \varepsilon_i \qquad \text{with } E(\varepsilon_i|x_i) = 0$$

The Local-linear regression method estimates a conditional mean for each point in our data for a subset of observations. See Fan and Gijbels (1996) for a good reference on local linear regression. The minimisation problem is given by

$$\min \gamma \sum_{i=1}^{n} \{y_i - \gamma_0 - \gamma_1'(x_i - x)\}^2 K(x_i, x, h) \qquad \text{where } \gamma = (\gamma_0, \gamma_1')'$$

The solution to the equation is similar to OLS. However, the slope and constant are interpreted differently. The constant γ_0 is the conditional mean at a specific point x. The slope γ_1 is the derivative of the mean function with respect to x. In our case this is the wage. Solving this least-squares problem

provides the mean function and its derivative for each element of x. The optimization is repeated to obtain for each point x the entire mean function and its derivatives. In practice, this means that the estimates of this mean function are robust to misspecification of the functional form.

The second difference from OLS is the weights used in the optimization problem. The kernel function $K(.)$ defines these weights. The smaller h is, the greater the weight assigned to points between x_i and x. This method is comparable when drawing a density function out of discrete data. Selecting the correct bandwidth is important as it affects the bias and variance of the estimator. We select the bandwidth using cross-validation (see Li and Racine, 2004) with the STATA command 'npregress'. In practice, this means we use an Epanechnikov kernel with a bandwidth optimized to minimize the integrated mean squared error of the prediction.

The result provides an initial indication of the relationship between wages and the likelihood to stay permanently in Germany. Hence, *y* in our case is a binary outcome, i.e. the probability to stay permanently in Germany. x is the hourly wage computed by dividing the monthly gross income by the number of contractual hours worked (reported weekly).

$$wage_{it} = \frac{gross\ monthly\ income}{weekly\ hours\ worked} \cdot \frac{12}{52}$$

Difference-in-Differences with Two-Way Mundlak Regression

To dig deeper into the subject, we exploit the minimum wage as an exogenous wage increase and observe how subsequent return intentions have changed. Put simply, there are two groups of migrants: The first group are those who have benefited from the minimum wage as they have earned less beforehand. The second group consists of those who have earned (slightly) above the minimum wage before its introduction and have not experienced (a direct) wage increase. This means that not every employee is affected by the minimum wage in the same way, calling for a Difference-in-Differences or Two-Way-Fixed-Effects (2WFE) estimation strategy - a common and straightforward approach to evaluating a policy event like this (see for instance Bachmann et al. 2022 or Caliendo et al 2019). However, things become more complex on closer inspection. In the case of a bivariate dependend variable, which we wish to use (i.e. $y = 1$ for migrants who wish to stay permanently and 0 otherwise), individual or year fixed effects may not be included. Otherwise all model parameters would be biased due to the incidental-parameter problem. Nevertheless, we can control for fixed effects by inserting the respective means of all the covariates in the corresponding dimensions as control variables (see Mundlak 1978; Chamberlain 1982, 1984; Wooldridge 2010; Wooldridge 2021). Wooldrige (2021) has developed this method. This method, known as Two-Way Mundlak Regression circumvents fixed effects by using the means of all covariates in the corresponding dimensions.

Wooldridge (2021) shows that two-way fixed effects estimates and his pooled OLS Mundlak (or Two-Way Mundlak) are equal using a balanced panel with linear and non-linear outcomes.

Another issue concerning TWFE has recently emerged: the ATT may not be discovered because the TWFE design uses cases in which treated individuals act as controls. This happens when staggered interventions are involved. Therefore, Callaway and Sant'Anna (2020), as well as Wooldridge (2021), define the control group as comprising only individuals who have never been treated. They then estimate the ATT for each cohort to ensure that cohorts do not have ambiguous ATTs. However, if we ignore this issue and use simple averaging over all cohorts, we may introduce negative weights and obtain biased estimates (see Goodman-Bacon, 2021; Chaisemartin & D'Haultfoeuille, 2020).

To address these issues, I adopt Wooldridge's (2021) approach, using his Two-Way Mundlak fixed-effect estimation strategy, which enables heterogeneous robust identification of the ATT. Rather than controlling for all individual FE and all year FE, the Two-Way Mundlak estimation strategy uses cross-sectional and time averages.

A balanced panel is required for the linear case, but not for non-linear equations like the Probit or binary outcomes used in our case. However, Probit with a binary outcome may lead to diverging results; that is to say, the ATT will not be identified. Therefore, we use a linear binary model. To ensure that no predicted outcome is negative, we use categorical variables as controls only.

Consequently, the estimation equation is

$$y_{it} = \alpha + d_i\,\gamma_i + \sum_{t=2015}^{2017} w_{it}\,\delta_t + f_t\,\tau + x_{it}\,\beta + w_{it}\cdot x_{it}\,\theta + w_{it}\cdot \overline{xd_\iota}\,\varphi + d_i\cdot x_{it}\,\vartheta + \varepsilon_{it}$$

$$\in w_{it} = d_i \cdot f_{t=Post}$$

where d_i is a dummy indicating the treated group. These are immigrants, who have earned less than the minimum wage (8.50€/h) before its introduction in 2015. We restrict the treated group to individuals that earn at least two periods less than the minimum wage prior to its introduction, i.e. in 2014 and 2013. This reduces the natural mean reversion (natural upwards wage mobility) of low wage workers. Moreover, in this way we take care of expected spillovers in terms of wage increases (Biewen, Fitzenberger und Rümmele 2022; Bossler und Schank 2023). We define three groups to adress that problem. A treated group [0;8.50€/h), a spill-over group [8.50€/h;15€/h) and a control group with individuals earning more than 15 €/h and less than 25 €/h in 2014 and 2013 ($d_i = 0$).

The letter f_t corresponds to year dummies for all periods. w_{it} is the interaction term between the treatment and the beginning of the treatment, i.e. the minimum wage comes into force for the individuals treated. In our case the corresponding δ_t is the coefficient of interests, which identifies the

ATT if assumptions hold, i.e. linearity, common trend, SUTVA to name the most important. The rest are control variables and its interactions to the corresponding treatment variable w_{it} as suggested by Wooldridge (2021) introducing the Two-Way Mundlak method. $\overline{xd_i}$ are the demeaned control variables for each treatment cohort.

Duration Analysis with Cox-Regression

The duration analysis - the proportional Cox regression model (Cox 1972) - is used to investigate the broader determinants of return migration intentions. It determines the individual risk (hazard) of planning return migration, depending on the length of stay and individual characteristics. It is derived from a baseline risk (baseline hazard rate) of the reference group, from which it deviates proportionally. The base hazard rate itself remains undefined. The calculated coefficients (β) therefore indicate the proportionally increased or decreased risk of planning a return migration compared to the baseline group as a percentage. A crucial assumption of the method is that the explanatory factors are also linearly proportional to each other - i.e. there are no intersections and the curve remains uniform. For example, it is assumed that the risk of migrant women with a medium level of education is lower across all years than the risk of migrant women with a low level of education. This assumption is checked for plausibility in the subsequent descriptive analysis.

Accordingly, the risk of actually planning to emigrate in year t (hazard rate $\lambda_i(t)$) is estimated as a function of the individual characteristics X_i:

$$\lambda_i(t) = \lambda_0(t) e^{X_i(t)\beta}$$

The product of the parameter β to be specified linearly with the individual characteristics X_i forms the exponent of the e-function. This in turn is multiplied by the basic hazard rate λ_0 and results in the hazard rate $\lambda_i(t)$ of an individual i at time t. In the context of the analysis of return migration, the explanatory variables X_i are gender, education, origin, employment status, income and various push and pull factors.

The maximum likelihood method is used to estimate the strength of the influences, β. A distinction must be made between individuals who had already entered Germany before their first interview (left-truncated), those who are observed to have plans to return, and those who had not made any concrete plans to return before the last interview and are still living in Germany, i.e. are right-censored (for more details see Kalbfleisch und Prentice 2011, 95 ff.).

Van den Berg 2001 motivates the model with the following argument. First, its dictated by economic theory. Second, it is easier to think about the exit probability given the already passed duration, then the unconditional probability of leaving in *t*. Third, it adopts to a model one has in mind. Forth, it effectively captures time-varying characteristics. Fifth, it deals with the right censoring

problem. However, the assumption that the hazard remains proportional to each other is hard to justify regarding the research question of this paper. Therefore, I show Kaplan-Meier plots that gain deeper insights on return migration (intentions), and will later on use the Two-Way Mundlak Regression to show some more profound estimates.

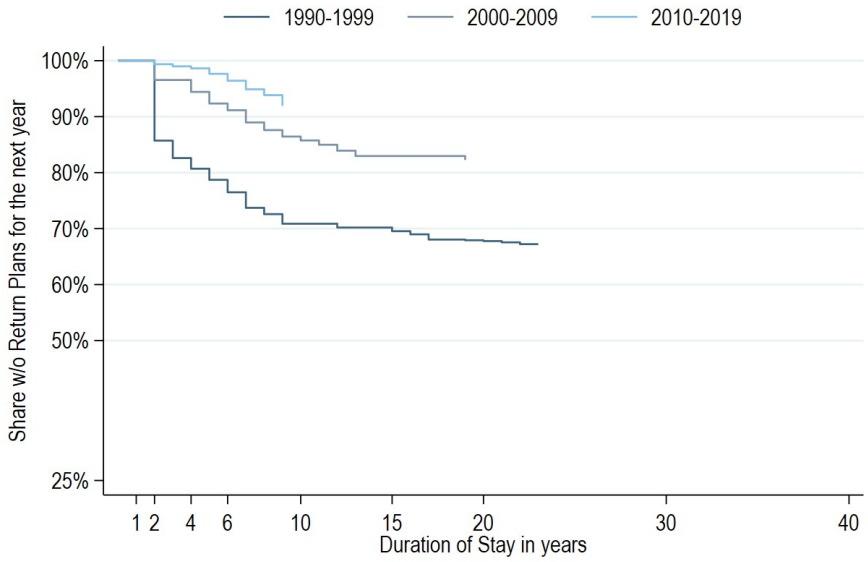
Descriptions of Return Intentions and the link to wages

Description of return intentions

The first question to address in the context of this paper is how long immigrants typically stay in Germany. This seemingly simple question is difficult to answer in the case of Germany. Administrative data on the inflow and outflow of foreigners is not linked to a panel, and the GSOEP only provides information on immigrants who have not yet left. A non-representative worldwide questionnaire by Boockmann et al. (2022), which used social media to reach participants, indicates an average completed duration of stay of 4.5 years, with a standard deviation of 5.8 years. The GSOEP can provide information about the duration of stays that have not been completed, i.e. immigrants who are still living in Germany. In 2019, the average duration of stay was 23 years (standard deviation of 15 years) for immigrants who were still living in Germany and 14 years for immigrants with the intention of returning within the next 12 months. This shows that the GSOEP is unable to capture very short stays due to the survival of the stayers. However, plain averages do not reveal the reasons why people stay or leave, or how durations differ depending on their socioeconomic 'identity'.

Therefore, we take a closer look and differentiate the hazard of leaving. The GSOEP observes whether migrants intent to return or leave Germany within the next 12 months. The panel also enables us to differentiate these intentions according to other characteristics, especially how long they have lived in Germany for. The latter is a crucial variable, as we expect the risk of leaving the host country to decrease the longer immigrants stay. Therefore, we use Kaplan-Meier figures, plotting the proportion of immigrants without return intentions on the y-axis and duration of stay on the x-axis. These figures illustrate that immigrants seem to be more mobile when the distance is shorter and immigration rules are less restrictive. Conversely, immigrants tend to stay longer in general in more recent decades.

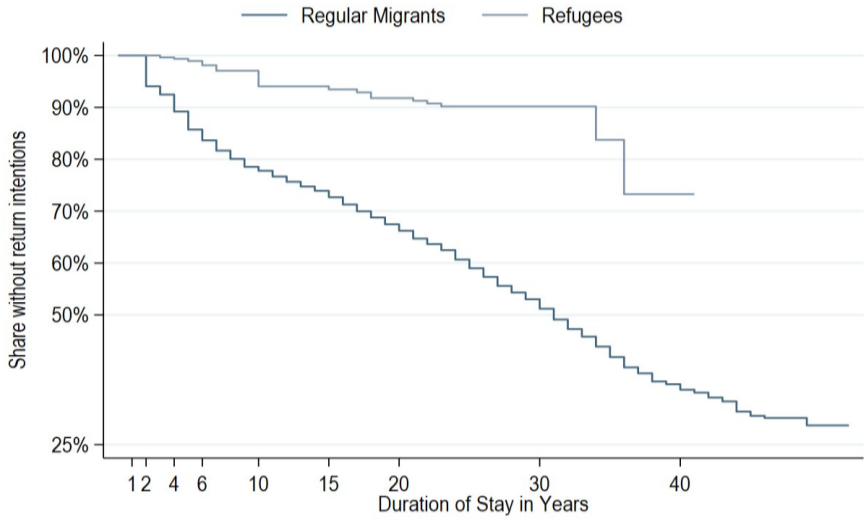
Fig. 4.3 *Kaplan-Meier Curve by decade of immigration (arrival)*



Source: GSOEP v36, own calculations.

Immigrants who have migrated to Germany in the 1990s show a proportional higher hazard to intend to return than those who have migrated after the millennium (see Figure 4.3). This may be due to more immigrant-friendly policies, a better economic situation in Germany, or a changing composition of immigrants over the last three decades. Especially the share of refugees in the sample show diverging hazards as Figure 4.4 shows. Refugees or asylum seeker tend to have lower hazard rates compared to 'regular' migrants. The very same is true of immigrants from countries facing an ongoing armed conflict according to the UCDP (not shown in a figure).

Fig. 4.4 *Kaplan-Meier Curve by region of origin*

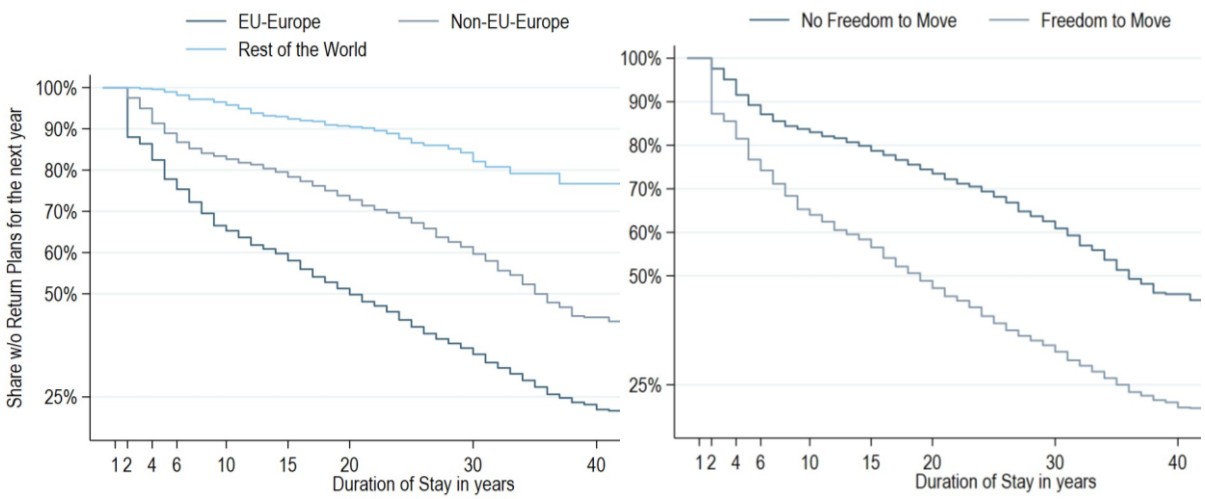


Source: GSOEP v36, own calculations.

Figure 4.5 categorises migrants into three regions based on their origin: EU Europe, non-EU Europe, and the rest of the world. The Kaplan-Meier curve for migrants from EU Europe decreases most sharply over time, indicating that people from the EU are the most mobile. More than one third

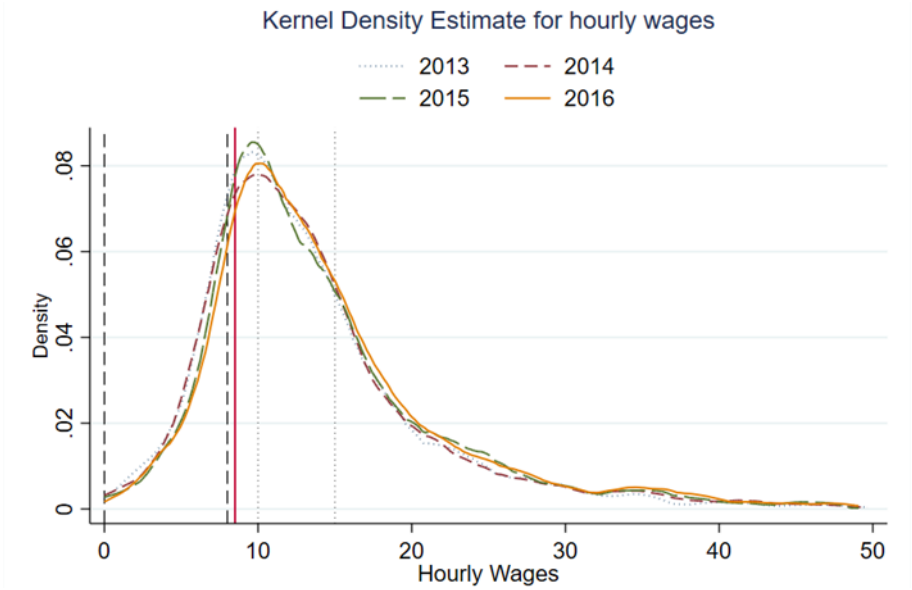
of EU immigrants have concrete plans to emigrate after ten years of residence in Germany. By contrast, emigration plans from non-EU Europeans and the rest of the world are much rarer. The difference between the EU and non-EU Europe shows that distance alone is not a sufficient explanatory variable for return migration intentions. Additionally, entry restrictions between countries appear to influence return intentions. Therefore, we categorise European immigrants according to the policy restrictions they encountered upon entry, i.e. the freedom to move was introduced many years after EU accession (coded manually). EU migrants who have benefited from the freedom to move tend to be more mobile in both directions. One reasonable explanation might be that the "costs" of migration decrease.

Fig. 4.5 *Kaplan-Meier Curve by region of origin*



Source: GSOEP v36, own calculations.

Fig. 4.6 *Distribution of wages of immigrants*



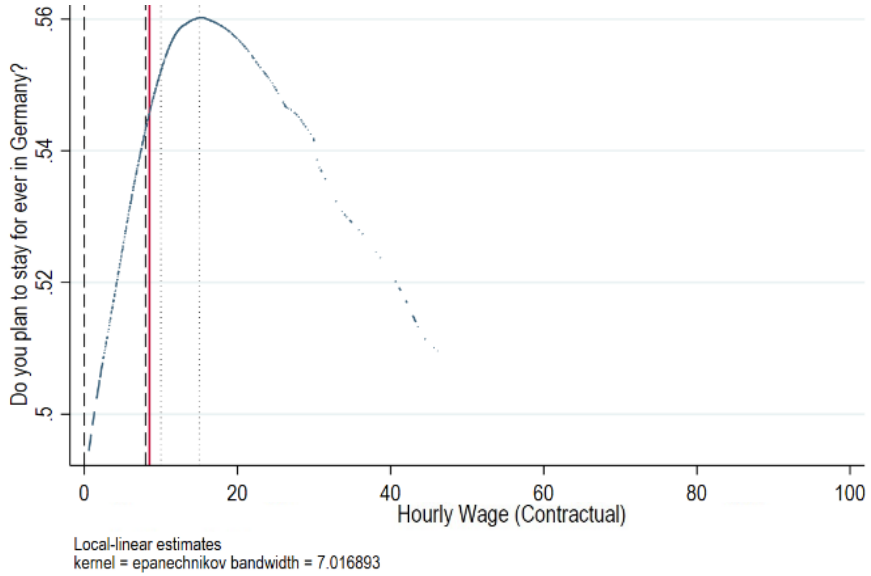
Source: GSOEP v36, own calculations.

Before we explore the link between return intentions and the wages of immigrants, it is worth considering what immigrants actually earn in Germany. In 2014, the mean hourly wage for immigrants was 13.60 €/h, while the median was 9.20 €/h. This increased to 16.09 €/h in 2019 (+18%), with a median wage of 11.16 €/h (+21%). Meanwhile, the mean hourly wage of native (non-migrant) employees was 16.20 €/h in 2014, while the median was 13.92 €/h. This increased to 19.32 €/h in 2019 (+19%), with a median of 16.50 €/h (+19%). The mean wage gap between migrants and native (non-migrant) employees was around 20% (19%) in 2019 (2014). The median wage gap was about 48% (51%) in 2019 (2014), i.e. it decreased by 3 percentage points.

Figure 4.6 illustrates the density function of hourly wages among immigrants in Germany between 2013 and 2016. The highest density is observed around the median wage, while the distribution shows the typical long tail towards the upper end. These selected years will be important later on, when we analyse the introduction of the minimum wage (8.50 €/h) in 2015 to identify the link between wages and return intentions. For now, we observe that the distribution of wages shifts to the right for lower wages after the introduction of the minimum wage, indicating a significant increase in wages. Furthermore, the long tails towards higher wages necessitate robust standard errors or the exclusion of the highest earners in the upcoming estimations.

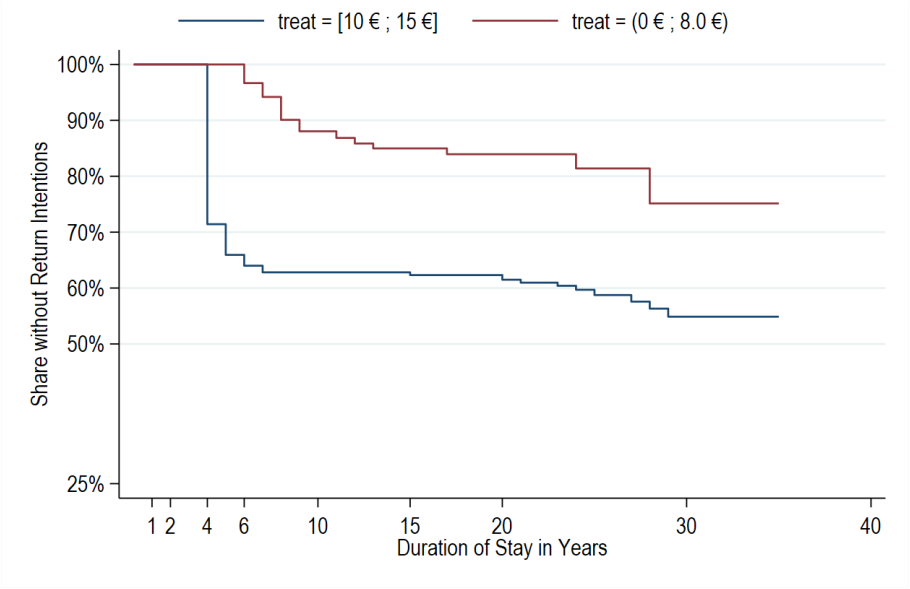
But are wages and the intention to return home related, at all? A local linear regression provides initial insights into this question (see Chapter Local Linear Regression for a methodological introduction to local linear regression). The estimates of the mean function present the relationship between hourly wages and the plans to stay in Germany permanently, without control variables (Figure 4.7). We observe a bell-shaped curve relating (hourly) wages and the plans to stay in Germany forever, peaking at around 20 €/h. At this point 56 % of immigrants intend to stay in Germany permanently in the mean. With the lower wages, this figure decreases to around 50 %. As the density function has already shown, most observations lie between 0 €/h and 20 €/h. In this section, the curve increases, i.e. higher wages correlate with a higher probability of staying permanently.

Fig. 4.7 *Mean function of wages on the plan to stay permanently*



Source: GSOEP v36, own calculations.

Fig. 4.8 *Kaplan-Meier Curve by Treatment Groups of Minimum Wage*



Source: GSOEP v36, own calculations.

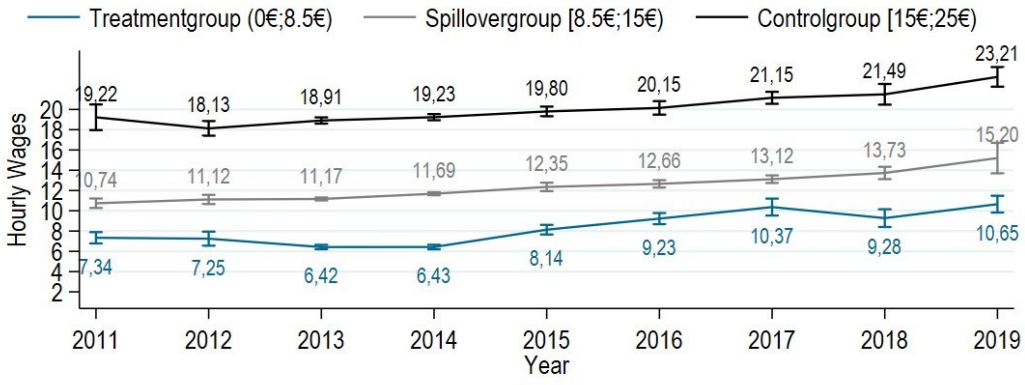
These phenomena can also be illustrated from a different perspective using a Kaplan-Meier curve and the introduction of the minimum wage. The advantage of this approach is that it considers the duration of stay within the model. In this case, the y-axis represents the survival function of immigrants with no plans to return within the next year. Figure 4.8 shows two groups. One group earned less than the minimum wage (<8 €/h) before its introduction. The second group earned ≥10 €/h and ≤15 €/h. The minimum wage was introduced in Germany in 2015. It amounts to 8.50 €/h. Individual hourly wages are calculated using survey data on gross monthly salary and weekly contractual working hours. This may result in moderate imprecise hourly wages. Therefore, group

categories are more broadly to ensure a clear distinction between the treated and non-treated groups (see Schroder et al., 2020, for a more detailed discussion of this issue). Figure 4.8 plots the survival curve for the subsequent years after the introduction of the minimum wage, depending on the duration of stay. We can see that immigrants who were affected by the introduction of the minimum wage (directly, as they earned less than it) have lower hazard ratios than immigrants who earned slightly above the minimum wage beforehand. This is merely a before-and-after comparison of two groups, with no controlling variables. However, it shows the same relationship as the local linear regression estimates. Within the low wage sector, a higher wage is correlated with a lower likelihood of leaving Germany (or a higher likelihood of staying permanently).

Description of minimum wage on return intentions

The descriptive results so far motivate further examination of the introduction of minimum wages as an exogenous event in order to observe the impact on the return intentions of immigrants. The method aims towards a Difference-in-differences setting using a Two-Way Mundlak Regression. The principles remain the same in terms of the crucial assumption compared to a standard Difference-in-differences approach. This chapter will take a descriptive look at two major assumptions. Firstly, it will examine whether the introduction of the minimum wage has led to a significant increase in wages, as suggested by Bachmann et al. (2022). This is necessary to demonstrate the relevance of the policy to our research. Secondly, it will examine whether a common trend can be observed between the different groups before the intervention took place. This assumption will also be tested later on. For now, we will form an initial impression of whether the idea works.

Fig. 4.9 *Hourly Wage by Treatment Groups*

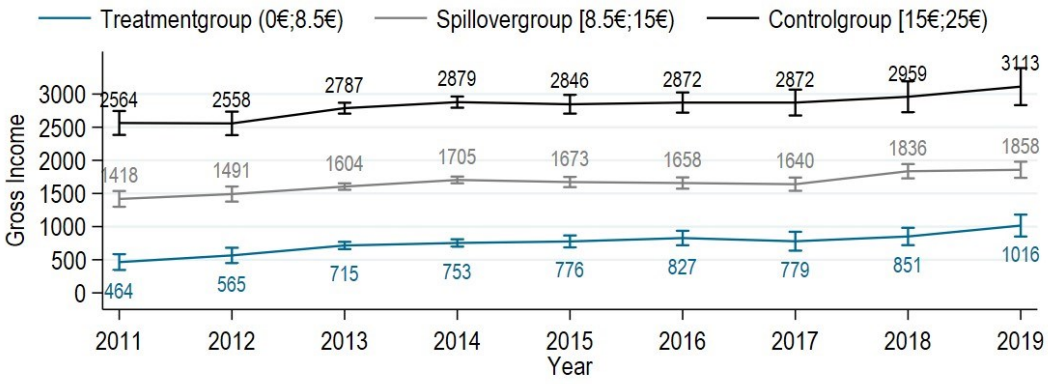


Source: GSOEP v36, own calculations.

It sounds both crucial and straightforward. We want to investigate the relationship between wages and the intention to return. Therefore, introducing a minimum wage may impact the income of those being treated. Figure 4.9 shows increasing wages for all groups. In 2015, the treatment group

experienced relatively large wage growth compared to the spillover group or control group following the introduction of the minimum wage. This extraordinary wage growth ended in 2017.

Fig. 4.10 *Gross Income by Treatment Group*

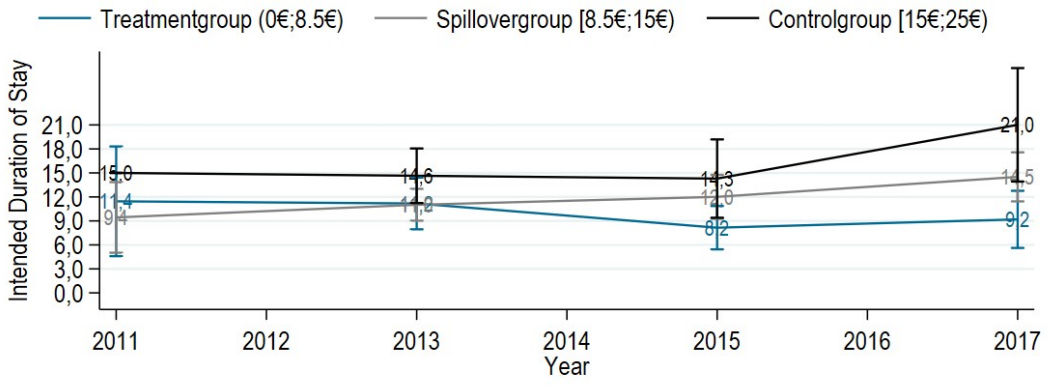


Source: GSOEP v36, own calculations.

However, hourly wages do not tell us whether the gross incomes of those affected have increased. This is particularly pertinent given that recent research shows that hours worked have decreased due to the minimum wage, with only a slight increase in gross income (Bachmann et al., 2022; Biewen et al., 2022).

Figure 4.10 also indicates a weaker increase in gross income for treated individuals. However, we can see that there has been a change in wages and incomes for immigrants affected by the introduction of the minimum wage.

Fig. 4.11 *Intended Duration of Stay by Treatment Group*

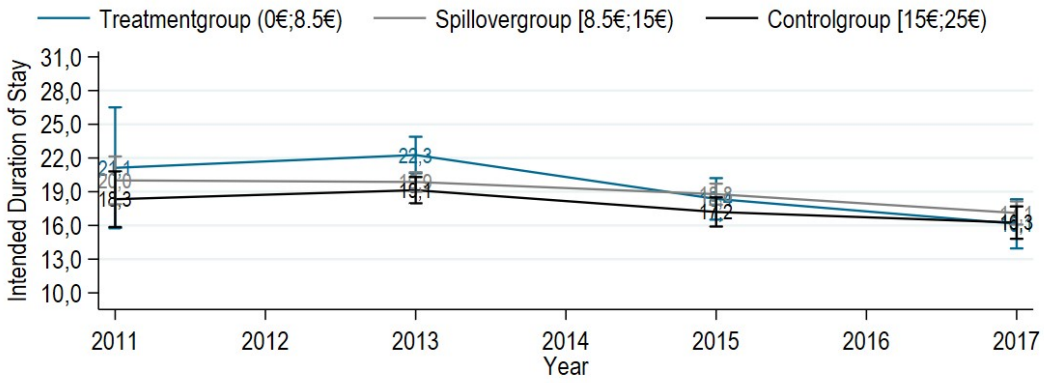


Source: GSOEP v36, own calculations.

Regarding the second crucial assumption, the figures suggest that a common trend cannot be rejected. Nevertheless, things are not that obvious, as all groups are very close to each other with overlapping standard deviations. Figure 4.11 and Figure 4.12 show the raw trends in the intended

duration of stay. Figure 4.12 includes those who intend to stay permanently, using the time until they retire as a proxy for their continued presence in the labour market. Hence, the intended duration of stay is longer compared to Figure 4.11. The null hypothesis may be rejected if there is a significant deviation from the corresponding group's trend. So far, we have observed only slight deviations which do not seem to be significant.

Fig. 4.12 *Intended Duration of Stay incl. those who want to stay permanently by Treatment Group*



Source: GSOEP v36, own calculations.

The description shows that there appears to be a link between wages and intentions to stay. Local linear regression and duration analysis using Kaplan-Meier curves revealed this link. Conversely, the impact of minimum wages on wage increases and subsequent intended duration of stay is less obvious. But these results are descriptive at this stage. The minimum wage has increased the wages of low-paid immigrants, and the general trend cannot be rejected so far. In the next step, we will further exploit the introduction of the minimum wage as an exogenous event to uncover the link between return migration and wages, applying a Two-Way Mundlak Regression.

Two-Way Mundlak Results

Before we examine the results, we will check the numbers in our sample specifically for estimation, i.e. the individuals observed in each group. The treatment and control groups are defined so that people receive a given wage within the boundaries of their respective groups for two consecutive years (2013 and 2014). This reduces the effect of natural upward wage mobility, which is stronger in lower wage groups and could lead to significant but false results. Furthermore, this method ensures persistent groups, which is particularly important for the control group, as they should never be treated at all.

Table 4.2 *Number of observations by Treatment Category*

Survey Year	(0€;8.5€)	[8.5€;15€)	[15€;25€)	.	Total
2010	46	136	88	2,133	2,403
2011	61	176	116	2,278	2,631
2012	63	197	123	2,172	2,555
2013	211	604	294	4,259	5,368
2014	211	604	294	3,958	5,067
2015	211	600	291	4,913	6,015
2016	196	554	266	9,925	10,941
2017	173	469	231	12,239	13,112
2018	151	420	206	11,634	12,411
2019	130	371	181	10,305	10,987
Total	1,453	4,131	2,090	63,816	71,490
Individuals	211	604	294		

Source: GSOEP v36, own calculations.

However, designing the groups in this way reduces the number of individuals in the sample (see table 4.2, Column 5). We implicitly exclude all individuals with varying wages across group boundaries, as well as those with missing wages in one year and unemployed migrants in one of the two relevant years (2013 and 2014). Nevertheless, a sufficient number of observations are still available for the proposed regression analysis. However, heterogeneity analysis and some of the more consuming robustness analyses are not possible.

Our modified Difference-in-Differences estimation strategy compares the development of return intentions among individuals who earned less than the minimum wage before its introduction with those who earned more than the minimum wage. Table 4.3 provides estimators of the marginal effects of the interaction term in two-way Mundlak estimates. This is the average treatment effect on the treated (ATT) – the effect of introducing the minimum wage on return intentions.

Table 4.3 *Estimation Results of Two-Way Mundlak*

	(1) Stay Permanently [0/1]	(2) Intended Years to Stay	(3) Intended Years to Stay Incl. Plans to stay permanently
Treatment Group (0€;8.5€)			
2015	0.054	-3.76	3.01**
	(0.047)	(5.61)	(1.49)
2017	-0.036	-14.65**	1.39
	(0.063)	(6.61)	(2.47)
Spill-Over Group [8.5€;15€)			
2015	0.014	-0.85	0.49
	(0.028)	(3.24)	(0.72)
2017	-0.011	-9.43*	3.13
	(0.057)	(5.68)	(2.37)
Education	x	x	x
Labour Force Status	x	x	x
Observations	3,707	232	2,631
Individuals	1,109	151	1,061
R^2	0.39	0.23	0.06
Common Trend Test (p-Values)	0.175	0.230	0.124

Note: The table shows marginal effects. Two-Way Mundlak Regression with * p < 0.1, ** p < 0.05, *** p < 0.01. Standard error in parenthesis. Control variables include as well year fix effects, group fix effects, interactions of control variables. No weights are used. Robust standard errors clustered at the individual level are used. Source: GSOEP v36, own calculations.

Generally, we cannot identify any significant long-term effects of wages on the intention to stay longer or shorter. Column (1) presents the result for the binary outcome of whether or not immigrants wish to stay in Germany permanently. There are small effects which are not significant. Neither for the treatment group nor the control group. However, we find a significant and relatively large negative effect on the intended duration of stay within the spill-over group (Table 4.3, Column 2). However, the subpopulation, especially for the estimated ATTs, is quite small, resulting in low-powered estimates. In 2015, the Spill-Over Group estimates relied on 35 individuals, whereas by 2017 this figure had fallen to 30 (Table 4.3, Column (2)), making them rather sensitive to changes alongside growing N. It may therefore be worthwhile considering the interpretation for that particular group. The introduction of the minimum wage substantially impacted the intended duration of stay, decreasing it by 14.65 (9.43) years for the treatment (spill-over) group by 2017.

As people are first asked if they wish to stay in Germany permanently (1), and subsequently, if not, how many years they wish to remain (2), the analysis is currently divided into two subsamples. However, I have decided to merge these two groups based on an educated guess in economic terms. The risk of leaving the country is directly connected to the risk of leaving the labour market. In economic terms, I could assume that staying permanently means intending to work in Germany until retirement. Hence, the resulting variable combining both groups is the expected number of years working for the German economy (see column 3). In this way, we implicitly weight the intended duration of stay for younger migrants more highly than for elderly migrants among those who wish

to stay permanently. Therefore, their intended duration of stay is equivalent to the retirement age of 67 minus their actual age.

Column (3) in Table 4.3 shows the results. The combined outcome variable reveals a significant short-term increase of 3.01 years in 2015, driven by younger migrants who now wish to remain permanently (see columns 1 and 3). However, this effect diminishes over time and becomes non-significant by 2017, just two years after the introduction of the minimum wage.

So far, estimates do not show any long-lasting effects of wages on return intentions. In 2015, a short positive response was observed due to the introduction of the minimum wage. Less important, but worth mentioning: There are no spillover effects either.

Robustness Checks

In the next step, results will be testes and checked whether they are robust to different tests and specifications. Neither the threshold defining the wage groups nor a variation of the outcome variable or the way I calculate wages nor a different identifying method changes the general interpretation of the results.

First, however, I need to test the common trend assumption, as this is crucial for the validity of the estimates. This means that, in the absence of the treatment (minimum wage introduction), the control group and the treatment group may follow a common trend. I cannot test this event as it has never happened, but I can hypothetically shift the introduction of the minimum wage. For the common trend to hold, the result must not differ significantly from zero. In other words, I will test the hypothesis that the interaction between the treatment group and the years 2011 and 2013 is zero ($w_{it} = d_i \cdot f_{t=2011,2013} \stackrel{\text{def}}{=} 0$). The null hypothesis of the **common trend** test cannot be rejected (see the last row in Table 4.3). This supports our assumption that the common trend holds.

Table 4.4 *Estimation Results of Two-Way Mundlak with Split at 8 €/h*

	(1) Stay Permanently [0/1]	(2) Intended Years to Stay	(3) Intended Years to Stay Incl. Plans to stay permanently
Treatment Group (0€;8€)			
2015	-0.040	-9.23	0.58
	(0.057)	(5.96)	(1.39)
2017	0.061	-14.94*	-0.38
	(0.073)	(7.60)	(2.19)
Spill-Over Group [8.5€;15€)			
2015	0.006	-0.77	-0.41
	(0.026)	(3.10)	(0.58)
2017	0.044	-4.49*	2.12
	(0.060)	(6.98)	(1.97)
Education	x	x	x
Labour Force Status	x	x	x
Observations	2,729	232	2,631
Individuals	1,075	151	1,061
R^2	0.32	0.24	0.06

Note: The table shows marginal effects. Robust standard errors clustered at the individual level are used. Source: GSOEP v36, own calculations.

One could argue to be more restrictive on the composition of groups. For instance, we could split groups more broadly away from the minimum wage rate considering measurement errors of the survey leading to interference and violating the SUTVA. A more reasonable split might be 8 €/h in order to exclude minor report errors of the participants in their working hour or wages, i.e. rounding errors. However, the effects remain unchanged compared to the main specification (compare Table 4.4 with Table 4.3). This supports the current group design, as the Hypothesis cannot be rejected that the group design does not lead to interference.

One of the most severe problems of the estimation is the lack of power resulting from few observations of individuals in the treatment group changing their intended years to stay. One reason is that for many migrants the actual working hours is reported but not the contractual, which is the relevant one when it comes to the calculation of the minimum wage. To get **more power for the estimates we use actual working hours** to fill the cells when contractual working hours is missing for one individual in a given year. We could justify this by assuming that many migrants in low wage sector do not understand the question properly anyway in order to differentiate its means. Hence some only answer actual working hours. In this way we gain a couple of observations more. Especially for the outcome variable *Intended Years to Stay* this is a crucial increase (see table 4.5, column (2)). We obtain 174 instead of 151 individuals leading to 22 (13) observations in the subpopulation to identify the ATT for the treated in 2015 (2017). This is more than before, but still not ideal. In general estimates are very close to those of the main specification, except that the Spill-Over Group loses its significance.

Table 4.5 *Estimation Results of Two-Way Mundlak including actual working hours*

	(1) Stay Permanently [0/1]	(2) Intended Years to Stay	(3) Intended Years to Stay Incl. Plans to stay permanently
Treatment Group (0€;8.5€)			
2015	0.044	-5.25	2.41**
	(0.043)	(4.02)	(1.23)
2017	-0.018	-13.21**	0.46
	(0.056)	(5.68)	(1.99)
Spill-Over Group [8.5€;15€)			
2015	0.023	-1.40	-0.41
	(0.027)	(3.08)	(0.66)
2017	0.028	-9.05	3.12
	(0.056)	(5.68)	(2.03)
Education	x	x	x
Labour Force Status	x	x	x
Observations	4,245	272	3,004
Individuals	1,268	174	1,211
R^2	0.38	0.24	0.07

Note: The table shows marginal effects. Robust standard errors clustered at the individual level are used. Source: GSOEP v36, own calculations.

Another way to increase the power of the analysis is to relax the way the treatment and control groups are defined. Due to the natural wage conversion of low-paid workers, we have set wage constraints for two consecutive years. However, if we relax this constraint and use only the 2013 wages to define the treatment and control groups, the number of observations doubles. By ignoring the natural mean wage conversion, we would expect to see more wage increases in the treatment group than would be purely expected by the minimum wage. As some of these increases may be non-exogenous, the results would presumably violate the SUTVA, but would provide more power. The inference of endogenous wage conversion towards the median wage diminishes the effect in size and significance (see table 4.6). Endogenous wage growth induced by mean wage conversion does not appear to extend the intended length of stay for immigrants.

Table 4.6 *Estimation Results of Two-Way Mundlak defining Groups with 2013 wages merely*

	(1) Stay Permanently [0/1]	(2) Intended Years to Stay	(3) Intended Years to Stay Incl. Plans to stay permanently
Treatment Group (0€;8.5€)			
2015	0.014	-3.89	0.15
	(0.034)	(3.68)	(1.21)
2017	-0.053	-3.34	-0.42
	(0.040)	(3.69)	(1.46)
Spill-Over Group [8.5€;15€)			
2015	0.021	-1.79	-0.11
	(0.023)	(2.25)	(0.63)
2017	-0.049	-4.90	-1.10
	(0.039)	(3.90)	(1.47)
Education	x	x	x
Labour Force Status	x	x	x
Observations	7,162	433	4,836
Individuals	2,373	307	2,215
R^2	0.37	0.14	0.05

Note: The table shows marginal effects. Robust standard errors clustered at the individual level are used. Source: GSOEP v36, own calculations.

Demean control variables do not change the results either. Results are in the appendix (Table 4.8 in the Appendix).

One more thing worth looking at is real emigration, as far as it can be observed. The GSOEP does not track migrants after they leave, but reports on why people have left the panel. One of the reported reasons is 'Moved abroad [91-93] '. We use this variable to create a dummy indicating whether immigrants are going to move in the subsequent year. As the number of exits for that specific reason is rather small, we add another event to the dummy. If, in the questionnaire, they said they were going to leave Germany within the next 12 months, and the interviewer was unable to reach them the following year, and if they were marked as a *personal dropout with no known reason*, I assume they have emigrated, too. This provides additional 30 to 60 emigrations in the treatment group each year. The main findings remain generally the same (see table 4.7, Column (2)). The estimates are much smaller and hardly relevant, though they may still be sensitive to growing N. The Spill-Over Group emigrated 0.6 percentage points more often after the introduction of the minimum wage in 2017 (significant at the 10% level). The main problem with the real emigration dummy is that hardly any emigrations were observed in the three relevant groups before the introduction of the minimum wage. In fact, only one migrant emigrated in the control group in 2014 (out of 294), and only one migrant emigrated in the Spill-Over group in 2014 (out of 604). This is certainly due to the group design, but observable emigration events remain rare in GSOEP. Therefore, it is better to base the investigation on return plans, which provide a little more power to the analysis.

Table 4.7 *Estimation Results of different Robustness Checks*

	(1) Stay Permanently [0/1], Excluding Missing Vlaues	(2) Observed Emigration [0/1]	(3) Intended Years to Stay Incl. Plans to stay permanently	(4) Cox-Regression
Treatment Group (0€;8.5€)				
2015	0.002	0.002	2.47	1.35
	(0.055)	(0.005)	(1.56)	(2.19)
2017	-0.095	-0.006	0.01	0.78
	(0.083)	(0.006)	(2.39)	(1.50)
Spill-Over Group [8.5€;15€)				
2015	0.009	-0.000	0.01	1.06
	(0.029)	(0.004)	(0.76)	(1.15)
2017	-0.057	0.006*	1.20	1.52
	(0.075)	(0.004)	(2.39)	(2.29)
Education	x	x	x	x
Labour Force Status	x	x	x	x
Time in Germany			**X**	o
Observations	2,729	3,707	2,632	7,982
Individuals	1,075	1,109	1,061	2,084
R^2	0.05	0.06	0.13	0.06

Note: The table shows marginal effects. Robust standard errors clustered at the individual level are used. Source: GSOEP v36, own calculations.

One drawback of our main estimation method so far is that it does not consider the duration at which immigrants have already been at risk, i.e. how long they have been living in Germany. One possibility would be to simply add another control variable while ignoring the challenges of left truncation and right censoring. The alternative would be to be a perform a Duration Analysis using a Cox-Regression which fully incorporates both issue, but is usually not used in DiD settings. The first option is presented in Table 4.7, Column (3). The significant positive effect of the treatment group diminishes, while the estimates generally become smaller. For the second option, we must first define the failure event: Emigration plans serve as events (failure events) in the duration of stay analysis when they occur for the first time. This is based on the annual question 'How long do you intend to stay in Germany?'. For the purpose of this analysis, it is assumed that if the respondent states that they intend to stay in the country for less than two years, they have concrete plans to emigrate. If interviewees could not be reached on multiple occasion and there were indications that they intended to leave the country, it is assumed they had plans to return one year before their departure. The results of the Cox-Regression may be interpreted in a proportional way (Table 4.7, Column (4)). If the coefficient is greater than one, the risk (hazard rate) increases by $(1 - \beta) \times 100\ \%$ compared to the reference group. Conversely, if the coefficient is less than one, the hazard rate is $(1 - \beta) \times 100\ \%$ lower than in the reference group. The results of the Cox-Regression are not statistically significant. The hazard rate tends to be slightly higher in the treatment and spill-over group than in the control group.

Discussion

The results of the multivariate regressions do not reveal a clear link between wages and return migration plans. This section will discuss the results so far. One focus is on whether what we have measured (the Estimator) in combination with the assumptions is what we want to know (the Estimand).

The estimation method using the Two-Way Mundlak Regression still incorporates some **assumptions**: parallel trends in absence of treatment; SUTVA (no endogenous change of the individual with respect to the treatment, no inference); salience to the treated (awareness, that their wages have increased). The minimum wage was widely debated before its introduction. Consequently, even less informed citizens had at least heard about its introduction. Furthermore, it was a key election promise by the Social Democrats. Moreover, hourly wages have increased for workers in the low-wage sector (Bachman et al. 2022). Overall, we might conclude that even migrants were salient about the introduction of the minimum wage or at least of their own increased wages. Another crucial point is ensuring that there is no endogenous change in individuals with respect to the treatment occurs, i.e. individuals swap between being treated and not being treated. Prior to the minimum wage introduction in 2013, we predefined treatment and control groups and do not allow changes in the treatment groups. Furthermore, we have carefully selected the Two-Way Mundlak regression to address recent issues with Two-Way Fixed Effects Models (see Goodman-Bacon 2021; Chaisemartin and D'Haultfoeuille 2020). Two-way Mundlak regression ensures a stable and robust control group, and more importantly, the control group is not treated at all. This is fine as long as we believe that migrants who earn considerably more than the minimum wage are unaffected by it in terms of their return migration plans. So far, the effect of wage compression has been more dominant than the wide-ranging spillover effects (Bossler and Schank 2023; Biewen, Fitzenberger and Rümmele 2022). The crucial assumption of parallel trends could not be rejected. We showed parallel trends with figures and tested this assumption. Moreover, we can argue that, from an individual perspective, the introduction of the minimum wage is an exogenous event that increases wages, as the influence of a single individual is very limited in the context of the introduction of the minimum wage.

The estimation results do not indicate a strong correlation between wages and return migration plans. The significant results of the main analysis could not be reproduced during the robustness checks. A slight shift in the hourly wage from 8.5 €/h to 8 €/h in 2013, which defined the control and treatment groups, caused the significance to vanish. Excluding those earning between 8 €/h and 8.5 €/h in 2013 and 2014 is enough to change the results for the intended years to include those who plan to stay permanently.

Increasing the power of the analysis by relaxing the group definition to a single year eliminates any significance in all outcome variables. We find neither a significant positive nor negative link between wages and return migration plans.

The results have some limitations: Firstly, the control groups may diminish the effect, as we observe slightly concave plans to stay permanently in the host country with respect to wages (see Figure 4.7).

Secondly, employers' behaviour may have changed, which could also bias the results. This is especially likely if the treated group lose their wages. However, the employment effects of minimum wages are small and are mainly driven by a decline in marginal employment (Caliendo et al., 2018).

Thirdly, the **power** of the regression remains to be the greatest challenge of the analysis. Even though the number of observations initially seemed to be promising at the first glance. Profound estimates require multiple conditions on the data due to the method, and the event, which diminished the number of observations in the sample and subsequently the groups. For instance, to rule out mean conversion of wages, migrants had to earn in two consecutive periods within the boundary of the corresponding group. Otherwise, they were excluded. Moreover, the general design requires three groups in two time periods (pre and post). This means, migrants had to employed, know their gross salary and contractual working hours, and be observed before and after the introduction of the minimum wage. Overall, the power of the estimates is substantially reduced, allowing only a cautious interpretation. The most powerful specification is to use the intended years to remain in Germany and measure its deviation upon the introduction of the minimum wage (Table 4.3, Column (3)). However, the positive and significant effect diminishes when the length of stay in Germany is taken into account as control variable or when actual working hours are incorporated.

Fourthly, increases in gross income remain lower than expected due to the introduction of the minimum wage, as working hours have been reduced. As our results rely on an increase in wages rather than gross income, it is reasonable that the effects can hardly be measured. Heterogeneity analyses are needed, which could easily be conducted with a larger sample size.

Apart from these drawbacks, the results are valid for low-wage migrants, as high-income migrants are not affected by minimum wages. Furthermore, the regression focuses on return migration plans rather than the decision to leave or stay. However, Boockmann et al. (2022) discovered a strong correlation between planned and realised duration of stay through a global social media survey of former migrants. Furthermore, plans provide expectations on duration, whereas the decision to leave or stay is binary, with cause and effect potentially occurring far apart in time. Expectations usually react more instantly, making them easier to identify in the short term.

Conclusion

This paper provides a few key takeaways. Firstly, it demonstrates that Germany is one of the most popular destinations for migrants among OECD countries. Secondly, the numbers of inflows and outflows are strongly correlated over time, i.e. an increase in inflows is followed by an increase in outflows in subsequent years. Thirdly, Kaplan-Meier curves show that the hazard of migrants leaving the country has slightly reduced when comparing the last three decades of migration in Germany with the GSOEP. Refugees demonstrate a low likelihood of leaving their host country while an armed conflict is ongoing in their home country. The freedom to move within Europe has increased mobility in both directions. More people immigrate and consequently emigrate again. The hazard of leaving has increased due to the freedom to move; trial and error may be a possible explanation, since the cost of migration has reduced. Fourth, the wage gap between migrants and natives in Germany was around 20% in 2019, using average hourly wages, and 48% using median hourly wages.

Fifthly, there seems to be a positive correlation between wages and the intention to stay permanently in Germany among migrants earning less than the median wage. However, I could not verify this positive correlation using Two-Way Mundlak Regression to exploit the introduction of the minimum wage. The results show no significant effects. They demonstrate that an absolute wage shock does not alter return intentions (plans) within the low-wage sector. Migrants do not tend to adjust their expected duration of stay in response to wage changes. Our results cannot support either the target earners motive or the life-cycle explanation using the data and empirics we have used.

Marginal wage changes certainly play a minor role, if any, in the expected duration of stay. For most migrants, this decision may involve more than simply making an income-maximising choice. Constant and Massey (2003), as well as Boockmann et al. (2022), found that return migration was strongly influenced by economic and social ties. This paper contributes to this body of work by demonstrating that marginal wage changes in the destination country play a minor role. Gibson and McKenzie (2011) also find that income maximisation plays a limited role. They have focused on the highly skilled, whereas we have focused on low-income earners.

Future research could provide more powerful analysis to obtain more robust estimates on return migration intentions. The IAB is developing the promising International Mobility Panel of Migrants in Germany, which will soon provide more reliable data. Furthermore, the significant wage gap between migrants and non-migrants requires closer analysis in the future.

We conclude that marginal wage changes are probably just not relevant enough in the context of such a complex decision. In economic terms, career expectations have not yet been considered and may also play an interesting role. However, this cannot yet be properly measured and will have to be left for future research.

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Table 4.8 *Estimation Results of Two-Way Mundlak with demeaned control variables*

	(1) Stay Permanently [0/1]	(2) Intended Years to Stay	(3) Intended Years to Stay Incl. Plans to stay permanently
Treatment Group (0€;8.5€)			
2015	0.055	-3.76	3.05**
	(0.047)	(5.61)	(1.50)
2017	-0.036	-14.65**	1.39
	(0.063)	(6.61)	(2.47)
Spill-Over Group [8.5€;15€)			
2015	0.014	-0.85	0.50
	(0.028)	(3.24)	(0.72)
2017	-0.011	-9.423	3.13
	(0.057)	(5.61)	(2.37)
Education	x	x	x
Labour Force Status	x	x	x
Observations	3,707	232	2,632
Individuals	1,109	151	1,051
R^2	0.38	0.23	0.06

Note: The table shows marginal effects. Robust standard errors clustered at the individual level are used. Source: GSOEP v36, own calculations.