Minimum Wage Effects in Germany and Europe – Four Essays

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Potsdam, March 2022 Linda Wittbrodt

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Chapter 1

General Introduction

1.1 Motivation

After a long lasting debate and as one of the last European economies, Germany introduced a nationwide statutory minimum wage in 2015.[1] It was initially set at a level of €8.50 gross per hour (see MiLoG §1). Since the law stipulated very few exemptions, the new wage floor was binding for nearly all dependent employees. At the time, this meant over 37 million workers. Being set at a rather high level, the wage floor implied a wage increase for a substantial number of people: According to the Federal Statistical Office (Destatis, 2016a) it affected about 4 million employees who earned less than €8.50 in 2014, amounting to 10.7 % of workers (see also Table 2.1).[2]

The minimum wage reform was preceded by the election of the German parliament ('Bundestag') in September 2013, which led to the constitution of a *great coalition* formed by the centre-right Christian Democratic Union (CDU), the Christian Social Union (CSU) as well as the centre-left Social Democratic Party (SPD). One of the conditions to agree to the coalition formulated by the SPD was the introduction of the minimum wage. Consequently, it was decided on in the coalition contract in November 2013, where it is explained with the necessity to provide workers with a minimum protection in light of the increasing reduction of collective bargaining coverage (CDU, CSU and SPD, 2013). In this rather brief explanation possibly

[1] The corresponding law was called Mindestlohngesetz (MiLoG) and was enacted in August 2014, see `https://www.gesetze-im-internet.de/milog/`, last accessed on February 10, 2022.

[2] However, due to data issues, precise ex-ante estimations were difficult, such that – depending on the source and calculation methods – the expected number of minimum wage beneficiaries was disputed. See for example Brenke (2014); Falck *et al.* (2013); Kalina and Weinkopf (2014); Lesch *et al.* (2014).

one thing does not become clear: The introduction of the minimum wage was the finale of a long-standing debate, which had already been held in the early 2000s. Before that, bargaining coverage was high and not even unions promoted a statutory national minimum wage, since they feared that would undermine their importance (Bispinck and Schulten, 2014).[3] However, this changed when the incidence of low wages increased. Accordingly, one argument of the minimum wage proponents was the reasoning that the low-wage sector had grown too much, that workers needed support where union coverage was low or where unions were not effective in negotiating higher wages (Bispinck *et al.*, 2004).[4] The issue of the erosion of the system of collective bargaining was brought up, arguably coinciding with reduced career advancement opportunities of low-wage employees. A minimum wage was seen as a tool to counteract the uneven power distribution on the German labour market (Bosch, 2007). Additionally, it was expected to promote fairness and potentially increase motivation and productivity and reduce dependency on social transfers (Bosch and Weinkopf, 2013; Kalina and Weinkopf, 2014; Heumer *et al.*, 2013). In the press release accompanying the minimum wage introduction in 2014, the Federal Minister of Labour and Social Affairs Andrea Nahles even expressed fears that social cohesion could be jeopardized if too many employees worked for too low wages and were thereby impeded from participating in the positive economic development (BMAS, 2014).

However, as Bispinck and Schulten (2014) point out, the political decision for the minimum wage introduction was taken despite the opposition of a considerable portion of both German employers and economists.[5] This was because opponents argued that a minimum wage would cause job losses. It was expected that especially those suffering from low wages, such as unskilled workers, women, employees in small firms or East German workers would be let go (see for example Brautzsch and Schultz, 2013; Brenke and Eichhorst, 2007; Kalmbach, 2007; SVR, 2013). Shortly before the minimum wage introduction, some studies estimated that with a wage floor of €8.50, job losses could range from 500,000 to 900,000 jobs in the long run (see for example Müller and Steiner, 2013; Knabe *et al.*, 2014). Additionally, it was argued

[3] For an overview of the complex interplay between unions, political parties, employers and economists before the minimum wage introduction see also Kitagawa and Uemura (2015).

[4] It was even argued that Germany gained wage cost advantages by the rise of the low-wage sector, which were detrimental to other countries in the euro area and partly caused current account deficits and employment issues for them (Kromphardt, 2014).

[5] Yet, Manning (2013) also argues that by 2012 some major international organisations that had traditionally opposed minimum wages had already changed their opinions on the grounds that it might raise labour force participation (see also ILO *et al.*, 2012).

that minimum wages would not reduce the risk of poverty, which was seen to be especially driven by having children, being unemployed or divorced, all of which a minimum wage could not directly affect (Ribhegge, 2008). Moreover, it was also reasoned that a minimum wage would be distributionally inefficient, since the subgroups affected by low wages would not be targeted effectively. For example, young workers were not seen to be in need since they would still have future opportunities to improve their income. Neither were low-wage employees, who often have supplementary jobs and live with higher-income earners (Heumer *et al.*, 2013). Many low-income households, though, receive in-work benefits and due to high withdrawal rates of earned income they would not benefit from a wage floor (Knabe *et al.*, 2014; Müller and Steiner, 2010). In light of the perceived ineffectiveness on the one hand and the large job losses on the other, many critics argued that other tools would be more suitable to benefit low-wage employees, such as changes in the tax and transfer system (Kalmbach, 2007; Ragnitz and Thum, 2008; Straubhaar, 2008).

Yet, minimum wage proponents replied that employment losses were mere hypotheses, based on the theoretical assumptions of the neoclassical model. In the competitive labour and product markets that are assumed in the neoclassical model, a universal minimum wage higher than the equilibrium wage increases the marginal cost of production. As a result, the price of the output good increases while demand for it is reduced. Accordingly, production falls. Moreover, since labour is relatively more expensive, it is substituted with capital, causing labour demand to decrease (Neumark and Wascher, 2008). However, proponents of the minimum wage argued that the assumptions of the neoclassical model such as perfect competition would not apply in reality, which is why the expected negative employment effects would not necessarily manifest themselves (Kromphardt, 2014). Another model that is often mentioned in this context and that could explain a lack of employment effects or even an increase in employment is that of a monopsony, where a monopsonist (a single firm) is responsible for the labour demand, while the labour supply is provided by various identically paid workers. In this setting, the firm sets wages such that the marginal cost of labour equals the marginal revenue product of labour, which leads to lower employment than the competitive model. Accordingly, a moderate minimum wage (being set at no more than the wage in a competitive setting) can cause employment to increase (Neumark and Wascher, 2008). However, these are theoretical considerations and the actual effects dependent on the prevalent market structure and its specific form. Accordingly, it is insightful to take a look

at the international empirical research, even though both critics and proponents argue that transferring findings from other countries is not necessarily feasible (Ribhegge, 2008).

Internationally, the minimum wage research has a long standing tradition, which resulted in the "new minimum wage research" in the last decade of the 20^{th} century. While the positive minimum wage effects on the wage distribution and wage inequality are not particularly disputed[6], the effect on employment is somehow more controversial. Accordingly, the academic debate on whether or not negative employment effects of minimum wages can be credibly identified – and for which subgroup – has been intense, especially for the United States of America (US) (for summaries see for example Belman and Wolfson, 2014; Neumark and Wascher, 2008).[7] Differences in the results possibly stem from studies having implemented different identification methods with a large range of data, for various outcome measures, regions, subgroups, sectors and time periods. Yet, most research finds no or at most modest evidence for a negative effect on employment (Doucouliagos and Stanley, 2009; Dube, 2019; Wolfson and Belman, 2019). Similar conclusions are drawn by a variety of studies from the United Kingdom (UK), but also other countries.[8] However, there is also evidence suggesting stronger negative or at least ambiguous effects.[9] Accordingly, the debate on how to decrease the uncertainty, especially about larger minimum wage increases or new introductions, is still ongoing (Neumark, 2019). Moreover, Manning (2021) argues that economists presumably do not dispute that wage floors could cause substantial negative employment effects when being set at too high a level and suggests that it could be time to focus on understanding what impacts this exact tipping point. He even sees this aspect as the next task of minimum wage research.

The effects of minimum wages are thus neither clear-cut in theory nor are they soundly predictable by studying other countries or settings. Accordingly, economists have argued that the introduction of a minimum wage should be accompanied by a thorough evaluation (Arni *et al.*, 2014; Möller *et al.*, 2014). This dissertation follows up on that and contributes to the

[6]See for example Autor *et al.* (2016); DiNardo *et al.* (1996); Dolton *et al.* (2012); Lee (1999); Lemieux (2008); Swaffield (2014); Teulings (2003).

[7]Most of the debate is concentrated on employment effects identified via cross-state variation of minimum wages. See for example the debate between Card/Krueger and Neumark/Washer (Card and Krueger, 1994, 2000; Neumark and Wascher, 2000), but also others (Allegretto *et al.*, 2011, 2017; Cengiz *et al.*, 2019; Dube *et al.*, 2010; Meer and West, 2015; Neumark *et al.*, 2014a,b; Neumark and Wascher, 2017).

[8]For studies on the UK see for example Dolton *et al.* (2010, 2012, 2015); Metcalf (2008); Manning (2013, 2021); Stewart (2004). For other countries see Broecke *et al.* (2017); Chletsos and Giotis (2015); de Linde Leonard *et al.* (2014); Dolado *et al.* (1996); Harasztosi and Lindner (2019); Jiménez Martínez and Jiménez Martínez (2021).

[9]See for example Boockmann (2010); Nataraj *et al.* (2014); Neumark and Shirley (2021).

existing research by identifying and summarizing minimum wage effects in Germany as well as broadening the scope to include a European perspective.

1.2 The German Minimum Wage

In order to gain a better understanding of the minimum wage in Germany, this section will briefly address a few key aspects of the reform, its introduction and enforcement. The minimum wage was introduced at €8.50 and was binding as of 1 January 2015. Its implementation and effects are to be evaluated and monitored by a minimum wage commission established for this purpose. The commission re-evaluates the level of the minimum wage every two years, being tasked to provide worker protection on the one hand and to ensure fair and functioning market competition on the other hand, while at the same time avoiding to endanger jobs (see MiLoG §9). Accordingly, the minimum wage was raised after the first decision of the commission, and was set at €8.84 as of January 2017. Additional increases followed in 2019 and the subsequent years. By July 2022 it is to reach €10.45 (Mindestlohnkommission, 2016a, 2018a, 2020a). However, the minimum wage discussion has gained new momentum with the most recent election of the German parliament in autumn 2021, when the SPD emerged as the largest party. They went into the coalition talks with the demand to raise the minimum wage to €12, backed by the third largest party 'Alliance 90/The Greens' ('*Bündnis 90/Die Grünen*'). Accordingly, the raising of the wage floor was stipulated in the coalition contract both partied concluded with the Free Democratic Party (FDP) in December 2021 (see SPD, Bündnis 90/die Grüne and FDP, 2021). In February 2022, it was announced that the wage floor would be raised to €12 as of October 2022 (BMAS, 2022).[10]

Minimum wage compliance is monitored by the German Customs Authority. The Financial Monitoring Unit for Illicit Employment is tasked with executing inspections of firms and enforcing compliance. When employers fail to comply with the minimum wage they can be punished with fines of up to €500,000. However, according to the Mindestlohnkommission (2020b) inspections are not dense enough and not sufficiently focused on sectors where the statutory minimum wage is relevant. This might contribute to the fact that despite these regulations, evaluations find a substantial incidence of non-compliance. Estimations of the magnitude of that issue differ depending on the data source, the assumptions made and the

[10] At the same time, the threshold for marginal employment, i.e. mini-jobs, will be raised from €450 to €520 in order to enable workers to stay in marginal employment without having to reduce their hours.

time frame, though. Summarising a number of studies, the minimum wage commission finds that in 2018 between 1.3% and 6.8% of employees earned less than the wage floor (Mindestlohnkommission, 2020b). The fact that some workers do not get paid what they are owed might potentially also be caused by a lack of minimum wage publicity. Based on three representative worker surveys conducted between the end of 2018 and the end of 2019[11], Bruttel and Dütsch (2020) point out that while nearly all employees knew that there was a minimum wage (95%), only 18% could determine the exact level at the end of a year it had been increased. About every second worker was able to pinpoint the rate with a range of ± €0.25. Interestingly, and contrary to expectations, among employees earning less than €11, the share of people being able to either exactly or roughly determine the level of the wage floor was lower.

While most employees in Germany are eligible for the minimum wage, the law also stipulates some exemptions. The first has already run out, since it granted a transition period for sectors with pre-existing minimum wages below €8.50, which were allowed to slowly adapt their wage floors until January 2017. Permanent exemptions apply to some long-term unemployed, minors, trainees, specific interns[12] and volunteers (see MiLoG §22). Yet, these exemptions have been disputed beforehand. For example, Amlinger *et al.* (2014a,b) criticize excepting workers because it creates incentives for circumventing the wage floor by exploiting exempted personnel. However, at least for long-term unemployed, Umkehrer and vom Berge (2020) show that the exemption is very rarely used. Overall, Destatis (2016a) estimates that in 2014, 5.5 million jobs were paid below €8.50, with 4 million of them being also eligible to the wage floor. The majority of the exempted workers were trainees, interns and minors. In sum, 10.7% of employees were estimated to have been subject to a wage increase due to the minimum wage introduction in 2015 (see also Table 2.1).

The share of affected employees is one way to measure the 'bite' of the minimum wage, i.e. the degree to which the wage floor cuts into the wage distribution: the higher the share of employees earning less prior to an introduction or increase, the more workers are affected by the minimum wage. It is used as the main bite indicator in this dissertation and henceforth also called '*fraction*'. It is easily computable, focuses on the group of affected individuals and can be adapted in relation to specific subgroups, such as for regions or female employees. However,

[11] The data is provided by Ipsos (2020).

[12] Excluded interns are those with a compulsory internship ('Pflichtpraktikum'), a voluntary orientation or a voluntary accompanying internship ('freiwilliges Orientierungspraktikum' or 'freiwilliges ausbildungsbegleitendes Praktikum') lasting less than three months or an entry-level qualification ('Einstiegsqualifizierung').

it does not account for the distance to the minimum wage threshold and therefore cannot measure how strongly affected employees' wages have to be raised. Another bite measure is the '*Kaitz index*'. It is defined as the ratio of the minimum wage to the average or median wage of employees. Thus, the greater the distance between the wage floor and the centre of the wage distribution, the lower the minimum wage impact and accordingly also the Kaitz index (Garnero *et al.*, 2015). However, due to the fact that it relies on the mean or median wage, it can be affected by movements in other parts of the wage distribution. Additionally, it cannot account for the amount of employees actually earning below or around the wage floor.[13]

1.3 Contribution of this Thesis

This thesis consists of four independent chapters, some of which have already been published. Three of them focus on the minimum wage introduction in Germany while the last widens the scope to include a European perspective. In this section, I will briefly present each chapter, giving a short overview of their motivation, research questions and contributions as well as their main findings. After the general introduction, the thesis continues with Chapter 2, which reflects the German minimum wage reform and summarizes the literature looking at its short-term effects. The third and fourth chapters provide empirical estimations of particular effects of the wage floor, both relying on a regional Difference-in-Difference (DiD) approach. While Chapter 3 focuses on its effect on employment, Chapter 4 estimates the impact on the gender wage gap. Chapter 5 connects to that and looks at gender-specific minimum wage effects in European Union countries. The last chapter summarizes the results as well as the caveats of this thesis and gives an outlook into future research perspectives.

Overall, this dissertation contributes to the research by mapping out the German minimum wage reform and its impacts. As was seen in section 1.1, the international debate on minimum wage effects is still ongoing. Accordingly, adding pieces to the puzzle can help to gain a better understanding of the mechanisms behind the impacts of a wage floor. With Germany being the biggest European economy in terms of gross domestic product[14], this thesis gives important insights into effects of a minimum wage introduced on a large scale

[13]The latter is circumvented by a third measure, the employment spike. It is based on Teulings (2003) and measures how many jobs cluster around the minimum wage. Yet, this measure neglects the fact that employees could be paid below the minimum wage, which can rather be captured by the *fraction* (Garnero *et al.*, 2015).
[14]Source: Eurostat.

and at a comparatively high level. Additionally, by looking at gender-specific effects in the European Union, it expands its scope beyond Germany while at the same time tapping into another policy-relevant topic: female inequality.

Chapter 2: The Causal Effects of the Minimum Wage Introduction in Germany
The second chapter of this dissertation, "The Causal Effects of the Minimum Wage Introduction in Germany – An Overview", is joint work with Marco Caliendo and Carsten Schröder. It has been published in the *German Economic Review*, 2019, Vol. 20(3), pp. 257–292.

The chapter poses an important contribution to the minimum wage research: We summarize the short-run effects of the German wage floor found in the literature, focusing on evidence collected up to three years after the minimum wage introduction. As described in Section 1.1, the reform was rather controversial which is why estimations on its impacts on the whole range of potential outcome variables were widely anticipated. Additionally, due to the rather high rate of the wage floor, Germany serves as an interesting case study also for international researchers, since previous literature often had to rely on low-impact wage floors or incremental changes. Accordingly, we provide a concise overview on the reform and the concerning ex-post literature, aiming to answer the following main research questions: What are the difficulties in estimating the minimum wage effects, especially with respect to the calculation of hourly wages, the suitability of data sources and the measurement of non-compliance? What are the potentially affected socio-economic outcome variables and how are they found to be impacted in causal short-run studies? What conclusions can be drawn from this with respect to the improvement of data sources as well as regarding potential future research avenues? In this sense, we provide a first summary paper for the German minimum wage research. For this dissertation, the chapter also operates as an opening, while the following chapters present more detailed analyses.

First, we provide a summary of the minimum wage legislation, describing exempted subgroups but also the designated process of increases and enforcement. Moreover, we discuss potential outcome variables and how they have been found to be impacted by minimum wages in other countries, such as wages, employment at the extensive and intensive margin, monthly incomes, perceptions of fairness, satisfaction or labour productivity. We briefly summarize the data sources that are most commonly used for German minimum wage research and identify their advantages and drawbacks, applying a special focus on the Structure of Earnings Sur-

vey (SES) and the Socio-economic Panel (SOEP). Finally, we provide a systematic review on the previous causal evidence, identifying whether effects are found to be positive, insignificant or negative. We do this for the whole range of outcomes that had been researched three years after the reform and categorize by the methodological approach the corresponding studies employ.

Our summary of the short-term evidence on minimum wage effects identifies three main results. First, the wage floor has substantially increased wages at the bottom of the distribution. There are some subgroups that have particularly benefited from the reform, among them marginally employed[15], women and employees with a migration background. However, one year after the reform there was still substantial non-compliance, meaning that a sizeable number of eligible employees was paid less than €8.50 gross per hour. Second, there were small negative employment effects, which were mainly caused by a more substantial effect on marginal employment.[16] And last but not least – and contrary to the hopes and expectations –, poverty and inequality were not found to have substantially decreased in the short run. This can be partly explained by the fact that working hours decreased, which is why the higher hourly wages did not translate into greater monthly earnings.

Chapter 3: Short-Run Employment Effects of the German Minimum Wage

The third chapter of this thesis, "The Short-Run Employment Effects of the German Minimum Wage Reform" was written in co-authorship with Marco Caliendo, Alexandra Fedorets, Malte Preuß and Carsten Schröder. It has been published in *Labour Economics*, 2018, Vol. 53, pp. 46–62. This chapter dives more deeply into the employment effects of the German minimum wage reform, providing one of the first estimations in this area. Its research relevance therefore derives from the importance of identifying whether or not the feared job losses had manifested themselves in the short-run. This is also the main research question in this chapter. Additionally, we follow up with subsequent questions: Which type of employment was affected especially? Were mini-jobs transformed into part-time employment or were marginally employed mainly let go? From a chronological perspective, the chapter was written and published before Chapter 2, which is why its results are already entailed in the previously summarized overview.

[15]Marginal employment is a specific type of part-time employment in Germany, defined by earning less than €450 a month. Marginal employees, also called mini-jobbers, have to pay nearly no employee-sided social security contributions and are exempt from income taxation. For more information see also Section 3.2.

[16]See also the recent paper by Dustmann *et al.* (2022), who do not find employment effects.

Since the minimum wage was introduced at the same level nationwide, empirical identification strategies that can be applied to identify effects are limited. In this chapter (as well as in Chapter 4) we employ the regional DiD approach suggested by Card (1992). It makes use of the fact that German regions were affected to different degrees. Due to a variety of reasons, such as structural, environmental or historical ones, wages differ considerably across local areas. The same level of a minimum wage thus does not have the same impact over the whole nation. While it can mean a considerable intervention for the wage distribution in one region, it may affect nearly nobody in another one. Accordingly, the 'bite' of the minimum wage is possibly very different throughout the country (see also Section 1.1). We make use of this idea, arguing that more affected regions should also display larger minimum wage effects after the reform. We use a classification of 141 regions that also accounts for commuter flows and labour market seclusion and construct two commonly used bite measures as our treatment indicator, the regional *Kaitz index* (i.e the relation of the minimum wage to the regional mean wage) and the *Fraction* (the regional fraction of affected employees). In order to do so, we rely on the SES 2014, containing detailed individual wage information but also relevant data on working hours and minimum wage eligibility, enabling us to construct rather precise bite measures. Our approach relies on the idea that wages would have developed similarly across the regions in absence of the treatment. Since the SES does not provide yearly data, we support this claim by testing the common trends before the reform with the SOEP. We first test whether the SOEP and SES data provide comparable bite indicators and then look at pre-treatment trends and possible anticipation effects. These steps also constitute important contributions to the literature: By comparing both data sources, we make the case that future research can indeed rely on the comparatively smaller SOEP when looking at pre-reform regional data. Additionally, we show that there were no considerable anticipation effects, which is relevant to all DiD identification approaches. Finally, comparing different bite measures we give insight into how to measure the degree to which regions are affected. More precisely, we construct indicators on different calculation bases, i.e. hourly wages derived from both actual and contractual working hours as well as monthly wages.

For our employment data we use information provided by the Federal Employment Agency (FEA). We estimate effects on regular employment (part- and full-time) and mini-jobs, the latter either carried out as sole employment or add-on jobs. Additionally, we also estimate effects on regular employment, which we calculate as the sum of both. As the post-treatment

period, we only look at the very short run, including FEA data from 30 June 2015. Our results suggest that the minimum wage caused a slight reduction in overall employment, which was mainly driven by a decrease of mini-jobs. We find that overall employment reduced by about 0.4% or about 140,000 jobs, respectively. For marginal employment, we find a reduction by about 3%, translating into roughly 190,000 jobs. However, we also refer to a few reasons why these numbers should be treated with caution. Yet, the results are robust to a variety of sensitivity tests, such as employing different bite measures or other area classifications.

We further contribute to the minimum wage research in Germany by shedding light on whether marginal employment was lost or simply transformed. In order to do so, we look at dynamics behind the estimated effects by differentiating between sole and add-on mini-jobs. The rationale behind this is that employees with a different primary job could not easily transfer to part-time employment. Since we find only a reduction in sole mini-jobs, whereas add-on mini-jobs were not affected, we argue that contract adaptations to part-time employment were the main reason for a reduction in marginal employment. We find another indication for this in the fact that the reduction in mini-jobs mainly took place immediately after the minimum wage came into effect.

Chapter 4: Effects of the German Minimum Wage on the Gender Wage Gap
The fourth chapter is called "Did the Minimum Wage Reduce the Gender Wage Gap in Germany?". It is joint work with Marco Caliendo and has been published as a CEPA Discussion Paper, 2021, No 40. Additionally, it is currently in a revise and resubmit process at *Labour Economics*.

The motivation of the chapter is based on the fact that women are often found to be overrepresented among low-wage earners. Accordingly, a minimum wage could be of particular benefit to them. If their wages were increased disproportionately compared to men's, this could reduce gender wage inequalities. While this relationship has been discussed broadly, causal evidence for European Union member states is scarce. The contribution of this paper consists in filling that void by providing an estimation of the minimum wage effect on the gender wage gap in Germany with a credible causal identification approach. The case of Germany is especially interesting since the country has a comparably high level of gender wage inequality on the one hand, and a considerably high minimum wage rate on the other hand. Our main research question is whether the minimum wage actually led to a decrease of

the gender wage gap. In order to do so, we identify the effects at the 10^{th} and 25^{th} percentiles as well as the mean of the underlying gender-specific wage distributions. Picking up on the identification strategy used in Chapter 3, we employ a regional DiD approach and interact the regional bite measure with a post-reform variable. In order to better map out the situation of women across the local areas, we construct the bite as the regional fraction of affected female employees in our main estimation.

We use data from the SES 2014 and 2018, aggregating the individual data of eligible employees for 257 labour market regions. In order to check the common trend assumption, we additionally use 2010 data. We are able to confirm that pre-treatment trends were similar and our common trend assumption holds. However, we also elaborate on differences in the data structure of the earlier wave and potential issues resulting from them. As sensitivity tests, we use different bite measures, other regional classifications and different weighting processes. Our results are robust to these alterations and imply that for eligible employees the gender wage gap at the 10^{th} percentile decreased by 4.6 percentage points between 2014 and 2018 in high-bite regions compared to low-bite regions. We estimate this to be a reduction of 32% compared to 2014. Higher up the distribution – i.e. at the 25^{th} percentile and the mean – the effects are smaller and not as robust.

Chapter 5: Gender-Specific Minimum Wage Effects in the European Union

The fifth chapter, "Gender-Specific Minimum Wage Effects - Evidence from the European Union" was written in single authorship and has not yet been published.

This chapter follows up on the previous one by keeping the gender-specific emphasis on minimum wage effects. However, in contrast to the rest of the dissertation, it is not as focused on Germany but widens the scope to include other European Union (EU) member states. It is motivated by the fact that although gender equality is one of the founding principles of the EU, it has mostly not yet been accomplished. Female inequality is still an important issue across the member states and countries are very heterogeneous in this respect. Following the rationale of the previous chapter, women could potentially benefit particularly from a minimum wage. This is also acknowledged by the EU (2021), who argues that the gender pay gap could be reduced by 'adequate minimum wage' across the EU, for which the groundwork was laid by a proposal for a directive of the European Parliament and of the Council (European Commission, 2020). However, women being disproportionately affected

by wage increases could also mean that they are more prone to suffer from the possibly induced job losses or reductions in working hours. Accordingly, this paper contributes to the research by summarizing minimum wage effects on gender equality in labour market outcomes identified by studies with a focus on EU member states. The main research question is thus how minimum wages in these countries are related to the national gender wage gaps. Additionally, it explores the question whether wage floors are found to lead to employment losses or decreases in working hours that particularly affect women. Moreover, the paper aims to give on overview on the current situation of women in the labour market in European comparison.

I find that there are large differences between the member states when it comes to both the relevance of the minimum wage as well as to gender equality on the labour market. For example, the share of EU workers earning at least 10% below the minimum wage rate and at most 10% above it ranged from about 3% to around 15% in 2018. Moreover, gender wage gaps varied from 1 to 22%, the employment gap amounted to 1 to 20 percentage points (pp). Additionally, these measures are interrelated, with a trade-off between lower gender wage gaps and higher female employment rates. Where more women work, this is partly achieved by (usually less paid) part-time employment, which in turn increases the wage gap. With respect to the minimum wage I find evidence that higher wage floors are associated with lower wage gaps. While women do not suffer larger employment losses than men per se, there is evidence for a particular minimum wage impact on part-time employees. Since these are disproportionately female, they seem to be more prone to job losses after all. Additionally, this advises caution with respect to the negative minimum wage relation with gender wage gaps. It cannot be ruled out that it is associated with these low-paid part-time employees losing their jobs. This working arrangement should therefore be specially focused on in the context of minimum wages. However, women were not affected by changes in working hours more often than men.

Chapter 2

The Causal Effects of the Minimum Wage Introduction in Germany - An Overview

The chapter is joint work with Marco Caliendo and Carsten Schröder and has been published in the *German Economic Review*, 2019, Vol. 20(3), pp. 257–292.

Abstract

In 2015, Germany introduced a statutory hourly minimum wage that was not only universally binding but also set at a relatively high level. We discuss the short-run effects of this new minimum wage on a wide set of socio-economic outcomes, such as employment and working hours, earnings and wage inequality, dependent and self-employment, as well as reservation wages and satisfaction. We also discuss difficulties in the implementation of the minimum wage and the measurement of its effects related to non-compliance and suitability of data sources. Two years after the minimum wage introduction, the following conclusions can be drawn: while hourly wages increased for low-wage earners, some small negative employment effects are also identifiable. The effects on aspired goals, such as poverty and inequality reduction, have not materialized in the short run. Instead, a tendency to reduce working hours is found, which alleviates the desired positive impact on monthly income. Additionally, the level of non-compliance was substantial in the short run, thus drawing attention to problems when implementing such a wide reaching policy.

2.1 Introduction

In January 2015, Germany introduced a major labour market intervention: For the first time, a nationwide statutory minimum wage was implemented.[1] It was binding for nearly all 37 million dependent employees and, set at €8.50 gross per hour, it unfolded a substantial 'bite': about 10 to 14 percent of the eligible work force earned less than the minimum wage in the year prior to the reform.[2] The German minimum wage provides an interesting case for international research. While most international minimum wages exhibit either a low bite or changes over time that are incremental (see, e.g. Neumark, 2019), the German wage floor was initially set at a relatively high level. Additionally, in contrast to other minimum wages, its introduction affected nearly the whole population. This is why avoidance measures, such as displacing eligible individuals in favour of exempted workers, were not feasible on a large scale. For these reasons, the reform can give insights into potential minimum wage effects even beyond the German context, where overall wage floor effects can be inferred.

From an implementation perspective, it is crucial to monitor and evaluate the minimum wage. However, this depends upon exact information on hourly wages. Most contracts and paychecks in Germany do not stipulate hourly wages, rather monthly salaries and agreed working hours are specified. The actual working hours, with detailed information on unpaid, paid, and otherwise compensated overtime, is not always available. This is true for documentation within establishments, thus complicating both firms' compliance and governmental controls. It is also true for scientific evaluations, which rely on exact data in order to identify how many people earned less than the wage floor and how their wages developed. In Germany, data that could provide such information is limited and its precision is reduced by various factors, including employers' incentives to report compliance and possible measurement error in employee surveys. Moreover, the information provided in survey or administrative data is often available with a considerable time lag.

So far, however, the available data suggests a substantial amount of non-compliance in the short run. This poses a problem for effect evaluations, since possible impacts of the wage floor might be underestimated if there is non-compliance. It also reflects that the enforcement of the minimum wage regulations was insufficient. Thus, control and enforcement mechanisms must be more clearly regulated and executed, with circumvention strategies precluded. In

[1] Previously, individual sector specific minimum wage agreements existed for specific industries.

[2] Different data sources and estimation strategies lead to differing numbers; see Amlinger *et al.* (2016); Brenke (2014); Destatis (2016a); Falck *et al.* (2013); Kalina and Weinkopf (2014); Lesch *et al.* (2014).

addition to implementation and data issues, the universality of the wage floor also provides a challenge for evaluations since it reduces the toolbox of possible identification strategies. Apart from descriptive evidence, most analyses exploit different intensities of minimum wage exposure (i.e. bite), thereby relying on regional or firm level variation. Another strategy relies on a more standard treatment and control group approach on the individual level, sorting workers by their hourly wages before the reform.

The minimum wage introduction in Germany was preceded by a large debate about potential threats and benefits of a wage floor. On the one hand, advocates stressed positive distributive effects, fairness aspects, and a reduced dependence of workers on social transfers (e.g. Bosch, 2007; Kalina and Weinkopf, 2014; BMAS, 2014). Their arguments are also supported by international empirical literature (see Lee, 1999; Teulings, 2003; Addison and Ozturk, 2012; DiNardo *et al.*, 1996; Autor *et al.*, 2016). On the other hand, opponents emphasized the possible negative effects of the minimum wage, predicting a decrease of employment by 500,000 to over a million jobs in the long run (Bachmann *et al.*, 2008; Müller and Steiner, 2011, 2013; Knabe *et al.*, 2014). Furthermore, achieving the main target of poverty alleviation was also questioned, since many low-income households receive in-work benefits, and withdrawal rates of earned income are high (from 80 to 100 percent). As a result, low-income households would gain nothing more than an increase in the risk of job loss caused by a strong rise of employers' labour costs (Knabe *et al.*, 2014; Müller and Steiner, 2010).[3]

Prior to the reform, Germany already had a number of sector-specific minimum wages in place. In 2013, more than 3 million employees were covered by such a wage floor (Schröder, 2014). Evaluations of these individual wage floors provided one source for predictions of potential minimum wage effects. One of the most comprehensive analyses of sectors is provided by the Federal Ministry of Labour and Social Affairs.[4] As the German Minimum Wage Commission (Mindestlohnkommission, 2016b) points out, overall there are no statistically significant negative employment effects found for most sectors, whereas wage increases are apparent, especially for East Germany (see also Bosch and Weinkopf, 2012; Möller, 2012; SVR, 2013). Yet, negative employment effects are also found for single sectors, specific indicators, and particular time frames (see also Aretz *et al.*, 2013; Schuster, 2013; vom Berge *et al.*, 2013). The

[3]For further criticism see also SVR (2013, 2014). For international literature on negative effects see Neumark and Wascher (2008) or Askenazy (2003).

[4]It entails evaluations of eight sectors, namely the waste industry (Egeln *et al.*, 2011), the main construction trade (Möller *et al.*, 2011), the roofing industry (Aretz *et al.*, 2011), electrician trade (Boockmann *et al.*, 2011a), facility cleaning services (Bosch *et al.*, 2011b), care sector (Boockmann *et al.*, 2011c), painters and varnishers (Boockmann *et al.*, 2011b), and laundry services (Bosch *et al.*, 2011a).

causal analysis of the effects is challenged by Fitzenberger and Doerr (2016), who critically discuss the identifying assumptions, the difference-in-differences approach, and the control groups used (see also Möller, 2012). Yet previous studies, which focus on the first German sectoral minimum wage for the construction sector in 1997, arrive at the same conclusions. The analyses find positive wage effects for East Germany as well as either no employment effects or negative employment effects only for East Germany, whereas West Germany experienced an increase in employment (see Bachmann *et al.*, 2018; Frings, 2013; König and Möller, 2009; Müller, 2010; Rattenhuber, 2014). Indications for negative employment effects are also small for the electrical trade (Boockmann *et al.*, 2013). Overall, the evidence suggests no or small negative employment effects and positive wage effects of the sectoral minimum wages.

With the general minimum wage in place for well over three years, the number of *ex-post* impact assessments based on actual data is rapidly increasing. These focus on the main measures of concern, which are employment and distribution effects, but also on a large variety of other outcomes: working hours and work intensity, poverty and inequality, training and self-employment, or reservation wages and prices. Since, at this point, the minimum wage has existed for less than four years, the time horizon of the measured effects is short: most studies look at effects having emerged one to two years after the introduction of the wage floor. The most recent descriptive studies also include information from 2017.

The aim of this paper is to give an overview of the short-run effects of the minimum wage introduction for different socio-economic outcomes. We also discuss implementation issues, the suitability of the existing data sources, and related measurement issues. Overall, the literature suggests small negative employment effects, mainly driven by a disproportionately large negative effect for marginal employment. The evidence regarding the effects on regular employment is mixed, but the size of the estimated effects is usually small. Additionally, gross wages increased, especially for low-wage earners. However, there is also a substantial share of non-compliance. After the reform came into force, a large share of eligible employees still earned less than the €8.50 they were entitled to, pointing to an incomplete enforcement of the reform. The increase in gross hourly wages also does not translate into higher monthly or yearly earnings due to a simultaneous decrease in working hours. In addition to reducing working hours, other adaptation strategies were also used, such as price increases and a rise of work intensity. While poverty is not found to be reduced, life and job satisfaction for affected individuals was increased.

This article proceeds as follows. Section 2.2 focuses on the minimum wage legislation and possible impact channels for various outcomes. Section 2.3 discusses the information content of existing data sources, implementation and measurement issues, as well as descriptive evidence. Section 2.4 gives an overview of the identification strategies used and summarizes evidence on the causal effects of the minimum wage on different outcome variables. Section 2.5 sums up, discussing the results and outlining avenues for future research.

2.2 Minimum Wage Legislation, Potential Effects and Outcome Variables of Interest

2.2.1 Minimum Wage Legislation

The general statutory minimum wage became effective in Germany on January 1, 2015, and was introduced at a level of €8.50 gross per hour.[5] In order to monitor and evaluate its implementation and effects, a Minimum Wage Commission was established. It is required to re-evaluate the level of the wage floor every two years in order to provide appropriate protection to workers, while at the same time ensuring fair and functioning market competition without endangering jobs (see MiLoG §9). The minimum wage was raised to €8.84 effective January 1, 2017 and will further increase to €9.19 in 2019 and to €9.35 in 2020 (Mindestlohnkommission, 2016a, 2018a). Prior to 2015, several sector-specific minimum wages were in place, introduced, for example, in construction and roofing (in 1997), for painters and varnishers (in 2003), and the care sector (in 2010).[6] Nearly all employees in Germany are eligible for the statutory gross minimum wage. However, sectors with existing minimum wages that lay below €8.50 were granted a transition period through January 2017, allowing them to slowly increase their wage floors. Permanent exemptions apply to minors, trainees, interns,[7] volunteers, and long-term unemployed (see MiLoG §22).

In order to both comply with the minimum wage and monitor it, the computation of hourly wages has to be regulated specifically. However, those provisions are rather complex. The minimum wage must be paid for every actual working hour, at the latest at the end of the month following the month the work was performed. This also applies to overtime hours,

[5] MiLoG, `https://www.gesetze-im-internet.de/milog`, last accessed on December 14, 2017.

[6] For an overview of the sectors see WSI Tarifarchiv.

[7] Excluded interns are those with a compulsory internship ('Pflichtpraktikum'), a voluntary orientation or a voluntary accompanying internship ('freiwilliges Orientierungspraktikum' or 'freiwilliges ausbildungsbegleitendes Praktikum') lasting less than three months or an entry-level qualification ('Einstiegsqualifizierung').

as long as they are not compensated by the current monthly wage. However, if there is a working time account agreed on in writing, overtime hours can be paid or compensated by paid time off up to twelve calendar months after their recording. Single bonus payments (such as Christmas or vacation bonuses) can be added to the hourly wage as long as they are not determined by a generally binding collective agreement. However, they can only be credited against the monthly salary of the month they were paid in. Also premiums for overtime, work on Sundays and holidays etc. can be included in the hourly wage, whereas night shift premiums cannot.[8]

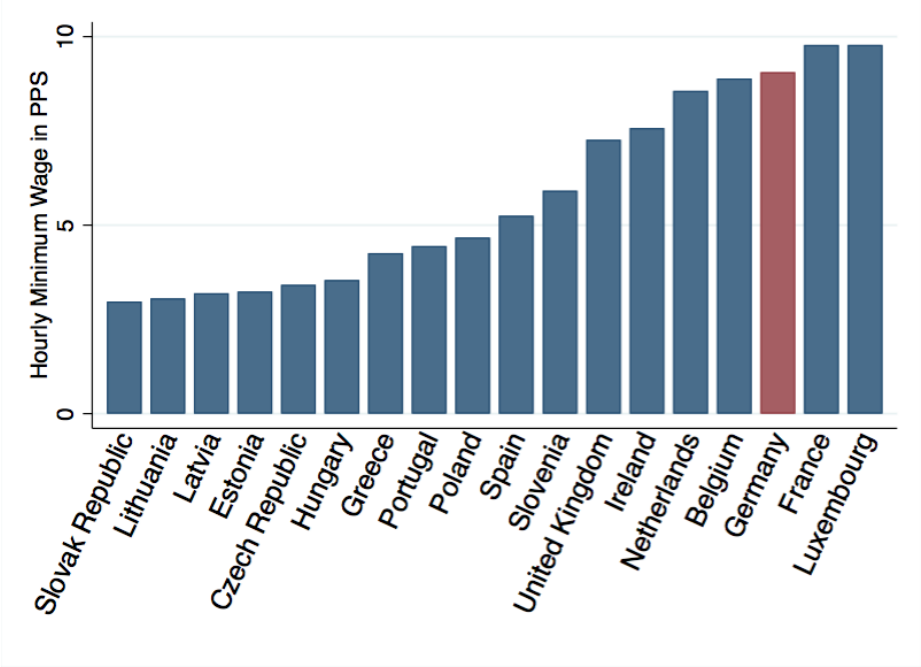
The German Customs Authority monitors firms' compliance with the wage floor. Specifically, the responsibility for conducting inspections of employers and enforcing compliance with social security laws and the Minimum Wage Act lies with the Financial Monitoring Unit for Illicit Employment. Should they detect non-compliance, employers can be punished with fines of up to €500,000. The Federal Ministry of Labour and Social Affairs provides a hotline for both employers and employees to get specific information on minimum wage issues and voice complaints.

The German minimum wage was introduced at a comparatively high level, despite the fact that there was no experience with a nationwide wage floor that reached beyond sectoral minimum wages. At the time of its introduction, it ranked among the highest wage floors among European countries when accounting for purchasing power (see Figure 2.1). Combined with the limited number of legal exemptions, this high bite translated into a rather large number of affected employees. In 2014, around 10 to 14 percent (depending on the data source) of the eligible employees earned less than €8.50 per hour.[9] For instance, the German Federal Statistical Office estimates that 10.7 percent of employees are affected, which amounts to 4 million workers (see Table 2.1). Moreover, they find that some groups are especially affected. Among them are females and marginally employed. Marginal employment or 'mini-jobs' are defined by monthly earnings of up to €450, which are not subject to social security contributions. Another disproportionately affected group are East German residents, which is visible in Figure 2.2, which shows the Kaitz-index (i.e. the ratio between minimum wage and average monthly gross earnings of full-time employees) for German districts in 2014. It

[8] For further information and specific regulations see `http://www.zoll.de/DE/Fachthemen/Arbeit/Mindestarbeitsbedingungen/Mindestlohn-AEntG-Lohnuntergrenze-AUeG/Berechnung-Zahlung-Mindestlohns/berechnung-zahlung-mindestlohns_node.html`.

[9] See Amlinger *et al.* (2016); Brenke (2014); Destatis (2016a); Falck *et al.* (2013); Kalina and Weinkopf (2014); Lesch *et al.* (2014).

Figure 2.1: Hourly Minimum Wages in 2015 in Purchasing Power Parities (in €)



Source: OECD. Note: Real hourly minimum wages are statutory minimum wages converted into a common hourly pay period for the selected countries. The resulting estimates are deflated using the consumer price index taking 2017 as the base year. The data are then converted into a common currency unit (US-$) using Purchasing Power Parities (PPPs) for private consumption expenditures in 2017. Eventually, the data is converted into €.

shows that the monthly salary of a full-time employee paid at the minimum wage makes up between 40 and 65 percent of the average earnings of full-time workers for most regions. Moreover, the Figure shows that there are considerable differences in the degree to which a region is affected. While for large parts of West Germany the Kaitz-index amounts to less than 50 percent, it lies above that for all of East Germany, except Berlin. This is another indicator that the minimum wage bit quite hard into the wage distribution. Additionally, it provides the first indication that regions are affected differently, a fact that is exploited by a number of causal studies (see Section 2.4).

2.2.2 Outcome Variables and Potential Channels

Hourly Wages The introduction of an hourly minimum wage has an immediate impact on the hourly wage distribution: If the minimum wage is binding and respected by all market participants, it leads to a left-cut wage distribution. The actual effect on the distribution of

Table 2.1: Minimum Wage Beneficiaries in 2014

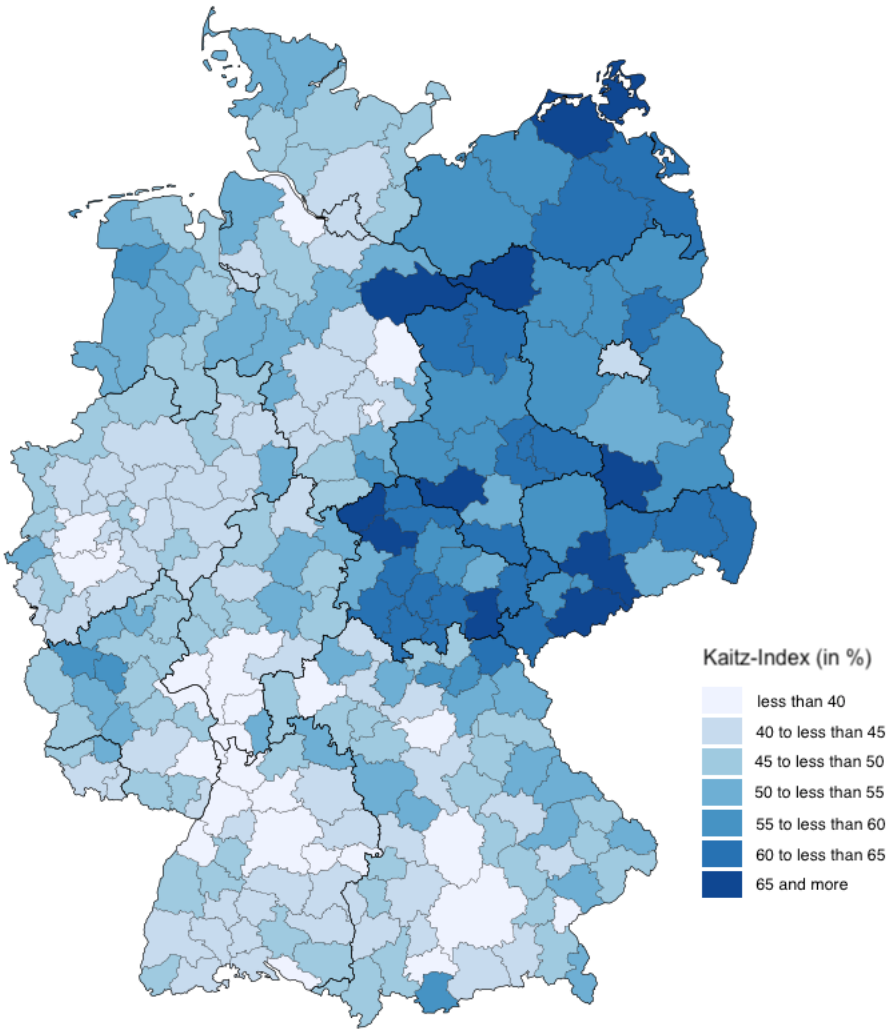
	Absolute (in mio.)	Share (in %) of employed	Share (in %) of affected
Employed	37.4	100	-
Wage <€8.50	5.5	14.7	-
Wage <€8.50 and eligible	4.0	10.7	100
thereof			
West-German residents	2.9	7.8	72.9
East-German residents	1.1	2.9	27.1
Full-time employment	0.9	2.4	22.4
Part-time employment	0.9	2.4	22.4
Mini-jobs	2.2	5.9	55.1
Women	2.5	6.6	61.7
Men	1.5	4.0	38.3

Source: Destatis (2016a). Note: Numbers are based on the Structure of Earnings Survey (SES) 2014 and include public sector employees.

hourly wages, however, may be more differentiated. While there could be no impact other than on the affected population, spillover- and compression-effects could also cause other parts of the wage distribution to change (Aretz *et al.*, 2013; Belman and Wolfson, 2014; Dickens and Manning, 2004; Lee, 1999; Neumark *et al.*, 2004; Neumark and Wascher, 2008). Moreover, violations of the law (i.e. non-compliance), could lead to an absence of any such effects (Metcalf, 2008; Weil, 2005). This is why the key empirical questions arising are how much wages increased and for whom. When addressing this issue, it is important to distinguish between gross and net wages. While the minimum wage is defined as a gross wage, the effects on net wages could potentially differ between individuals, depending on the individual tax rates.

Monthly Income and Working Hours Assessing the immediate effect on wages is also essential to understand the effects on other economic outcomes. A central aim of the minimum wage introduction in Germany was to improve the material situation of low-wage workers and reduce their dependence on governmental transfers, i.e. in-work benefits (*'Aufstocker'*). Both the financial situation and the net transfer position in the context of taxes and social security are, thus, potentially affected by a wage floor (Atkinson *et al.*, 2017; MaCurdy, 2015; Sabia and Nielsen, 2015). They depend, however, on individual monthly earnings and those of a partner (if any). This is why monthly (and possibly yearly) earnings are also important

Figure 2.2: Kaitz-Index for Germany 2014



Source: Destatis, available at `https://www.destatis.de/DE/ZahlenFakten/GesamtwirtschaftUmwelt/VerdiensteArbeitskosten/Mindestloehne/Karte/Mindestloehne.html`. Note: The Kaitz-index measures the monthly income earned in a minimum wage job for 40 weekly working hours (€1473) in percent of the regional average earnings of full-time employees.

outcomes when evaluating the effects of the reform. However, they do not necessarily increase when hourly wages rise, since working hours could adapt – both on employers' and employees' initiative – such that monthly earnings stay constant. This is why working hours are also a key variable of interest.

Employment Not only the effect at the intensive margin, i.e. working hours, is crucial, but also the extensive margin: employment effects depend on labour supply and demand elasticities as well as, in general, on the structure and performance of the labour market. The

neoclassical model predicts a reduction in demand for the factor that becomes relatively more costly – in this case workers in the low wage segment. This is why the critics of minimum wages expect employment losses. Search models, however, also predict a positive effect of minimum wages because of rising marginal costs of labour from frictions (see Stigler, 1946; Brown *et al.*, 2014). If the labour market is characterized as (partly) monopsonistic, labour demand effects could be positive when employees are paid below the marginal product of labour beforehand. On the supply side, models predict that individuals whose reservation wages lie between the minimum wage and the previous market wage now enter the labour market, causing an increase in involuntary unemployment. For the above considered reasons, employment effects are not clear *ex-ante*. Thus, identifying them is a key task of any minimum wage evaluation. The literature is not unanimous on the time frame in which employment effects are likely to arise. Moreover, other adjustment channels could be used in order to avoid or postpone job losses, such as prices, profits, or non-compliance (Stewart and Swaffield, 2008; Draca *et al.*, 2011; Metcalf, 2008).

Other factors In addition, the introduction of minimum wages may impact other socio-economic decisions and psychological indicators. On the side of the employees, it can serve as a reference point for reservation wages but it can also impact perceptions of fairness and attitudes toward work – for employees in the low-wage segment and beyond (Clark *et al.*, 2009; Delfgaauw and Dur, 2007). The sense of fairness and satisfaction can also be altered by minimum wages; again this may impact labour productivity (Falk *et al.*, 2006; Fehr *et al.*, 2009). Minimum wages can also influence the decision to become self-employed or to invest in education (Agell and Lommerud, 1997). Again, the direction of the effect is unclear *ex-ante*. For example, an additional incentive for self-employment or higher education efforts may result from the expectation that the minimum wage increases the risk of unemployment; a disincentive from rising opportunity costs associated with not taking up a job. Firms, in turn, can respond to rising hourly wages with a whole set of measures including labour demand, endowment of workplaces, implementation of productivity-enhancing measures, or price adjustments (Lemos, 2008; Belman and Wolfson, 2014).

2.3 Data Sources, Implementation Issues and Descriptive Evidence

2.3.1 Data Sources

A rigorous and comprehensive assessment of the minimum wage effects requires appropriate data. Most importantly, the data should provide information on hourly wages and represent the entire (eligible) population. Ideally, the data also has a panel structure allowing to take trends and dynamics into account. A wide range of micro data suited for empirical labour-market analyses, including both administrative data and survey data, is now available for Germany. Table 2.2 gives an overview over data sources that are used in the evaluation of the German minimum wage.

Administrative data A large part of the existing studies on minimum wage effects is based on administrative data. One important provider of such data is the FEA. The data entail monthly aggregated data on employment, unemployment and job vacancies that are available in the short run. It is available for a variety of regional classifications and can be disaggregated even to small regional levels, such as the 11,000 municipalities. Another large data provider is the Research Data Centre of the Institute for Employment Research (IAB). It provides administrative data collected by social security notifications of employers and process-generated data of the FEA, which are then subsumed into the Integrated Employment Biographies (IEB).

This administrative data supposedly has a small measurement error in earnings, its sample size is large, and its panel structure makes it suited for causal analyses. One drawback, however, is that hourly wages are not collected. Instead, these must be computed from monthly earnings and a categorical variable on working hours, which only differentiates full-time, part-time, and marginal employment. As a result, computing the key variable hourly wage is not possible without making strong assumptions – particularly for part-time and marginally employed workers. For this reason, many studies focus on full-time employees, although low wages are markedly more prevalent among part-time and marginally-employed workers (see also Table 2.1). Moreover, civil servants and self-employed are not included in the data.

Table 2.2: Data Sources for the Evaluation of Minimum Wage Effects in Germany

Data Source	Type	Form	Observation Period	Level	Number of Observations	Data on	Comment
FEA – Federal Employment Agency	A	P/CS	since 1950, monthly	R	universe	(un)employment, job vacancies, remuneration	
IBS – ifo Business Survey	S	P	since 1949, monthly	E	7,000	planned changes in prices and employment	
IEB – Integrated Employment Biographies	A	P	since 1975, yearly	I	universe	duration of employment, welfare benefits, job search	no info on hours worked, civil servants, self-employed
IAB-EP – IAB Establishment Panel	S	P	since 1993, yearly	E	16,000	supply side infomation, including info on wages, number of employees affected by wage floor	
LPP - Linked Personnel Panel	S	P	since 2012, biennial	E/I	800 / 7,000	supply and demand side simultaneously	some industries excluded, only firms with at least 50 workers liable to social security
PASS – Panel Study Labour Market and Social Security	S	P	since 2007, yearly	HH	9,000	contexts and dynamics of households living in poverty	focus on transfer recipients, shows significant differences to SOEP / Microcensus
SES – Structure of Earnings Survey	S	CS	since 1951, since 2006 quadrennial	E/I	60,000 / 1 million	monthly wages and working hours	
SOEP – Socio-economic Panel	S	P	since 1984, yearly	I/HH	30,000 / 15,000	comprehensive individual data, including monthly wages and working hours	

Note: The table entails datasets frequently used for causal minimum wage evaluations. Abbreviations represent:
Type: A = Administrative, S = Survey. *Form*: CS = Cross-Section, P = Panel. *Level*: E = Establishments, HH = Households, I = Individuals, R = Regions.

Survey Data The available survey data in Germany either addresses individuals/households or firms. One important survey data provider is the German Federal Statistical Office. It provides two payroll-based data sets on individual monthly earnings and working hours. One is the obligatory Structure of Earnings Survey (SES). It is an extensive survey of about 60,000 firms in 2014, with a combined employment of 1 million, that are obliged to provide information on wages, working hours, and other working conditions. Thus, it is possible to construct precise hourly wages and account for overtime payments. Unfortunately, the SES is only conducted every four years, with its last wave collected in 2014. Therefore, it is not possible to study post-reform outcomes at this time. The second data set is the voluntary follow-up Earnings Survey (ES), a voluntary survey specifically conducted for minimum wage evaluation that took place in 2015, 2016, and 2017. However, the data is cross-sectional and, thus, does not allow for panel estimation techniques. Furthermore, several methodological differences between SES and ES challenge the inter-temporal comparability of the data: most importantly, participation in the ES was rather scarce. In 2015, only 12.8 percent of the contacted establishments returned the questionnaire. In 2016 (2017), the return rate lay at 6.3 (14.8) percent (Destatis, 2017a,b, 2018b).[10]

A central household survey data set for Germany is the Socio-Economic Panel (SOEP). It is an ongoing representative longitudinal panel survey with about 30,000 survey participants across 15,000 households per year (see Goebel *et al.*, 2019). Next to a comprehensive set of socio-economic, psychological, and health variables, which can be used as outcome and control variables, detailed wage information is available. Similar to the SES, hourly wages can be derived from reported monthly earnings and weekly working hours.[11] The SOEP contains both actual and contractual working hours as well as information on (un)paid overtime, allowing for the construction of both actual and contractual wages. However, the SOEP has a smaller sample size than SES, which puts limits on the possible level of regional differentiation.

Other relevant survey data is provided by the IAB. Here, the longitudinal data sets IAB Establishment Panel (IAB-EP), the Panel Study Labour Market and Social Security (PASS), and the Linked Personnel Panel (LPP) are of particular interest. The PASS shares several features of SOEP but puts its focus on job seekers. For this reason, PASS contains about the same number of transfer recipient households as households from the overall population.

[10] In 2015, 6,609 out of 51,651 establishments (12.8 percent) participated. In the following year, the sample was largely increased, such that 125,000 establishments were contacted, out of which 7,862 participated (6.3 percent). In 2017, 8,544 out of 58,000 firms (14.8 percent) responded.

[11] For a comparison between both data sets see Dütsch *et al.* (2017).

Comparisons of PASS with SOEP and Microcensus data, however, indicate significant differences (Beste *et al.*, 2018). Among others, the relative poverty risk is different from that found in the SOEP or the German Microcensus. In the context of minimum wage assessments, this is problematic because in-work poor are particularly affected by the minimum wage. The IAB-EP is an annual representative survey that entails up to 16,000 establishments per year. It covers various topics and has a special focus on the determinants of labour demand. Next to the PASS and IAB-EP, the IAB also provides the LPP. It is a linked data set of employers' and employees' information entailing data on human resources management, individual job quality and firm performance. Additionally, the ifo Institute supplies the Business Survey, which is a monthly employer survey asking about their planned changes in prices and employment (see Table 2.2).

Overall, none of the available data sources are ideal, but each can serve a specific purpose. However, the differences between data sets, their respondents as well as issues with the computation of hourly wages can also be a reason for diverging estimations of minimum wage effects across studies.

2.3.2 Implementation, Measurement Issues and Non-Compliance

A crucial point to any minimum wage evaluation is the actual implementation of the wage floor. Corresponding issues include enforcement and compliance, as well as possibilities of monitoring and evaluation.

The legislation sets the minimum wage on an hourly basis, leading to a number of problems for evaluations and compliance studies. In order to estimate how many people are affected by the minimum wage and what their wages are, detailed information on hours and earnings must be available. Two data sets meet this request: the SES/ES and the SOEP (see also Section 2.3.1). While the SES/ES entails data on wages provided in a survey of employers, the SOEP respondents are individuals in private households. Thus, both data sources potentially capture different forms of bias. Employees' responses may be subject to noise caused by possible misremembrance or rounding of key variables. Employers may have an incentive to conceal potential non-compliance with the wage floor, an issue that is also potentially relevant in the voluntary ES 2015-2017 (see Mindestlohnkommission, 2018b). Moreover, neither data set directly contains hourly wages; instead, this must be derived from monthly earnings and weekly working hours. In doing so, it has to be decided whether actual or contractual working

hours are used and how paid/unpaid overtime can be, and is, taken into account (see also Brenke and Müller, 2013; Dütsch *et al.*, 2017; Burauel *et al.*, 2017). Further, the handling of bonuses or yearly special payments, which are not stipulated to be entailed in the minimum wage, must be addressed. SOEP and SES differ also in other aspects, though. First, until 2016, the SOEP entails detailed information on hours only for the main employment of an individual, whereas the SES contains both main and add-on employment. Second, in the SOEP, respondents are assigned to the region where they live, the SES allocates according to the place of work, which can possibly lead to differences in regional analyses (Dütsch *et al.*, 2017). Thus, derived hourly wages depend on a number of working assumptions, which potentially lead to different results obtained by SOEP and SES.

The estimation of compliance is further complicated by the exemptions stipulated in the minimum wage regulations (see Section 2.2). In order to calculate the eligible population, these exemptions must be identifiable in the underlying data source. Due to the complexity of the regulations, this is not always possible. Individuals in industries granted a transition period to adapt own minimum wages below €8.50 are especially difficult to identify. The same holds true for long-term unemployed, minors, and individual interns, since their exemption relies on very specific circumstances that cannot always be captured in the data. For the above-mentioned reasons, estimating non-compliance is not straightforward and depends on the used data source and assumptions made.

All evaluations of compliance find a substantial number of eligible employees still earning less than the new wage floor in the short run. According to estimates derived from contractual wages in the SOEP by Caliendo *et al.* (2017), for example, this is the case for about seven percent of eligible employees in the first half of 2015. Indications for non-compliance are also reported in Bachmann *et al.* (2017); Bruttel *et al.* (2018); Burauel *et al.* (2017); Mindestlohnkommission (2018b); Pusch (2018); Pusch and Seifert (2017). Even based on the SES/ES – i.e. employer survey data – 4 million employees earned below the minimum wage in 2014, and still 1 million in 2015, 750,000 in 2016 and 800,000 in 2017 (see Mindestlohnkommission, 2018b; Destatis, 2018a).[12] Using SOEP data, the Mindestlohnkommission (2018b) estimates 2.8 million (2.1 and 1.8) employees earning less than €8.50 for 2014 (2015 and 2016). Although these numbers have to be treated with caution due to the known measure-

[12]The number of employees earning less than the wage floor is measured by people earning less than €8.45 for the years 2015 and 2016 and less than €8.84 for the 2017.

ment issues, they imply that (non-)compliance with the wage floor is an important issue.[13] Moreover, they show marked differences between the SOEP and the ES data. This is possibly caused by the aforementioned difficulties in estimating compliance as well as the potential bias introduced by answers from both the employer and employee perspective. It can also be caused by employers' avoidance strategies that may cover non-compliance in employers' responses. In that sense, a qualitative study by Koch *et al.* (2018) reveals that workers experienced increased supervision of work times, similarly for actual work and break time as well as for time used for preparation and follow-up. Moreover, there are reports suggesting a reduction of special payments, no or smaller payment for waiting or stand-by time, as well as the stipulation of piecework rates. Additionally, workers report receiving payment in kind and suffering a deduction of costs for working materials from their wages (Bruttel *et al.*, 2018; Burauel *et al.*, 2017; Mindestlohnkommission, 2016b). As Fedorets and Schröder (2017) find, two-thirds of affected respondents of a special SOEP omnibus survey had either directly experienced avoidance strategies or knew someone who had.

This substantial non-compliance raises questions about the enforcement of the minimum wage law. As Burauel *et al.* (2017) point out, controls by the Customs Administration are mainly conducted where violations are expected, thereby leading to a selective monitoring procedure. Bruttel *et al.* (2018) find that controls are mainly concentrated on operations in the shadow economy and that out of 43,637 screened employers in 2015, only 705 preliminary proceedings for non-compliance were instituted (see also Mindestlohnkommission, 2016c). Burauel *et al.* (2017) further argue that the already scheduled increase of customs inspection posts had not progressed sufficiently, making it more difficult to enforce the law due to a staff shortage. Insufficient regulation of requirements for documentation, such as recording the starting time, the ending time, and the number of hours worked, complicate controls further. Moreover, the responsibility of proving violations of the law lies with the employees, discouraging many workers from actually demanding their rights. Burauel *et al.* (2017) conclude that extensive measures are needed to enforce the minimum wage.

From an implementation perspective, a number of issues should be considered when stipulating a wage floor. First, the minimum wage regulations should be designed in a way that

[13]For comparison, when the minimum wage was introduced in the UK compliance was higher than 80 percent one month after the reform and increased shortly after when firms were able to adapt their systems and workers challenged their employer for a wage increase (see Low Pay Commission, 2000). In 2014, non-compliance with minimum wages for adults is estimated to be about 0.8 percent in the UK and 2.2 percent in the US (see Rani, 2016).

facilitates the monitoring of compliance. Therefore, it should be considered whether the chosen measure – e.g. hourly wages – is verifiable in employers' records. In that sense, electronic recording of work-time could help to provide credible and comparable documentation. Second, precise data should be available to the scientific community in order to ensure credible policy evaluations. Third, strong precautions have to be taken such that employers' circumvention of the wage floor is difficult and sufficient controls of establishments are performed. The legislation should include regulations as to how the minimum wage can be enforced and the state should be responsible for it.

2.3.3 Descriptive Trends

Despite considerable non-compliance, many employees were paid according to the new minimum wage legislation. The following sub-section briefly presents first descriptive evidence for the development of key outcomes after the minimum wage introduction (see Mindestlohnkommission, 2016b, 2018b, for more extensive overviews), while the causal evidence is discussed in Section 2.4.

Overall, the descriptive literature finds positive effects of the minimum wage reform on gross hourly wages, particularly (and not surprisingly) in the low-wage segment. Especially women, low-skilled, and East German workers, marginally employed and employees in small firms experienced above average wage increases (e.g. Amlinger *et al.*, 2016; Burauel *et al.*, 2017). Spillover effects also increased wages in wage groups above €8.50, although this effect fades out at wages higher than €15 (Mindestlohnkommission, 2018b). Some studies also find a significant compression of the wage structure, with higher wages being cut (Lesch and Schröder, 2016; Schubert *et al.*, 2016). Moreover, there is some evidence that wages were already increasing in anticipation of the reform (Kubis *et al.*, 2015; Bellmann *et al.*, 2015).

Despite ex-ante apprehension of job losses, employment developed very positively after the reform. Possibly due to the strong performance of the economy, there were nearly no job losses but rather evidence for a reluctance for hiring. The positive employment growth was largely driven by an increase in regular employment. Marginal employment, on the other hand, decreased following the minimum wage reform, mainly right at the beginning of 2015. About half of those mini-jobs were found to have transformed into regular employment, though (e.g. vom Berge *et al.*, 2016a, 2018; Groll, 2016).

Average weekly working hours decreased significantly after the reform, which is why hourly

wage increases did not translate into large increases in gross monthly earnings (e.g. Bellmann *et al.*, 2016; Grabka and Schröder, 2018). Similarly, the number of in-work welfare recipients decreased only slightly (e.g. Bossler, 2016; Bruckmeier and Wiemers, 2016). Also, the main goals of the minimum wage reform, alleviation of poverty and reduction of inequality, are not found to be met (e.g. IAB, 2018; Grabka and Schröder, 2018).

The wage increases translated into disproportional increases of consumer prices in affected sectors, without a noticeable effect on the overall price index (Mindestlohnkommission, 2018b). However, firms are also found to have adapted through other channels, such as reducing investments (Schubert *et al.*, 2016), concentrating work and increasing intensity (Bellmann *et al.*, 2016; Bruttel *et al.*, 2018; Koch *et al.*, 2018) or changing job requirements (Gürtzgen *et al.*, 2016). Also, some firms no longer offer any internships or restrict their maximum duration to three months (Bossler *et al.*, 2018; Koch *et al.*, 2018).

2.4 Causal Evidence

2.4.1 Identification of Causal Effects

To estimate causal effects of the minimum wages, the international literature relies on a variety of identification approaches. One strategy relies on legislative variation in the minimum wage regulation. For example, in the US, differences in state-level minimum wages are used to evaluate diverging wage and employment trends (see Card and Krueger, 1994, 1995; Neumark and Wascher, 2008; Dube *et al.*, 2010). This approach is appealing, especially if federal state labour markets only marginally differ with respect to their regulations and structure. In Germany, this strategy is not applicable, though, as the minimum wage applies in all regions. Other studies rely on a comparison of exempted sectors or individuals with their non-exempted counterparts. Yet, since these groups are systematically different, the necessary assumption that both groups would have developed equally in the absence of the reform and, thus, would share a common trend, is likely violated, which makes this approach equally unfeasible for the German case.

Therefore, the evaluation studies in Germany mainly rely on three approaches. The first one was suggested by Card (1992) and relies on regional variation in the degree to which an area is affected by the wage floor (for applications in the UK see Stewart, 2002; Dolton *et al.*, 2010). Since there is considerable regional heterogeneity in wage levels, the bite of the reform

– e.g. measured by the proportion of employees with hourly wages below €8.50 before the reform – varies despite of the uniformity of the wage floor. Thus, the change in wages relies on the regional bite level. The more strongly a region is affected by the minimum wage, the stronger the expected impact on wages and, in turn, the stronger the supposed reaction in the examined outcome. The causal effect can then be obtained in a difference-in-difference (DiD) framework, where it is captured by the coefficient of an interaction term between a post-reform dummy and the bite measure. This approach can be applied either on the level of regions or also on the individual level.

The second DiD strategy relies on a standard approach of defining a treatment and a control group. Treated individuals are those employees with an hourly wage below €8.50 in the year prior to the reform, while control individuals are those with a wage slightly above €8.50. The method then compares the difference between pre- and post-treatment outcomes of control individuals with those of treated individuals. However, this identification approach is potentially sensitive to spill-over effects, in which case treatment and control group cannot be clearly disentangled (Mindestlohnkommission, 2016b).

Finally, the DiD approach can also be applied on the firm level. Assuming that establishments are affected differently, treated firms are those with a high share of employees paid below €8.50 before the reform, whereas controls are those with a small share or no affected employees at all. The causal effect is then captured by the coefficient of the interaction term between the post-treatment dummy and the treatment dummy.

In what follows, we refer to the first approach as the Regional Difference-in-Difference (DiD-R) identification and the second as the Individual Difference-in-Difference (DiD-I). The firm level strategy is referred to as Establishment-Level Difference-in-Difference (DiD-E). Applying these methods, the minimum wage evaluations look at a variety of outcomes that could potentially be affected (see also discussion in Section 2.2.2). They range from the most apparently affected outcomes, such as hourly wages and employment, over monthly income and poverty effects, to working hours and other adaptation methods. Additionally, studies examine the impact on other outcomes, like self-employment, well-being, and reservation wages (for an overview see Table 2.3).

Table 2.3: Causal Evidence on Minimum Wage Effects in Germany by Effect Sign

Outcome / Effect	Significant Negative Effect (−)	No Significant Effect (=)	Significant Positive Effect (+)
Hourly Wages			Ahlfeldt *et al.* (2018) ◊ Bossler and Gerner (2016) □ Burauel *et al.* (2018) ▽ Caliendo *et al.* (2018b) ◊ Caliendo *et al.* (2017) ◊
Overall Employment	Bonin *et al.* (2018) ◊□ Bossler and Gerner (2016) □ Bossler *et al.* (2018) □ Caliendo *et al.* (2018a) ◊ Schmitz (2017) ◊	Ahlfeldt *et al.* (2018) ◊ Garloff (2018) ⊙ Link (2018) □	
Regular Employment	Caliendo *et al.* (2018a) ◊ Schmitz (2017) ◊	Bonin *et al.* (2018) □◊	Garloff (2018) ⊙ Holtemöller and Pohle (2017) ⊙
Marginal Employment	Bonin *et al.* (2018) □◊ Caliendo *et al.* (2018a) ◊ Garloff (2018) ⊙ Holtemöller and Pohle (2017) ⊙ Schmitz (2017) ◊		
Unemployment	Ahlfeldt *et al.* (2018) ◊	Bonin *et al.* (2018) ◊ Garloff (2018) ⊙	
Self-employment		Bossler and Hohendanner (2016) □ Bossler *et al.* (2018) □	
Working Hours	Bonin *et al.* (2018) ▽ Bossler and Gerner (2016) □ Caliendo *et al.* (2017) ◊ Caliendo *et al.* (2018b) ◊		

Table continued on next page.

Table 2.3 – continued from previous page.

Outcome / Effect	Significant Negative Effect (−)	No Significant Effect (=)	Significant Positive Effect (+)
Monthly Earnings		Caliendo *et al.* (2017) ◊ Caliendo *et al.* (2018b) ◊	
Benefit Recipients	Schmitz (2017) ◊	Bruckmeier and Becker (2018) ▽	
Poverty		Bruckmeier and Becker (2018) ▽	
Prices			Link (2018) □
Reservation Wage			Fedorets *et al.* (2018) ◊
Training	Bellmann *et al.* (2017) □ Bossler *et al.* (2018) □		
Internships		Bossler and Wegmann (2019) ◊□	
Satisfaction			Bossler and Broszeit (2017) ▽ Gülal and Ayaita (2018) ▽ Pusch and Rehm (2017) ▽

Note: This table summarizes 20 causal minimum wage studies sorted by the effect sign they identify for 15 different outcome variables. The studies are based on different causal identification approaches that are represented by the following signs:
□ = Establishment-Level Difference-in-Difference (DiD), ▽ = Individual DiD, ◊ = Regional DiD, ⊙ = Combinations and Others.

2.4.2 Hourly Wages

The causal literature is unanimous about positive effects on gross hourly wages.[14] Relying on a DiD-I identification strategy and using SOEP data, Burauel *et al.* (2018) find that between 2014 and 2016, the minimum wage introduction induced an additional wage growth of about six to seven percent for eligible employees in the wage segment below 8.50 in 2014. This corresponds to about €0.50 per hour. This effect tends to be stronger for groups that commonly receive lower wages, such as mini-jobbers, employees without a completed education, women and foreigners. However, due to small sample sizes these subgroup results have to be treated with caution. The authors do not find evidence in favour of spillover effects for wages beyond the minimum wage threshold. A DiD-R identification strategy is implemented in Caliendo *et al.* (2017), using SOEP data and comparing wages in 2014 and 2015. They find that in a region with an average treatment intensity (normalized to be 1.0), wages in the bottom quintile of the region-specific wage distribution grew about six to seven percent faster than in a region with zero treatment intensity. Consistent with Burauel *et al.* (2018), they find no indications of positive treatment effects for higher quintiles of the region-specific wage distributions. This is also substantiated by Ahlfeldt *et al.* (2018), who employ a DiD-R strategy based on IEB data and also find higher wage growth in low-wage than in high-wage counties, which is especially apparent for the left tail of the wage distribution. Applying a DiD-E specification, Bossler and Gerner (2016) find a treatment effect for the affected establishments, showing an increase in mean wages by about 4.8 percent for them.

Overall, the existing evidence for Germany testifies to an increase in gross wages of low-wage workers and no strong evidence in favour of spillover effects on higher wage segments. However, the positive effect is hampered by the large amount of non-compliance (see Section 2.3.2). Moreover, while the current literature focuses on gross hourly wages, the effects on net hourly wages could potentially differ, since they strongly depend on the individual tax rates. For example, while mini-jobbers' gross income equals their net income, regular employees' increase in net wages is reduced by tax deductions and social security contributions. This is an issue that is largely neglected so far. In this sense, the impact on net household income, which possibly depends on the partner's earnings and the tax and transfer deduction rates, is also an important outcome variable (see also Section 2.4.4).

[14] See Ahlfeldt *et al.* (2018); Bossler and Gerner (2016); Burauel *et al.* (2018); Caliendo *et al.* (2017, 2018b).

2.4.3 Regular Employment, Marginal Employment and Unemployment

Overall Employment Descriptive evidence shows that employment developed positively in the first years after the reform, with some evidence for reduced hiring. However, it is unclear whether the development is actually attributable to the minimum wage reform. There are also a number studies estimating the causal impact of the minimum wage on employment, with most identifying small negative or zero employment effects.[15]

Both Bossler and Gerner (2016) and Bossler *et al.* (2018) employ an establishment level DiD with the IAB-EP data and find that overall employment decreased by about 1.7 to 1.9 percent for affected establishments, translating into 46,000 to 60,000 jobs. They, too, find that this was rather due to reduced hiring than to displacements. Using the DiD-R approach, Bonin *et al.* (2018) find significant, but only small, effects on employment. In comparison to low-bite regions, employment decreased in highly affected regions by about 0.5 percent. The authors also substantiate their results by employing a DiD-E identification.

With a similar regional level identification, Caliendo *et al.* (2018a) find that overall employment was reduced by about 140,000 jobs, i.e. 0.4 percent due to the minimum wage reform. Similar results are obtained by Schmitz (2017), who estimates disemployment effects of up to 260,000 jobs using a DiD-R specification with FEA data. Ahlfeldt *et al.* (2018), on the other hand, employ a similar identification but with IEB data and find no evidence of significant job losses in low-wage regions compared to high-wage regions. This is supported by Link (2018), who finds an insignificant effect of the wage floor on firms' planned employment changes. His results are obtained using a DiD estimation on firm level with the ifo Business Survey data. However, the author also adds that in light of potential measurement error, negative employment effects cannot be eliminated. Moreover, it remains unclear whether the common trend assumption is likely to hold. Garloff (2018), who relies on a combination of individual and regional DiD by calculating the bite for cells by region, age, and gender with data from the FEA and the remuneration statistic finds a slightly positive relationship between the bite and overall employment. However, his results are not robust across specifications and only small in magnitude. The differences in the results found in the literature are partly driven by differences when estimating the singular effects on regular (i.e. full- and part-time employment) and marginal employment.

[15] See Bonin *et al.* (2018); Bossler and Gerner (2016); Caliendo *et al.* (2018a); Garloff (2018); Holtemöller and Pohle (2017); Schmitz (2017).

Regular Employment Caliendo *et al.* (2018a) find a slight (but not robust) significant decrease for regular employment, meaning that the overall effect found in this study does not translate directly into a drop in full- and part-time employment. Schmitz (2017), who uses FEA data with a DiD-R approach, comes to similar conclusions. His back-of-the-envelope calculation arrives at an employment decrease of up to 57,000 regular employed caused by the minimum wage, which falls into the interval of 52,000 to 78,000 jobs suggested by Caliendo *et al.* (2018a). Bonin *et al.* (2018) do not find a significant effect on regular employment. Garloff (2018), once again, identifies an increase in regular employment (so do Holtemöller and Pohle, 2017) and explains this difference by the fact that the DiD-R approach captures only geographical dynamics, since regional variation alone is not necessarily related to the minimum wage. In sum, most causal studies find weakly negative or insignificant effects on regular employment, while others also point in the direction of a slightly positive impact. There is thus little consensus on the actual effect of the minimum wage on regular employment, suggesting that the mostly found overall negative effects seem to be driven by the decrease of marginal employment.

Marginal Employment The descriptive evidence unanimously finds a decrease in marginal employment. This can also be causally attributed to the wage floor reform. As Caliendo *et al.* (2018a) find, their overall employment effect was mainly caused by a strong decrease of marginal employment, which amounted to about 2.8 percent, or up to 180,000 jobs. Schmitz (2017) estimates that mini-jobs have decreased by up to 202,000. Using data on monthly wages, Garloff (2018) also identifies a significant decrease in the number of mini-jobs. Further, Bonin *et al.* (2018) find that marginal employment was reduced after the minimum wage introduction and that this decrease was 1.6 to 2.0 percentage points higher in regions where the minimum wage bites stronger than in low-bite regions. A reduction of mini-jobs is also found by Holtemöller and Pohle (2017), who employ idiosyncratic trend estimations. Overall, a decline of marginal employment as a consequence of the minimum wage is established in both the descriptive and the causal literature and, thus, seems undisputed.

Transitions A question that remains when comparing effects across employment types is whether the minimum wage reform induced some kind of transition between the employment forms. Since marginal employment implies an upper limit for monthly earnings (€450), an hourly wage increase could easily cause mini-jobbers to exceed this threshold, thus either

having to reduce working hours or transitioning into regular part-time employment. Unfortunately, the existing evidence relies on aggregate regional or establishment-level employment data, which makes it impossible to identify single employment spells required for tracking individual transitions. However, descriptive evidence suggests that about half of the decrease in mini-jobs is due to contract adaptations to regular employment (see Section 2.3.3). Similarly, Bonin *et al.* (2018) find evidence of a substantial increase in transitions, but also argue that the reduction of marginal employment was not totally compensated by an increase in regular employment (see also Caliendo *et al.*, 2018a). Holtemöller and Pohle (2017) also find that the increase in regular employment found in their analysis is not systematically related to the decrease of mini-jobs. Thus, it is likely that a good part of formerly marginally employed were able to turn their contract into regular employment, but this cannot explain the entire decrease. Overall, labour market flows are largely neglected in the previous evidence. However, they are highly important for understanding the underlying structures and to identify job loss and hiring effects and should therefore be examined more closely in the future (see, e.g., Bachmann *et al.*, 2018).

Unemployment Since a reduction of different employment types could be caused by inter-type transitions or a general change in the labour force, disemployment effects do not necessarily imply increased unemployment. Thus, the impact of the minimum wage on unemployment is also an interesting object of investigation. The causal literature finds either no effect or even a small reduction. Neither Bonin *et al.* (2018) nor Garloff (2018) find stable evidence that the minimum wage affected regional unemployment. Ahlfeldt *et al.* (2018), on the other hand, find that regions with a higher share of low-wage workers also display lower unemployment rates in the two post-reform years. They argue that in 2015 this was driven by a decrease of the labour force in highly affected regions, whereas in 2016 it was caused by increasing employment levels.

Self-employment When addressing employment effects induced by the minimum wage examining the impact on self-employment is also an important issue. Since the self-employed are exempted from the wage floor, firms might prefer to employ freelancers as opposed to hiring employees, leading to disguised employment (*'Scheinselbständigkeit'*). The initial descriptive evidence does not seem to find this effect. Bossler and Hohendanner (2016), estimating the minimum wage effect on the employment of freelancers based on the IAB-EP, do not find

evidence that employing freelancers is used as a circumvention strategy. The result is confirmed by Bossler *et al.* (2018). If any, a moderate increase by 0.2 percentage points (9,300 freelancers) in 2016 can be seen in comparison to unaffected establishments. However, this largely occurs in industries that already had a large proportion of freelancers before the minimum wage reform. Thus, initial evidence does not suggest an increase of self-employment or disguised employment caused by minimum wage reform.

Overall, evaluation studies suggest a reduction of marginal employment, with some evidence that at least part of it was transformed into regular employment. Results on overall employment suggest small negative employment effects. The magnitude of the found short-run effects (job losses of up to 260,000) is below the predicted long-run effects (job losses of up to over a million). However, these effects are only short-run evidence and it needs to be examined how they develop in the medium and long run. Moreover, a substantial amount of non-compliance (see Section 2.3.2) possibly leads to a cushioning of the impacts. Since both descriptive and causal evidence suggest a reluctance in new hiring after the wage floor reform, disemployment effects are likely not as perceptible. One reason why regional employment effects could differ is that they depend upon the labour market structure. Using a semi-structural approach, Bachmann and Frings (2017) look at the effect of the degree to which a labour market can be described as monopsonistic and find that labour supply elasticities differ significantly between industries, mainly caused by differences in worker composition and worker representation through works councils. While retailing, the hotel/restaurant industry, and agriculture are found to be monopsonistic labour markets, other services and manufacturing of food products are not. This might produce diverging employment reactions across industries and regions and could begin to explain opposing results. Another reason why employment effects could vary are differences between the structures of the firms' product markets. This aspect, however, is not yet studied.

2.4.4 Working Hours, Monthly Earnings and Poverty

Working Hours Since increasing wages mean rising labour costs, employers could use working hours as an adaptation mechanism. Yet, employees might also have an incentive to adapt working hours, due to increased hourly compensation. A negative effect on hours is substantiated by a number of causal studies. Using a DiD-R approach with the SOEP, Caliendo

et al. (2017) study the implications for contractual and actual working hours, finding that the minimum wage affected both measures negatively (see also Caliendo *et al.*, 2018b). The effect is found to be slightly larger for contractual than for actual hours worked, suggesting an increase in overtime. Pusch and Rehm (2017) evaluate the effects on actual working hours in a DiD-I design using PASS data. Their estimations suggest a negative effect on actual working hours for the treatment group of employees, i.e. those with wages below €8.50 in 2014, with part of the effect resulting from a reduction of overtime work. Bonin *et al.* (2018) also apply a DiD-I approach, but use the SOEP as data source. They show that contractual hours decreased by 5 percent, whereas the reduction of actual hours is not significant, implying that the working hours mainly adapted on paper. Overall, all cited studies find a reduction of contractual working hours and most also find evidence for a decrease of actual working time.

Monthly Earnings The identified reduction of working hours can also influence monthly earnings. If hours are reduced very strongly, this reduction could even compensate the hourly wage increase, leading to a reduction in monthly earnings. The study of Caliendo *et al.* (2017) suggests that the reform did not improve gross monthly earnings for employees with low-paid jobs as they experienced a reduction of working hours roughly proportionate to their wage increase.[16] This is also clear in Caliendo *et al.* (2018b), who argue that working time reductions caused the gross income of the lowest quintile of the wage distribution to remain nearly stable, changing from €1,166 in 2014 to €1,193 in 2015.

In-Work Benefit Payments While descriptive evidence finds a slight decrease of in-work benefit recipients after the reform, causal studies find mixed effects of the wage floor. Bruckmeier and Becker (2018) find evidence that in-work welfare recipients transitioned from marginal to regular employment in course of the minimum wage. However, they do not find that working welfare recipients were able to leave benefit receipt. Schmitz (2017), on the other hand, testifies to a reduction of in-work welfare recipients, which he calculates to amount to about 39,000. His results suggest, though, that about half of this reduction is the result of losing a supplementary job, rather than by a rise in the household income. In sum, Schmitz (2017) argues that the minimum wage was only somewhat effective at reducing welfare dependency.

[16] Neumark *et al.* (2004) find a similar result in the United States.

Poverty Risk and Inequality Using a DiD-I identification with the FEA data and the PASS, Bruckmeier and Becker (2018) find no significant effect of the minimum wage on the poverty risk.[17] In addition, the intensity of poverty (i.e. the gap between the equivalent household income and the poverty line) is not significantly affected. Supported by the descriptive evidence, these results suggest that while the minimum wage was effective in raising hourly wages for low-income earners, its alleviating effect on poverty is not readily apparent. This might be due to the fact that high poverty risk individuals are largely not working at all and, thus, are not affected by a wage floor, whereas only a small share of minimum wage earners actually lives in households with a high poverty risk. Additionally, the high poverty risk of working individuals usually results from low working hours rather than low hourly wages (Bruckmeier and Becker, 2018; Mindestlohnkommission, 2018b). An outcome that is not covered sufficiently in the context of poverty and inequality is the net household income. As mentioned in Section 2.4.2, the impact on the household income depends on the monthly earnings of the household members but also on the rate of tax deductions and social security contributions. Thus, an increase in gross hourly wages does not translate into a uniform increase in net household income across all employees, even if working hours remained constant. In turn, poverty and inequality and also labour supply decisions could be affected differently. In that sense, further research on that aspect is needed.

2.4.5 Other Outcomes

Firm Level Adaptations On the basis of the ifo Business Survey and employing a DiD approach on establishment level, Link (2018) find that the probability of firms to increase their prices is higher, the higher the degree to which they were affected by the minimum wage. Conditional on being equally affected, manufacturing firms and service companies increase their prices similarly. The same holds true for firms in West and East Germany. Relying on administrative producer price indices, the author finds that the firms' price increases are quantitatively large, with the overall level of producer prices in Germany increasing by about 0.2 percent. Using a DiD-E specification with IAB-EP data, Bossler *et al.* (2018) also analyse a variety of other firm level effects. They find that the expected business volume decreased by 0.8 percentage points in affected establishments compared to unaffected firms. Moreover, the probability for deficits for affected firms excels that of their unaffected counterparts by

[17] A household is considered at risk for poverty if the disposable equivalent income is lower than 60 percent of the median equivalent income of the population.

2 percentage points. The authors also find a negative effect on firms' profitability, which is largely explained by increased wage costs. However, they cannot identify significant minimum wage effects with regard to productivity (measured as turnover per employee) or competitive pressure. Further, investments into physical capital that could substitute for labour or human capital (measured as apprenticeships and training intensity) are not found to be affected.

Reservation Wages Fedorets *et al.* (2018) study how the minimum wage introduction changed reservation wages of non-working individuals. Employing a DiD-R design, they find that reservation wages of respondents who reported low reservation wages increased by approximately four percent. Interestingly, this correspondents with the increase in observed wages. Thus, the results suggest that the minimum does not lead to higher job acceptance rates of low-wage earners but to an adaptation of the distribution of reservation wages. However, this does not hold true for the whole population, since the authors also find that immigrants adjust their reservation wages less than German citizens. They argue that speaks to a strategic non-adjustment among immigrants, which reduce their reservation wages caused by lower expected job arrival rates.

Training and Internships Bellmann *et al.* (2017) analyse the effect of the wage floor reform on both training incidence and intensity. Employing a DiD-E approach with the IAB-EP, the authors do not find a reduction in the training incidence, i.e. the decision to provide training to at least one employee. However, they do find a reduction of training intensity – measured as the number of trained employees relative to all employees – at highly affected firms, amounting to 0.4 percentage points per 10 percentage points increase in the fraction of affected employees (see also Bossler *et al.*, 2018). This negative effect on training intensity is caused by firm-financed training. Training that is fully or partially financed by employees is not affected. On the worker level, there is evidence for a decrease of training for medium- and high-skilled employees, whereas the effect on low-skilled employees' training is insignificant. The authors relate this to the fact that firms do not want to further diminish the productivity of low-skilled employees and, hence, cut training costs of the employees unaffected by the minimum wage. Bossler and Wegmann (2019) analyse the effect of the minimum wage on internships. Applying a DiD approach separately for states, regions, and establishments, they do not find a significant effect on the log number of internships or the share of interns relative to all employees. The analysis does not take into account the length of internships

and does not distinguish between voluntary and compulsory internships, though.

Satisfaction and Motivation Since the minimum wage was designed to improve the situation of employees, an increase in well-being and satisfaction could speak to a achievement of this goal. To that end, Gülal and Ayaita (2018) study life, job, and pay satisfaction of SOEP respondents. Applying a DiD-I approach, the authors find that all of these measures increased after the reform. The increase in life satisfaction after the minimum wage introduction amounted to 0.1 standard deviations. This effect was largely driven by East Germany, where the share of affected employees is especially high. Interestingly, the positive effect remains even when former employees who lost their job are included. However, the common trend assumption is likely at risk, especially for the job satisfaction measure. Moreover, the authors argue that effects on job satisfaction are potentially overestimated, since the control individuals are possibly negatively affected by the minimum wage introduction. Using the same identification strategy on the PASS, Pusch and Rehm (2017) find that satisfaction with wage, overall work, as well as the compatibility of family and work increased after the minimum wage started, even though the amount of work also rose. The authors do not further elaborate on whether the common trend assumption holds, though, and whether employees above the minimum wage experience a decrease of satisfaction. Using the LPP, Bossler and Broszeit (2017) find a positive effect on pay satisfaction. Moreover, this resulted in higher job satisfaction for employees feeling positively affected. The authors do not find any evidence for spillover effects to higher pay groups, indicating that really the absolute position in the wage distribution determines satisfaction rather than the relative position. Since increased satisfaction could also result in a higher motivation and thus in a higher individual productivity, Bossler and Broszeit (2017) also look at the impact on work engagement or turnover intention but do not find an effect. However, the data comprises of only two waves and the authors, therefore, cannot completely assert common trends. Overall, the results testify to an increase in different measures of satisfaction for those employees affected by the minimum wage. Yet, the studies cannot completely identify common trends and face the problem of a negatively affected control group, which is why the results should be treated with caution.

2.5 Outlook and Conclusion

Most of the international literature investigates *incremental* changes of existing minimum wages or wage floors with a small bite, sometimes only relevant for a small fraction of the population. In contrast, by introducing a statutory minimum wage at an unprecedentedly high level in January 2015, Germany provides a most interesting case study in order to better understand non-incremental minimum-wage adjustments. The evolving literature on the causal effects of the German experiment has already brought to light a number of interesting short-term results, which are summarized in this paper.

First, and foremost, one to two years after the introduction of the minimum wage, hourly wages at the bottom of the distribution have substantially increased. The employees who seem to have benefited the most are low educated, marginally employed, women, and people with a migration background. There is no strong evidence for spill-over effects to higher wage groups. Whilst this wage increase in the low-wage segment was one of the inherent goals of the minimum wage introduction, it has to be noted that there is also substantial evidence for non-compliance: In 2016 there were at least 750,000 eligible employees that were still paid less than the minimum wage, showing that the actual wage increases were not large enough for some employees.

Second, the introduction of the minimum wage did not have a significant positive effect on the livelihood of affected persons. This means that the intended goal of poverty and inequality reduction was not achieved in the short run and the amount of in-work benefit recipients decreased only slightly. This is, inter alia, due to the fact, that the reform caused a sizeable reduction in working hours such that monthly earnings for low-wage workers nearly stagnated.

Third, a small negative effect on overall employment can be stated. This effect is mainly driven by reduced hiring and a reduction of marginal employment (where some mini-jobs seem to have been transitioned into regular employment). However, compared to the ex-ante long-run predictions, these short-run effects are very moderate and it seems that the reform did not trigger substantial negative employment effects in the short run. While this could very well speak to the innocuousness of the wage floor and recommend further increases, the employment effects could also have been mediated by a variety of other factors. First, the non-negligible share of non-compliance might have caused labour costs to have increased less than expected. Second, the reform occurred during a time of economic upswing and a robust

economy which might have cushioned negative employment effects.[18] Third, while labour was not significantly substituted by capital in the short run, such substitution effects may be stronger in the long run. For all of these reasons, the absence of large job losses in the short run cannot yet be seen as a prediction for long-term labour market effects of a higher minimum wage. This is also why it might still be too early to consider larger increases of the wage floor. What seems to be more imminent is to ensure that all eligible employees also receive the current minimum wage. After all, public support of the reform is high and even increased after the introduction (see Fedorets and Schröder, 2017) and now the wage floor has to be fully enforced.

The avenue for more minimum wage research in Germany is wide open. First of all, many topics on the individual (e.g., labour market flows, net wages, household income, etc.), the establishment (e.g., profits, competition, etc.) and macroeconomic level are not studied exhaustively yet (for further fields of research see also Mindestlohnkommission, 2018b). Second, only future research can tell us something about the important medium- and long-term effects of the reform (see also Neumark and Wascher, 2007). This research will have to incorporate that special legal arrangements will expire and the minimum wage will be continuously adjusted. It was already raised to €8.84 effective 2017, and will increase to €9.19 and €9.35 in 2019 and 2020. Third, more research is needed on the suitability of available data. So far, all causal studies face the challenge that existing data sources have limitations. While administrative data often lacks information on working hours, survey data is potentially prone to measurement error. Therefore, investments in better data infrastructure but also more accurate recording of working hours would potentially help to improve evaluations and lead to a better understanding of the minimum wage effects. Initiatives that link administrative with survey panel data are an important step in this direction.

[18] See also Bossler and Möller (2018) and Neumark (2019).

Chapter 3

The Short-Run Employment Effects of the German Minimum Wage Reform

The chapter is joint work with Marco Caliendo, Alexandra Fedorets, Malte Preuß and Carsten Schröder and has been published in *Labour Economics*, 2018, Vol. 53, pp. 46–62.

Abstract

We assess the short-term employment effects of the introduction of a national statutory minimum wage in Germany in 2015. For this purpose, we exploit variation in the regional treatment intensity, assuming that the stronger a minimum wage 'bites' into the regional wage distribution, the stronger the regional labour market will be affected. In contrast to previous studies, we construct two regional bite indicators based upon detailed individual wage data from the SES 2014 and combine it with administrative information on regional employment. Moreover, using the SOEP, we are able to affirm the absence of anticipation effects and verify the assumption of a common trend in wages before the reform. In sum, we find only moderate negative effects on overall employment of about 140,000 (0.4%) jobs, which are mainly driven by a sharp decline of marginal employment ('mini-jobs'), while we do not find pronounced significant effects for regular employment in most specifications. Our results are robust to a variety of sensitivity tests.

3.1 Introduction

In January 2015, the German labour market was exposed to a massive intervention in its wage structure with the introduction of a national statutory gross minimum wage of €8.50 per hour applying to nearly all employees. The introduction of the minimum wage was preceded by a long debate among German economists and policy-makers about its potential risks and benefits. Advocates emphasised the primary policy targets of poverty prevention and inequality reduction (Bosch, 2007; Kalina and Weinkopf, 2014; BMAS, 2014), while opponents stressed the economic burden of the reform. Due to its high level and – with only a few exemptions – universal character, it was expected to affect more than one in ten employees in Germany, potentially leading to extensive job destruction (SVR, 2013, 2014; Müller and Steiner, 2010, 2011, 2013; Knabe *et al.*, 2014). Accordingly, the aim of our paper is to examine whether these earlier expectations have actually proven to be true in the short run.

In theory, the potential effects of minimum wages on labour demand depend on the market structure. While negative employment effects are expected in a competitive price-taker setting, a monopsonistic labour demand does not imply negative effects in general. Depending on the minimum wage level, the demand for labour may increase when employees are paid below the marginal product of labour. Moreover, the time frame in which employment effects should arise has not been determined and other adjustment channels might be used in the short run to postpone displacements, e.g. working hours (Stewart and Swaffield, 2008), profits (Draca *et al.*, 2011) or simply non-compliance (Metcalf, 2008). Identifying employment effects is therefore an empirical question that has been addressed with a variety of strategies and – in most cases – has come up with no or weak negative employment effects (Neumark and Wascher, 2007; Card and Krueger, 1995). Unfortunately, due to the universal validity of the reform, the set of empirical identification strategies is considerably restricted in the German case. We base our analysis on the approach suggested by Card (1992), which relies on the degree to which regional labour markets are affected by the minimum wage. Between regions, earnings and wages differ due to structural and environmental differences. This variation implies that a nominal minimum wage affects regions to different intensities. The stronger that a minimum wage 'bites' into the regional wage distribution, the stronger the regional labour market is affected. We adapt this approach to the German case and apply a difference-in-difference framework to analyse short-term effects of the minimum wage on employment for the first year after the policy reform.

Since the definition of the treatment indicator – i.e. the degree to which a region is affected by the minimum wage – is crucial to our identification strategy, we construct two commonly-used bite measures: the *Fraction* and the *Kaitz index*. The Fraction reflects the share of affected eligible employees per region, while the Kaitz index displays the relation of the minimum wage to the regional mean wage. Moreover, the construction of the regional bite also calls for a definition of a suitable area classification. We rely on 141 distinct Regional Labour Markets (RLMs) as proposed by Kosfeld and Werner (2012). Since this approach aggregates areas according to economic performance and commuter flows even across federal states, it allows constructing credible RLMs. We draw upon data from the comprehensive Structure of Earnings Survey (SES) 2014, which contains detailed individual information on wages, working hours and minimum wage eligibility. Therefore, we are able to compute precise bite measures based on the regional hourly wage distributions. This is a crucial advantage to previous studies (e.g. Garloff, 2016) which relied on monthly income of full-time workers only, imposing strong assumptions on distributions of hours and wages. This is to say, as opposed to data from previous studies, the SES enables the derivation of precise bite measures.

The minimum wage introduction was preceded by a legislative process that allows for potential anticipation effects, which have largely been neglected by previous studies on Germany. Since our identification strategy depends on the assumption that wages would have developed equally among low- and high-bite regions in the absence of the minimum wage reform, we have to test the notion that wages were not adapted in anticipation. For this purpose, we need to make use of data on the pre-treatment period. Unfortunately, the SES is only available every four years. Therefore, we additionally employ the SOEP, which is smaller in sample size but conducted every year. With the annual SOEP data, we can consider hourly wages in a time frame before the minimum wage was decided upon, allowing us to explore wage effects and their potential anticipation. The bite measures constructed with SES and SOEP display strong positive correlations. Accordingly, despite the SOEP's smaller sample size, its measures identify a similar variation in regional treatment intensity compared with the SES indicators. The pre-treatment analysis reveals no anticipatory effects, meaning that prior to the reform wages followed the same trend across regions with different treatment intensities. However, with the policy reform, the share of individuals earning less than the minimum wage substantially decreased in affected areas, while the mean wage – and thus the Kaitz – has hardly been affected.

For our estimation of employment effects, we combine our bite measures with administrative data on employment stocks from the Federal Employment Agency ('Bundesagentur für Arbeit', FEA), measuring the development of employment from 2012 onwards. As a first step we look at overall employment. Out of 37.4 million employed individuals in 2014, about 4 million were eligible for the minimum wage and earned less than €8.50 gross per hour. The degree to which workers are affected varies strongly with their employment type. Employees in full- and part-time employment (throughout the paper also called 'regular employment' that entails social security contributions) are generally less affected by low wages than people in marginal employment. Marginal employment (also called 'mini-jobs') is a specific type of employment in Germany with an income of up to €450 per month, which is exempted from income taxation and requires (almost) no employee-sided social security contributions, while the employer pays 30 percent flat charges. To disentangle effects on the regular employed from effects on the generally more affected marginally employed, we additionally look at both employment subtypes separately in a second step. In sum, we find a small but significant reduction in overall employment, indicating that an increase of one percentage point in the bite indicator is accompanied by an employment reduction of 0.03 percent. This is foremost caused by a reduction in marginal employment rather than a decrease of full- and part-time employment. While we do not find pronounced effects for regular employment, marginal employment reduced by 0.18 percent per percentage point increase in the bite indicator. These results are robust across all specifications and a variety of sensitivity tests. Our results are roughly in line with the previous literature on short-term employment effects in Germany, indicating that adaptation within the extensive margin of labour demand was less strong than expected. Using employer survey data, Bossler and Gerner (2016) find that the minimum wage led to the absence of about 60,000 new hirings, while Garloff (2016) – also applying the identification strategy by Card (1992) – identifies no effect on regular employment, but finds evidence of a shift from marginal to regular employment. When we predict the average employment effect that results from these estimations, we find that roughly 140,000 jobs (0.4 percent) were lost in total.

The remainder of this paper proceeds as follows. Section 3.2 introduces the legal framework of the German minimum wage and discusses expectations and previous findings. Section 3.3 considers the identification strategy, its implementation and our data sources. Subsequently, Section 3.4 displays the descriptive results for our bite measures and the employment data,

while Section 3.5 presents the main analysis of employment effects as well as robustness analyses and considerations about possible channels and dynamics. Finally, Section 3.6 concludes.

3.2 Institutional Details and Expectations

Institutional Details On January 1, 2015, the Minimum Wage law ('Mindestlohngesetz') entered into force, introducing a minimum wage of €8.50 gross per hour. Until then, wage floors were set by collective, voluntary agreements within specific sectors.[1] The formal decisions about future adjustments to the minimum wage are to be made by the German Minimum Wage Commission ('Mindestlohnkommission'). In light of minor short-term employment effects, the Minimum Wage Commission has recommended raising the statutory minimum wage by 34 cents per hour starting from January 1, 2017 (see Mindestlohnkommission, 2016a).

With the 2015 regulations, almost any employee in Germany is eligible for the minimum wage. Restrictions have only been introduced with respect to two dimensions. First, specific groups are excluded, namely the self-employed, trainees, specific types of interns[2], minors without vocational training, volunteers and the long-term unemployed. Second, albeit temporarily, sector-specific minimum wages under the national level of €8.50 remained valid until December 2016 and had to be adjusted afterwards. The exemption for the long-term unemployed is rarely drawn upon (vom Berge *et al.*, 2016c) and only few sector-specific minimum wages have been below the minimum wage (Mindestlohnkommission, 2016b; Amlinger *et al.*, 2016). Nonetheless, the exception for trainees and adolescents reduces the number of eligible individuals to a great extent. Table 3.1 summarises the number of beneficiaries. In 2014, about 5.5 million employees earned less than €8.50 per hour, of which 4.0 million (72 percent) were eligible for the minimum wage (Destatis, 2016a).

The timeline of the minimum wage introduction allows for potential anticipation effects, which previous studies on Germany have largely neglected. In September 2013, the German parliament ('Bundestag') was elected. Given that the major centre-left wing party (SPD) announced their uncompromising stance for a universal, nationwide minimum wage of €8.50

[1] Sector-specific minimum wages had been introduced over the last two decades in several sectors, including the construction sector or the roofing sector (in 1997), hair dressing (in 2013) and security services (in 2011). Most sector-specific minimum wages are higher than the statutory minimum wage and have been increased after the uniform minimum wage (Amlinger *et al.*, 2016). See Fitzenberger and Doerr (2016) for an overview.

[2] Interns are excluded if the internship is compulsory ('Pflichtpraktikum'), is either a voluntary accompanying or voluntary orientation internship ('freiwilliges Orientierungspraktikum' or 'freiwilliges ausbildungsbegleitendes Praktikum') that lasts less than three months or if it is an entry-level qualification ('Einstiegsqualifizierung').

Table 3.1: Minimum Wage Beneficiaries in 2014

	Absolute (in mio.)	Share of employed	Share of affected
Employed	37.4	100%	-
Wage <€8.50	5.5	14.7%	-
Wage <€8.50 and eligible	4.0	10.7%	100%
thereof			
West-German residents	2.9	7.8%	72.9%
East-German residents	1.1	2.9%	27.1%
Full-time employment	0.9	2.4%	22.4%
Part-time employment	0.9	2.4%	22.4%
Mini-jobs	2.2	5.9%	55.1%
Women	2.5	6.6%	61.7%
Men	1.5	4.0%	38.3%

Source: Destatis (2016a).
Note: Numbers base on SES 2014 and include public sector employees.

per hour, the inclusion of such a policy in the coalition contract with the major centre-right wing party (CDU/CSU) in December 2013 was commonly expected. Announced as a high-priority project in January, the law was then passed by the two German parliaments in July 2014. These decisions may have had instantaneous effects. According to Bossler (2017), employers affected by the minimum wage reported greater employment uncertainty in summer 2014. The potential anticipation of the new regulations could thus have affected employers' behaviour even before the minimum wage introduction. Therefore, in Section 3.4 we examine the bite indicators in the pre-treatment period.

Expectations and Previous Findings The wage floor of €8.50 places Germany in the middle of the international minimum wage ranking (OECD, 2015, p. 37). Expectations on its effects on employment were predominantly negative, even though both theory and empirics are not conclusive on this topic (see Neumark and Wascher, 2007, 2008). For the long run, ex-ante simulations predicted a reduction of 500,000 to 900,000 jobs, while positive effects on poverty prevention are small due to the German in-work benefits regulations and withdrawal rates from 80 to 100 percent for households with low income (Müller and Steiner, 2013; Knabe *et al.*, 2014). Then the reform has little impact on the budgets of employees but relaxes the public budget constraints. The largest impacts on earnings are to be expected in East Germany, for women and mini-jobbers since a great share of the beneficiaries belong to one of these groups (see Table 3.1). In fact, 2.2 million (55 percent) of the 4 million

beneficiaries were marginally employed. Moreover, one in five East-German residents earned less than the new wage floor in 2014, whereas only 9 percent within West Germany were affected. Differences in earnings also arise with respect to gender, since over 60 percent of all eligible employees who earned less than €8.50 were women (Destatis, 2016a).

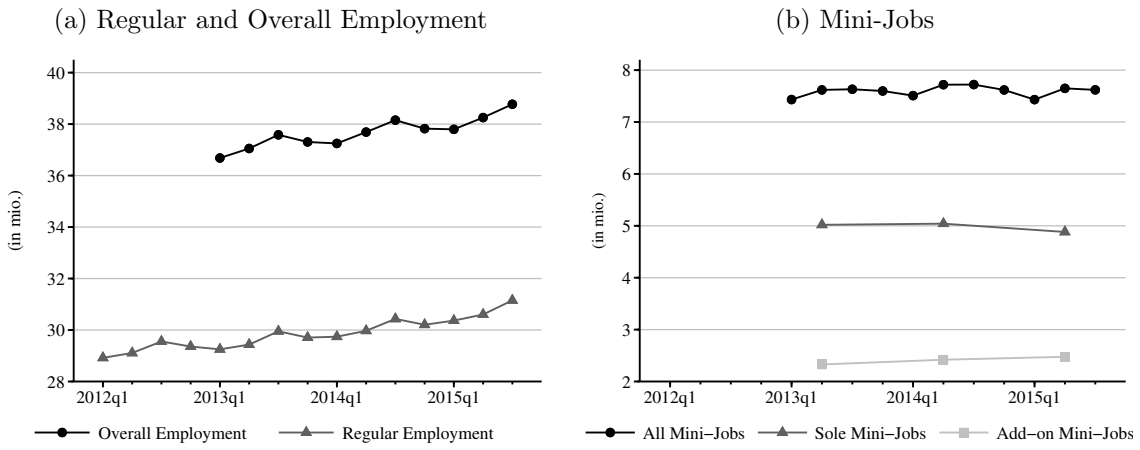
To shed more light on our target variable, Figure 3.1 shows the employment level between 2012 and 2015 (see Section 3.3.3 for a data description). First, we look at overall employment, which follows a seasonal trend but, in general, has been increasing over the last years. Secondly, we also distinguish between regular and marginal employment. This is due to the fact that mini-jobs hold special interest within the discussion about the minimum wage introduction since they display a high share of low hourly wages (see Table 3.1). Figures 3.1a and 3.1b show that the increase in overall employment before 2015 is caused by a rise in both considered employment types. However, only regular employment continued to increase after the minimum wage introduction. While in the second quarter of 2012 29.1 million people were regularly employed, this number had increased by 1.5 million three years later (see Figure 3.1a). Hence, from this descriptive perspective it does not seem as if the minimum wage affected regular employment. In comparison, marginal employment, which was following the upwards trend of regular employment (see Figure 3.1b) before the reform, hardly changed from 7.7 million to 7.65 million after the minimum wage introduction, giving a first descriptive indication that it was affected differently. This is likely a consequence of the special regulations on marginal employment.

Marginal employment is defined by a monthly income below €450 and can either be pursued as a primary/sole or a secondary/add-on job (although mini-jobs as add-on jobs are less frequent as indicated by Figure 3.1b). They require almost no employee-sided social security contributions and are exempted from income taxation, which is why gross income is nearly equivalent to net income for marginally employed.[3] The employer has to pay a linear income tax rate of 2 percent and social security contributions of approximately 30 percent.[4] However, when exceeding the mini-job threshold a worker can switch to a 'midi-job', which is defined by an income between €450 and €850. Then, the employers' social security contributions are reduced to the general amount of about 20 percent. For the employees, however, they follow a progressive design until the standard flat rate is reached at the upper threshold of the midi-job.

[3]Mini-jobbers are required to pay contributions to the social pension fund but can be liberated from that obligation upon request.

[4]Contributions are smaller if the employer is a private household.

Figure 3.1: Absolute Employment between 2012 and 2015



Source: FEA 2012-2015, own calculations.
Note: Data on marginal employment distinguished by add-on and sole employment are only available on a yearly basis measured at the end of the second quarter. See Section 3.3.3 for a description of the data.

Moreover, if it is an add-on job or the spouse is working, income taxes have to be paid as well. For these reasons, many marginally employed do not want to change their employment status. However, with the minimum wage introduction and the corresponding wage effect, some will exceed the threshold if their working hours are not reduced simultaneously. This transition, however, does not necessarily entail more net income for workers as they start to pay taxes and social-security contributions. Especially add-on jobs lose their financial attractiveness as taxes and social security contributions are paid in full. Sole mini-jobbers might take this chance to increase their labor supply since they exceeded the mini-job threshold anyway. But, since sole mini-jobs are typically chosen due to a limited time budget (e.g. by mothers) extending labor supply is often not desired (Bachmann *et al.*, 2017).

In sharp contrast, employers benefit from transferring mini-jobs into midi-jobs as their marginal social security contributions are strongly reduced as soon as employees enter a midi-job. In that sense, an increasing gross wage can lead to a decrease in total labor costs for the employer which is why transitioning workers to a midi-job might be beneficial to them. A reduction in mini-jobs thus does not necessarily result in rising unemployment, but may actually be associated with a demand side favoured increase in regular employment. The first evidence on such a substitution is discussed by vom Berge *et al.* (2016b,a). To investigate such possible effects we will not only look at overall employment but also distinguish between effects on regular and marginal employment separately in the empirical Section 3.5.1. Unfortunately,

our aggregate data does not allow to directly estimate transitions. We will therefore try to give some insight into potential underlying channels in Section 3.5.3.

In accordance with the descriptive perspective from above, the literature does not substantiate the huge negative employment effects that were foretold in ex-ante predictions (see Garloff, 2016; Bossler and Gerner, 2016). Applying the same identification strategy as we do, Garloff (2016) does not find any overall employment effects, but a shift from mini-jobs to part-time employment. He calculates that about 66,000 mini-jobs were lost after the reform. However, since he has no information on working hours, the analysis relies on strong assumptions when calculating the regional bite indicator. Due to a lack of working hours in the data, he determines workers to be paid below €8.50 per hour when they earn less than €1,400 a month, thereby assuming that full-time employees work 38 hours a week. This is a strong assumption which neglects the fact that full-time employment has a wide range of working hours.[5] Moreover, the analysis thereby completely disregards part-time and marginally employed, although they exhibit a vastly different wage distribution and are typically much more affected by low wages. These limitations can cause a measurement error that biases the estimates towards zero. Using employer survey data, Bossler and Gerner (2016) identify that 60,000 fewer jobs were created within firms affected by the minimum wage between 2014 and 2015.

3.3 Identification Strategy, Empirical Approach and Data Sources

3.3.1 Identification Strategy

The literature on minimum wage evaluation has used and discussed a variety of identification strategies for the causal evaluation of aggregated short- and long-term employment effects. However, a large part of the previous studies relies on legislative variation in the minimum wage regulation. This identification approach is not applicable to the German case, though, since there is no legislative variation between federal states. Moreover, exempted sectors and individuals differ substantially from their affected counterparts.[6]

Therefore, we follow Card (1992) who proposes an alternative identification approach using regional variation that does not depend on differences in legislation (for applications in

[5]Full-time employment is defined as working at least 30 hours a week. However, many contracts fix the number of weekly working hours to 40.

[6]The exempted sectors are the meat processing industry, hairdressers, agriculture and forestry sectors and – in East Germany only – temporary employment ('Leiharbeit') and textile producers.

the UK see Stewart, 2002; Dolton *et al.*, 2010). Conceptually, the intensity with which wages need to change in accordance with a minimum wage introduction is heterogeneous between areas. Where minimum wages 'bite' hard, adaptations in wages will be stronger and so should be the adaptation of labour demand. This approach is applicable to the German context and will be pursued in the subsequent analysis. With 2015 as the year of the minimum wage introduction, the following structural model summarises this relationship:

$$\Delta W_{j,2015} = \alpha + \beta Bite_{j,2014} + u_{1,j} \tag{3.1a}$$

$$\Delta E_{j,2015} = \gamma + \eta \Delta W_{j,2015} + u_{2,j}, \tag{3.1b}$$

where $\Delta W_{j,2015}$ describes the changes in aggregated wages for region j between 2014 and 2015, i.e. $\Delta W_{j,2015} = W_{j,2015} - W_{j,2014}$. The wage change during the minimum wage introduction depends on three elements: the average change (α), the lagged minimum wage bite in area j ($Bite_{j,2014}$) and an error term ($u_{1,j}$). Following Card (1992), β then describes the average effect of the minimum wage on wages. However, $Bite_{j,2014}$ does not affect employment (E_j) directly. Given a labour demand elasticity of η, only ΔW_j is transferred to employment changes. Substituting Equation (3.1a) into (3.1b) emphasises this relation, i.e.

$$\Delta E_{j,2015} = \gamma_0 + \eta\beta Bite_{j,2014} + \epsilon_j, \tag{3.2}$$

with $\epsilon_j = \eta u_{1j} + u_{2j}$ and $\gamma_0 = \gamma + \eta\alpha$. The product $\kappa = \eta\beta$ can then be interpreted as the causal effect of the minimum wage on employment.

3.3.2 Empirical Approach

Based on Equation (3.2), we estimate employment effects in accordance with Card (1992) and Stewart (2002). However, while they look at changes in the employment-to-population ratio, we decided to use the log employment level as the dependent variable. This is because the employment-to-population ratio not only reflects changes in employment levels, but also changes in the population, which held particular relevance in 2015 due to a large inflow of migrants. We thus analyse the log employment level but include population levels as a control variable in our specifications below (and test the robustness of this approach extensively in Section 3.5.2).[7] Furthermore and in advantage of the stylized model from above, estimations

[7] Highly-affected areas underlie a significant reduction in population until 2014, while population grows in high-wage areas. In light of the increasing number of refugees, the negative trend in low-wage areas stopped, although the differences between areas prevail from 2014 to 2015. Although the inflow of migrants increases

do not need to be restricted to the year of introduction. Additional years may be included in order to control for anticipation and contradiction with the common trend assumption. Then, instead of using the change in employment as left hand side variable, fixed effect estimations on employment levels are a more appropriate choice as they control for time persistent characteristics best. Accordingly, the annual log employment level is estimated by:

$$E_{j,t} = \gamma_j + \gamma' T_t + \theta_1' T_t \times Bite_{j,2014} + \delta X_{j,t} + \upsilon_{j,t}, \tag{3.3}$$

where $E_{j,t}$ denotes the log employment level (either for overall, marginal or regular employment as will be explained in Section 3.5.1) in period t, γ_j a region-fixed effect, $X_{j,t}$ a set of regional controls and $\upsilon_{j,t}$ the error term. T_t denotes a year vector, which we expand from the years around the minimum wage (2014 and 2015) to 2013 and 2012 to evaluate the identifying common trend assumption (which we also discuss graphically in Section 3.4.2). The model estimates the reform's effect based on the pre-treatment bite in year 2014.

The definition of $Bite_j$ is crucial to the analysis, which is why the literature discusses several alternatives. Most prominent is the Kaitz index (*'Kaitz'*), which measures the ratio between the minimum wage and the regional mean wage. The higher the Kaitz, the stronger that the minimum wage bites. However, its development is not exclusively determined by changes caused by the minimum wage. Movements in other parts of the wage distribution also affect this indicator. Card (1992), Stewart (2002) and Dolton *et al.* (2010, 2012, 2015) rely on the share of the employed population earning less than the minimum wage (*'Fraction'*). In contrast to the Kaitz, this definition rather focuses on the group of affected individuals. It depicts how many of the working population eligible for the minimum wage are actually affected by it. However, please note that the fraction neglects the density below the wage floor since the low-wage employed affect the indicator independently of their distance to the threshold. Therefore, the suitability of Fraction and Kaitz as a treatment intensity indicator hinges on the assumption that the relative wage distribution below €8.50 per hour or below the mean wage, respectively, is similar among all regions. We construct both bite measures on a scale from 0 to 1 and include them in our analysis. To evaluate the general robustness of our approach, we will also test binary specifications of the bite measures as well as different

the population size, most of them were not allowed to work yet, such that the absolute employment level is most likely not as affected. They are likely a result of the migration from the countryside to urban areas, as well as the migration from East to West Germany. Nonetheless, not controlling for these diverging trends may lead to wrong presumptions regarding the employment effects, which is why further analysis needs to control for population size.

bite definitions in the later sensitivity analysis in Section 3.5.2.

3.3.3 Data Sources

To identify the minimum wage effects on the basis of regional variation, we need comprehensive wage data on the eligible population as well as employment stocks on the regional level. Moreover, to control for pre-treatment trends we need data from the years 2012 to 2015. Thus, our analysis combines different data sources, taking advantage of the differing scopes of the data sets. For our employment measures, we rely on administrative information provided by the FEA (2016). However, the wage data is drawn from two different sources, given that we need extensive data both in terms of the time frame and concerning the number of observations. The longitudinal design of the SOEP allows us to test our identifying assumptions for the pre-treatment period. However, our identification strategy requires computing the regional average wage for a multitude of regions and the number of participants in the SOEP does not ensure a sufficient sample size per region. This is why we use the SES data for our analysis, which is considerably larger in sample size but only takes place at a four-year interval.

Wage Data To evaluate the minimum wage introduction, the availability of comprehensive information on earnings and working hours is crucial. Since the policy reform targets hourly wages, corresponding data is needed for the calculation of the bite. Additionally, marginally employed persons should be identifiable in addition to full-time employed, since they compose the most affected groups. Moreover, the proposed difference-in-difference approach relies on a common trend assumption with respect to wages. Information on the bite is thus not only needed during the reform, but also before any potential anticipation took place, which is why longitudinal data is needed. In the German case, one can rely on different alternatives with respect to regional wage data. In previous studies (see Garloff, 2016), administrative data such as the remuneration statistic ('Entgeltstatistik') has been used to derive regional bite levels.[8] Unfortunately, these statistics do not include detailed information about working hours. Hence, either full-time employment with the same number of working hours has to be

[8]The remuneration statistic is released by the Federal Employment Agency and reports information on regular employment and mini-jobs relationships, including both employment information such as average monthly payments and individual sociodemographic information. For further information, see `www.statistik.arbeitsagentur.de/Navigation/Statistik/Statistik-nach-Themen/Beschaeftigung/Entgeltstatistik/Entgeltstatistik-Nav`.

assumed for everybody or working hours have to be imputed. However, neither solution has been shown to provide a precise measure of hourly wages.

An alternative source of individual wage data is the SES 2014.[9] It is an employer data set with more than 70,000 firms with one million workers overall. In April 2014, representatively-chosen firms were legally obliged to provide detailed information on the income and working hours of their employees (and thus the data does not suffer from systematic bias caused by the sampling process or non-response). Due to its scope, the SES is perfectly suited to aggregate individual wage data at any regional level to derive precise bite measures. Although the SES is considered only representative at the level of federal states, it still contains considerably large sample sizes in our classification of 141 RLMs.[10] Unfortunately, the SES only takes place at a four-year interval. Thus, only data from the year prior to the minimum wage introduction is available, which makes it impossible to analyse short-term changes in bites or wages or to test for anticipation.[11]

For this reason, we rely on an additional data set, namely the annual Socio-Economic Panel (SOEP, 2016, v32). The SOEP is an ongoing panel survey with currently about 30,000 survey participants per year, conducted since 1984 (see Wagner *et al.*, 2007). Similar to the SES, the SOEP allows us to retrieve individual information about employment, earnings and working time. However, the SOEP has its own limitations since surveys are typically prone to measurement issues. Participants may refuse answers or misreport income or working hours, which can potentially lead to measurement errors. Moreover, the division of the data into small-scale areas results in small sample sizes, questioning the precision of the regional indicator. We will therefore first evaluate whether the SOEP is actually suited to derive bite measures on small-scale levels by comparing them to the SES indicators. Subsequently, we can use it to evaluate whether wages have adapted in anticipation of the reform and how they changed after its introduction. Finding no anticipation and no contradiction with the common trend assumption within the SOEP, we can make use of the more comprehensive SES bite measures from April 2014 for our regression analysis of employment dynamics.

[9]Source: FDZ der Statistischen Ämter des Bundes und der Länder, Verdienststrukturerhebung, 2014.

[10]However, note that the lack of representativeness at the RLM level might lead to measurement error and thus possibly to attenuation bias in our later analyses.

[11]In light of the minimum wage introduction, an additional, voluntary survey was conducted in 2015 ('Verdiensterhebung 2015'). However, only 6,000 firms among the original sample participated. The representative character of the SES 2014 is therefore at risk (Destatis, 2017a) and we refrain from using it for our analysis.

Hourly Wages We derive hourly wages in the SES by dividing information about gross monthly wages excluding compensation for overtime and surcharges by monthly paid hours without overtime for workers who are paid based on their working hours. For all others, the income is divided by regular weekly working hours multiplied by average weeks per month. As the SES does not provide regional information of civil servants at a smaller level than federal states, the overall sample size reduces to 780,000 observations. Therefore, mean wages could be underestimated. In order to prevent outliers in hourly wages from biasing our results, we winsorise the data and set the first and last percentile of the overall hourly wage distribution to the value of the corresponding percentiles. We exclude interns, trainees, and minors without formal training from the sample, since they are exempted from the minimum wage. However, since we cannot differentiate between agreements above and below €8.50, we do not drop employees with sectoral bargaining agreements. The SES provides weights for both firms and employees. However, they are constructed to weight observations at the level of federal states. Since we conduct our estimations at a smaller regional level, we refrain from weighting our estimations. Nonetheless, we include weights as a sensitivity check in Section 3.5.2.

Similar to the SES, hourly wages are not retrieved directly in the SOEP but can be computed as the ratio of gross monthly wages and weekly working hours adjusted by average weeks per month. As the SOEP not only includes contractual weekly working hours but also the actual working time – which includes paid and unpaid overtime – we derive the bite measures for both concepts and compare them with each other. In principle, using actual working time is the more accurate measure as minimum wages need to be paid for any working time. However, the SOEP asks for weekly working hours in general and for income for the previous month. Since both measures thus do not necessarily match each other, this possibly leads to measurement errors. Nonetheless, since the contractual wages are closer to the hours concept of the SES, our analysis focuses on contractual wages. Again, we restrict the SOEP sample to eligible employees reporting all necessary wage information, and apply a top and bottom recoding at the first and last percentiles to avoid measurement errors. Moreover, the sample is restricted to individuals participating after February, since the income question refers to the previous month and thus to the previous year in case of January.

Classification of Regions We use the two wage data sets to compute annual characteristics of regional wage distributions. For this purpose, each individual is assigned to one region.

Nonetheless, the choice of the regional level comes with a trade-off: the smaller the area classification, the better that heterogeneous labour market performances and their variation can be captured, although the more likely it is that the economic structure of a region is not picked up accurately, e.g. in areas with high commuter flows. In this sense, the sixteen federal states might be considered too broad a classification, since there is a substantial amount of heterogeneity within each state. The 401 administrative districts ('Landkreise') account for this dissimilitude more accurately, although they are also more prone to high commuter flows, which is why measures like GDP per capita may not reflect the actual economic performance of a district. Moreover, while the SOEP data provides information on the place of residence, the SES identifies the place of work of each respondent. The classification should therefore unite both concepts.

A common solution to these problems is using area classifications that account for commuter flows and general economic performance. In the following, we rely on 141 RLMs defined by Kosfeld and Werner (2012) as they consider commuter flows and regional labour market seclusion. Moreover, they do not coincide with the federal states, which is especially expedient for metropolitan areas and city states. As an additional sensitivity check, in Section 3.5.2 we replicate our estimation based on another area classification, the 96 planning regions ('Raumordnungsregionen', ROR). They divide Germany into segregated regions by commuter flows and economic structure and are defined by the federal states according to their own regulations (for details see BBSR, 2016).

Since the SOEP sample is not uniformly distributed in Germany, some regions have only few observations. Thus, in addition to the previous restrictions to the SOEP, we discard regions with fewer than 30 observations per year as they are strongly dependent on single individuals and thus are relatively volatile in their bite measures. For this reason, we omit 48 RLMs in the analysis with the SOEP. After this restriction, we observe 89 individuals on average per region and year. When using the SES 2014, we rely on average on 5,345 individuals per RLM in 2014 with the smallest (largest) region containing 366 (46,202) observations. Based on these regional wage distributions, we compute the previously-introduced Kaitz and Fraction bite measures for both data sets.[12]

[12]Please note that the SOEP and SES hourly wage data slightly differ. The SOEP only captures the main occupation, while SES also entails secondary employment. Moreover, SOEP respondents are registered according to the residence principle, whereas SES data is collected in compliance with the place of work principle. This is especially crucial for the regional classification in our analysis. However, we account for this by allowing for commuter flows. For a summary of differences between the two data sets and a review of the consequential computation of hourly wages, see Dütsch *et al.* (2017).

Employment Data Following our theoretical considerations, heterogeneous wage effects between regions can – depending on the labour demand elasticity – cause varying adaptation in employment. This will be analysed in our regression analysis in Section 3.5. For this purpose, we combine our regional bite measures with administrative data on employment stocks. We examine overall employment defined as the sum of regular and marginal employment, relying on administrative information provided by the FEA (2016), where marginal employment includes jobs carried out as sole employment as well as add-on mini-jobs. Data on regular employment is available from 2012 onwards and information on mini-jobs is at hand from 2013 onwards.[13] Since we only have the total employment head count, we cannot differentiate between sectors exempted from the national minimum wage and non-exempted industries. The labour market data is available for administrative districts ('Landkreise'), although for the aforementioned reasons we aggregate it to our RLM by summation. Although the data at hand is on a quarterly basis, we will focus on annual effects to abstract from any seasonal effects. As a reference point in time, we choose the second quarter of each year (June 30th), as the points of the legislative process (parliament election in September 2013, law passing in July 2014, see Section 3.2) lie around that date. However, we exploit the quarterly data for the discussion of underlying dynamics in Section 3.5.3.

In our regression analysis, we will make use of additional control variables, namely the regional population level and GDP, taken from Destatis (2016b). Like the outcome variables, they are only available for administrative districts. Hence, we also aggregate them by addition. Note that the indicators are measured at the end of each respective year. We impute the population level for each quarter by geometric weighting and assume a constant flow of migration within a year. As the population, we employ the working age population between 15 and 65 years. GDP per capita is manually computed as the GDP-population-ratio.

3.4 Descriptive Results

3.4.1 Bite Variation and Correlations

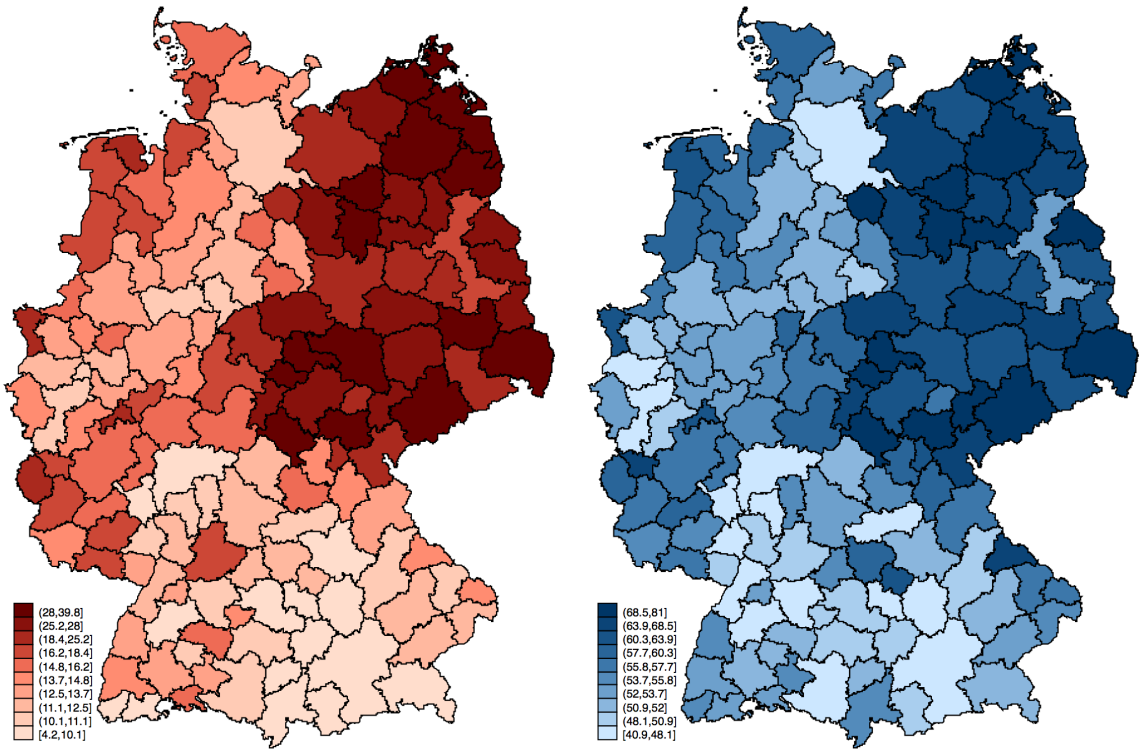
Regional Variation First, we evaluate the geographical structure of the bite as its variation is crucial for the identification strategy. Only if areas are affected differently can wage effects and thus employment effects be identified by the suggested model. Figure 3.2 presents

[13]The definition of marginal employment changed in 2013. Until 2012, employment was considered a 'Mini-Job' up to a threshold income of €400. The FEA does not provide coherent information concerning marginal employment for administrative districts before 2013.

both Fraction and Kaitz from the SES for the 141 RLMs.[14]

Figure 3.2: Degree to which Regional Labour Markets are Affected in 2014

(a) Fraction (b) Kaitz



Source: SES 2014, own calculations.
Notes: Bite measures are divided into deciles, such that each category contains about the same number of regions in 2014.

Both bite measures reflect the long-lasting and still significant structural differences between East and West Germany, since the bite is considerably higher in the eastern part. However, there is not only a substantial variation between East and West, but also within. Figure 3.2a shows that there are RLMs in the West that display a high Fraction. This holds true for some areas close to East Germany, such as Göttingen and Goslar. However, regions closer to the German borders – such as Cham to the east, the Volcanic Eifel, Bitburg and Cleves to the West and Emden and Wilhelmshaven to the North – also display a high Fraction. On the other hand, cities and their surrounding areas like Munich, Düsseldorf or Hanover, as well as highly-industrialised areas, such as east Lower Saxony or Rhine-Main report a relatively

[14]For reference, a histogram of the distributions of Fraction and Kaitz is presented in Figure C.1 (see Appendix).

lower bite compared with other parts of the corresponding federal state. Moreover, Figure 3.2b shows that the Kaitz and the Fraction do not always yield the same ranking of regions by bite. While the overall picture remains roughly the same, the Kaitz index displays more variation within Bavaria than the Fraction. There are also some differences for Baden-Württemberg and Rhineland-Palatinate. This is due to the fact that the Kaitz index measures the ratio between the minimum wage and the average wage. It is therefore not necessarily affected in the same way as the fraction of affected workers, since it only decreases when the average wage is in fact increased. Nonetheless, it can also be influenced by movements in other parts of the wage distribution. To get the full picture, we will therefore look at both measures in our following analysis. Overall, the graphical analysis shows that we observe considerable regional variation in the bite indicators, which can be used for the upcoming analysis.

Correlations between SES and SOEP Before analysing changes in bite indicators, we elaborate on whether the SOEP is suited to replicate the SES measures, despite its smaller sample size per region. For this reason Table 3.2 displays summary statistics for various indicators derived from the SES and SOEP for 2014. As the SOEP offers two wage concepts, bite indicators for contractual wages (SOEP_{con}) and actual wages (SOEP_{act}) are listed.
Table 3.2 shows that according to the SES data on average 17.1 percent of eligible workers earned less than €8.50 in 2014. In the SOEP, this amounts to 12.1 and 15.5 percent, respectively. This is roughly in line with the considerations of the German low-wage commission (Mindestlohnkommission, 2016b). The differences in levels between both data sets arise because the SES does not include any public sector employees at our regional level. Since only 0.9 percent of them are affected by the wage floor, excluding them increases the bite measures. This is also the reason why the table does not replicate the fractions of affected workers from Table 3.1.

The lower panel of the table displays the correlations between bite measures. The correlations between indicators from different data sets are considerably high. The correlation between SES and SOEP Fraction is 0.657 or 0.7, respectively. The Kaitz indices derived from the SES and the SOEP display a correlation of at least 76 percent. Due to these large correspondences, the identification of pre-treatment indicators with the SOEP is reasonable. We can attest that regardless of the known shortcomings of survey data, both SOEP and SES provide similar bite measures.

Table 3.2: Summary Statistics of Minimum Wage Bite Measures in 2014

	Fraction				Kaitz		
	SES	SES_{month}	$SOEP_{con}$	$SOEP_{act}$	SES	$SOEP_{con}$	$SOEP_{act}$
N	141	141	72	72	141	72	72
Mean	0.171	0.038	0.121	0.155	0.569	0.485	0.531
Sd	0.073	0.035	0.048	0.058	0.079	0.060	0.064
Min	0.042	0.002	0.029	0.044	0.409	0.368	0.406
33rd percentile	0.131	0.017	0.093	0.125	0.527	0.450	0.495
50th percentile	0.148	0.022	0.113	0.146	0.558	0.480	0.523
67th percentile	0.176	0.031	0.154	0.175	0.593	0.512	0.556
Max	0.398	0.157	0.238	0.276	0.810	0.640	0.716
			Correlation matrix				
Fraction SES	1.000						
Fraction SES_{month}	0.918	1.000					
Fraction $SOEP_{con}$	0.657	0.564	1.000				
Fraction $SOEP_{act}$	0.700	0.610	0.913	1.000			
Kaitz SES	0.896	0.847	0.616	0.679	1.000		
Kaitz $SOEP_{con}$	0.692	0.637	0.766	0.798	0.762	1.000	
Kaitz $SOEP_{act}$	0.704	0.653	0.759	0.815	0.775	0.988	1.000

Source: SOEP v32, SES 2014.
Note: Table presents bite measures divided by different definitions for Kaitz and Fraction. *SES* and *SOEP* denote measures based on corresponding data sets. $SOEP_{con}$ and $SOEP_{act}$ display measures based on contractual and actual working hours, respectively, while SES_{month} denotes the Fraction calculated with monthly wages of full-time employees only.

To relate our results to previous studies and make an additional contribution to the literature, we calculate one additional bite measure (Fraction SES_{month}) based on the monthly earnings of full-time employed.[15] Since many data sources lack information on working hours, using monthly wages of full-time employees only is common practice (see Garloff, 2016). However, the validity of this approach is strongly discussed. By neglecting the fact that especially part-time and marginally employed earned below the minimum wage, the fraction of affected workers could be strongly underestimated. Table 3.2 shows that the monthly bite for full-time employees amounts on average to only 3.8 percent, which is substantially lower than the bite based on hourly wages. This poses a problem for general statements about the degree to which regions were – or still are – affected. However, assuming that there are no systematical differences between regions, it is less crucial when the bite is used as a treatment indicator. Subsequently, it should only reflect a relative degree to which regions are affected,

[15]We follow Garloff (2016) and calculate the fraction of workers who earn less than €1,400 per month, are full-time employed and between 30 and 54 years old in proportion to all full-time employees of that age category.

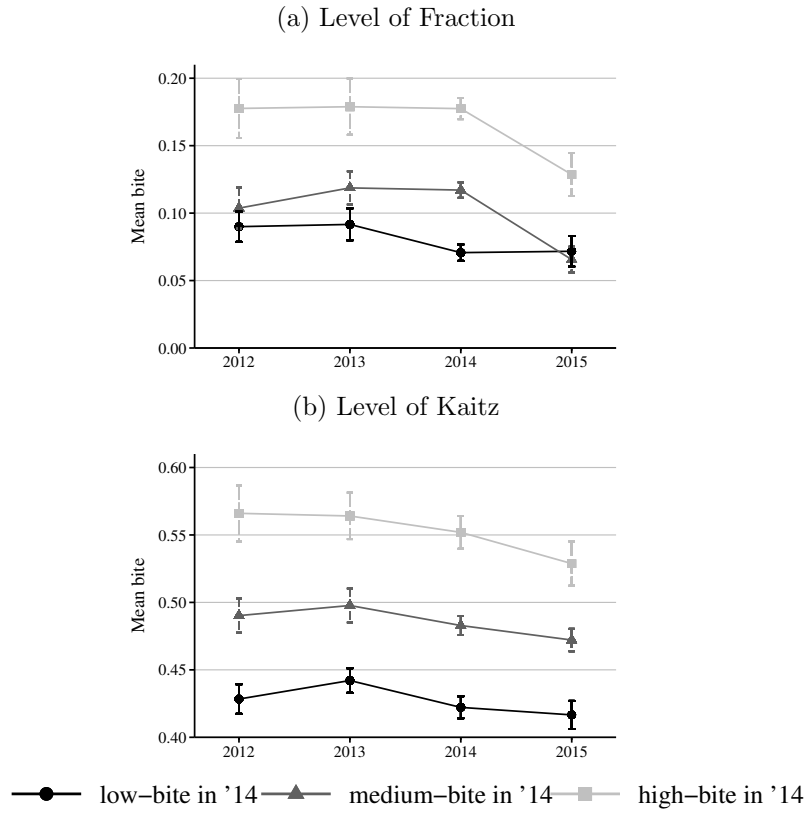
irrespective of its amount. The Fraction SES_{month} displays a very strong positive correlation with the SES Fraction derived by hourly wages ($corr = 0.918$). Accordingly, although the monthly bite is possibly less precise, both indicators measure the same relative treatment intensity. We will include the measure in our sensitivity analysis in Section 3.5.2 to test whether it yields similar results when used as a treatment indicator.

3.4.2 Anticipation and Common Trend

As already discussed, the SES does not allow evaluating wages in years other than 2014. However, this is possible with the longitudinal SOEP data. For the analysis, this is relevant for two reasons: first, as proposed by Equations (3.1a) and (3.1b), we only expect diverging changes in employment if wages adapt in light of the minimum wage reform; and second, the difference-in-differences framework does not only imply a common trend assumption concerning employment, but also in wages, namely that in the absence of the policy reform, wages and thus employment would have developed equally in all areas. Hence, we will now graphically examine whether we find equal trends in the pre-treatment time frame. To do so, we sort all considered regions into one of three groups – low-, medium- and high-bite areas – according to their bite level estimated with the SOEP in 2014. We follow Card (1992) and set the cut points in the respective bite such that each group comprises the same number of RLMs (see Table 3.2 for the corresponding thresholds, i.e. the 33rd and 67th percentile). Figures 3.3a and 3.3b present the average level of Fraction and the Kaitz-index in high-, medium-, and low-bite areas between 2012 and 2015.

In line with our definition, Fraction in high-bite areas is significantly higher compared to the other regions in all considered years (see Figure 3.3a). While the share of employed paid below €8.50 has been between 6 and 12 percent in low- and medium-areas between 2012 and 2014, we find shares above 15 percent in high-bite regions. The same ordering holds when we look at the Kaitz index (see Figure 3.3b). In the pre-treatment years, Fraction steadily remained large in high-bite regions, while it changed slightly in low- and medium bite regions. However, the differences are not strong, showing that trends have been roughly similar across regions. Moreover, Kaitz diminished significantly in all areas between 2013 and 2014. Accordingly, high-, medium- and low-bite areas shared a joint trend in wages before the reform when loking at this indicator. These results suggest that the common trend assumption for wages holds from this perspective, even though there are some slight differences in the

Figure 3.3: Level of Bite within High-, Mid-, and Low-Wage Regions



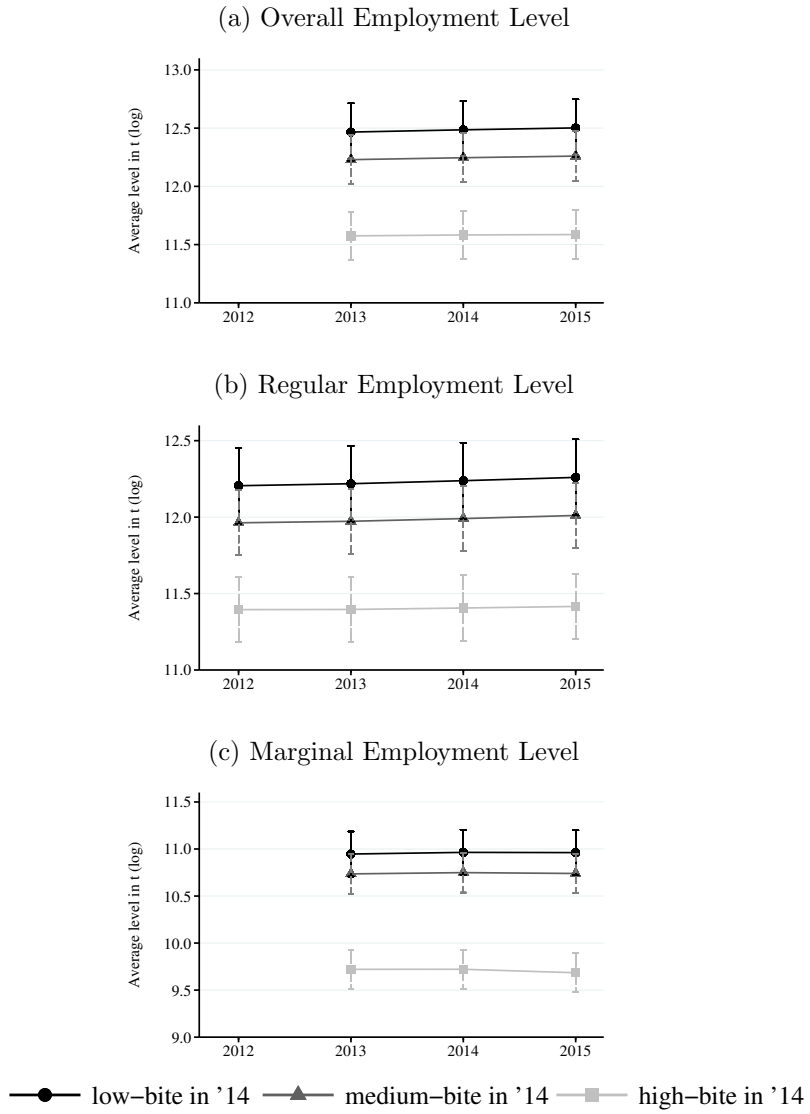
Source: SOEP 2012-2015, own calculations.
Notes: Figures 3.3a and 3.3b represent absolute levels of the bite indicator. Whiskers denote the corresponding 95 percent confidence interval. Each region has been sorted into the groups by its level of bite (*Fraction* or *Kaitz*) in 2014.

trends of the Fraction between regions.[16] After the minimum wage introduction, however, we see – as expected – diverging trends between regions. According to Figure 3.3a, Fraction decreased within medium- and high-bite areas by approximately 5 percentage points, while staying constant in low-bite areas. This indicates a positive wage effect in dependence of the regional bite level. However, the Fraction did not decrease to zero after the reform, seeing that the average Fraction in high-bite regions amounted to roughly 17.5 percent in 2014. The same pattern holds for medium- and low-bite regions. This is in line with the findings of Caliendo *et al.* (2017) and Burauel *et al.* (2017) who stress that the adaptation of wages – and thus Fraction – had not been executed completely in early 2015. Accordingly, regional mean

[16] We come to the same conclusion when we look at mean annual changes in the bite. We find no significant differences in changes in the bite between regions before the minimum wage reform. The results are available on request from the authors in the Supplementary Appendix.

wages and therefore Kaitz seem almost unaffected by the reform. Here, bite levels changed only marginally after 2014 and did not significantly differ between regional categories (see Figure 3.3b).

Figure 3.4: Mean of Logarithmic Employment Level within High-, Mid-, and Low-Bite Regions



Source: SES 2014, FEA 2012-2015, Destatis 2012-2015, own calculations.
Notes: Whiskers represent the corresponding 95 percent confidence interval. Each region has been sorted into the groups by its level of *Fraction* in 2014. Information on mini-jobs is only available from 2013 onwards. All values measured at the end of the second quarter (June 31st).

Since our estimation strategy also assumes that employment would have followed the same trend in absence of the reform, we need to look at this dimension, too. Regions are assigned into one of the three groups by their Fraction obtained from the SES, with the 33rd and 67th percentile of the bite's distribution posing as a threshold. Figure 3.4 shows the average

logarithmic levels for the three employment types – that will be used later on as outcome variables – in the bite groups. It can be seen that both overall and regular log employment are significantly lower in high-bite regions, which is a cause of very different population levels across the region types. However, Figure 3.4 shows that the three bite groups share a common trend over the years, implying that the regions would have developed equally in absence of the minimum wage reform. This holds true for the whole time horizon, even after the wage floor introduction. For marginal employment, differences in levels between regions are even more pronounced.[17] Yet, the common trend in employment before the reform also holds for mini-jobs. In contrast to regular employment, though, there appears to be a slight decrease in marginal employment between 2014 and 2015; potentially a reaction to the minimum wage introduction. However, the post-treatment effects are not as apparent in this illustration, since Figure 3.4 displays absolute log employment levels without accounting for the very different population levels. To provide additional evidence, we thus consider an alternative approach, where we look at conditional correlations between bite and employment level, thereby making use of the continuity of our bite indicator. We do this by regressing the log employment level on the bite measure interacted with the year dummies (and other control variables). As presented in Figure C.2 in the Appendix, the corresponding coefficients do not change much before the reform, indicating that the relationship between bite and employment was stable in the pre-treatment years. After the reform, only marginal employment seems to be affected. When additionally controlling for time fixed effects and population level (see Figure C.2b), the post-reform change for marginal employment becomes even more pronounced, while there is no difference before 2015. This analysis thus supports the common trend assumption. Moreover, it makes clear that the diverging population levels hamper the graphical evaluation of changes in employment levels. Population is shown to be a crucial component that we have to take into account in our further analysis.

3.5 Employment Effects

3.5.1 Main Effects

After looking at the employment dynamics descriptively, we will now apply a fixed-effects estimation to evaluate employment effects of the new minimum wage in a difference-in-difference

[17] While the different levels of absolute regular employment between high- and low-bite areas can be explained by differences in population, mini-jobs are more frequent in low-bite regions even in relative terms. Evidence on this is available on request from the authors and in the Supplementary Appendix.

framework. We derived our estimation equation (3.3) in Section 3.3.2:

$$E_{j,t} = \gamma_j + \gamma' T_t + \theta_1' T_t \times Bite_{j,2014} + \delta_1 GDP_{j,t-1} + \delta_2 POP_{j,t} + \upsilon_{j,t},$$

where $E_{j,t}$ denotes the log employment level and is measured on 30th June in each year.[18] As discussed above, we will estimate the effects on overall employment, as well as for marginal and regular employment separately in the following. The treatment effect is identified by the coefficients of the interaction term between bite and year (θ_1). We are especially interested in the coefficient of the interaction term between the bite and the year 2015 (denoted as Bite × D2015 in the following tables), which identifies the employment change in the year of the minimum wage introduction. However, to control for pre-treatment trends and test the common trend assumption, we also include pre-treatment years, such that our observation period ranges from 2012 to 2015. For comparability the reference year remains fixed at 2014, such that the employment effects of the interaction terms between the bite and previous years (Bite × D2013 and Bite × D2012, respectively) have to be interpreted in relation to the year 2014. Besides the inclusion of time- and region-fixed effects[19], we also control for regional differences in logarithmic GDP in $t-1$, assuming that a region's economic power will have an impact on its employment. As employment is additionally strongly affected by the population size (see also discussion in Section 3.3.2 and 3.4.2), we also include the logarithmic population size in t.[20]

Effects on Overall Employment First, we focus on short-term effects on overall employment, i.e. the sum of full-time, part-time and marginal employment. Panel A in Table 3.3 presents the corresponding results. Columns (1) to (4) use Fraction, adding control variables as well as pre-treatment periods iteratively. Columns (5) and (6) repeat the main specifications for the Kaitz index. All specifications use 2014 as the reference year such that treatment effects in 2015 can be compared between different specifications.

[18] Including not only one point in time per year but rather all quarters yields similar results (and will be discussed in Section 3.5.3). However, variation between quarters indicates strong seasonal dependency, which can be ruled out easily by focusing on annual changes only.

[19] Using the change in employment as dependent and neglecting regional fixed effects yield the same results. The results are available on request from the authors.

[20] We thus assume that the current population level is not affected by the minimum wage. Although this might not be the case – i.e. the population in t is potentially endogenous – we chose this specification over the population in $t-1$. This is because the population changed especially in 2015, due to the large inflow of migrants, which would not be captured by the population in $t-1$. Moreover, since we only look at the first six month after the reform, adaptations in the current population caused by the minimum wage are not very likely.

Columns (1) and (2) present the employment effects from 2014 to 2015 without and with controls for population and GDP per capita. While GDP's effect is negligible, controls for current population dynamics appear, again, to be crucial. Due to the direct interrelation between population and the employment level, the population in t explains a great share of within and between variation in employment. This holds major importance for the upcoming analysis. Because high-bite regions have a smaller population and a diverging population trend (see Figure C.3 in the Appendix), variation in employment growth due to diverging changes in population will be attributed to the minimum wage when these differences are not controlled for.[21] Since population would have affected employment in the absence of the policy reform, controlling for the current population level is obligatory. By contrast, differences with respect to GDP seem to be controlled for by regional fixed effects.

Accordingly, controlling for population effects reduces the highly significant treatment effect from -0.089 in Column (1) to -0.03 in Column (2), which means that an increase of Fraction by one percentage point is associated with a reduction in employment by 0.03 percent ceteris paribus. Column (3) additionally includes the year 2013. This allows us to revisit the common trend assumption and control for any pre-treatment trends (common to all regions), giving us a more precise treatment effect. This more extensive estimation will serve as our preferred specification. While the effect diminishes slightly to -0.025, it remains highly significant. Moreover, the pre-treatment interaction term (Bite $\times$ D2013) does not display a significant coefficient supporting our conclusions from the descriptive analysis in Section 3.4 that the regions followed the same trend before the reform. Repeating the specifications from Column (2) and (3) with the the Kaitz index in Columns (5) and (6) yields similar effects. We thus conclude that there was indeed a negative effect on overall employment in dependence of the minimum wage bite and will now further explore what drives these effects.

[21] Additional information on trends of population are available on request from the authors and in the Supplementary Appendix.

Table 3.3: Employment Effects on Log Employment Level

	Fraction				Kaitz	
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Overall Employment					
Bite × D2015	-0.089***	-0.030***	-0.025***		-0.018***	-0.014**
	(0.009)	(0.008)	(0.007)		(0.007)	(0.006)
Bite × D2013			0.004			-0.003
			(0.007)			(0.006)
GDP (log, t-1)		0.004	-0.006		0.006	-0.010
		(0.019)	(0.018)		(0.020)	(0.019)
Population (log, t)		0.858***	0.932***		0.926***	1.000***
		(0.075)	(0.056)		(0.075)	(0.057)
Constant	12.107***	1.396	0.566		0.533	-0.244
	(0.000)	(0.944)	(0.694)		(0.921)	(0.701)
Region & Year FE	Yes	Yes	Yes		Yes	Yes
Observations	282	282	423		282	423
R^2 within	0.811	0.904	0.949		0.898	0.947
R^2 between	0.151	0.995	0.995		0.995	0.995
R^2 overall	0.032	0.995	0.995		0.995	0.995
	Panel B: Marginal Employment					
Bite × D2015	-0.233***	-0.168***	-0.177***		-0.109***	-0.115***
	(0.022)	(0.028)	(0.025)		(0.026)	(0.023)
Bite × D2013			0.049***			0.004
			(0.018)			(0.019)
GDP (log, t-1)		0.069	0.034		0.081	0.016
		(0.061)	(0.055)		(0.067)	(0.059)
Population (log, t)		0.925***	0.807***		1.277***	1.187***
		(0.261)	(0.193)		(0.280)	(0.219)
Constant	10.480***	-1.688	0.119		-6.168*	-4.446
	(0.001)	(3.292)	(2.411)		(3.566)	(2.795)
Region & Year FE	Yes	Yes	Yes		Yes	Yes
Observations	282	282	423		282	423
R^2 within	0.727	0.761	0.704		0.709	0.642
R^2 between	0.310	0.924	0.923		0.922	0.920
R^2 overall	0.113	0.924	0.923		0.922	0.920
	Panel C: Regular Employment					
Bite × D2015	-0.080***	-0.017**	-0.011	-0.012*	-0.008	-0.004
	(0.009)	(0.008)	(0.007)	(0.007)	(0.008)	(0.007)
Bite × D2013			-0.005	-0.003		-0.006
			(0.007)	(0.007)		(0.006)
Bite × D2012				-0.001		-0.010
				(0.014)		(0.012)
GDP (log, t-1)		-0.023	-0.028	0.021	-0.023	0.017
		(0.020)	(0.019)	(0.018)	(0.021)	(0.019)
Population (log, t)		0.907***	0.997***	0.982***	0.971***	1.025***
		(0.086)	(0.065)	(0.057)	(0.089)	(0.060)
Constant	11.880***	0.807	-0.275	-0.539	0.015	-1.039
	(0.000)	(1.070)	(0.770)	(0.680)	(1.080)	(0.709)
Region & Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	282	282	423	564	282	564
R^2 within	0.886	0.941	0.962	0.955	0.940	0.955
R^2 between	0.120	0.994	0.994	0.995	0.994	0.995
R^2 overall	0.013	0.994	0.994	0.995	0.994	0.995

Source: SES 2014, Destatis 2012-2015, FEA 2012-2015, own calculations.
Note: Robust standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable is regular, marginal and overall employment in logarithmic terms, annually measured on June 30th. Marginal employment is characterized by a monthly income of up to €450. Bite measure is denoted by the first row. Reference year in all specifications 2014.

Revisiting our discussion from Section 3.2, where we argued that especially mini-jobs are of special interest for the wage floor evaluation, we will now investigate in a second step, whether employment effects are indeed largest for this group. To do so, we analyse the effects for marginal employment and regular employment separately.

Effects on Marginal Employment Panel B of Table 3.3 sheds light on changes in mini-jobs. As discussed above, this type of employment with monthly earnings of up to €450 is of specific interest as it is characterized by low gross hourly wages and small employee sided social security contributions. We follow the same structure as in Panel A. All columns of Panel B show that high-bite regions report a highly significant reduction in mini-jobs after the introduction of the minimum wage. Comparing Columns (1) and (2) shows that – very similar to overall employment – population size has a large impact on marginal employment throughout all specifications, while a region's GDP does not have a significant effect. Following Column (2) in Panel B, an increase in Fraction by one percentage point is associated with a reduction in marginal employment by 0.17 percent. Column (3) – which includes the year 2013 and the placebo test – yields a similar treatment effect of 0.177. However, since Column (3) of Panel B in Table 3.3 identifies a slight decrease in mini-jobs already from 2013 to 2014, we need to be cautious with the interpretation. With the reference year 2014, a one percentage point increase in Fraction translates to 0.05 percent more mini-jobs in 2013, pointing to the fact that mini-jobs already declined in highly-affected regions even before the minimum wage introduction. However, its magnitude is not as large as the effect from 2014 to 2015, which indicates that the additional reduction is likely due to the reform. Moreover, using the Kaitz index yields similar treatment effects without showing any anticipation, suggesting that the results are robust.

Effects on Regular Employment The estimation results for regular employment are reported in Panel C of Table 3.3. The overall picture is consistent with the previous results. Since regular employment makes up a large share of overall employment, the coefficients are closer to Panel A. However, the significance is not as prevalent as in Panels A and B, indicating that the main driver for the reduction in overall employment was the reduction in mini-jobs rather than a decline in full- and part-time employment. Column (2) implies a treatment effect of 0.017, meaning an increase in Fraction by one percentage point is associated with a reduction in regular employment by 0.017 percent. As in the previous Panels, Column (3)

appends the estimation by the year 2013. Since for regular employment we also have data for 2012, we additionally include Column (4) which even stretches the time horizon to 2012 and serves as our preferred specification for regular employment. The effect diminishes to -0.012 and is insignificant in Column (3) and close to being insignificant in column (4), meaning that the minimum wage affected regular employment only weakly at most. The pre-treatment interaction terms are insignificant, confirming the common trend assumption. Repeating the specifications from Column (2) and (4) with the the Kaitz-index in Columns (5) and (6) does not yield any significant effects.

Aggregated Employment Effects In order to provide a benchmark for the absolute nationwide impact of the minimum wage we will now translate the estimated treatment effects into aggregated effects. In the following, we use our results from Table 3.3 and predict the relative and absolute employment effect for the German labour market. For this prediction, we multiply our estimated treatment effects with the regional bite level. We assume that Fraction affects employment from zero onwards, i.e. a Fraction of zero is the counterfactual scenario. Since Kaitz has no such explicit level where a strictly positive minimum wage has no causal effect[22], we focus on Fraction here. After estimating relative employment effects, we put them in relation to the employment level in 2014 to compute absolute effects. Table 3.4 summarizes the predictions for all three employment categories and all main specifications using Fraction from above. The columns are numbered as in Table 3.3 and also include the corresponding coefficients.

On average, the minimum wage led to a relative reduction in overall employment from -0.42 percent to -0.52 percent. This amounts to a head count of 137,000 to 165,000 jobs less due to the wage floor. These estimations roughly correspond with Bossler and Gerner (2016), who estimate a negative effect of overall 78,000 jobs. For marginal employment, we compute an average relative employment effect of almost 3 percent translating to roughly 189,000 jobs. In comparison, vom Berge *et al.* (2016a) report a reduction of 94,000 mini-jobs between December 2014 and January 2015 in addition to any seasonal trends. We thus find an even larger negative effect on marginal employment from summer 2014 to 2015 than previous studies (see also Garloff, 2016). Looking at regular employment, we find no or, at most, weak negative effects of about 52,000 in our preferred specification which is to say that

[22]This is because the denominator, the mean regional wage, is a first raw moment of the wage distribution that provides little information on the 'bite' in the absence of information on higher-moments.

Table 3.4: Employment Effect Predictions with Fraction

	(2)	(3)	(4)
Overall Employment			
Coefficients	-0.030***	-0.025***	
Relative Prediction (in %)	-0.51	-0.42	
Absolute Prediction	-164,935	-137,342	
Marginal employment			
Coefficients	-0.168***	-0.177***	
Relative Prediction (in %)	-2.83	-2.97	
Absolute Prediction	-180,314	-189,432	
Regular employment			
Coefficients	-0.017**	-0.011	-0.012*
Relative Prediction (in %)	-0.30	-0.19	-0.20
Absolute Prediction	-78,013	-50,464	-52,450

Source: SES 2014, Destatis 2012-2015, FEA 2012-2015, own calculations.
Notes: Column numbers correspond to Table 3.3, 'Coefficients' displays the coefficients from each corresponding estimation, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 'Relative' is the unweighted average of all regions. 'Absolute' is the sum of all absolute regional employment effects.

the estimation does not imply strong effects after the minimum wage introduction on this dimension. However, since we assume a counterfactual scenario where the bite is zero and our baseline estimation for marginal employment questions the common trend assumption, the reduction of 189,000 should be interpreted with caution and considered as an upper bound.

Moreover, the fact that the estimated absolute effects of marginal and regular employment do not add up to overall employment has several reasons. First and foremost, marginal employment is not distributed evenly across the country (see again Figure 3.4). The neglected heterogeneity between regional labor markets may thus bias the treatment effect for overall employment. Moreover, the treatment effect of regular employment has a considerable standard error, which also manifest itself in a wide confidence interval of the presented aggregated effects. The difference may thus not be statistically significant. Finally, we see a slight significant decrease of marginal employment already before the introduction of the wage floor, which could cause an overestimation of the effects if it is a general trend in marginal employment. Consequentially, the combination of these issues could cause a divergence between the overall and the separate effects. Nevertheless, the results show that there was a reduction in overall employment in the short run.

3.5.2 Robustness Analysis

Thus far, we have found strong effects of the minimum wage on overall employment, mainly driven by mini-jobs, while regular employment has only been slightly affected. We will now test the robustness of our results in different directions, i.e. we employ another area classification, a weighted SES bite measure, the SES bite based on monthly wages, a bite measure constructed with the SOEP and two binary indicators. The results of the robustness tests are summarized in Table 3.5. The following tables use the Fraction indicator and include all pre-treatment years, since this is the most comprehensive specification.

Area Classification As discussed in Section 3.3.3, we apply another area classification to test the extent to which our results are dependent on the chosen definition of regions. Therefore, we re-estimate the effects based on the 96 planning regions. The results are displayed in Column (2) of Table 3.5. For reference, our baseline estimation from above is presented in Column (1). Both the magnitude and the significance of the effects are very similar to the results obtained using the RLMs, thus substantiating our findings.

Weighting of the SES Bite Measure The SES provides weights for firms and employees. However, they are constructed to be representative at the level of federal states. Since we use a smaller area classification, we have used unweighted data thus far. Nonetheless, since this decision is arbitrary, we conduct a sensitivity test by including weighted data. Column (3) of Table 3.5 provides the corresponding results. Once again, the results are very similar to our baseline results in terms of magnitude and significance.

Bite Measure Based on Monthly Earnings As discussed in Section 3.4, previous studies use information on monthly earnings only, thus relying on critical assumptions on working hours (see e.g. Garloff, 2016). We can review this alternative approach with our data and rely on a bite based on the share of full-time employed earning less than €1,400 per month. Column (4) of Table 3.5 displays a highly significant coefficient of -0.042 for regular employment in Panel B. While this effect is much higher in magnitude than our baseline result, it has to be evaluated in relation to the average Fraction. Given the lower level in the monthly Fraction (on average 3.8 percent, see Table 3.2), this translates into an average employment effect of -0.16 percent and thus an employment loss of only 37,000 jobs. While this is close to our baseline estimation, differences with respect to marginal employment are higher. Here,

Table 3.5: Robustness Analysis (Summary)

	(1) Baseline	(2) ROR	(3) Weights	(4) Month	(5) SOEP	(6) p25/p75	(7) p50/p50	(8) Ratio
	Panel A: Overall Employment							
Bite × D2015	-0.025***	-0.027***	-0.028***	-0.055***	-0.018*	-0.005***	-0.002*	-0.020***
	(0.007)	(0.007)	(0.007)	(0.014)	(0.011)	(0.001)	(0.001)	(0.006)
Bite × D2013	0.004	0.001	0.010	0.014	-0.004	0.001	-0.000	-0.001
	(0.007)	(0.008)	(0.007)	(0.013)	(0.009)	(0.002)	(0.001)	(0.005)
Observations	423	288	423	423	216	216	423	423
	Panel B: Marginal Employment							
Bite × D2015	-0.177***	-0.187***	-0.186***	-0.340***	-0.147***	-0.030***	-0.015***	-0.164***
	(0.025)	(0.030)	(0.025)	(0.048)	(0.043)	(0.005)	(0.003)	(0.020)
Bite × D2013	0.049***	0.064***	0.075***	0.094***	0.017	0.006	0.001	0.035**
	(0.018)	(0.024)	(0.016)	(0.033)	(0.031)	(0.005)	(0.002)	(0.014)
Observations	423	288	423	423	216	216	423	423
	Panel C: Regular Employment							
Bite × D2015	-0.012*	-0.012*	-0.014*	-0.042***	0.003	-0.004**	0.000	-0.011*
	(0.007)	(0.007)	(0.007)	(0.015)	(0.012)	(0.001)	(0.001)	(0.006)
Bite × D2013	-0.003	-0.010	-0.002	0.005	-0.016*	0.000	-0.001	-0.005
	(0.007)	(0.007)	(0.007)	(0.013)	(0.009)	(0.001)	(0.001)	(0.005)
Bite × D2012	-0.001	-0.017	0.000	0.019	-0.030*	0.002	-0.001	-0.004
	(0.014)	(0.017)	(0.014)	(0.027)	(0.017)	(0.003)	(0.001)	(0.011)
Observations	564	384	564	564	288	288	564	564

Source: SES 2014, Destatis 2012-2015, FEA 2012-2015, own calculations.
Note: Robust standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01. The table displays only the relevant coefficients of the estimations. Full results are available upon request from the authors in a Supplementary Appendix. Dependent variable is overall employment (Panel A), marginal employment (Panel B) or regular employment (Panel C) in logarithmic terms, annually measured on June 30th. Fraction is used as continuous bite measure in all specifications except Columns (6) and (7), where it is binary. Reference year in all specifications 2014. Controls, regional and time fixed effects are included. Controls are GDP in $t-1$ and Population in t. Specification (1) is the baseline estimation, (2) applies the regional concept of ROR, (3) relies on weights to compute level of Fraction, (4) applies Fraction of full-time employed earning less than €1,400 per month, bite indicators computed with SOEP 2014 are used in (5). Specifications (6) and (7) include the Fraction as a binary measure, with cut-off points at first and last quarter of the distribution and at the median, respectively. Specification (8) includes the employment-to-population ratio as the dependent variable.

the estimation in Column (4) shows a coefficient of -0.34, which means an average treatment effect of -1.27 percent and thus a reduction in marginal employment of 67,000 only. While this is exactly the effect obtained by Garloff (2016), it amounts to only one third of our previously-derived results. As discussed above, relying on monthly income of full-time employees only might bias the results. The difference could thus be explained by the lack of information on part-time employed and mini-jobbers within the computation of the bite, which appears to be crucial for the estimation of corresponding employment effects.

Bite Measure Based on SOEP Thus far, we have used SOEP data only to evaluate changes in bite over time, while we refrained from using it in our analysis of employment effects since it contains only little data for some RLMs and is somewhat prone to measurement error. However, we will employ the bite derived from contractual hours as an alternative data source for the employment effects estimation as a sensitivity test. The corresponding results are presented in Column (5) of Table 3.5. RLMs with fewer than 30 observation in the SOEP are not considered in the estimation, which is why the number of observations decreases quite drastically. The estimated treatment effects for overall employment and marginal employment remain significant but are considerably smaller in comparison to our baseline estimation (e.g. the treatment effect on marginal employment is now -1.3 percent compared to -2.97 percent in the baseline estimation). The effects on regular employment, on the other hand, are not significantly different from zero and there are weakly significant negative effects even before the introduction, implying that the common trend assumption does not hold. These differences could be due to the fact that the dropped 48 RLMs are foremost those with a low population density, typically also being areas with relative high bite levels. Clearly, this loss in information makes the estimation less precise and is not desirable in our situation.

Binary Treatment Indicator The lack of representativeness at the RLM level within the SES might cause a measurement error on the bite level, which could bias our estimates if the error correlates with bite or the outcome variable. To test the robustness regarding errors at the bite level, we additionally derive two binary treatment indicators, sorting regions into treatment and control groups based on the Fraction's distribution. Accordingly, the exact level is not decisive for the estimation, but rather the distinction between low- and high-wage regions only. In Column (6), regions in the highest quarter of the distribution are considered treated and the lowest quarter are sorted into the control group. Regions in between are discarded.[23] Column (7) alters the cut-off to the median, such that no RLMs are dropped. Using binary indicators yields similar results compared to our baseline estimation. Column (6) identifies a significant effect for regular employment. This means that regions in the highest quarter of the bite distribution had a significant reduction of 0.4 percent in regular employment compared to regions in the lowest quarter. As for mini-jobs, the results

[23]This leaves us with 36 RLMs in each group. What needs to be kept in mind with this separation is, that 32 of the high-bite regions in the treatment group are in East Germany, whereas all of the low-bite regions are in West Germany.

are again highly significant. Column (6) yields a treatment effect of 3 percent, which is nearly the effect of our baseline estimation. The softening of the treatment assignment in Column (7) diminishes the average treatment effect for both employment types. Given the less disjunctive definition, this is to be expected and the results substantiate our baseline estimations.[24]

Employment-to-Population Ratio As discussed in Section 3.3.2, we include the employment level in logarithmic terms as the dependent variable in our baseline estimation. We thus decide not to employ the widely used employment-to-population ratio on the right hand side, since it makes it difficult to disentangle the effects of employment and population. To test the sensitivity of this approach, we display the results of a regression of the ratio in Column (8) of Table 3.5. The coefficients and significances are very close to our baseline results, leading to slightly smaller predictions of absolute job losses.

3.5.3 Potential Channels and Dynamics

So far we have shown that particularly marginal employment decreased in course of the wage floor reform. However, it is possible that marginal employment was transitioned to part-time employment (see Section 3.2). Unfortunately, with our aggregate data we cannot disentangle whether mini-jobs were mainly lost or transformed into regular part-time employment. We will thus try to shed some further light on potential channels and dynamics behind these effects.

Examining Sole and Add-on Mini-jobs One way to examine the underlying mechanisms is to look at add-on and sole marginal employment separately. As discussed in Section 3.2, add-on mini-jobbers might not be as willing to transition to part-time as exclusively marginally employed. We would therefore expect a stronger reaction for contract adaptations in sole than in add-on mini-jobs, whereas unemployment should affect both groups similarly.[25] When we re-run the model with the level of add-on and sole mini-jobs separately (see Table C.1), we find a significant, negative treatment effect for sole mini-jobs only, whereas the number of add-on mini-jobs is not affected. With respect to the considerations from above,

[24] Using a binary specification that divides the treatment and control group at p33/p66 as well as constructing binary indicators with the Kaitz yield essentially the same results. The results are available on request from the authors.

[25] We cannot rule out potential differences between sole and add-on mini-jobbers, though, which can also explain these differences. If the latter are, for instance, more productive, job losses could be less likely for them.

the effect being only driven by sole mini-jobbers is an indication that the employment effect was mainly caused by transitions to part-time rather than to unemployment.

Seasonal Patterns and Quarterly Analysis Furthermore, we exploit the more detailed seasonal data on employment and evaluate *when* the discussed effects took place by estimating quarterly treatment effects (see Figure C.4). We find that regular employment was negatively affected by the Fraction in summer and autumn after the minimum wage was introduced. Yet, at the end of the fourth quarter of 2015 no difference to the previous years can be identified. Effects on regular employment may thus especially occur in summer, affecting seasonal or short-term hiring only. While marginal employment displays a seasonal dependence, too, the effect of the minimum wage occurred right after the introduction in the first quarter of 2015. After that, no strong further effects can be identified, implying that the reduction was mainly taking place immediately. In connection with vom Berge *et al.* (2016a) who report that part-time transitions were the main reason for a reduction in marginal employment in January 2015, this could also hint at the fact that mini-jobs decreased due to contract adaptations (which is easily done from an administrative point of view) rather than due to a change to unemployment. However, Figure C.4b also shows that a small reduction took place even in the months before the wage floor reform. Overall, our findings hint at the fact that a substantial share of mini-jobs was transformed rather than lost.

Non-Compliance While transforming marginal into regular employment is one adjustment channel for employers, another often discussed (illegal) alternative is simple non-compliance. As descriptively discussed in Section 3.4, the fraction of affected workers decreased after the minimum wage introduction. This is also supported by Caliendo *et al.* (2017) who find a positive effect on wages at the bottom of the wage distribution. However, it is also seen that the fraction did not decrease to zero in 2015, indicating that the wage floor was not fully enforced. Looking at contractual wages, Burauel *et al.* (2017) show that still eight percent of the eligible employees were paid less than €8.50 per hour, translating into roughly 2 million workers. These high levels of possible non-compliance could thus also explain why the short-term employment effects we find were not as strong as anticipated.

3.6 Conclusions

The introduction of the minimum wage in Germany was preceded by many – predominantly negative – expectations for the development of employment. In worst-case scenarios, a loss of 500,000 to 900,000 jobs was predicted for the long-run (Müller and Steiner, 2013; Knabe *et al.*, 2014). The evaluation of the effects of the minimum wage, however, is complicated by the universality of the policy, which reduces the set of potential identification strategies. The application of the minimum wage on almost any employee in every region does not allow for the use of the law for a quasi-experimental setting. For this reason, our analysis relies on regional differences in wage levels as a source of variation, as proposed by Card (1992). The approach exploits the assumptions that the higher the impact of the minimum wage on the regional wage distribution, the more strongly that the regional labour market is affected.

Therefore, we identify two commonly-used regional bite measures – the Fraction and the Kaitz index – which give us a broader picture of the degree to which regions are affected by the minimum wage reform. Using 141 RLMs, we are able to account for regional dependencies in economic performance and commuter flows. To assure a valid assessment of the minimum wage effects, we combine different data sets. Our main data source is the SES 2014, which entails comprehensive wage data from the year prior to the reform. However, since it is only available every four years, we extend our analysis by adopting the longitudinal SOEP, allowing to test our assumptions about anticipation in more pre-treatment periods. When examining the bite measures obtained by different concepts and different data sources more closely, we establish strong correlations between these concepts, thus declaring their application valid. This is substantiated by our estimation results, which do not yield considerably different results. Despite certainly being less precise, we can even validate the employment of the treatment intensity constructed with the monthly earnings of full-time employees only, if hourly wage data is not available in future research.

We use the SOEP to analyse potential anticipation effects in wages and show that overall there were no relevant anticipation effects before the introduction of the reform, such that we can assume that regional wages would have followed the same trend in the absence of the minimum wage. In summary, we find a significant negative effect of the minimum wage on total employment. Following our baseline estimation, overall employment reduced by 0.025 percent per percentage point increase in Fraction due to the minimum wage, which we predict to be roughly 140,000 jobs and approximately 0.4 percent of total employment. Our results

are mainly driven by a reduction in marginal employment. In accordance with descriptive studies (e.g. vom Berge *et al.*, 2016a) we find indications that at least parts of the losses in marginal employment could have been transferred into regular employment. But, while the effects on mini-jobs are considerably robust across specifications, estimations on regular employment do not allow for a clear conclusion. Here, we find no or, at most, weak negative effects of the minimum wage. However, transitions from mini-jobs to regular employment could have covered an underlying decrease of full- and part-time jobs triggered by the reform. Unfortunately, we cannot disentangle these effects since the data does not allow to follow individual employment spells. However, such transitions should not affect overall employment. Because overall employment decreased, we thus conclude that the policy reform affected labour demand negatively in the short-run. Yet, one limitation with our study is, that the SES data only ensures representativeness at the level of federal states. Thus, we induce possible measurement error by disaggregating to RLMs, which could bias our results towards zero. However, we address this issue by using weighted data as well as testing a binary indicator in our robustness analysis and the results do not hint at any major problems.

Although our results lie above the effects found in previous studies, they are still well below the predictions of 500,000 to 900,000 job losses. One possible explanation for this is that we only identify short-term effects, while the effects predicted in ex-ante studies are catered to the long run. However, job losses could have also failed to appear because other channels of adjustment were chosen. Correspondingly, employer surveys show that alternatives to displacements such as the reduction in working hours or an increase in prices, have been preferred to date (Bellmann *et al.*, 2016; Mindestlohnkommission, 2016b; Sauer and Wojciechowski, 2016). An additional explanation could be the existence of a monopsony, where the current minimum wage lies below the marginal product of labour, in which case theory would not predict any job losses. Moreover, while in a companion paper (see Caliendo *et al.*, 2017), we show that wages did in fact increase for employees at the bottom of the wage distribution, there is strong evidence that wages had not adapted fully in 2015, and about 8 percent of the eligible population were still earning less than the minimum wage per hour shortly after the introduction (Burauel *et al.*, 2017). This points to a generally-postponed adjustment, in which case labour demand would not have fully adjusted in 2015 either. The near absence of an effect on regular employment could then also be explained by non-compliance. In this case, it might be too early to derive policy conclusions. However, the

reduction in the number of marginally employed – which was the most affected group with relatively easy adjustment possibilities – might serve as a precursor for more severe effects on regular employment in the long run once labour demand has fully adjusted and the minimum wage is fully enforced. Thus, the effects of the minimum wage will have to be re-evaluated when sufficient long-term information is available. Moreover, the adaptation of the wage floor from €8.50 to €8.84 introduces new movement and a possible route for further evaluations.

Chapter 4

Did the Minimum Wage Reduce the Gender Wage Gap in Germany?

The chapter is joint work with Marco Caliendo and has been published as a CEPA Discussion Paper, 2021, No 40. It is currently in a revise and resubmit process at *Labour Economics*.

Abstract

In many countries, women are over-represented among low-wage employees, which is why a wage floor could benefit them particularly. Following this notion, we analyse the impact of the German minimum wage introduction in 2015 on the gender wage gap. Germany poses an interesting case study in this context, since it has a rather high gender wage gap and set the minimum wage at a relatively high level, affecting more than four million employees. Based on individual data from the Structure of Earnings Survey, containing information for over one million employees working in 60,000 firms, we use a difference-in-difference framework that exploits regional differences in the bite of the minimum wage. We find a significant negative effect of the minimum wage on the regional gender wage gap. Between 2014 and 2018, the gap at the 10^{th} percentile of the wage distribution was reduced by 4.6 percentage points (or 32%) in regions that were strongly affected by the minimum wage compared to less affected regions. For the gap at the 25^{th} percentile, the effect still amounted to -18%, while for the mean it was smaller (-11%) and not particularly robust. We thus find that the minimum wage can indeed reduce gender wage disparities. While the effect is highest for the low-paid, it also reaches up into higher parts of the wage distribution.

4.1 Introduction

The differences between men's and women's earnings have been studied extensively over recent years. One of its dimensions is the fact that women are often overrepresented among the low-paid employees (Kahn, 2015; Card *et al.*, 2016). This leads to the existence of a wage gap at the bottom of the distribution, also called 'sticky floors', and one possible way to alleviate this issue is the introduction of a minimum wage. If female employees are more prevalent among the wage floor beneficiaries than their male counterparts, they should also be disproportionately affected by a minimum wage, which would reduce wage disparities and thus the gender wage gap. In their seminal paper, DiNardo *et al.* (1996) find that labour market institutions such as a minimum wage can indeed reduce inequality, especially so for women. This line of research has been continued by an ample number of studies establishing a negative relationship between the minimum wage and the gender wage gap for a variety of countries.[1]

In this context, Germany poses an interesting case study. On the one hand, it is a country with a relatively high gender wage gap, especially at the bottom of the wage distribution. In 2014, the difference between the earnings of full-time employed men and full-time employed women amounted to 17.2 percent in Germany, placing it in the upper third of countries with the highest gaps, above the OECD average (see Figure D.1a).[2] At the first decile of the wage distribution, Germany even shows a wage difference of 18.2% in 2014, placing it among the eight countries with the highest gaps among low-income earners, and well above the United States and the United Kingdom (see Figure D.1b). It is thus evident that Germany suffers from high gender wage discrepancies in an OECD comparison, especially among low-wage earners. On the other hand, Germany introduced a nationwide minimum wage in 2015, which was set at a comparably high level and had only few exemptions.[3] With €8.50 gross per hour, it ranked among the highest wage floors in Europe in terms of purchasing power (Caliendo

[1] See for example Bargain *et al.* (2019) and Robinson (2005) for UK and Ireland, Broadway and Wilkins (2017) for Australia, Hallward-Driemeier *et al.* (2017) for Indonesia, Li and Ma (2015) for China, and Majchrowska and Strawinski (2018) for Poland.

[2] In this paper, we look at the *unadjusted wage gap*, also called the raw gap. A substantial part of the raw gap can be explained by observable factors, such as differences in schooling, working hours, etc. For Germany, these factors can explain around three quarters of the gap (Destatis, 2020). The remaining unexplained part is called *adjusted gap*.

[3] The law only stipulated a few exemptions, mainly minors, trainees, specific interns, volunteers and long-term unemployed. Additionally, employees in sectors with pre-existing wage floors below €8.50 were exempted until the end of 2016 (for details, see also Caliendo *et al.*, 2019). For the Minimum Wage Act, see `https://www.gesetze-im-internet.de/englisch_milog/index.html`, last accessed on November 11, 2021.

et al., 2019). Its level is to be re-evaluated every two years, and it was increased to €8.84 in January 2017, being followed by additional increases in 2019 and the subsequent years (Mindestlohnkommission, 2016a, 2018a, 2020a). Caused by its high initial value, the reform affected about four million employees, i.e. about ten percent of the eligible workers (Destatis, 2016a). Among them, women were vastly overrepresented, since they accounted for two-thirds of affected employees (Mindestlohnkommission, 2016b; Burauel *et al.*, 2017). This has been attributed to insufficient regulations of the low-pay sector and gender segregation, which lead to an unequal coverage of collective bargaining agreements for men and women (Grimshaw and Rubery, 2013; Herzog-Stein *et al.*, 2018). Accordingly, the introduction of the wage floor could have an influence on gender inequality. In a simulation study for Germany, Boll *et al.* (2015) predict that in the absence of job losses, the minimum wage could reduce the average gender wage gap by 2.5 percentage points. However, with job losses, the effect could even be larger. This points to a potential caveat, namely that low-paid women quitting or losing their jobs would cause a decrease in the gap. However, since ex-post evaluation studies on Germany find only small employment effects and no strong evidence of gender-specific job-losses, we assume that there are no significant heterogeneous employment effects (see Caliendo *et al.*, 2019; Bonin *et al.*, 2018; Pestel *et al.*, 2020).

In our analysis, we mainly employ data from 2014 and 2018, which we obtain from the Structure of Earnings Survey (SES), a large obligatory survey that comprises detailed wage information for over one million employees working in 60,000 firms. First, we descriptively look at the wages of male and female employees who are eligible for the minimum wage and compare the wage gap in 2014 with the one in 2018. Second, following Card (1992), we make use of regional variation in the "minimum wage bite", i.e. the degree to which a region is affected by the minimum wage, and analyse the causal effects of the minimum wage introduction on regional gender wage gaps.[4] We are able to identify a significant effect on the wage gap at the regional level, especially for low-paid employees. Between 2014 and 2018, the gap at the 10^{th} percentile was reduced by 4.6 percentage points (or 32%) in regions that were strongly affected by the minimum wage compared to less affected regions. For the gap at the 25^{th} percentile, the effect still amounted to -18%, while for the mean it was smaller (-11%) and not particularly robust. Using a continuous bite measure, we can show that an

[4]Since our post-reform data is from 2018, in our analysis we estimate the effect of both the initial introduction of the minimum wage as well the first increase from 2017. However, previous research shows that the reform had mainly significant wage effects in 2015, whereas there were no relevant effects after the first increase in 2017 (Burauel *et al.*, 2020b; Fedorets *et al.*, 2019).

increase in a region's bite by ten percentage points, reduces the wage at the 10^{th} percentile by 3.3 percentage points. This thus shows that the minimum wage reform led to a significant decrease in the wage gap especially at the bottom of the wage distribution.

The remainder of this paper proceeds as follows. Section 4.2 reviews the previous literature on gender-specific minimum wage effects on wages and presents the data that we use. Additionally, it offers a first descriptive overview of the gender-specific wages before and after the reform, as well as the regional differences in the wage gap and the bite measure. Section 4.3 then presents the identification strategy and displays the results of the main estimation and the robustness analyses. In section 4.4, we summarise our results.

4.2 Previous Evidence, Data and Descriptives

4.2.1 Previous Evidence

The previous evidence on gender-specific wage effects of the German minimum wage is scarce. In a descriptive analysis with the Socio-Economic Panel (SOEP), Herzog-Stein *et al.* (2018) find that the wage gap at the tenth percentile had reduced by seven percentage points (from 22% to 15%) two years after the reform, which the authors attribute to the minimum wage. Interestingly, the gender gap stagnated for the lowest paid five percent (at 18%), which they relate to circumvention strategies. Ohlert (2018) looks at wages below €10 in 2014 and 2015 and finds that women's wages increased more strongly (14.6%) than men's wages (11.3%) in East Germany, while in the former West German states there was no gender-specific difference.[5] Additionally, some causal studies on wage effects have included some gender-specific heterogeneity analyses. However, they do not estimate the effect on the wage gap itself and suffer from small samples sizes. Burauel *et al.* (2018) find that between 2014 and 2016 the minimum wage effect on hourly wages was significant for women and amounted to about 5.9%. While the male effect was larger, it did not satisfy the common trend assumption, challenging the results for this subgroup. Bachmann *et al.* (2020) find that female employees earning less than €8.50 in 2014 experienced a statistically significant wage growth of 5.7% directly after the reform, while the effect for men was postponed: they benefited from a wage growth of about 10.2% from 2015 to 2016 and 14.8% in the following year. There was no such effect on women's wages at this time. Overall, previous studies find indications that wages

[5]However, the results are based on the voluntary Earnings Survey (ES) and are potentially influenced by selectivity issues (see Ohlert, 2018; Dütsch *et al.*, 2017).

among the low-paid employees rose due to the minimum wage introduction.[6] Moreover, there is evidence that women's wages rose more strongly shortly after the reform and men's effects were possibly postponed. However, none of these studies have explicitly examined the effects of the minimum wage on the gender wage gap.

4.2.2 Data Sources and Preparation

There are only a few datasets that are suitable for evaluating the minimum wage effects in Germany. All of them have different advantages but also caveats and neither is completely ideal. The differences as well as the associated issues have been widely discussed before (see, e.g., Caliendo *et al.*, 2019; Mindestlohnkommission, 2020b). In this paper, we rely on the SES[7], provided by the Federal Statistical Office of Germany (Destatis). It is a rich earnings survey in which employers are obligated to participate. Next to firm level data, the SES entails detailed individual information taken directly from payroll accounting or personnel statistics. It has first been collected in 1951 and takes place every four years since 2006. In this paper, we mainly make use of the most direct pre- and post-reform waves that are available, namely 2014 and 2018. Additionally, we look at the data from 2010 in Section 4.3.3 in order to check the pre-treatment trends.

The SES 2014 and 2018 are well suited for our analysis since they are large datasets, that each contain more than 1 million employees working in over 60,000 firms. The businesses were obliged to provide information on wages, working hours, and other working conditions, allowing us to construct precise hourly wages. However, two caveats should also be noted. First of all, it does not take place yearly, forcing us to look at a rather long time-frame. Second, it is not a panel study but largely comprises repeated cross sections. The recent waves include employees from nearly all economic sectors that have at least one employee. Nonetheless, we exclude public sector employees, since for them there is no detailed regional information available beyond the federal state. However, since this sector does not typically suffer from low wages, it is not strongly affected by the minimum wage. The SES is representative at the level of the sixteen federal states. In order to exploit the regional variation of the minimum wage bite, we follow previous studies (see for example Bachmann *et al.*, 2020) and rely on the smaller 257 labour market regions (*"Arbeitsmarktregionen"*). Moreover, in a sensitivity

[6] See also Ahlfeldt *et al.* (2018), who find that wages in low-wage counties increased more rapidly than in high-wage counties, particularly so for low-paid employees.

[7] Source: FDZ der Statistischen Ämter des Bundes und der Länder, Verdienststrukturerhebung, 2010, 2014 and 2018.

analysis we also look at planning regions (*"Raumordnungsregionen"*) and districts (*"Kreise und kreisfreie Städte"*), the former being a less and the latter a more disaggregated regional classification (see Section 4.3.4).

Since the SES does not collect data on hourly wages, we compute them using monthly income and working hours. We employ the gross monthly earnings and subtract earnings received for premiums and overtime worked. We divide this by the number of paid working hours (excluding overtime).[8] In our analysis, we only look at employees who are eligible for the minimum wage. We thus exclude trainees, interns as well as minors without a vocational training.[9] We generate the gender wage gap at the regional level as the difference between men's and women's wages divided by men's wages. We do this for regional mean wages but also with the wages at the 10^{th} and 25^{th} percentiles of the gender-specific wage distributions. This allows us to look at wage differences not only at the mean but also specifically among low-paid employees.

4.2.3 Descriptive Statistics

Wage Gap and Fraction of Affected Employees As a first step in our empirical analysis, we examine different aspects of the gender wage gap descriptively.[10] As shown in Table 4.1, we rely on information for 755,431 individuals in 2014, 46% of them being female. In 2018, there are 742,716 individuals included, 44.9% of which are women. The mean wage of employees in our sample amounted to €16.80 in 2014. While men eligible for the minimum wage earned about €19.0 on average, their female counterparts earned only €14.4. This translates into a gender wage gap of 24.5%.[11] Four years later, wages had increased overall, but women's wages more strongly. Consistently, the wage gap at the mean decreased to 21.9% in 2018.

[8] If the number of paid working hours is not known, we divide by regular weekly working hours times 4.33, which is the average number of weeks in a month.

[9] Since we cannot identify employees working in sectors with pre-existing wage floors below €8.50, we follow previous studies and include them in our analysis (see e.g., Ohlert, 2018). This should not affect our results as these were only few sectors and regulations ran out at the end of 2016 (Mindestlohnkommission, 2016b). Nevertheless, we will address this issue in our robustness analysis.

[10] The SES provides individual weights that we use for the general descriptives and the calculation of regional wages and wage distributions. While the weights yield representative results on the level of federal states, they do not necessarily do so for a smaller regional level (see FDZ, 2019, for details). Therefore we exclude the weights as a sensitivtiy check in Section 4.3.4.

[11] It should be noted that the gender wage gap in our sample is larger than that for the overall population found in other studies. This is caused by different factors. First, we do not include public sector personnel, whose inclusion would lead to the gap being smaller. Second, we only look at workers who are not exempt from the minimum wage. Finally, we focus on the raw/unadjusted gender wage gap, while many studies examine the (lower) adjusted gap.

Table 4.1: Descriptive Statistics on Wages, 2014 and 2018

	2014				2018			
	All	Men	Women	Gap (%)	All	Men	Women	Gap (%)
Share earning								
<8.50 (%)	12.66	9.34	16.28		0.69	0.62	0.76	
<8.84 (%)	15.47	11.54	19.76		2.83	2.39	3.33	
Mean	16.80	19.03	14.36	24.54	18.71	20.86	16.29	21.91
SD	11.56	13.75	7.86		12.98	15.65	8.46	
p10	8.01	8.56	7.70	10.05	9.46	9.82	9.21	6.21
p25	10.00	11.01	9.29	15.62	11.18	12.21	10.36	15.15
p50	14.11	15.67	12.54	19.97	15.58	17.10	14.17	17.13
p90	28.62	33.23	22.93	31.00	31.38	36.06	25.58	29.06
N	755,431	407,894	347,537		742,716	409,571	333,145	

Source: SES 2014 and 2018, own calculations.
Note: We only include eligible employees that are not employed in the public service. The used waves include employees from sectors A to S from the WZ 2008 classification. The SES contains workers that are employed throughout the whole month of April of the respective wave, but does not entail employees who are not paid for the full month because they were newly hired or let go.

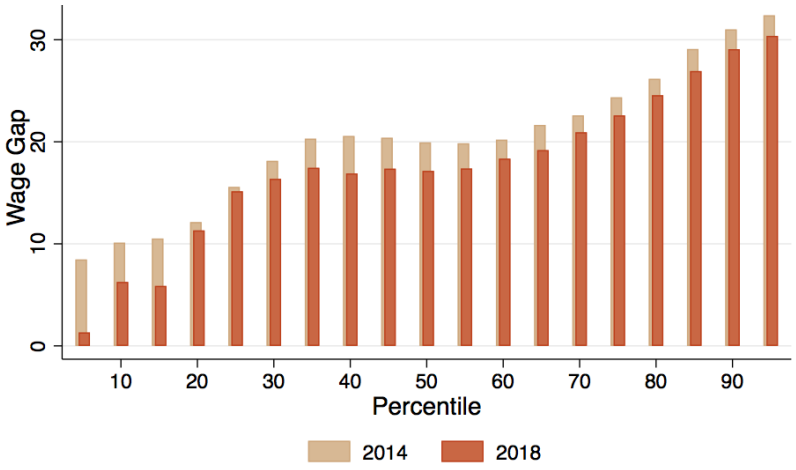
It also becomes apparent that women were overrepresented in the low-wage sector. In 2014, 9.3% of men but 16.3% of women were earning less than €8.50 per hour. Looking at the first minimum wage increase to €8.84, nearly every fifth woman (19.8%) in our sample was affected in 2014, compared with only 11.5% of men. Four years later, i.e. after the introduction and first increase of the minimum wage, less than one percent of either gender earned less than €8.50 and 2.8% of employees earned less than €8.84. However, among women the percentage was slightly larger than among men, suggesting that women were disproportionately affected by non-compliance with the wage floor.[12]

Wage Gap Over the Distribution Our sample displays a higher wage gap at the top of the wage distribution: in 2014, it amounted to 31% at the 90^{th} percentile, but to only about 10% at the first decile. However, the largest decrease in the gap is found at the bottom: at the 10^{th} percentile, the gap decreased by 3.84 percentage points. A more detailed representation of this is displayed in Figure 4.1. Here, we depict the wage gap at twenty quantiles. Between 2014 and 2018, the gap decreased along the whole distribution, but especially up to the 15^{th} percentile. Remembering that 16.3% (19.8%) of women earned less than €8.50 (€8.84) in 2014, this indicates a connection to the minimum wage introduction. The strongest reduction

[12] All evaluations of (non-)compliance find that a substantial number of eligible workers were paid less than the minimum wage even after the reform. Estimations of the magnitude of that issue differ depending on the data source. Summarising a number of studies, the minimum wage commission places non-compliance in the range of 1.3% to 6.8% for 2018 (Mindestlohnkommission, 2020b).

took place at the fifth percentile, where the gender wage gap in hourly wages decreased from 8.5% to 1.3% among eligible employees in our sample. There was also a strong reduction in the gap at the 10^{th} and 15^{th} percentile. However, at the 20^{th} and 25^{th} percentiles – i.e. above the share of affected women – it remained nearly constant.

Figure 4.1: Wage Gaps at Percentiles of the Gender-specific Wage Distribution, in %



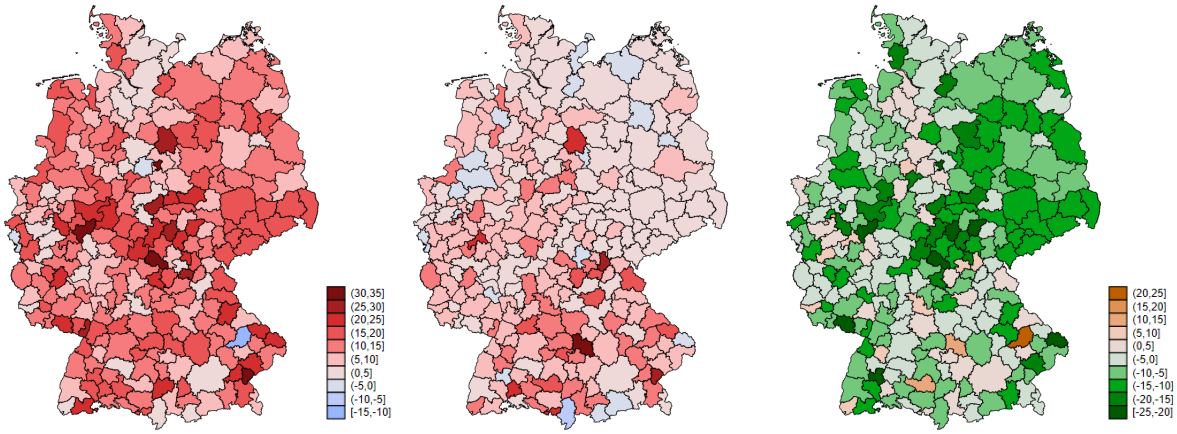
Source: SES 2014 and 2018, own calculation. Includes only employees eligible to the minimum wage, without public sector personnel.

Regional Variation and Development Our findings are also underlined by Figure 4.2, which displays the regional variation in the gender wage gap at the first decile, i.e. one of the dependent variables in our later analysis. Figure 4.2a and 4.2b map the gap in 2014 and 2018, where red colouring indicates a positive wage gap and blue colouring a negative gap. The higher the absolute value, the higher the colour intensity. Figure 4.2c displays the difference in the gap between the two years.[13] It highlights the fact that the gap among the low-paid decreased from 2014 to 2018 in most regions. Additionally, we see that in the East of Germany, the picture is rather homogeneous: the gap at the 10^{th} percentile was reduced in all East German regions, in many of them between five and fifteen percentage points. However, in the West, the development is more diverse. There are some regions that experienced a strong decrease in the gap, but also regions in which the gap substantially increased.

[13]Note that in the regional descriptives, information for Saxony is averaged over all Saxon labour market regions due to data approval issues. However, in our later analysis, we use detailed data on the ten Saxon regions.

Figure 4.2: Regional Wage Gap at 10^{th} percentile in 2014 and 2018 and Difference in Gaps between both Years (in Labour Market Regions)

(a) Gap in 2014 (in %) (b) Gap in 2018 (in %) (c) Difference (in p.p.)

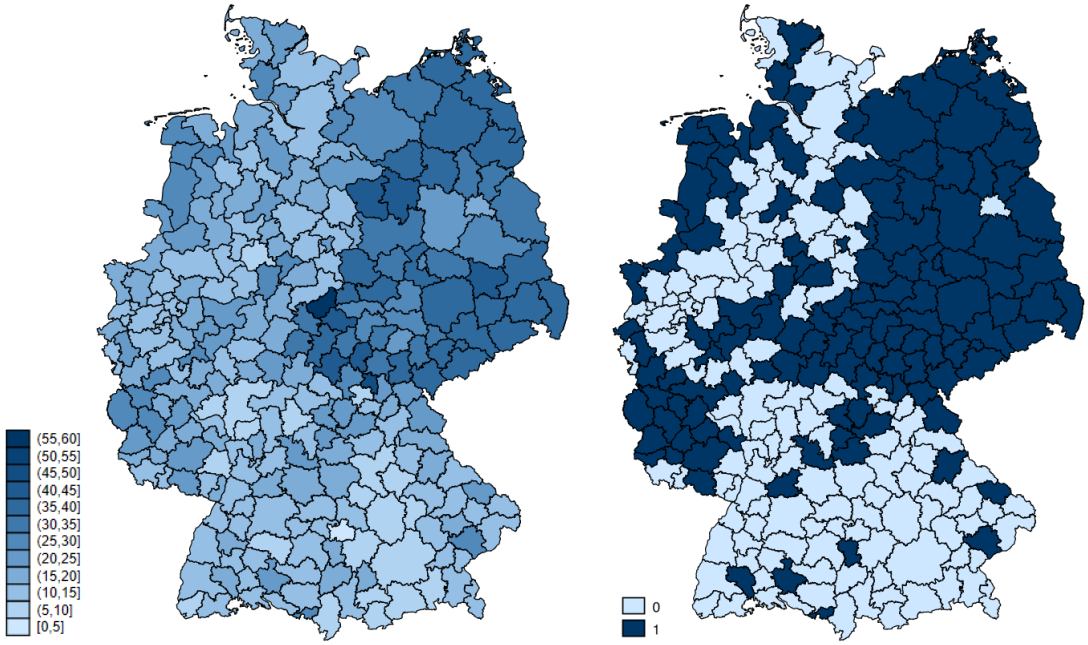


Source: SES 2014 and 2018 and BKG (2021), own calculations. Note: Due to clearing restrictions, data for Saxon regions is averaged for whole Saxony.

The Minimum Wage Bite Another variable central for our analysis is the minimum wage bite, measuring the degree to which a region was affected by the minimum wage in 2014. The literature features a variety of bite measures such as the fraction of affected employees or the Kaitz index (see also Caliendo *et al.*, 2018a). However, the Kaitz index indicates the ratio between the minimum wage and the regional mean wage and it is thus also affected by other determinants of the mean wage, which is why we focus on the fraction of affected employees. More precisely, we employ the regional share of eligible women earning less than the wage floor in relation to all eligible women (henceforth called 'fraction'), since we aim to identify regions in which women were especially affected by the minimum wage. Figure 4.3a displays the fraction for the 257 labour market regions in 2014. It can be seen that – in contrast to the wage gap – the bite is generally higher in the East of Germany. However, there are also regions in the West where women are affected above average. For the empirical analysis later on, we will use a binary "high-bite" indicator and split the sample into high- and low-bite regions at the median bite level of 17.15%. Figure 4.3b shows the regional distribution of this new indicator. While all regions in East Germany are high-bite regions (equal or above the median) and receive a value of 1 for this indicator, it also becomes clear that high-bite regions are found across the whole country.

Figure 4.3: Fraction of Affected Female Employees in 2014 (in Labour Market Regions)

(a) Continuous (in %) (b) Binary



Source: SES 2014 and BKG (2021), own calculation. Note: Due to clearing restrictions, data for Saxon regions is averaged for whole Saxony.

4.3 Methodological Approach and Results

4.3.1 Empirical Approach

In this paper, we aim to identify the impact of the German minimum wage on the gender wage gap. In our identification strategy, we follow Card (1992), who proposes an approach relying on regional variation (also employed by other authors, e.g. Stewart, 2002; Dolton *et al.*, 2010, 2012, 2015; Caliendo *et al.*, 2018a). In contrast to other methods, it therefore does not depend on differences in legislation, which do not occur in the German case (see also Caliendo *et al.*, 2018a). The approach incorporates the intuition that regional wages have to adapt to varying degrees in accordance with the introduction of a minimum wage. In regions where wages were lower prior to the introduction, the minimum wage is assumed to bite harder into the wage distribution and its effect on potential outcomes is thus expected to be stronger. The corresponding fixed effects estimation equation of the gender wage gap is defined as follows:

$$Gap_{j,t} = \alpha_r + \beta T_t^{2018} + \delta T_t^{2018} Bite_j^{2014} + \gamma X_{j,t} + \upsilon_{j,t}, \qquad with \quad t = \{2014, 2018\} \tag{4.1}$$

where $Gap_{j,t}$ denotes the gender wage gap in percent in region j at time t. α_r is a region-fixed effect and T_t^{2018} is a dummy taking the value of 1 in the year 2018. $X_{j,t}$ is a set of regional controls and $\upsilon_{j,t}$ is the error term. $Bite_j^{2014}$ denotes the regional bite. While we use the fraction of affected women in our main analysis, we also make use of the fraction of affected employees over both genders in our sensitivity analysis. In order to obtain a clearer picture, we employ the fraction as a binary bite measure, dividing regions at the median fraction. In the robustness checks, we also include the fraction as a continuous variable. $X_{j,t}$ entails control variables provided by the INKAR database.[14] It comprises of the regional GDP per capita, the population density, the share of women, the female employment rate as well as the uptake of childcare for children under three and children between three and five. All of those are measured in the previous year to avoid endogeneity issues.[15] To control for time persistent regional characteristics, we employ a fixed effect estimation with robust standard errors.

The identifying assumption of the difference-in-difference approach that we employ is that the treatment regions (i.e. high-bite regions) and control regions (i.e. low-bite regions) have common trends in the wage gap in the absence of the treatment. In order to test this, we estimate a placebo regression in Section 4.3.3, assuring us that the common trend assumption holds. However, we first turn to our main results in the following section.

4.3.2 Main Results

In our main analysis we estimate equation (4.1) described in Section 4.3.1 with fixed effects. As already done in Section 4.2.3, we look at the wage gap at three different points of the distribution: the 10^{th} and the 25^{th} percentile as well as the mean. Accordingly, our results are divided with respect to these outcome variables, which is indicated by the three different panels in Table 4.2. Within each panel, we include the control variables in six steps, captured

[14] The "Indicators, Maps and Graphics on Spatial and Urban Monitoring" database is provided by the *Federal Institute for Research on Building, Urban Affairs and Spatial Development* (BBSR, 2021).

[15] The control variables are precisely defined as follows: gross domestic product in €1,000 per inhabitant, inhabitants per km^2, the share of women among inhabitants in percent, the share of women with contracts subject to social security contributions among all women in working age, the share of children under three years in childcare among all children under three, the share of children between three and six years in childcare among all children between three and six.

by columns (1) to (6). The table shows the coefficients and indicates their significance.

The first panel estimates the effect on the gap at the tenth percentile. Without the inclusion of further control variables (column 1), there is a strongly significant treatment effect of -5.2 percentage points. This is to say that – in comparison to regions with a low bite in 2014 – high-bite regions experience a reduction of the wage gap at the tenth percentile of 5.2 percentage points in 2018, ceteris paribus. This is accompanied by a general reduction of the gap of 3.6 percentage points between 2014 and 2018. When we include additional control variables iteratively, the magnitude of the treatment effect only slightly decreases and the significance is unchanged. In the most comprehensive specification (column 6), high-bite regions experience a reduction in the wage gap of 4.6 percentage points from 2014 to 2018 due to the minimum wage. This corresponds to a reduction of about 32% in the wage gap (compared to the level in 2014 which was 14.4% in these regions). It is interesting to note, that the significance of the dummy for 2018 vanishes after controlling for the employment rate of women, meaning that the wage gap for low-paid employees only reduced for highly affected regions, and very strongly so. When looking at the wage gap at the 25^{th} percentile (see Panel B), the treatment effect is still highly significant but not as large. It ranges from -2.8 to -3.4 percentage points depending on the specification. In our preferred specification (column 6) it corresponds to a relative effect of -18% compared to the level in 2014 (which was at 18.3%). For the wage gap at the mean (Panel C), the effects are least pronounced: in the most comprehensive specification (column 6), the significant treatment effect amounts to -2.3 percentage points, corresponding to a relative reduction of 11% (compared to the 2014 level which was 20.4%). The results suggest that there was a general reduction of the wage gap at the mean between 2014 and 2018, but regions in which women were particularly affected by the minimum wage experienced an additional decrease.

Overall, the analysis shows that there is a strong effect of the minimum wage, which is especially large at the bottom of the wage distribution. The gap at the tenth percentile is reduced by 4.6 percentage points (32%) between 2014 and 2018 in regions that were strongly affected by the minimum wage compared with less affected regions. Higher up in the distribution, the magnitude and significance of the results decrease. However, an effect can still be detected and it ranges from 18% at the 25^{th} percentile to 11% at the mean.

Table 4.2: Fixed Effects Regressions of Wage Gaps at 10^{th} Percentile, 25^{th} Percentile and Mean (in Labour Market Regions)

	(1)	(2)	(3)	(4)	(5)	(6)
A: Wage Gap at p10						
Bite x 2018	-5.228***	-5.085***	-4.870***	-4.735***	-4.550***	-4.550***
2018	-3.568***	-4.286***	-4.539***	-5.419***	-3.684	-3.754
GDP per capita (t-1)		0.153	0.152	0.126	0.147	0.158
Pop. Density (t-1)			0.024	0.020	0.017	0.015
Share of Women (t-1)				-2.722	-2.837	-2.610
Empl. Rate Women (t-1)					-0.412	-0.457
Childcare 0-2 (t-1)						0.044
Childcare 3-5 (t-1)						-0.061
B: Wage Gap at p25						
Bite x 2018	-3.352***	-2.814***	-3.122***	-3.111***	-3.191***	-3.336***
2018	-1.454**	-4.154***	-3.792***	-3.865**	-4.612	-4.079
GDP per capita (t-1)		0.576**	0.578**	0.576**	0.567**	0.541**
Pop. Density (t-1)			-0.034	-0.034	-0.033	-0.027
Share of Women (t-1)				-0.225	-0.176	0.010
Empl. Rate Women (t-1)					0.178	0.280
Childcare 0-2 (t-1)						-0.239
Childcare 3-5 (t-1)						0.048
C: Wage Gap at Mean						
Bite x 2018	-1.609*	-1.339	-1.634*	-1.684*	-2.139**	-2.297**
2018	-2.154***	-3.511***	-3.163***	-2.838*	-7.110**	-6.581**
GDP per capita (t-1)		0.289	0.292	0.301	0.249	0.229
Pop. Density (t-1)			-0.032	-0.031	-0.023	-0.018
Share of Women (t-1)				1.006	1.289	1.662
Empl. Rate Women (t-1)					1.014*	1.092*
Childcare 0-2 (t-1)						-0.228
Childcare 3-5 (t-1)						0.007
Observations	514	514	514	514	514	514
Groups	257	257	257	257	257	257

Source: SES 2014 and 2018, INKAR; own calculations.
Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The table displays the results of fixed-effects estimations in a difference-in-difference framework with region-fixed effects and robust standard errors. The minimum wage bite is binary and the regions are the 257 Labour Market Regions. In Panel A the dependent variable is the unadjusted wage gap at the 10^{th} percentile of the regional gender-specific wage distribution. Accordingly, in Panel B (C) the dependent variable is the gap at the 25^{th} percentile (the mean). The reference year is 2014.

4.3.3 Common Trend

In order to analyse the common trend assumption, we make use of the 2010 wave of the SES. However, the structure of the SES significantly changed between 2010 and 2014. In 2010, there were fewer sectors included and – most important to our case – there were no firms with fewer than ten employees subject to social security contributions. Additionally, the month of data collection changed. Overall, there is information on over 1.9 million employees in about 34,000 businesses included in 2010, in comparison to about one million workers

in 60,000 businesses in 2014 (Destatis, 2013; FDZ, 2019). Therefore, given that we cannot simply compare our previous results to the 2010 data, we make use of a subsample of firms, which we identify via a variable that is provided by the federal statistical office and that adapts the 2014 and 2018 data to the structure of the 2010 wave (FDZ, 2019). This leads to a slightly different subsample in comparison to our main analysis (see Table D.1): here, a smaller share of male and female employees was affected by the introduction of the wage floor. Accordingly, the wages that they display are higher. One reason for this is that the subsample does not include firms with fewer than ten employees, which usually pay less. Additionally, the gender wage gaps are slightly different between the two samples. However, the two samples are comparable overall.

In Table 4.3 we show the results of re-running the equation 4.1 with our subsample (columns 4 to 6) as well as the equation adapted to the pre-reform period (columns 1-3), which is defined as follows:

$$Gap_{j,t} = \alpha_r + \beta T_t^{2014} + \delta T_t^{2014} Bite_j^{2014} + X_{j,t} + \upsilon_{j,t}, \qquad with \quad t = \{2010, 2014\} \tag{4.2}$$

The table shows that when looking at the time before the minimum wage introduction, i.e. between 2010 and 2014, the wage gap evolved similarly in high- and low-bite regions, which is indicated by a statistically insignificant interaction term. This confirms our assumption of a common trend in the gap before the reform. When turning to the replication of our main regressions for the years 2014 and 2018 with the subsample, we observe very similar results to our main specification presented in Section 4.3.2. Again, we find significant effects for the 10^{th} and 25^{th} percentile that are also comparable in magnitude. However, in contrast to our main results in Table 4.2, we do not find a significant effect for the mean gap. Nonetheless, our results give us confidence that the common trend holds and that the wage gaps would have developed similarly in high- and low-bite regions in absence of the reform, especially for the lower parts of the distribution.

Table 4.3: Fixed Effects Regressions of Wage Gaps at 10^{th} Percentile, 25^{th} Percentile and Mean, with Subsample, from 2010 to 2014 and 2014 to 2018 (in Labour Market Regions)

	2010-2014			2014-2018		
	p10 (1)	p25 (2)	Mean (3)	p10 (4)	p25 (5)	Mean (6)
Bite x 2014	2.277	2.031	1.075			
2014	-2.523	1.376	2.973			
Bite x 2018				-4.947***	-2.831**	-1.242
2018				-0.236	-2.608	-5.149
GDP per capita (t-1)	0.353	0.075	0.372	-0.130	0.374	0.011
Pop. Density (t-1)	0.013	0.031	-0.010	-0.006	-0.026	-0.025
Share of Women (t-1)	0.578	0.197	1.019	-1.377	4.160	2.737
Empl. Rate Women (t-1)	0.050	-0.016	-0.627	-0.171	0.352	1.027*
Childcare 0-2 (t-1)	-0.169	-0.244	-0.337	-0.074	-0.249	-0.098
Childcare 3-5 (t-1)	0.246	-0.047	0.119	-0.013	0.038	0.014
Observations	514	514	514	514	514	514
Groups	257	257	257	257	257	257

Source: SES 2014 and 2018, INKAR; own calculations.
Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The table displays the results of fixed-effects estimations in a difference-in-difference framework with region-fixed effects and robust standard errors. The minimum wage bite is binary and the regions are the 257 Labour Market Regions. Columns (1)-(3) include regressions entailing the years 2010 and 2014, Columns (4)-(6) show regressions containing the years 2014 and 2018. In Columns (1) and (4) the dependent variable is the unadjusted wage gap at the 10^{th} percentile of the regional gender-specific wage distribution. Accordingly, in Columns (2) and (5) the dependent variable is the gap at the 25^{th} percentile, in Columns (3) and (6) it is the gap at the mean. The reference year is 2010 for Columns (1)-(3) and 2014 for Columns (4)-(6). The subsample is identified via the variable *GG2010*, which is provided by the federal statistical office and which adapts the 2014 and 2018 data to the structure of the 2010 wave (FDZ, 2019).

4.3.4 Robustness Analysis

In the following, we will test the sensitivity of our results to a number of practical implementation issues. We look at different computations of the minimum wage bite, the exclusion of employees working in sectors with sector-specific minimum wages, different regional classifications, a non-weighted bite measure and weighted labour market regions.

Different Bite Measures First, we employ other variations of the bite measure. As discussed in Section 4.2.2, our main analysis relies on the ratio of affected female employees in 2014 to all female employees in 2014. However, we could also employ the minimum wage bite for all employees, irrespective of their gender. We thus re-run the estimation using the overall fraction (see Panel B in Table D.2, Panel A repeats the results of the main analysis for comparison). The results mirror those of the full estimation of our main analysis, although

the effects are not as large in magnitude. This is caused by the fact that the fraction of affected employees calculated for women only better identifies the regions that are particularly subject to reductions in the wage gap. However, regions where men are also highly affected are not expected to experience a pronounced decrease in the wage discrepancies. A second bite variation that we employ is the continuous bite (see first Panel C in Table D.2). In our main estimation, we only distinguished between low-bite and high-bite regions, cut off at the median bite. Now we include the fraction of affected female employees as a continuous variable, allowing for more variation in the treatment. The variable ranges from 3.8% to 55.8%. Again, the structure of the results is rather similar to our main results, although the interpretation of the coefficients is slightly different. Here, a ten-percentage-point increase in the bite corresponds to a reduction of the wage gap at the tenth percentile by 3.3 percentage points in 2018. Similarly, the gap at the mean decreases by 1.8 percentage points.

Excluding Sector-Specific Minimum Wages The law stipulates that employees in sectors with pre-existing minimum wages below €8.50 were exempted from the wage floor until the end of 2016. Unfortunately, we are not able to identify these sectors in our data. Since these were only few sectors and regulations ran out at the end of 2016, this should not affect our results. Nevertheless, in order to test the robsutness of our results in that direction we can exclude all employees subject to *any* sector-specific minimum wages (most of them were above €8.50, see Mindestlohnkommission, 2016b, for more details) and re-run our estimation (see Panel D in Table D.2).[16] Again, the results are similar to the main estimation and do not challenge our results.

Regional Classifications Moreover, we check the sensitivity in terms of the regional classification. In our main estimation, we decided to make use of the 257 labour market regions since they supply more variation than the 96 planning regions and include more observations per region than the 401 districts. In order to check whether this decision influences our results, we now make use of these other regional divisions (see Panels E and F of Table D.2). The results are very similar to our main results, although the effects sizes are again slightly smaller and the treatment effect at the mean becomes insignificant. However, the effects at the 10^{th} and 25^{th} percentiles are robust and mostly highly significant.

[16] We thus exclude 98,076 men and 79,654 women in 2014 and 94,602 men and 71,567 women in 2018.

Weights Finally, we check whether our results are sensitive with respect to weighting issues. First, we focus on the individual weights provided by the Federal Statistical Office. As discussed before, our main analysis incorporates individual weights when calculating the minimum wage bite and the regional wage distributions. However, these are designed to yield representative results on the level of federal states. Since we run estimations on a smaller regional level, we check whether the inclusion of those weights influences our results. We therefore re-run our estimation without them (see Panel G in Table D.2). Additionally, since our main analysis does not include regression weights, we now repeat the analysis using the regional employment in 2014 as estimation weights (see Panel H in Table D.2). The results of both these sensitivity analyses are in line with the previous estimations, yielding very similar results compared to our main regression.

Overall, our analyses confirm the robustness of our results for low-paid employees with respect to different implementation decisions that we made in our main estimation. We can thus conclude that there is a negative and highly significant effect of the minimum wage on the gender wage gap at the bottom of the distribution. However, for the gap at the mean, the results are not as robust.

4.4 Conclusion

The introduction of the minimum wage in Germany was a major intervention into the labour market, which was – among other things – expected to reduce poverty and alleviate inequality (Bosch, 2007; Mindestlohnkommission, 2016b). One of the dimensions that could be affected is gender wage differentials. Germany poses an interesting research case, since it exhibits both large wage gaps and a high minimum wage. In our paper, we have thus analysed whether the wage floor did indeed lead to a decrease of the gender wage gap and – if so – at which point(s) of the distribution. For this purpose, we employed a regional difference-in-difference approach, making use of the regional variation in the degree to which female employees were affected by the minimum wage. Using comprehensive data from the Structure of Earnings Survey, we identify regional wage gaps in 2014 and 2018 as well as the regional fraction of affected female workers. Descriptively, we observe a reduction in the wage gap between 2014 and 2018 for the whole distribution. However, we also see that it was particularly reduced among the low-paid employees.

In our difference-in-difference analysis, we find that there was a significant decrease in the gender wage gap in regions in which women were strongly affected by the minimum wage in comparison to regions where women were less affected: high-bite regions experience a reduction of the wage gap at the 10^{th} percentile of 4.6 percentage points from 2014 to 2018 due to the minimum wage, and this effect is robust across specifications. The magnitude of this effect is large, corresponding to a relative reduction of about 32% compared to the level in 2014. When looking at the bite in a continuous definition, we find that increasing a region's bite by ten percentage points leads to a reduction of the wage gap at the 10^{th} percentile by 3.3 percentage points. Our placebo estimations show that there was no such effect before the reform, i.e. that the common trend assumption is reasonable. Additionally, we find that these effects fade out higher up the distribution. For the gap at the 25^{th} percentile, the effect still amounted to -18%, while for the mean it was smaller (-11%) and not particularly robust.

Overall, our analysis suggests that the minimum wage can be an effective tool for reducing the gender wage gap, especially at the lower end of the wage distribution. However, we do not incorporate employment effects. As discussed above, strongly affected women could be driven to quit or lose their jobs, which would artificially reduce the gender wage gap. While previous studies do not find strong evidence of this effect, future research should focus on this more thoroughly. Additionally, it would be insightful to determine the long-term effects on the gender wage differentials. Thus far, we are restricted to data from 2014 and 2018, which allows us to identify a medium-term effect but neglects possible long-run adaptation processes. Moreover, while our survey period includes the first minimum wage increase, an analysis of the subsequent raises could also yield relevant information. Finally, it would be interesting to disentangle the effects on the adjusted gender wage gap, not least because the distinction between the two is often a very essential issue in the gender wage gap debate.

Chapter 5

Gender-Specific Minimum Wage Effects - Evidence from the European Union

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Abstract

On average, women earned 15.3% less than men in the EU-28 countries in 2018. While raw gender wage gaps varied across the member states, ranging from 1% in Luxembourg to 22% in Estonia, women are disproportionately affected by low wages in nearly every country. These persisting wage differentials at the bottom of the wage distribution are one of the issues that could be potentially addressed by a recent initiative on a promotion of 'adequate minimum wages' in the European Union. In order to sketch the effects minimum wages have on female equality, this paper summarizes the existing literature that focuses on gender-related minimum wage effects on labour market outcomes in the EU. I conclude that minimum wages are found to be associated with lower gender wage gaps. While they do not seem to affect women's employment more strongly than men's per se, some evidence suggests a negative effect on part-time employment. Women, who constitute a large share of part-time employees, are thus likely to be more prone to job losses. Adjustments of working hours do not display gender-related differences.

5.1 Introduction

Gender inequality is still an issue in the EU: In 2018, women earned 15.3% less than men on EU-28 average, the gender employment gap amounted to 10.5 pp.[1] This is in spite of the fact that the promotion of gender equality is a fundamental EU value that belongs to its founding principles. More than that, it is considered a driver for economic growth (EU, 2016), which is partly driven by the fact that as Europe is facing an increasing labour shortage, female labour force participation becomes a strategic production source for European economies. Achieving gender equality, however, is a complex task, which requires a multidimensional approach, including actions such as an improvement of equality in economic sectors and occupations, the promotion of equal chances in education, banning gender discrimination or introducing family-oriented policies that help lifting the burden of care work carried out by women (EU, 2016). Another one is, naturally, to ensure equal pay, thus reducing the gender wage gap.[2] However, the path to reaching this is not particularly obvious. One possible tool could be a minimum wage.[3] Since women are represented disproportionately among low-wage employees in many countries, more women than men could benefit from the wage increases induced by a wage floor. This is also stated by the EU (2021), which argues that more than half of the minimum wage earners in the member states are women and concludes that 'adequate' minimum wages could lead to smaller gender pay gaps. In fact, this mechanism is also listed as a reason to advance adequate wage floors in a recent proposal for a directive of the European Parliament and of the Council (European Commission, 2020). This first step towards a European strategy on minimum wages comes after a long-lasting debate on the topic (see for example Schulten, 2008; Vaughan-Whitehead, 2010). Accordingly, Müller and Schulten (2020) consider this initiative a 'watershed', since it is the first time the European Union is thinking of taking action to promote appropriate wage floors in their member states.[4]

[1] Source: Eurostat.

[2] For an overview over the evolution of the gender wage gap as well as its driving factors see for example Blau and Kahn (2003, 2017); Weichselbaumer and Winter-Ebmer (2005).

[3] See Blau and Kahn (2017); DiNardo *et al.* (1996); Fortin and Lemieux (1997); Rubery *et al.* (2005); Rubery and Grimshaw (2011).

[4] The proposal for the directive does not define specific target values for minimum wages but speaks to the adoption of national benchmarks to define and assure adequate wage floors (see EU, 2021; Lübker and Schulten, 2021, for a more detailed overview). However, a reference that is mentioned in the proposal is setting minimum wages at 60% of median wages. Eurofound (2021a) calculates that this would result in a minimum wage increase by about 30% for some countries (Luxembourg, Czech Republic, Estonia, Malta, Netherlands) and still 20% for others (Belgium, Latvia and Germany). For some countries (Bulgaria, Romania, the UK or Cyprus) the effect would be marginal and even negative for a few (France, Portugal, Hungary, Poland).

At the same time, single countries are already further increasing their minimum wage levels.[5] In this sense, it is important to identify whether the existing minimum wage regulations in the EU have already fostered gender equality and where they might have been detrimental. After all, wage floors are often feared to cause job losses or decrease working hours, although research is not unanimous on whether this is true and if so to what extent and for which subgroups. Looking at overall employment, most research finds no or only modest negative minimum wage effects (Belman and Wolfson, 2014; Card and Krueger, 1995; Doucouliagos and Stanley, 2009; Dube, 2019; Neumark and Wascher, 2008). However, following the rational of disproportionately affected women, females could not only benefit more strongly from a wage increase but could experience stronger disemployment effects as well. On the other hand, higher wages could also encourage non-employed women to seek gainful employment.

This paper summarizes previous studies that focus on the gender-specific effects of minimum wages on labour market outcomes, considering research on gender wage gaps but also employment and working hours. I include information on the EU-28, i.e. all current members states of the European Union and the UK.[6] I find that the EU member states display large differences concerning both the importance of the wage floor in the countries as well as the labour market equality measures. In 2018, about 7% of employees in the EU earned around the national minimum wage[7] but this number varies from 3% (the Netherlands and the Czech Republic) to around 15% (Romania, Poland and Portugal, see Eurofound, 2021a). Differences in the gender wage gap are even larger, with females earning 1% less than men in Luxembourg but 22% less in Estonia. And also gender differentials in the employment rate vary substantially, ranging from around 1pp to 20pp. Additionally, measures of gender inequality are intertwined. Gender wage gaps and female employment rates are positively correlated. Moreover, part-time employment, which is mainly carried out by women, is usually more common where female employment is high. Since it is often low-paid and faces a wage penalty, it also increases gender wage gaps.

The summary of previous research on gender-specific minimum wage effects yields three key findings. First, the countries that introduce a minimum wage or increase an existing one, tend to have lower gender wage gaps. Second, there is no strong evidence that employment

[5]For example, the German has decided to strongly increase the wage floor to €12 (BMAS, 2022).

[6]The UK has left the European Union as of January 2020. However, it has been an important EU member for the last decades and a lot of EU based research is associated with it. This is why it plays an important role in understanding recent relationships and is thus included in this paper.

[7]This is measured as earning at least 10 % below the minimum wage rate and at most 10% above it.

effects affect women more strongly per se. However, there seems to be a particular impact on part-time employment, leading to women being affected indirectly. Additionally, it cannot be ruled out that the negative impact on wage gaps is not associated with these low-paid part-time individuals being let go. The results thus suggest to lay a larger focus on this working arrangement in the context of minimum wages. Third, adaptations of working hours are not found to display gender-specific patterns, although evidence is scarce in this context.

The remainder of this paper is structured as follows. The second section first gives an overview over minimum wage policies in the EU member states and examines gender gaps in wages and employment for these countries. The third section summarizes studies on the relationship between minimum wages and gender wage gaps, as well as gender-heterogeneous effects on employment and hours. The fourth section recaps the results and gives an outlook into possible future research paths and policy implications.

5.2 Minimum Wage Policies and Gender Gaps Across Europe

5.2.1 Minimum Wage Policies

Minimum wages have been present in Europe since the second half of the 20th century. Romania was the first EU country to introduce a nationwide wage floor in 1949, Germany was the last in 2015 (see Table 5.1). Now there are only five EU member states which do not have a statutory minimum wage, namely Austria, Cyprus, Denmark, Finland, Italy and Sweden.[8] Since there is a wide variety concerning the year of the minimum wage introduction across the countries, there is an equally wide variety of experience with the wage floor. Most countries have experienced multiple steps of increases, some have also changed legislation such as rules for exempted employees (e.g. Spain) or introduced new minimum wages (e.g. UK). Moreover, every country's minimum wage is regulated uniquely. They differ for example with respect to the wage setting process or the range of exempted workers. Many countries have introduced sub-minimum wages for special subgroups. A large share of them apply to young employees (e.g. Belgium, France, Ireland, Netherlands or UK), some to disabled (France) or

[8]However, in those states wage floors are not completely unknown, since they have sectoral regimes set by law or collective agreements (ILO, 2016; Schulten, 2014). Moreover, there are discussion about a potential minimum wage introduction in Cyprus and Italy, which have been disrupted by the COVID-19 pandemic, though (see Eurofound, 2021c). While these types of wage setting processes are also comparable to minimum wages, they are not as comprehensive. Thus, I will only look at nationwide statutory minimum wages in this paper.

unskilled workers (Luxembourg) or convicts (Latvia).[9] Moreover, the relevance of minimum wages varies across the Member States, since the share of employees that actually earn them is very different for each country. Comparable data concerning the degree to which a country is affected by the minimum wage (also called the 'minimum wage bite') is scarce. As Eurofound (2021a) points out, about 7% of employees earned around the minimum wage[10] in the EU in 2018. Yet, this number differs between about 3% (the Netherlands and the Czech Republic) and around 15% (Romania, Poland and Portugal).[11]

European wage floors also differ in their level and their design. Some countries specified a monthly minimum whereas others opted for a weekly or hourly wage floor (see Table 5.1). When converting the countries' national rates in 2018 to monthly values measured in Euros, Luxembourg displayed the highest minimum wage with about €2,000. Bulgaria, on the other hand, has the lowest minimum wage which, amounts to €261.[12] While both countries remain at the top and the bottom when measuring in Purchasing Power Standards (PPS), the range of values in PPS is not as large. However, even when factoring in purchasing power, there are large differences in minimum wages across Europe (see Figure 5.1a). While wage floors are much lower in the Eastern European states, they are highest in Western states, such as Luxembourg, the Netherlands, Belgium and Germany. However, Eurofound (2021a) points out that minimum wage rates have converged in the European Union over the last decade. While many countries with lower wage floors in 2009 have experienced strongly rising minimum wages, increases in member states with higher wage floors have been modest.

[9] For an overview over the specific regulations see for example Adema *et al.* (2018); Eurofound (2021c); Eurostat (2021).

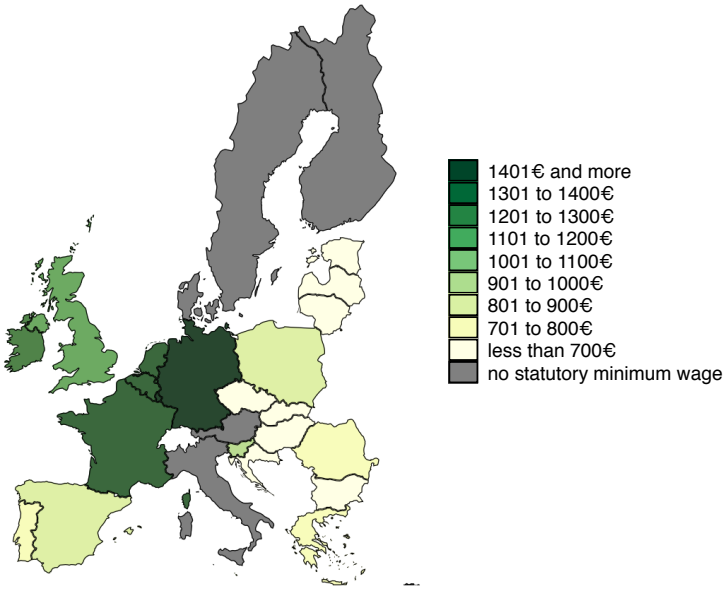
[10] It is defined as earning between 10% less and 10% more than the minimum wage.

[11] The minimum wage bite can be measured in different ways. Next to the proportion of workers earning around the wage floor it is often calculated as the fraction of employees earning at most the minimum wage or as the ratio between the minimum wage and the mean or median wage (called the 'Kaitz' index).

[12] For reasons of comparability with gender-related data, I display minimum wages in 2018. However, the range of wage floors had not substantially changed in 2021, with Luxembourg (€2,202) and Bulgaria (€332) still representing the upper and lower bounds. Source: Eurostat.

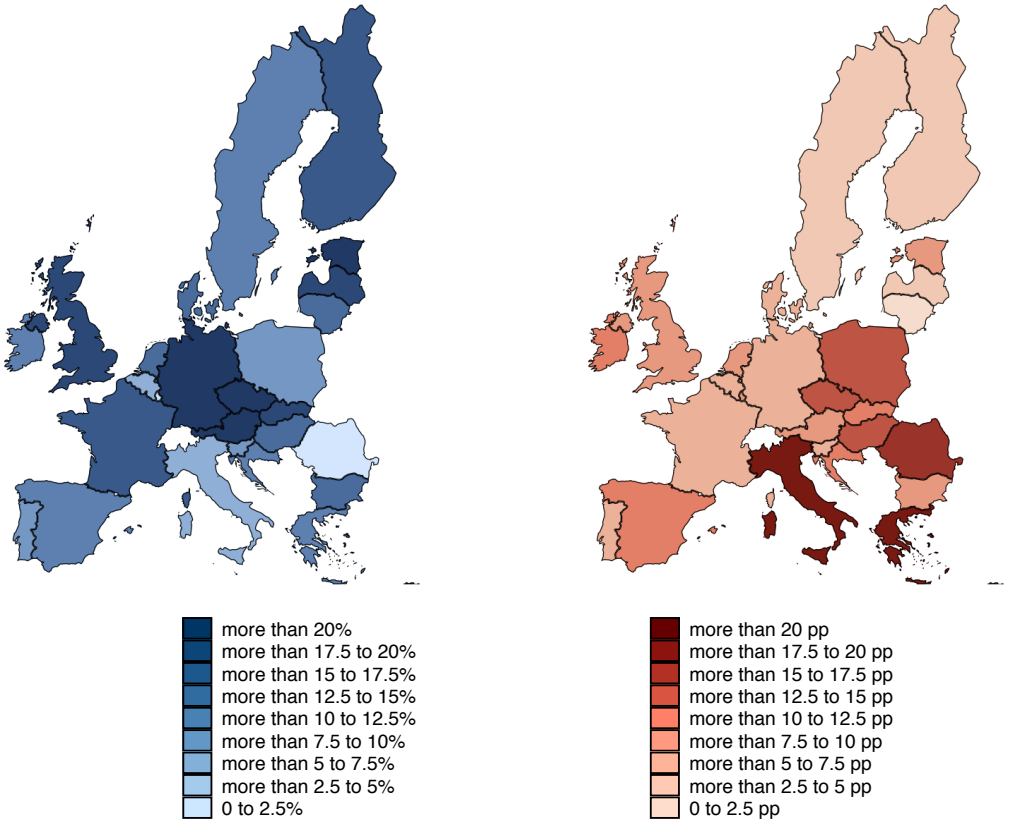
Figure 5.1: Minimum Wages and Gender Wage and Employment Gaps in EU-28 countries in 2018

(a) Minimum Wage in PPS



(b) Gender Wage Gap

(c) Gender Employment Gap



Source: Eurostat (last accessed 03 March 2022).
Note: Minimum wage is measured in Purchasing Power Standards (PPS). *Wage Gap*: Data displays the unadjusted wage gap, measured as the difference between average gross hourly earnings of male paid employees and of female paid employees as a percentage of average gross hourly earnings of male paid employees. *Employment Gap:* Data shows the absolute difference between the male and the female employment rates for employees aged 15-64, measured in percentage points (pp).

Table 5.1: Minimum Wage Policies in EU-28 countries in 2018

Country	Year of Introduction	National basis	Monthly values in EUR	in PPS
Luxembourg	1973	monthly	1,999	1,575
Ireland	2000	hourly	1,614	1,203
Netherlands	1969	monthly	1,578	1,380
Belgium	1975	monthly	1,563	1,363
Germany	2015	hourly	1,506	1,424
France	1970	hourly	1,498	1,315
United Kingdom	1999	hourly	1,400	1,176
Spain	1963	monthly	859	891
Slovenia	1995	monthly	843	968
Malta	1974	weekly	748	864
Greece	1991	monthly	684	788
Portugal	1974	monthly	677	768
Poland	1970	monthly	503	826
Estonia	1991	monthly	500	603
Slovakia	1991	monthly	480	568
Czech Republic	1991	monthly	478	643
Croatia	2008	monthly	462	657
Hungary	1991	monthly	445	656
Latvia	1991	monthly	430	561
Romania	1949	monthly	408	732
Lithuania	1990	monthly	400	593
Bulgaria	1966	monthly	261	503

Source: Eurostat (last accessed 03 March 2022).
Note: Data refers to January 2018. For the countries where the minimum wage is not fixed at a monthly rate, its hourly or weekly rate is converted into a monthly rate according to conversion factors supplied by the countries (based on 39.1 weekly working hours for Germany, 39 hours for Ireland, 35 hours for France and on mean basic paid hours per week for full-time employees in all sectors for UK). For the non-euro area countries, the minimum wages in their national currencies are converted into euro by applying the monthly exchange rate of the end of the previous month. Values in the column 'PPS' refer to Purchasing Power Standards, displaying results achieved by using special conversion rates, the Purchasing Power Parities (PPPs).

5.2.2 Gender-related Labour Market Aspects

So far, I have discussed minimum wage policies across Europe, which have been in place for at least thirty years in most countries. Before their gender-specific effects across the European countries are summarized in the next section, this subsection gives a short overview on recent developments of gender-related labour market aspects. I will examine how women are affected by minimum wages and consider the status quo of gender wag gaps and female (part-time) employment in the EU-28.

Table 5.2: Gender Gaps in Wages and Employment in EU-28 countries in 2018

	Wages	Employment Rate		Part-Time Rate	
	Gap %	Women %	Gap pp.	Women %	Gap pp.
Austria	20.4	68.8	9.0	46.9	36.9
Belgium	5.8	61.7	7.0	41.0	31.0
Bulgaria	13.9	63.4	8.6	2.0	0.3
Croatia	11.4	55.4	10.3	6.8	3.0
Cyprus	10.4	64.4	9.8	14.4	6.9
Czech Republic	20.1	68.3	13.9	10.9	8.3
Denmark	14.6	71.0	6.4	34.3	19.8
Estonia	21.8	71.8	8.1	15.3	8.1
Finland	16.9	70.7	2.7	20.6	10.6
France	16.7	62.1	6.8	28.8	21.0
Germany	20.1	72.7	7.5	46.3	36.7
Greece	10.4	45.3	19.9	13.2	7.1
Hungary	14.2	62.6	14.0	6.3	3.8
Ireland	11.3	63.8	10.6	29.9	19.3
Italy	5.5	49.6	17.9	32.4	24.4
Latvia	19.6	70.3	3.1	9.8	5.1
Lithuania	14.0	72.2	1.4	8.9	3.7
Luxembourg	1.4	63.8	8.4	31.8	26.0
Malta	13.0	62.2	20.3	22.8	16.3
Netherlands	14.7	73.6	8.5	75.6	48.1
Poland	8.5	60.3	14.0	9.7	5.9
Portugal	8.9	66.9	6.2	10.5	4.8
Romania	2.2	56.0	16.7	6.9	0.7
Slovakia	19.8	61.9	12.5	7.0	3.8
Slovenia	9.3	68.4	6.7	14.3	8.4
Spain	11.9	57.4	11.1	23.9	17.2
Sweden	12.1	75.6	2.8	33.3	20.4
United Kingdom	19.8	70.6	8.9	39.7	28.6
EU-28 average	15.3	63.6	10.5	31.3	22.6

Source: Eurostat (last accessed 16 December 2021).
Note: *Wage Gap:* Data displays the unadjusted gender wage gap, measured as the difference between average gross hourly earnings of male paid employees and of female paid employees as a percentage of average gross hourly earnings of male paid employees. All employees working in firms with ten or more employees, without restrictions for age and hours worked, are included. *Employment Rate:* It shows the employment rates for women aged 15-64. The gap displays the absolute difference between the male and the female employment rates. *Part-Time Rate:* It shows the part-time employment as percentage of the total employment for female employees aged 15-64. The gap is measured as the absolute difference between the female and male rates.

Overrepresentation Among Low-Wage Earners Previous research shows that the group of low-wage earners that are potentially affected by minimum wages are not homogeneous. A subgroup that is particularly overrepresented among them is women. This is, for example, established by Eurofound (2019) based on the European Union Statistics on Income and Living Conditions (EU-SILC) 2017. Bulgaria and Estonia are the only Member

States where the share of females among the working population nearly equals the share of women among minimum wage earners. In all other states, women are more affected by the minimum wage than men. When looking at employees who earn around the wage floor (i.e. 90%-110% of the minimum wage), the Czech Republic, Belgium, Slovakia and Croatia display the strongest overrepresentation of women. The strongest discrepancy is found in the Czech Republic where women account for only 47% of employees, but nearly four out of five workers earning around the minimum wage are female. For the EU median these numbers are not as far apart but still amount to 48% versus 58% and among employees earning less than 90% of the minimum wage even 62% are women. This shows that in most countries low wages is an issue that disproportionately affects women. Accordingly, minimum wages could benefit women especially and could also generate gender-specific effects.

Gender Wage Gaps The fact that there are more women among the low-paid than men points to a crucial indicator of women's equality in a country: the gender wage gap. One measure of this is the *raw* (or *unadjusted*) gap, which is usually calculated as the difference between average wages of men and women as a share of male wages. It is easily computable and comparable across countries. However, a substantial part of it can be explained by observable factors, such as individual and job-specific differences.[13] This is why a second measure has evolved, the *adjusted* (also called unexplained) gender wage gap, measuring the remaining gap after accounting for observable drivers.[14] In 2018, the raw gender wage gap amounted to 15.3% on EU-28 average. However, the diversity of European countries is also captured in the differences in the gender wage gaps they display. Looking at cross-country differences, it becomes apparent that the raw gender wage gap in 2018 has a large range: from about 1% in Luxembourg to nearly 22% in Estonia (see Figure 5.1b and Table 5.2). Interestingly, Eastern European countries tend to display lower gender wage gaps, whereas most West European states are suffering from higher raw gaps.[15] Additionally, they evolve very differently over

[13]This could be differences in schooling, experience or working hours or selection into occupations, industries and firms. Also non-cognitive skills, norms and institutional regulations influence the pay gap. For a comprehensive literature overview see Blau and Kahn (2017).

[14]The unexplained gap is often attributed to discrimination. However, it can also be caused by other unobserved or unobservable factors such as other negotiation skills or lower salary expectations of women. For example, Adriaans *et al.* (2020) find that both women and men find it fair if women earn about 3% less than men for the same work. Vice versa, the explained gap is not necessarily free of discrimination as women's work is often valued less and differences in explanatory factors are also consequences of social / institutional norms and restrictions (Boll and Leppin, 2015; Gould and Schieder, 2016; Klenner, 2016; Wrohlich and Zucco, 2017).

[15]However, this partly changes when focusing on the adjusted gender wage gap. For example, Leythienne and Pérez-Julián (2021) estimate the unexplained gender wage gap and order the countries according to the new measure. Interestingly, compared to the raw gap, Germany, Austria, Denmark, the Netherlands and Sweden

time. Most countries experienced a constantly decreasing gap between 2008 and 2018, with Greece, Cyprus and Luxembourg showing the largest decreases in this decade (see Figure E.1). Yet, some member states also suffered from increases (e.g. Latvia, Malta or Slovenia), or at least nearly stagnating gaps (e.g. Portugal, France and Italy) in this period. These differences across countries show that gender wage equality is not necessarily increasing in the European Union. And even in many countries in which the gaps have been declining, they are still rather large. Accordingly, the reduction of the wage gap is elemental to fostering gender equality.

Female Employment Countries that display high pay gaps also often have a high female employment rate and vice versa. This can be attributed to the fact that modest female labour market participation is often caused by an unequal gender division of household and care work and small employment opportunities for unskilled females. Accordingly, the women that do participate are often high skilled and well paid, which leads to a smaller gender wage gap (Eurofound and JRC, 2021). This largely corresponds to the data depicted in Figures 5.1b and 5.1c and might explain the lower gender wage gaps in the Central and Eastern European countries.

Figure 5.1c shows the absolute difference between male and female employment rates for the EU-28 countries (see also Table 5.2). For all members of the European Union and the UK, employment rates are smaller for women than for men, translating into positive gender employment gaps. The country with the largest female employment rate in 2018 was Sweden (75.6%), the lowest rate for women is seen in Greece (45.3%). Accordingly, Greece (together with Malta) also has one of the highest employment gaps, amounting to about 20pp. In Finland, Sweden and Lithuania, the gap is below 3pp. Most of the countries with high female employment (i.e. a lower gender employment gap) also display high gender wage gaps. However, there are also exceptions to the rule. For example, Belgium and Sweden have comparably low employment and wage gaps. In EU-28 average the gender employment gap amounted to 10.5pp in 2018. According to the European Commission (2017) this is the case although women are well qualified and even better educated than men, seeing that 44% of women in 2016 had at least a tertiary education or higher, whereas this was only true for 34% of men. This is to say that the lower employment rates are not necessarily a consequence

moved at least 10 positions downwards, while, Slovenia, Portugal, Poland and Romania moved at least 10 position upwards. The authors conclude that for the latter countries the gaps increased when accounting for worker's characteristics, reflecting a selection bias in the group of female workers.

of less favourable labour market characteristics but rather speak to underlying difficulties for women to enter the work force. Castellano and Rocca (2018) compare 26 European countries and find that labour market conditions and opportunities for women are very diverse, partly caused by differences in economic frameworks as well as policies and welfare systems but also social stereotypes and cultural gender models. A similar argument is made by The European Institute for Gender Equality (EIGE, 2021), which finds that the biggest gender gaps in full-time equivalent employment rates are found between women and men taking unpaid care of children (27pp) and between foreign-born men and women (21pp), and conclude that gender roles and stereotypes still have a negative impact on female labour market participation. Moreover, this data underlines the fact that maternal employment relies on the availability of childcare (Vuri, 2016).

Part-time Employment One route often taken by women trying to combine household/care work and paid work is part-time employment. Accordingly, countries that have a high female employment rate often display particularly high part-time employment rates. However, countries are again diverse in this respect. Some do not have a considerable share of employees working part-time, neither women nor men. Foremost among them is Bulgaria, with a female part-time rate of 2% and no markable gender gap (see Table 5.2). Also other Eastern European countries display very low part-time employment. On the other hand, West European countries have a more substantial share of women working part-time, the country with the highest share being the Netherlands (75.6% in 2018). Accordingly, they also display large gender gaps in this respect (e.g. 48pp in the Netherlands). In Germany, for example, nearly every second woman works part-time, while only 10% of men do.

Twenty years ago, Connolly and Gregory (2002) argued that gender wage inequality has become an issue mainly driven by the full-time/part-time gap, which describes the fact that employees – and in this case disproportionately women – experience a wage penalty for working fewer hours that does affect both their monthly earnings and hourly wage rates. This still seems to be true. Matteazzi *et al.* (2018) show that the wage gap is higher in countries that have more part-time employed workers. However, part-time employment alone does not necessarily explain a large part of the overall national gender wage gap. They see more importance in the nature of part-time employment and the institutional context. For example, they distinguish between settings where it is mainly taken up to preserve the work–family

balance or settings where it is imposed by employers for flexibility reasons. It is thus not entirely clear how part-time employment should be evaluated in the context of gender equality. Due to the fact that it is mainly drawn upon by women, its drawbacks are also disproportionately female issues. An important one is the fact that part-time workers often earn lower wages compared to full-time employees. This has been explained by different factors, among them are differences in characteristics and work experience, the occupations and industries part-time employment is mostly found in but also gender discrimination (see Matteazzi *et al.*, 2018, for a summary). However, there are also other drawbacks such as ineligibility for certain social benefits and the limitation of career prospects (Bollé, 1997). As a result of limited future employment and earnings prospects, it is often seen as a trap (Connolly and Gregory, 2010) and in contrast to full-time employment it is found to manifest the traditional division of labour (Stier and Lewin-Epstein, 2000). Additionally, it leads to lower life-time earnings and, as a result, higher pensions gaps (Bonnet *et al.*, 2022; Mavrikiou and Angelovsk, 2020). However, it also allows flexibility and gives some women the chance and motivation to enter the labour market or stay in work during childcare years. Overall, Connolly and Gregory (2010) argue that its role is not homogeneous. While it allows some women to pause and ultimately maintain their full-time careers during times of childcare, it can be a dead-end to others, who get stuck in low-wage jobs with limited career prospects.

Overall, the discussed elements of female inequality are associated with each other. There seems to be a trade-off between having low gender wage gaps on one hand and high female employment rates on the other (Boll and Lagemann, 2018, see also Figure E.2a). Countries with high female employment usually allow more flexible working conditions and foster part-time employment, empowering more women to access the labour market. However, this also decreases average female wages because of the type of work these women do.[16] Altogether, reducing part-time employment is not necessarily the way to improve women's situation and lower the gender wage gap. Boll and Lagemann (2018) rather propose reducing the part-time wage penalty and overcoming gender stereotypes.[17]

[16]This tendency is also seen in Figures E.2b and E.2c. However, this relationship is not as strong, since it also depends on a variety on other factors.

[17]Additionally, they deduce the prevention of long hiatuses, and the promotion of women's careers and their leadership skills as policy implications.

5.3 Gender-Specific Minimum Wage Effects

One way to influence pay and thereby possibly improve gender equality is a minimum wage. As was already visible in Figure 5.1, countries that have higher minimum wages also display slightly narrower gender wage and employment gaps (see also Figure E.3). Additionally, and possibly linked to the interrelations discussed above, these countries have higher gender gaps in part-time employment. These observations are only describing the status quo, however. Therefore this section gives an overview of the existing evidence on gender-related minimum wage effects for the EU-28.

5.3.1 Gender Wage Equality

The Connection to Minimum Wages The following subsection focuses on gender-specific minimum wage effects on wages and the gender wage gap in EU member countries.[18] This line of research has been followed for some time. In their seminal paper, DiNardo *et al.* (1996) look at the relationship between labour market institutions and the wage distribution. Employing counterfactual densities they find that a decline in the real US minimum wage in the 1980s has led to higher wage inequality, especially for women.[19] A connection between minimum wages and/or collective bargaining with gender wage disparities is also found for European countries (see for example Arulampalam *et al.*, 2007; Christofides *et al.*, 2013; Felgueroso *et al.*, 2008; Ramos *et al.*, 2016). However, Arulampalam *et al.* (2007) argue that unions do not necessarily present the interests of women, especially the low-paid. Moreover, women are not as frequently covered by union-membership as men are (Booth and Francesconi, 2003). Accordingly, universal minimum wages might be a better tool to fight gender inequality.[20] This is why this paper focuses on statutory minimum wages, summarizing the direction of their effects and affected subgroups.[21] However, comparing exact effect sizes is not straightforward, partly because they depend on a number of factors. As Robinson (2002) points out, the effect on the gap is contingent upon the level of the wage floor, the proportion of women among the low-paid and the size of the gender wage gap

[18] An association of the minimum wage with reduced gender wage gaps has also been found for non-European countries, see for example Broadway and Wilkins (2017); Hallward-Driemeier *et al.* (2017); Li and Ma (2015); Moon (2019).

[19] Further literature looking at this time frame but using different estimation methods corroborates these results (see Autor *et al.*, 2016; Fortin and Lemieux, 1997; Lee, 1999).

[20] For an examination of how minimum wages and collective bargaining can complement or oppose each other and how this varies across selected countries, sectors and time periods see Grimshaw *et al.* (2014).

[21] The paper also focuses on ex-post studies in contrast to ex-ante calculations as are for example provided by Dex *et al.* (2000) or Boll *et al.* (2015).

before the minimum wage introduction or increase. As seen in Section 5.2.2, the European member states are very diverse with respect to these dimensions. In addition to these very time and country-specific differences, the examined studies all use different approaches and sample definitions. The effects found in the studies with respect to female wage equality are summarized in Table 5.3. More detailed information is also available in Table E.1.

Table 5.3: Association between Minimum Wages and Female Wage Equality

	negative association with female wage equality	no association with female wage equality	positive association with female wage equality
All employees		Bargain *et al.* (2019)*, Robinson (2002)*	Caliendo and Wittbrodt (2021), Robinson (2005), Bargain *et al.* (2019)*, Blau and Kahn (2003)*, Butcher *et al.* (2012)*, Huertas *et al.* (2017)*, Matteazzi *et al.* (2018)*, Majchrowska and Strawinski (2018)*
Full-time empl.	Goraus-Tańska and Lewandowski (2019)*		Ferraro *et al.* (2018b)*, Grimshaw and Rubery (2013)*, Schäfer and Gottschall (2015)*
Young employees	Cerejeira *et al.* (2012)		

Source: Own summary. Note: Table summarizes the studies discussed in Section 5.3.1 according to the minimum wage effects on female wage equality they find in the studied subgroup. Bargain *et al.* (2019) is entailed twice since they find differences for UK and Ireland. More detailed information on the studies is available in Table E.1.
*Effect only descriptive.

Descriptive Evidence Since causal studies on the relationship between minimum wages and gender wage gaps in Europe are scarce, the consistent picture painted by descriptive evidence gives a first orientation as to the relationship. However, since they are not as insightful, they are identified in with a (*) in Table 5.3. For example, there are cross-country analyses such as the one provided by Blau and Kahn (2003), who use microdata from on 22 countries[22] over the years 1985 to 1994. Running a regression of the gender wage gaps on the minimum wage bite, they find a negative correlation between both. Similar evidence is provided by Schäfer and Gottschall (2015) who focus on full-time employees working in 25 European countries. They examine cross-country differences in three exemplary industries and find a negative and significant relation between the minimum wage bite and the gender earnings gap for most countries, arguing that a minimum wage is associated with an increase of gender

[22]More than half of them are in Europe.

wage equity for full-time employees. Matteazzi *et al.* (2018) additionally look at part-time employees since they are over-represented among low-paid jobs and seldom unionised, which makes it unclear whether the part-time wage gap is also reduced. Using EU-SILC 2009 data for 11 European countries[23], they find that wage-setting institutions especially affect wages at the bottom of the distribution, which is why the full-time/part-time gap for women reduces with higher minimum wages and more strongly so than the full-time gender wage gap.[24]

This is especially interesting in relation to the discussion in Section 5.2.2, which identified the reduction of the part-time wage penalty as a way to reduce the gender wage gap. Since part-time employed women are disproportionately found at the bottom of the wage distribution a minimum wage could be a way to reduce this penalty. Another positive relationship between a wage floor and gender wage equality is also provided by Grimshaw and Rubery (2013). Using data from the EU-SILC, the Organisation for Economic Co-operation and Development (OECD) and the SES on 16 countries, they look at the correlation between the bite of the minimum wage (measured as the Kaitz index) and the national gender wage inequality. They find that the higher the minimum wage level, the lower gender gap in incidence of low wage employment. Huertas *et al.* (2017) also look at this relationship, but for different regions in Spain. They regress the regional raw and adjusted gaps on a variety of factors, among them the regional minimum wage bite. They find that the higher the bite, the lower the unexplained regional wage gap.

Another approach used in a variety of studies is to examine the evolution of the wage gap after a minimum wage introduction. For example, Robinson (2002) conducts a regression of the real hourly wage using data from the UK Labour Force Survey (LFS) from 1995 to 2000. She finds that after the introduction of the minimum wage the gender gap decreased only slightly. An additional quantile regression does not suggest a decrease in the gap at the bottom compared to the time prior to the minimum wage introduction, which she attributes to the minimum wage of £3.60 being set at too low a level.[25]

A similar finding is provided by Bargain *et al.* (2019), who study the minimum wage introduction in both the UK and in Ireland. They construct counterfactual wage distributions and estimate gender differences before and after the minimum wage implementation. They

[23]They had to exclude a variety of countries due to small numbers of part-time employees and large non-response rates for workplace information.

[24]Unfortunately, the study does not include part-time employed men due to sample sizes, which is why gender wage gap reduction can only be identifies for full-time employees.

[25]In a simulation she suggests that the minimum wage would have had to reach £5.00 in order to reduce the average pay gap by 3 percentage points.

find that the wage floor is accompanied by a reduction in the gender wage gap for Ireland, especially for the low-wage workers. At the bottom of the distribution it was almost eliminated. However, they do not find a significant decrease in the UK, which they explain by a lack of compliance that was higher for the UK.

For Poland, Majchrowska and Strawinski (2018) estimate the effect of a substantial 2008–2009 minimum wage increase on the gender wage gap. They generate gender-specific counterfactual wage distributions and decompose the actual and counterfactual gender wage gaps, assuming that any temporal change is only caused by the minimum wage increase. They find that the estimated gender wage gap decreased, which was mainly driven by a decrease among young workers, i.e. those for whom the minimum wage was most binding.

Ferraro *et al.* (2018b) analyse the effect of minimum wage on the wage distribution for Estonia. Although they do not estimate the effect on the gender wage gap directly, they analyse heterogeneous wage effects for men and women. They find that up to the 30^{th} percentile the effects are greater for women than for men with especially strong differences at the bottom of the distribution. They conclude that the higher the minimum wage, the more women's wages catch up, meaning that a minimum wage increase can help reduce the gender wage gap. Another study that identifies gender differences in the minimum wage effect on wages is provided by Butcher *et al.* (2012). The authors examine the wage inequality in the UK between 1998 and 2010 and find that wages were compressed among both men and women. For women this effect was greater, though, and reached higher up into the distribution. For example, the decrease in the percentile ratio of the 50^{th} to the 5^{th} percentiles for women was about 10%, whereas it amounted to 4% for men. Moreover, they find that for females under 30 about 50% of the decline in the log of the ratio of the 50^{th} to the 5^{th} percentile can be attributed the minimum wage, while for young men the effect on inequality is not as strong. However, Butcher *et al.* (2012) point out that due to data and methodological restrictions these effects should be interpreted with caution.

Causal Evidence Causal evidence on the effect of minimum wages on the gender wage gap in Europe is scarce. One is provided by Robinson (2005) who, in a subsequent paper to her previously discussed study, conducts a causal estimation for Britain. Using LFS data she employs a DiD approach, comparing wages for men and women in differentially affected

regions over time.[26] Using variation provided by the fact that the fraction of men and women earning low wages differs across regions, she identifies the effect of the introduction of the national minimum wage. Looking at 11 regions in Britain, the author estimates the effect on the log hourly wage in a triple difference setting, focussing on the interaction of gender, post-reform period and region. She finds that the gender pay gap decreased by 1 to 2 percentage points more in regions where women are overrepresented among the low paid or where the average distance between the pre-reform hourly wage and the minimum wage was higher. However, some effects are statistically insignificant.

A regional approach is also used in the causal paper by Caliendo and Wittbrodt (2021). The authors analyse the effect of the German minimum wage introduction on regional gender wage gaps. Using 2014 and 2018 data from the Structure of SES, they employ a regional DiD approach. Based on Card (1992), it assumes that wage floor effects are stronger in regions with higher minimum wage bites. They distinguish high- and low-bite regions, cut off at the median fraction of women earning less than the wage floor prior to the reform. They find that the gap at the tenth percentile of the gender-specific wage distributions was reduced by 4.6pp in high- compared to low-bite regions. This translates into a reduction of the wage gap among the lowest paid by 32%. For the gap at the 25^{th} percentile they find a reduction by 3.4pp (-18%), while at the mean it was reduced by 2.3pp (-11%). In a specification based on a continuous bite measure, they find that a 10pp increase in a region's fraction of affected female employees leads to a reduction of 3.3pp in the wage gap at the tenth percentile. The authors thus state that the minimum wage introduction reduced the gender wage gap significantly. While this was especially true among the low-paid, it also reached up to higher points in the distribution.

Circumventions and Non-Compliance So far, most evidence speaks to a negative relationship between a minimum wage and the gender wage gap, partly shown to be causal. However, there is also evidence that points in another direction, albeit that it is always related to insufficient enforcement or employers' circumvention strategies. For Portugal, Cerejeira *et al.* (2012) explore the effect of the 1998 increase in the wage floor for minors[27] and next to the

[26]The DiD approach relies on the definition of a treatment and a control group and a pre- and post-treatment period. It compares the outcome changes over time in the treatment group with those in the control group. Its identifying assumption is that the trends in the outcome variable would be the same for both groups in absence of the treatment.

[27]Before that, workers younger than 18 years of age were entitled to 75% of the full minimum wage, afterwards this increased to 100%.

base wage also look at additional dimensions of employee compensation that allow for circumventions.[28] Using a DiD approach, they find that there were particularly relevant increases in the gender gap for overtime payments and fringe benefits, which were redistributed in favour of men. In sum, they find that the wage gap for minors increased 2.7pp more than for other groups, preventing a positive effect on gender wage equality. Analysing potential channels for this, they point to industries exhibiting different flexibilities as well as discrimination.

A positive effect on women's equality could also be impeded when they are more often subject to minimum wage violations. A study looking at differences in compliance is provided by Goraus-Tańska and Lewandowski (2019), who analyse minimum wage infringements for 10 Central and Eastern European (CEE) states from 2003-2012.[29] In a probit regression they find that women were significantly more likely to earn less than the wage floor than men. The same holds true for young employees and workers in small firms or the service sector. They thus argue that non-compliance affects particularly those employees who are meant to benefit from a wage floor, since they have the weakest bargaining power. Moreover, a higher minimum wage bite is associated with a higher incidence of non-compliance in the studied countries.

The fact that the establishment of a minimum wage does not completely abolish female inequality is touched on by Avram and Harkness (2019). They compare probabilities to transition out of minimum wage jobs across regions with different fractions of minimum wage employees in the UK. They find that about 50% of minimum wage workers are able to transition to better paid jobs within one year. However, they point out that women have lower probabilities of moving to high paid jobs, along with part-time employees and persons that have an unemployment experience.

All in all, the studies discussed above paint a rather homogeneous picture for European countries: Minimum wages are mostly found to be related to lower levels of gender wage inequalities. Some studies are also able to identify a causal relationship, showing that wage floors reduce the gender wage gap, especially for the low-paid. Exemptions from this either relate to circumventions or non-compliance or the wage floor being set at too low a level. This evidence suggests that promoting minimum wages could mean an improvement of women's wage position in comparison to men, at least at the bottom of the wage distribution. Accord-

[28] They look at fringe benefits, overtime payments and overtime hours.

[29] They include Bulgaria, the Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, Romania, Slovakia and Slovenia.

ingly, wage floors might be seen as an effective tool for reducing gender wage gaps. However, the majority of the studies only identify a correlation. The two studies that identify a causal negative minimum wage effect on the wage gap do not focus on employment effects. While they both look at previous evidence and argue that employment effects in the studied cases were not strong, they cannot be ruled out. The next section will examine gender-related employment effects more closely.

5.3.2 Employment

So far, I have mostly disregarded potential employment effects at the intensive and extensive margins. As discussed in Section 5.2.2, however, employment is also a very important pillar of female equality. Additionally, it is not entirely unrelated to wage gap effects. Women could deliberately reduce working hours or leave employment completely. More importantly, they could be hit harder by disemployment effects in the wake of a minimum wage. This could lead to a decrease in the gap that is of a technical nature, being driven by the fact that low-paid women – rather than earning more – leave their jobs relatively more often than men.

The debate on minimum wage effects on overall employment is long and controversial. It is mainly found that minimum wages do not have any or at most small employment effects (for an extensive overview see for example Dube, 2019). That lack of effects can be caused by a variety of factors, among them the reduction of employment at the intensive margin, labour market frictions, incomplete compliance, price increases and a reduction in profits (Metcalf, 2008). Additionally, Eurofound (2021b) points out that the magnitude of effects also relies on a country's individual circumstances such as the minimum wage bite or its economic situation. However, the effect on female employment has not been in the centre of attention that often. Previous studies on non-European countries do partly find negative employment effects for women (see for example Kambayashi *et al.*, 2013; Shannon, 1996). Others find no effects for either gender (Menon and Rodgers, 2017) or small negative effects for both (Williams and Mills, 1998). In the following section I will look at studies that focus on gender-specific employment effects of minimum wages in Europe. The studies are summarized in Table 5.4 according to their effect on female versus male employment.

Employment Effects Particular to Women The most comprehensive collection of studies on gender-related employment effects is available for the UK. They either look at the introduction of the National Minimum Wage (NMW), the introduction of the National Living

Wage (NLW) or their subsequent upratings.[30] Most studies examining the gender-specific employment effects of the first introduced NMW do not identify an impact particularly detrimental to women. One of the first is provided by Stewart (2002). Making use of the regional variation in a DiD framework, he finds that employment growth was not significantly lower in highly affected regions, with no diverging effects for women. In a subsequent paper, Stewart (2004) uses additional data sources and relies on an individual level DiD approach, looking at full-time employees earning less than the wage floor before the reform as the treatment group and differentiating by age and gender. The control group consists of employees earning slightly above the wage floor before the introduction. Again he finds no significant adverse employment effects for the considered subgroups or any of the data sources. The same approach is chosen by Dickens *et al.* (2009). Employing LFS and Annual Survey of Hours and Earnings (ASHE) data from 2001 to 2006, they look at minimum wage upratings and also do not find strong evidence for adverse employment effects. Although they find some scattered negative effects of the minimum wage on job retention for both adult women and men in some specifications, the results are not robust. Together, the studies identify no strong negative employment effects induced by the UK's minimum wage, neither for men nor women. There is also evidence from other European countries that falls in line with the previous results and does not identify employment effects that affect women especially.[31] For example, Abowd *et al.* (2000) compare minimum wage effects on employment between France and the United States, looking at gender-specific effects.

In a DiD framework, they analyse employment transition probabilities conditional on a person's position in the wage distribution. They show that an increase in the real minimum wage was associated with a decrease in the future employment probability of affected individuals compared to other employees. Yet, while differences between male and female workers were not large, the effects for low-paid men were actually slightly stronger, albeit that more women were paid in proximity to the minimum wage. The results thus suggest that when there were gender-related differences, men were more detrimentally affected.

[30] While the NMW was introduced in 1999, the NLW came into effect in April 2016. It was set at £7.20, and thus exceeded the NMW by more than 7% at this time. Moreover, workers over 24 were eligible to it, whereas the adult rate of the NMW had applied to employees older than 20 since 2010 (see Aitken *et al.*, 2019).

[31] In order to identify whether women were affected more strongly than men, I only include studies that also examine effects on male employment. For example, Addison and Ozturk (2012) employ a cross-country analysis for 16 OECD countries from 1970 to 2008 and find that minimum wage increases correspond with lower female employment and participation rates. However, since they only look at women, it is unclear whether this effect is particular to them.

Table 5.4: Minimum Wage Impact on Employment for Men and Women in Comparison

	men more negatively affected	women more negatively affected	both negatively affected	both not affected	both positvely affected
All employees	Abowd *et al.* (2000)		Baranowska-Rataj and Magda (2015), Pestel *et al.* (2020)	Dickens *et al.* (2009), Stewart (2002)	
Full-time empl.			Ferraro *et al.* (2018a)	Aitken *et al.* (2019), Capuano *et al.* (2019), Dickens *et al.* (2015), Fialová and Mysíková (2021), Stewart (2004)	
Part-time empl.		Aitken *et al.* (2019)	Capuano *et al.* (2019), Dickens *et al.* (2015)*, Pestel *et al.* (2020)**		
Young employees	López Novella (2018)		Kabatek (2015)		Dickens *et al.* (2014)
Permanent empl.			Kamińska and Lewandowski (2015)		
Temporary empl.		Kamińska and Lewandowski (2015)			

Source: Own summary. Note: Table summarizes the studies discussed in Section 5.3.2 according to the minimum wage effects on employment they find for men and women in the studied subgroup. Studies are entailed twice when they identify different effects for subgroups. More detailed information on the studies available in Table E.2.
*Effect not estimated for men but is potentially the same. ** Effect measured for marginal employment, which is a specific form of part-time employment.

For Estonia, Ferraro *et al.* (2018a) analyse the effect of minimum wage increases that took place in 2013–2016. They look at the impact on the retention probability of full-time workers in a DiD framework. Using the Estonian LFS they find that the wage floor increases had no or at most small effects on employment retention, with no differences for men and women.[32] The findings are underlined by Fialová and Mysíková (2021) who analyse the effect of the minimum wages in the Czech and Slovak Republics from 2005 to 2017. They use a fixed effects model on the individual level and examine the effect of an interaction between the wage floor increase and the position of an employee in the wage distribution on the subsequent probability of staying employed. However, they do not find a significant effect in either country with no deviations for women, younger and older or low-educated workers.

Consideration of Part-Time Employees So far, the studies have merely concluded that there were no employment effects that affected women more strongly than men. Yet there are also papers that do not fall in line with the previous evidence. This is mainly because the previously discussed studies do not look at part-time employment. There seems to be an indirect effect on women caused by the fact that they are more likely to be part-time employed. One of the studies making this case is that by Dickens *et al.* (2015). The authors use the individual DiD approach adopted in previous studies, re-examining the evidence on effects of the UK NMW introduction but considering subsequent increases as well as different subgroups. Although they continue to find no employment effect for full-time workers, they establish a decrease in employment retention among part-time female workers, the most affected group. The wage floor reduced their employment retention – which lay at 75% – by about 3 pp. These effects increased in the recession after 2008. The authors conclude that while the minimum wage increased wages and reduced inequality overall, it might have modestly decreased the employment prospects of part-time employed women. However, they do not estimate effects on male part-time employees due to sample size but note that they might expect to find impacts among them as well. Therefore it is unclear whether this part-time effect is gender-specific. In any case, women constitute the major share of part-time workers, which causes them to be more negatively affected by the employment effects.

This is also corroborated by Aitken *et al.* (2019), who examine the effects of the 2016 introduction of the NLW. They use a DiD approach on the individual level in order to analyse the impact of the introduction and the previous year's increase on said outcomes.

[32] However, the authors point out that they have only small samples sizes.

While they find no negative effects on employment retention for full-time employees and men, they identify evidence for a decrease in retention for part-time employed women, which amounted to roughly 2.5pp. A similar study design is chosen by Capuano *et al.* (2019), who also look at the effect of the NLW and subsequent increases on employment and hours worked. Following the previous research in employing a DiD estimation, they find that the introduction of the NLW induced a reduction in employment retention for part-time employed but not for full-time employees. Accordingly, they are in contrast to Aitken *et al.* (2019) who did not find such effects for men, which they attribute to different time frames used. [33] In numbers, they estimate that a 1% rise in the NLW led to a decrease in employment retention of about 0.56% for part-time employed women. They thus conclude minimum wage increases of that magnitude should be considered with caution.

For Germany, Pestel *et al.* (2020), look at the employment effects of the 2015 minimum wage introduction. Employing different data sources in a regional DiD approach they find a small reduction in employment but no clear heterogeneous effects. At the most they identify a slight decrease in men's probability of being regularly employed which has to be interpreted with caution due to small sample sizes and possible measurement error in the data source. However, the authors find that there was a decrease in mini-jobs, a special type of part-time employment.[34] While this is true for both men and for women in this subgroup it could imply that women are more affected by a reduction in employment seeing that the majority of marginally employed workers is female. On the other hand, it is not entirely clear whether these jobs were lost or transformed into regular part-time employment. As Caliendo *et al.* (2018a) deduce, a substantial share of them could indeed have been transformed rather than lost.

An analysis that looks at another vulnerable subgroup is provided by Kamińska and Lewandowski (2015). Differentiating between permanent and temporary employment they identify stronger effects for the more precariously employed. They estimate the minimum wage effects on job separations in Poland in 2002-2013 in a DiD framework with propensity score matching. The authors find that the minimum wage induced job separations that were larger for temporarily employed workers than for permanent employees. Especially affected were temporarily employed women, who accounted for more than half of employees that lost

[33] Aitken *et al.* (2019) look at 2007-2017 data whereas Capuano *et al.* (2019) employ data from 2010-2018.

[34] Mini-jobs, also called marginal employment, are defined by monthly wages below €450. The particular effect of the minimum wage mainly on marginal employment is corroborated by many other studies for Germany (see Caliendo *et al.*, 2019, for a summary).

their jobs due to the minimum wage. Employment effects for permanent workers did not differ by gender.

With a similar identification strategy, Baranowska-Rataj and Magda (2015) focus on the subgroup of young employees in Poland. They examine the effect on the risk of job separation induced by minimum wage increases for employees aged 18-29. They find that the increases in the wage floor led to a decrease in the probability of remaining employed, for both genders alike. Interestingly, part-time employees were less likely to have lost their jobs after a minimum wage increase among this specific subgroup of young employees. While they do not differentiate by gender, it can be assumed that women are thus more positively affected than men.

Minimum Wage Exemptions for Youths There is another strand of literature looking at a different setting by identifying effects of minimum wage exemption rules. Dickens *et al.* (2014) estimate the effect of a legislated minimum wage increase in UK of about 16-22% at the age of 22. Looking at low-skilled workers a few months younger or older, they employ a regression discontinuity approach and find an increase of 3-4pp in their rate of employment. Interestingly, they find gender differences in the source of the additional employment.[35] For women, they tie the positive effect on employment to a decrease of inactivity. Men seem to be coming out of unemployment instead, although their effects are even a little less robust. These positive employment effects could be caused by an increase in labour supply or job search intensity after a 20% wage increase.

Another paper that looks at the age-dependent minimum wage regulations is Kabatek (2015), who focuses on youth employment in the Netherlands. He makes use of the wage floor legislation which defines the increases in the minimum wage with the age of the employee until reaching the adult minimum wage rate when a workers turns 23. In an employment duration analysis he shows that for workers younger than 23, the probability of job loss increases by 1.1% in the months before their birthdays. However, he does not find differences according to gender.

López Novella (2018) looks at the impact of abolishing the lower minimum wage rates for 18 to 20 year olds in 2013 and 2014, which translated into a wage increase of 6 to 18% depending to the age of the employee. She analyses the effect on retention and accession rates (i.e. the probabilities of remaining in a firm and of entering a new firm) with a DiD approach.

[35] However, they are not as robust as the main findings due to smaller sample sizes.

The author finds slightly significant increase in retention probability for women, amounting to 4 to 6pp. Effects for men were insignificant. Accession probabilities however, are mainly reduced for men, while there is no significant effect for women. The results thus suggest that in the specific case of sub-minimum wages being abolished for young people, women have higher chances of keeping a job and men have lower chances of finding a job.

In conclusion, the evidence on gender-specific minimum wage effects on employment is not as unanimous as in the case of wage gaps. Most studies identify no or negative employment effects, but they are similar for both genders, which means that they are not expected to affect female equality (see Table 5.4). Two studies find that men are more negatively affected than women, one of which is looking at minors, though. Two studies find the opposite effect, women being more negatively affected than men, but this is among part-time employees and temporary workers. That relationship is also visible in another issue: While most studies among part-timers find that women and men are affected similarly, it has to be kept in mind that the majority of part-time employees is female in the EU. Accordingly, a negative effect in this subgroup possibly translates into higher effects for women. For full-time employees there do not seem to be gender-specific employment effects.[36] This gives a strong hint that the lower paid part-time employment is really the issue that makes women more vulnerable to job losses, thus being one driver for gender inequalities, as was already discussed in Section 5.2.2. It also underlines that hours of work are relevant to this topic. In this sense, it is also very interesting to asses how minimum wages influence them.

5.3.3 Working Hours

So far I have discussed evidence on the extensive margin of employment. Yet, minimum wages can also affect the amount of hours worked. Accordingly, this section will focus on gender-specific effects on working hours (see also Table 5.5).

Connolly and Gregory (2002) look at UK women in a DiD framework, applying a special focus on women working part-time but do not find significant changes in hours worked, neither

[36] A similar conclusion is drawn in the meta-regression analysis performed by Hafner *et al.* (2017), who look at effects of the NMW on employment in the UK including 22 empirical studies. They do not find negative overall employment effects. However, part-time employees seem to have experienced a stronger reduction in employment than other sub-groups. Interestingly, for women in general the findings suggest a positive employment effect at the extensive margin. The authors see this in relation to the fact that almost 80% of the estimates focusing on part-time employees are related to females, so the remaining effect for women might be caused by a supply effect for full-time female workers.

for full- nor for part-time employed women. The same result is also obtained by Aitken *et al.* (2019) for the NLW. Capuano *et al.* (2019) largely corroborate this, even though they find cautious evidence that full-time employed men experienced a reduction in working hours after the introduction of the NLW.

Table 5.5: Minimum Wage Impact on Working Hours for Men and Women in Comparison

	women more negatively affected	both negatively affected	both not affected
All employees	Baranowska-Rataj and Magda (2015)*	Stewart and Swaffield (2008)	Dickens *et al.* (2009)
Full-time empl.			Aitken *et al.* (2019), Capuano *et al.* (2019), Connolly and Gregory (2002)
Part-time empl.			Aitken *et al.* (2019), Capuano *et al.* (2019), Connolly and Gregory (2002)

Source: Own summary. Note: Table summarizes the studies discussed in Section 5.3.3 according to the minimum wage effects on hours they find for men and women in the studied subgroup. Studies are entailed twice when they identify different effects for subgroups. More detailed information on the studies available in Table E.3.
*This effect is related to absence of an effect on young women and a positive effect on young men.

Stewart and Swaffield (2008) estimate the effect of the minimum wage introduction on the working hours of the low-paid. They adopt a DiD approach with the New Earnings Survey (NES) and LFS and identify a negative effect on hours. However, the results do not differ between men and women. They thus do not identify gender-specific differences in the effects. In their above mentioned paper, Dickens *et al.* (2009) also find only scarce evidence of a consistent minimum wage effect on hours. Some estimations speak to the fact that larger increases in 2001 and 2003 might have had a negative impact on basic hours for adult men.

In a DiD setting, Burauel *et al.* (2020a) look at the effects of the minimum wage introduction in Germany. With respect to contractual hours they find a highly significant negative effect on hours of full-time employees of 5.5% one year after the reduction but an insignificant impact for part-time employees. Unfortunately, they do not look at gender-specific effects among those subgroups. However, for all employees subject to social security contributions, they identify effects on men and women and find a decrease of similar magnitude for females. It is unclear whether this differs from men's effects, though, since they are also significantly affected but their estimation does not satisfy the common trend assumption. For young em-

ployees in Poland, Baranowska-Rataj and Magda (2015) show that hours of work increased after the minimum wage. They argue that this is because employers compensated job separations by raising the hours of employees that remained at the firm. However, in a heterogeneity analysis they find that this was only true for men, while there was no effect for women.

Overall, the results for the gender-specific effect on hours are rather concordant in that there does not seem to have been a strong impact on hours for women, at least not stronger than on men. However, it has to be noted that the evidence is rather limited, especially with respect to the countries it applies to. Further research in this area could be insightful in order to yield generalisable effects.

5.4 Outlook and Conclusion

This paper summarized gender-specific effects of minimum wages on labour market outcomes for the EU-28 countries. The states display large ranges when it comes to minimum wages, gender wage gaps and female and part-time employment. In this sense, they pose an interesting case for identifying minimum wage effects. There are some broad relationships to be detected. Large gender gaps often come hand in hand with a higher female employment rate and also more part-time employment, especially among women. The option of working fewer hours seems to empower female workers to enter the labour market whilst maintaining a work-family balance. In this sense, part-time employment is a meaningful feature for increasing female labour force participation and thus fostering women's equality. However, part-time employment is also often subject to a wage penalty, which in turn has a negative impact on the latter.

Minimum wages have been proposed in order to tackle issues of gender inequality. With regard to the relationship between wage floors and gender equality measures, this study identifies a few consistent key findings. First, minimum wages seem to be associated with lower gender wage gaps, a relationship that has been established for a variety of countries, although only sometimes shown to be causal. However, it is subject to the restriction that there is to be no non-compliance or circumvention strategies that disproportionately affect women. Additionally, the existing literature does not specifically factor in potential job losses of low-paid women, which is why they cannot be ruled out as a reason for the reduction. Yet, the summary of studies shows no strong evidence for impacts on employment that are specific to the subgroup of women, especially not among full-time employees. However, it

is apparent that there might be a particular impact on part-time employment. Since this is mainly executed by women, they might be affected more strongly after all. Moreover, some studies even identify a specific effect on part-time and temporarily employed females. In this sense, it cannot be ruled out that these potential job losses of the more vulnerable and low-paid employees lead to the reduction of the gender wage gap that was discussed before. Possible adaptations of working hours in the wake of a wage floor do not seem to display gender-specific patterns. However, the evidence to this effect concentrates on only a few countries and more research on this topic is needed. While this shortcoming is especially visible for the analysis of working hours, it is also true for the other dimensions studied. There are large research blanks for some countries. Additionally, not many studies identify a causal effect for wage gaps, and non directly look at gender (part-time) employment gaps, an issue that could be addressed by future research. Moreover, since lower wages, working part-time and having a lower working life duration are the main drivers of the gender pension gap (Bonnet *et al.*, 2022; Mavrikiou and Angelovsk, 2020), a minimum wage could also reduce this additional dimension of inequality. However, this relationship is largely unresearched.

The identification of minimum wage effects on part-time employment suggests putting more emphasis on this working arrangement in the context of minimum wages. It is often found to be subject to a wage penalty in comparison to full-time employment. Cross-country correlations do indeed show that this penalty is lower in the presence of minimum wages. However, more research is needed to understand this relationship. The causality and underlying mechanism for this effect are especially relevant. If the full-time/part-time gap as well as the gender wage gap are only reduced by a minimum wage because low-paid part-time employees are let go, this does arguably little for female equality. This study has found indications that this might be the case, with minimum wages seeming to lead to lower retention possibilities of part-time employees. Additionally, a reduction in part-time employment is problematic in itself, since it has been found to be beneficial in terms of female equality and labour market participation. One possible way to disentangle this could be the reduction of the full-time/part-time gap, making part-timers less vulnerable. This is not straightforward, though, and calls for a better understanding of its nature. One part of it is addressed by Manning and Petrongolo (2008) who argue for the UK that a large part of the penalty can be explained by differences in the jobs that are carried out and argue that policy measures have had little effect on its reduction. Similar drivers are found by Matteazzi *et al.* (2018), but they

also argue that disparities between female full- and part-time employees are to a large extent explained by vertical and horizontal segregation. Boll and Lagemann (2018) summarize that the part-time wage penalty is heterogeneous between sectors, being dependent upon the time and leadership practices they implement, which is why sectors and firms should be drawn upon in the endeavour of addressing this problem.

Overall, this paper has shown that a European initiative to promote 'adequate' minimum wages might reduce gender wage gaps. This is especially relevant since they are still comparatively high in some countries. Additionally, inequality has been recently exacerbated by the COVID-19 pandemic, which has been shown to affect women – and especially the low-paid – disproportionately (Profeta, 2021; Queisser, 2021). However, in order to improve women's situation as a whole, it should come along with a large focus on part-time employment and possible channels to reduce the part-time wage penalty. Additionally, wage floors have to be fully enforced such that women are not disproportionately affected by non-compliance. However, minimum wages cannot be the panacea in the fight against female inequality. One of many reasons for that is that they are designed to focus on inequality at the bottom of the distribution, with potential spillover effects. Yet, 'glass ceilings' are also a relevant aspect of gender inequality. Additionally, they do not affect the sources of modest female labour market participation, such as views on the traditional division of labour. On the other hand, they are not a requirement for female equality either, since wage gaps can also be comparably small and female employment high in the absence of minimum wages as is shown by Sweden.

Chapter 6

General Conclusion and Outlook

This thesis provided both a general overview on the German minimum wage reform and more detailed analyses of specific wage floor effects. Overall, the chapters have shown that the minimum wage reform has increased wages at the bottom of the distribution in Germany, even though a substantial share of non-compliance still existed in the short run. There were small disemployment effects at the extensive margin, mainly caused by a reduction of marginal employment. However, some hopes of minimum wage proponents did not materialize in the short run, since poverty and overall inequality did not seem to have decreased. Yet, at least one measure of equality seems to be affected positively: the gender wage gap. We have shown that due to the minimum wage, the gap between male and female hourly wages was reduced, especially among the low-paid. This association between the two can be found for other countries in Europe, too. However, there is also EU related evidence that points to the fact that part-time employees – which are in many cases female – are more often prone to job losses in the wake of a minimum wage. Thus, the overall effect on gender equality on the labour market needs to be researched more thoroughly. In this last chapter, I briefly recap the main findings of the previous chapters and elaborate on their limitations and future research paths. After that, I offer some concluding remarks on the minimum wage in Germany.

Chapter 2 summarizes the short-run literature on wage floor effects for Germany and showed that the minimum wage led to a substantial increase of wages among the low-paid. Women, employees in mini-jobs, low-skilled and foreigners benefited particularly. At the same time, there was also non-compliance in the short-run, which meant that a considerable share of eligible workers was still paid below the wage floor. Moreover, small negative employment effects were found, largely driven by a reduction in marginal employment. However, it is not

entirely clear whether these employees were actually let go or transferred to regular part-time employment. Finally, the goals of poverty and inequality reduction were not found to have met in the short run. This can be partly attributed to the fact that working hours were reduced, causing monthly earnings to have stayed largely constant. These effects have to be taken with a grain of salt, though, since there are a few issues that might have mediated the short-run effects of the wage floor. For example, the minimum wage introduction fell into a time of economic upswing, which could have muffled negative employment effects. Moreover, the substantial incidence of non-compliance shows that the minimum wage was not fully enforced. Employers' circumvention strategies might have additionally undermined the wage floor regulations or enabled them to transfer the minimum wage costs. Another issue that has to be considered is that these effects are all measured in a very short time frame, which does not permit to draw conclusions for a the long run. This also highlights a future research avenue: It would be insightful to repeat a systematic overview on the minimum wage effects after a reasonable amount of time to incorporate a larger time frame (including the minimum wage increases) and thus also consider long-term adjustments. Moreover, Chapter 2 carves out the difficulties with current data sources and emphasizes that a stronger data base is needed in order to better understand minimum wage effects.

The third and fourth chapters of this thesis present empirical analyses that estimate the effects of the German minimum wage on employment and the gender wage gap, respectively. Chapter 3 estimates the short-run employment effects in a regional DiD framework, relying on the fact that the minimum wage did not unfold the same bite across all regions: since local areas displayed different wages before the reform, they are also expected to be affected by the minimum wage to different degrees. Making use of SOEP, SES and FEA data, we are able to contribute a few key findings to the literature. First, we argue that there were no relevant anticipation effects that caused increasing wages before the minimum wage reform. Second, we show that the minimum wage led to a small reduction in overall employment by about 0.4 percent. This is largely driven by a decrease of mini-jobs, which shrank by about 3 percent and thus much more strongly than overall employment. However, since mini-jobs are defined by earning less than €450 a month, increasing hourly wages with stable working hours can cause marginal employees to exceed that threshold. Accordingly, they could adapt their contracts, leaving marginal and entering part-time employment. We do find hints that this might have been the case for many marginal employees, which did not lose their jobs

but transferred to part-time employment. This could have masked a stronger reduction in regular employment induced by the wage floor. Unfortunately, we cannot disentangle these effects due to data restrictions. Another limitation of our paper is the fact, that the SES data is strictly speaking only representative at the level of federal states. It is therefore possible that we induce measurement error by employing the smaller level RLMs. We try to minimize this issue by employing weighted data as well as a binary bite measure in our sensitivity analysis and do not find cause for concern. We also explore possible reasons why our results fall behind the ex-ante predictions of 500,000 to 900,000 job losses. These considerations are similar to the issues discussed in Chapter 2. One possible explanation is of course the time frame: While we look at short-run effects, the previously predicted effects are estimated for the long run. Additionally, next to non-compliance, employers could have taken other routes of adjustments, such as decreasing working hours or increasing prices. Moreover, in relation to the theoretical considerations discussed in Section 1.1, the reason for a lack of disemployment effects could be the existence of a monopsony, in which the minimum wage is smaller than the marginal product of labour. In any case, the minimum wage effects on employment should be constantly evaluated and re-estimated more thoroughly when long-term information is at hand.

Chapter 4 looks at the effect of the minimum wage on the gender wage gap in Germany, especially for low-paid individuals. We employ the same identification strategy that was used in the third chapter and estimate the effects in a regional DiD approach. With SES data from 2014 and 2018, we find that the minimum wage decreased the gender wage gap at the 10^{th} percentile by 4.6 percentage points for eligible employees in high-bite regions compared to low-bite regions. This translates into a reduction of 32% between 2014 and 2018. Moreover, we also look at the wage gap at the 25^{th} percentile and the mean. Here the effects are smaller and not as robust. One limitation of this chapter, however, is that we are not able to account for employment effects. It is possible, that low-paid women that should have benefited from a wage increase instead quit or lost their jobs. In this case, the gender wage gap would have in fact decreased. We are confident that this is not a large scale issue, since previous studies do not find strong evidence of heterogeneous employment effects, though. However, this is certainly an important avenue for future research. Moreover, the time frame limitation discussed in the previous chapters applies here, too: We establish effects for the short run, which is why this research question could be picked up in the future.

The fifth chapter summarizes previous evidence on gender-specific minimum wage effects across EU member states. It examines the association between wage floors and the national gender wage gaps in these countries and looks at minimum wage effects on employment and working hours that are particular to women. I find that member states are heterogeneous with respect to the importance of minimum wages and with regard to the status quo of gender equality on the labour market. However, previous literature finds that greater wage floors are associated with narrower wage gaps. While both genders' adaptations of working hours were not affected differently, there is evidence that part-time employees might be especially prone to job losses. Since they are often female, this can be interpreted as a larger employment effect on women. Thus, I can not rule out that the negative association with wage gaps is related to the fact that low-paid women lose their jobs relatively more often. As was already mentioned, this relationship has to be investigated more thoroughly in future research. Additionally, the interpretation of results is also complicated by the fact that part-time employment plays an important double role for female equality. On the one hand, it enables women to participate in the labour market while simultaneously balancing family and care work. On the other hand, it offers lower wages and less career prospects, potentially leading into the 'part-time trap' and exacerbating other aspects of gender inequality such as gender pension gaps (Connolly and Gregory, 2010; Mavrikiou and Angelovsk, 2020). I therefore argue that part-time employment should be given special attention in the context of minimum wages, both by researchers and by policy makers. Especially the part-time/full-time wage gap is an issue that should be focused on. Additionally, the chapter also uncovers other areas that lack credible research for EU countries. Especially causal studies on minimum wage effects on the gender wage gap are scarce. Moreover, while minimum wages have been in place for many years in most countries, there are large blanks when it comes to estimations of effects on other gender gaps, be it in employment, part-time or pensions. These are routes that could be taken by future research.

Finally, I want to finish with some remarks on the wage floor in Germany. While this thesis is being written, the minimum wage research is still evolving. For example, there is an ongoing discussion on the question if and why predicted employment effects did not materialize. For example, Bruttel *et al.* (2019) conclude that the ex-post research refutes the ex-ante expectations of enormous job losses and attribute this – among other things – to a

distorted interpretation of the (inter-)national evidence, too simple modelling and unrealistic setting of parameters. Heise and Pusch (2021) even coin the term the 'Waterloo of labour market economics', since they argue that the accompanying research has disproved the large employment losses expected by opponents. However, Knabe *et al.* (2020) respond to this criticism by arguing that the estimated effects do in fact not differ substantially from the predicted effects once you account for non-compliance as well as decreases in working hours by employing full-time equivalents. This is to say, that the interpretation of minimum wage effects is still disputed, not only internationally, but also in Germany. In this sense, it is even more interesting that this dissertation is finished at a suspenseful time for minimum wage researchers. Not only did the EU start to pave the way for a European understanding of the relevance of minimum wages (European Commission, 2020), but the federal government of Germany has just announced to increase the minimum wage to €12 as of October 2022 (BMAS, 2022).[1] In doing so, it wants to increase the wage floor to reach 60% of the median wage in Germany, thereby also complying with EU proposals. However, this is done without including the minimum wage commission in the process, which had decided to increase the wage floor to €10.45 as of July 2022. Moreover, it is especially interesting since it coincides with the aftermath of the COVID-19 pandemic, in which minimum wage establishments have been shown to be particularly affected (Börschlein and Bossler, 2021). Accordingly, the minimum wage debate has been reignited, with both proponents and critics putting forward their arguments again. Proponents argue that such an increase is necessary in order to ensure fair pensions, that it will lead to substantial income increases among the low-paid, cause rising consumption and productivity (Krebs and Drechsel-Grau, 2021; Pusch and Schulten, 2019). Opponents argue, however, that the government's decision is an affront for the minimum wage commission and that the apparent pursuing of the concept of living wages constitutes a paradigm change that deviates from the idea that redistribution is achieved via the tax and transfer system (Lesch and Schröder, 2022). Moreover, it is found that it would mean a redistribution up to the middle class, which does nearly nothing against old-age poverty (Schröder and Kestermann, 2020). Additionally, Caliendo (2020) points out that even a minimum wage of €12 does not lead to a pension that reaches the basic income, which is why investing in

[1] Additionally, the government has announced that it will increase the threshold for mini-jobs to €520 in order to enable minimum wage employees to work up to ten hours a week (BMAS, 2022). However, Fedorets (2021) argues that this presents a potential conflict of goals. While the wage floor aims to improve the situation of low-wage workers, raising the mini-job threshold might lead to an increase in marginal employment, a working arrangement that does not offer prospects for upwards mobility.

human capital would be a more reasonable long-term goal. Moreover, while the government's decision was with no doubt related to the general understanding of comparatively small employment effects found in recent research endeavours, it is not clear how strongly an increase of this magnitude would change the situation. It is estimated that such a substantial wage floor increase would affect about 6 to 9 million employees (DGB, 2022; Pusch, 2021). Yet, Gürtzgen (2021) points out that predicting employment effects could still be complicated, since previous evidence cannot necessarily be seen as a preview for future effects. And even if, the COVID-19 pandemic has left the labour market in a different state than it was in in 2015. This is also in line with this dissertation, since – among many other things – it has shown that predicting minimum wage effects is difficult. It will be interesting to see whether the findings of this thesis will be confirmed in light of such a substantial increase in the wage floor and in a different economic situation.

Appendices

Appendix Chapter 2

Table B.1: More Detailed Information on Causal Studies Focusing on Minimum Wage Effects in Germany

No.	Author (Year)	Main Outcome	Main Method/ Identification	Main Data Source	Time Frame	Main Findings
1	Ahlfeldt *et al.* (2018)	employment, wages	DiD-R	IEB	2011-2016	more rapid increase of wages in low-wage regions compared to high-wage regions, no negative effect on employment, regions with higher share of low-wage workers experienced lower unemployment rates
2	Bellmann *et al.* (2017)	training	DiD-E	IAB-EP	2011-2015	small reduction of training intensity at affected firms, mostly driven by employer- financed training; reduction of training for medium- and high-skilled workers.
3	Bonin *et al.* (2018)	employment	DiD-R, DiD-I, DiD-E	FEA, IEB, SOEP, SES	2012-2016	significant but small effect on employment, mainly marginal employment reduced, no effect on regular employment, contractual hours decreased by 5%, decrease of actual hours (2.5%) not significant, no significant effect on unemployment
4	Bossler and Broszeit (2017)	satisfaction	DiD-I	LPP	2013-2015	positive effect on pay satisfaction, imprecisely negative for adversely affected individuals, no effect on work engagement and turnover intention
5	Bossler and Gerner (2016)	employment, wages	DiD-E	IAB-EP	2011-2015	mean wage rose by 4.8%; employment decreased by 1.9% (60,000 jobs), mainly due to reduced hiring, contractual working hours of full-time employees decreased by 0.2 hours (0.6 percent)

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No.	Author (Year)	Main Outcome	Main Method/ Identification	Main Data Source	Time Frame	Main Findings
6	Bossler *et al.* (2018)	employment, self-employment, firm-level adaptations	DiD-E	IAB-EP	2015-2016	average negative effect on employment of about 1.7%, amounting to 46,000 to 59,000 jobs, mainly driven by reduced hirings, no strong increase of freelancers, decrease of expected business volume, higher probability for deficits for affected firms reduced firm profitability largely driven by increased wage costs, no effect on productivity, competitive pressure, investments into human and and physical capital..
7	Bossler and Hohendanner (2016)	self-employment	DiD-E	IAB-EP	2013-2015	no implication that employment of freelancers is used to circumvent the minimum wage
8	Bossler and Wegmann (2019)	internships	DiD-E, DiD-R	IAB-EP, Google search data	2011-2017	no reduction in the number of internships or the number of searches for internship positions, reduction in Google search for 'generation internship'
9	Bruckmeier and Becker (2018)	benefit recipients, poverty	DiD-I	FEA, PASS	2014-2016	more regular employment among working benefit recipients, no significant effect on poverty reduction, welfare recipients transition from marginal to regular employment
10	Burauel *et al.* (2018)	wages	DiD-I	SOEP	2010-2016	Wage growth of affected workers increased by 6.5 % (€0.5) until 2016, no spill-over effects
11	Caliendo *et al.* (2017)	wages, hours	DiD-R	SOEP	2012-2015	positive wage effect for low-wage earners; stronger for contractual than actual hourly wages; negative effect on hours, resulting in constant monthly wages, non-compliance

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Table B.1 – continued from previous page.

No.	Author (Year)	Main Outcome	Main Method/ Identification	Main Data Source	Time Frame	Main Findings
12	Caliendo *et al.* (2018a)	employment	DiD-R	FEA, SOEP, SES	2012-2015	slight decrease of overall employment (0.4%), mainly driven by marginal employment, only weakly significant small decrease of regular employment, evidence for transitions
13	Caliendo *et al.* (2018b)	wages	DiD-R	SOEP	2014-2016	contractual wages increased in lowest decile by 6 % between 2014 and 2015, monthly wages increased descriptively but this cannot be traced back to minimum wage, actual and contractual hours decreased
14	Fedorets *et al.* (2018)	reservation wages	DiD-R	SOEP	2013-2015	increase in reservation wages of 4% at low end of distribution, corresponds with increase of observed wages
15	Garloff (2018)	(un-)employment	DiD-I/R	FEA	2013-2015	positive relationship between bite and growth of regular employment, stronger negative relationship for marginal employment, potentially caused by transitions, small positive effect on overall employment not stable, faster unemployment growth in highly affect cells, but also not stable
16	Gülal and Ayaita (2018)	satisfaction (life, job, pay)	DiD-I	SOEP	2012-2016	life satisfaction increased by 0.1 standard deviations, largely driven by East Germany, effect remains when considering employment losses
17	Holtemöller and Pohle (2017)	employment	idiosyncratic trend estimation	FEA	2010-2015	negative effect on marginal employment and a positive effect on regular employment; no evidence for transitions of mini-jobs to regular employment

Table continued on next page.

Table B.1 – continued from previous page.

No.	Author (Year)	Main Outcome	Main Method/ Identification	Main Data Source	Time Frame	Main Findings
18	Link (2018)	employment, prices	DiD-E	IBS	2010-2015	insignificant employment effect, affected firms increased prices more frequently, magnitude of the price effect is quantitatively large (overall level of producer prices increased by about 0.2%)
19	Pusch and Rehm (2017)	satisfaction	DiD-I, matching	PASS	2014-2015	affected workers' satisfaction with wage, overall work and compatibility of family and career increased while work amount increased
20	Schmitz (2017)	employment, welfare-dependency	DiD-R	FEA	2012-2015	negative effect on marginal employment, indication for slight reduction of regular employment, number of working welfare recipients reduced

Note: This table presents the causal studies on minimum wage effects and summarizes outcomes studied, methods and data sources used as well as key findings. The abbreviations entailed in the table represent: DiD-E = Establishment-Level Difference-in-Difference (DiD), DiD-I = Individual DiD, DiD-R = Regional DiD. Abbreviations of data sources are explained in Table 2.2.

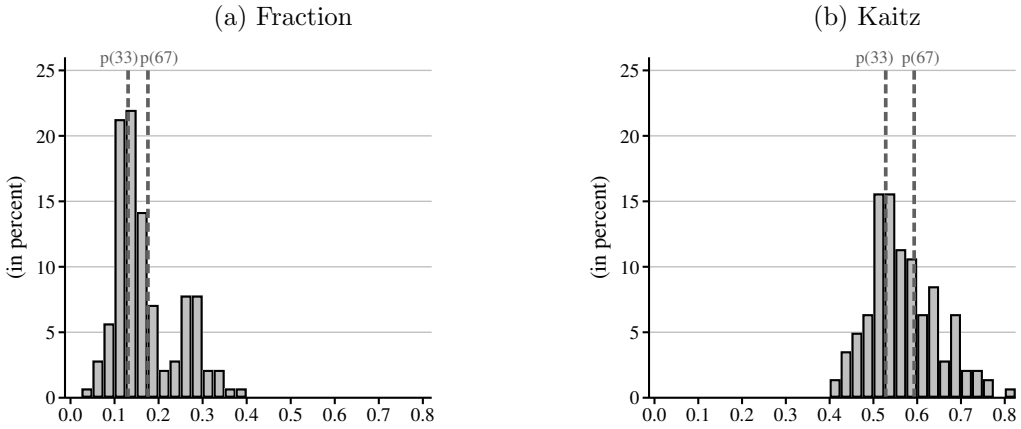
Appendix Chapter 3

Table C.1: Employment Effects on Mini-Jobs (log Employment)

	Fraction				Kaitz	
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Sole Mini-Jobs					
Bite × D2015	-0.232***	-0.190***	-0.186***		-0.120***	-0.122***
	(0.022)	(0.032)	(0.028)		(0.027)	(0.023)
Bite × D2013			0.035*			0.008
			(0.021)			(0.018)
GDP (log, t-1)		0.055	0.025		0.067	0.013
		(0.080)	(0.052)		(0.085)	(0.054)
Population (log, t)		0.592*	0.661***		1.009***	0.972***
		(0.325)	(0.231)		(0.329)	(0.225)
Constant	10.068***	2.191	1.611		-3.124	-2.151
	(0.001)	(4.179)	(2.861)		(4.257)	(2.842)
Region FE	Yes	Yes	Yes		Yes	Yes
Year FE	Yes	Yes	Yes		Yes	Yes
Observations	282	282	423		282	423
R^2 within	0.876	0.882	0.835		0.856	0.808
R^2 between	0.254	0.946	0.943		0.943	0.941
R^2 overall	0.044	0.946	0.943		0.943	0.941
	Panel B: Add-On Mini-Jobs					
Bite × D2015	-0.052**	-0.027	-0.033		-0.002	-0.003
	(0.024)	(0.028)	(0.026)		(0.026)	(0.024)
Bite × D2013			-0.037			-0.069***
			(0.024)			(0.021)
GDP (log, t-1)		0.165**	0.163**		0.163**	0.144**
		(0.064)	(0.070)		(0.065)	(0.067)
Population (log, t)		0.332	0.235		0.505*	0.499**
		(0.273)	(0.211)		(0.276)	(0.209)
Constant	9.257***	3.585	4.805**		1.442	1.694
	(0.001)	(3.263)	(2.368)		(3.288)	(2.389)
Region FE	Yes	Yes	Yes		Yes	Yes
Year FE	Yes	Yes	Yes		Yes	Yes
Observations	282	282	423		282	423
R^2 within	0.666	0.687	0.880		0.684	0.883
R^2 between	0.423	0.863	0.869		0.856	0.855
R^2 overall	0.015	0.863	0.868		0.856	0.855

Source: SES 2014, Destatis 2012-2015, FEA 2012-2015, own calculations.
Note: Robust standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Dependent variables are primary mini-jobs in Panel A and secondary mini-jobs in Panel B, each in logarithmic terms, annually measured on June 30th. Bite measure is denoted by the first row. Reference year in all specifications 2014.

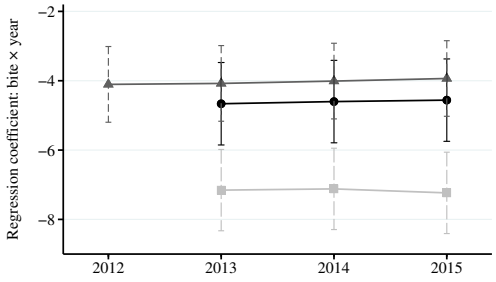
Figure C.1: Fraction and Kaitz Distribution



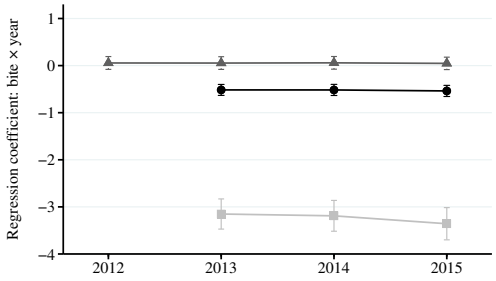
Source: SES 2014, own calculations.
Notes: Band width set to 0.025. $p(33)$ and $p(67)$ denote the 33th and 67th percentile of the distribution.

Figure C.2: Correlation of Bite and Logarithmic Employment Level by Year

(a) No Additional Controls

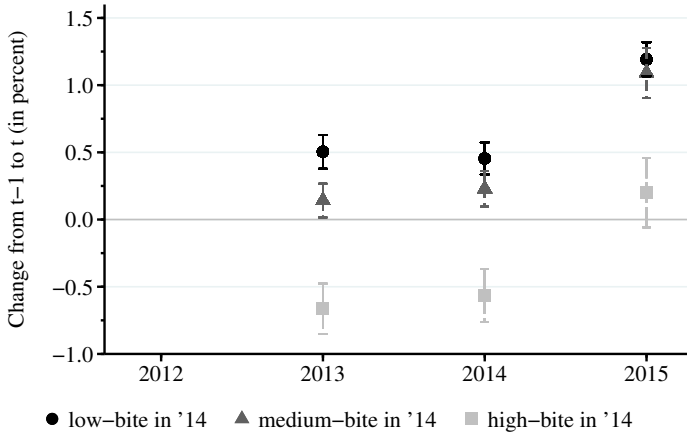


(b) Time Fixed Effects and Logarithmic Population as Controls



Source: SES 2014, FEA 2012-2015, Destatis 2012-2015, own calculations.
Notes: Figure C.2a presents the coefficients of a regression of the logarithmic employment level on the bite interacted with year dummies. Figure C.2b adds time fixed effects and logarithmic population level as controls to this regression. Whiskers represent the corresponding 95 percent confidence interval based on robust standard errors. Information on marginal employment is only available from 2013 onwards. Bite is defined as Fraction. Employment levels are measured at the end of the second quarter (June 30th).

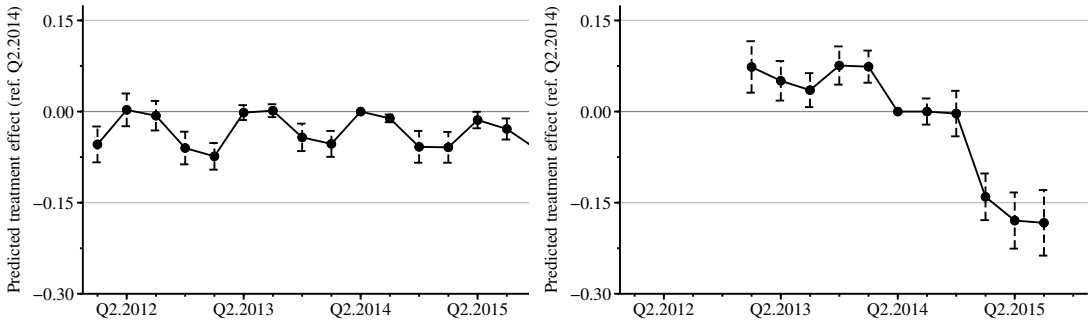
Figure C.3: Average Population Change within High-, Mid-, and Low-Bite Regions



Source: SES 2014, FEA 2012-2015, Destatis 2012-2015, own calculations.
Notes: Each point represents the average log level in population. Whiskers represent the corresponding 95 percent confidence interval. Each region has been sorted into the groups by its level of *Fraction* in 2014.

Figure C.4: Predicted Effect of Fraction on Employment by Quarter

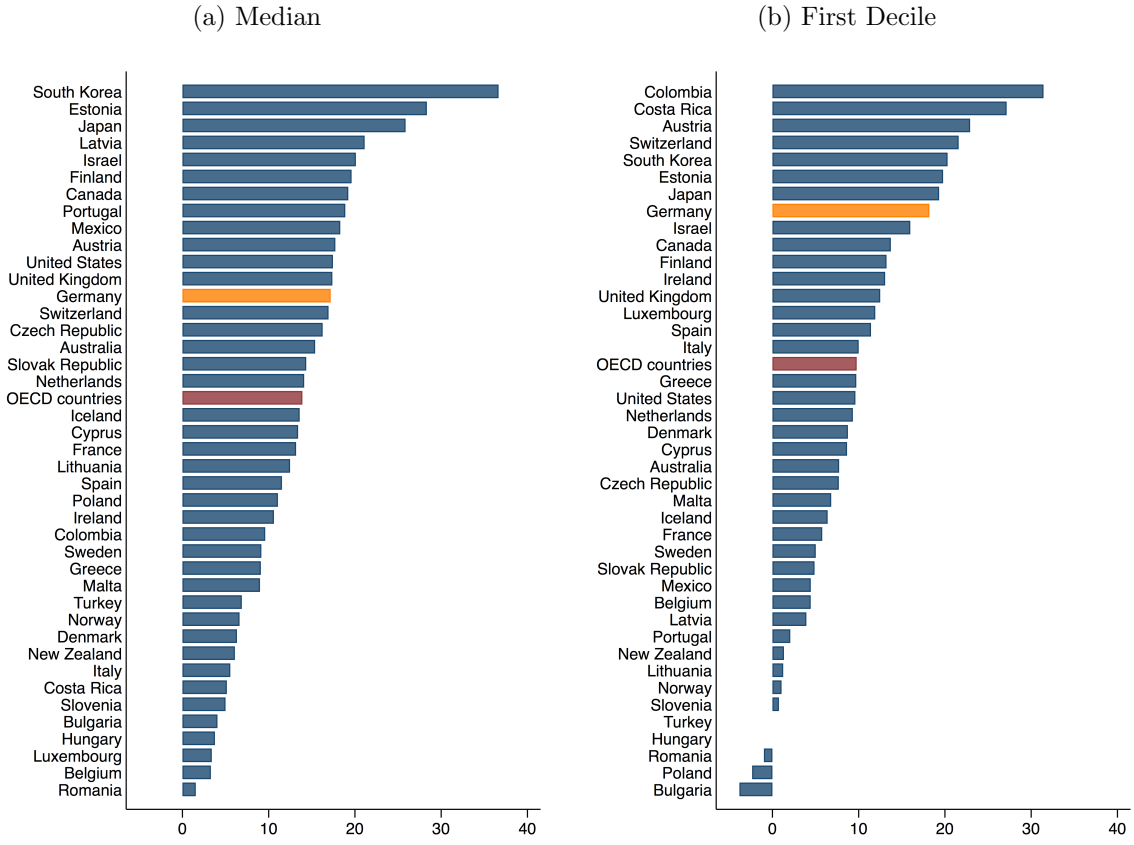
(a) Regular Employment (b) Mini-Jobs



Source: FEA 2012-2015.
Note: Employment level in logarithmic terms as dependent variable. Predicted treatment coefficient with Q2 in 2014 as reference point. Whiskers denote the 95% confidence interval. Population level (in logarithmic terms) included as control variable. Full estimation results are available upon request from the authors in a Supplementary Appendix.

Appendix Chapter 4

Figure D.1: Unadjusted Gender Wage Gaps Across Different OECD Countries in 2014, in %



Source: OECD (2020), Gender wage gap (indicator). doi: 10.1787/7cee77aa-en (Accessed on 22 September 2020).
Note: The gender wage gap is unadjusted and is defined as the difference between earnings of full-time employed men and full-time employed women relative to earnings of full-time employed men. Wages are measured at the median (Figure D.1a) and the tenth percentile (Figure D.1b).

Table D.1: Descriptive Statistics on Wages of Subsample 2010-2018

	2010			2014			2018		
	Men	Women	Gap (%)	Men	Women	Gap (%)	Men	Women	Gap (%)
Share earning									
<8.50 (%)	9.95	19.44		6.25	11.85		0.54	0.71	
<8.84 (%)	11.34	22.02		7.91	14.65		1.76	2.62	
Mean	19.16	14.43	24.67	20.54	15.64	23.88	22.35	17.55	21.50
SD	12.84	7.67		14.33	8.45		16.48	9.19	
p10	8.50	7.48	11.95	9.26	8.20	11.50	10.30	9.55	7.28
p25	11.65	9.21	20.94	12.36	9.94	19.58	13.41	11.14	16.93
p50	16.20	13.13	18.96	17.24	14.02	18.71	18.56	15.53	16.32
p90	32.65	22.75	30.32	35.17	24.82	29.44	38.29	27.68	27.70
N	892,994	612,996		345,701	265,859		328,844	244,403	

Source: SES 2010, 2014 and 2018, own calculations.
Note: We only include eligible employees that are not employed in the public service. The subsample is identified via the variable *GG2010*, which is provided by the federal statistical office and which adapts the 2014 and 2018 data to the structure of the 2010 wave (FDZ, 2019).

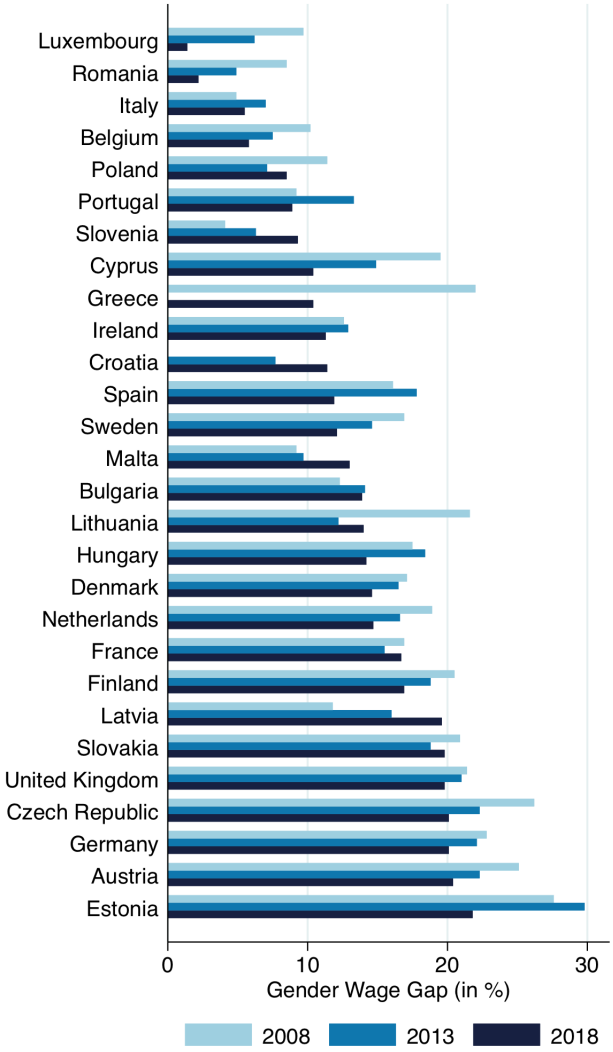
Table D.2: Sensitivity Analyses: Fixed Effects Regressions of Wage Gaps at 10^{th} Percentile, 25^{th} Percentile and Mean

	A: Main Results			B: Bite for All		
	p10	p25	Mean	p10	p25	Mean
Bite x 2018	-4.550***	-3.336***	-2.297**	-2.617***	-2.272**	-1.792*
2018	-3.754	-4.079	-6.581**	-4.000	-4.084	-6.471**
Controls	yes	yes	yes	yes	yes	yes
Observations	514	514	514	514	514	514
Groups	257	257	257	257	257	257
	C: Continuous Bite			D: No MW Sectors		
	p10	p25	Mean	p10	p25	Mean
Bite x 2018	-0.333***	-0.277***	-0.183***	-4.207***	-3.051**	-3.323***
2018	2.016	0.877	-3.336	-3.598	-6.162*	-7.601**
Controls	yes	yes	yes	yes	yes	yes
Observations	514	514	514	514	514	514
Groups	257	257	257	257	257	257
	E: ROR			F: Districts		
	p10	p25	Mean	p10	p25	Mean
Bite x 2018	-3.730***	-2.185*	0.133	-3.951***	-3.196***	-1.286*
2018	-1.881	0.113	0.461	-0.739	-3.612	-3.275
Controls	yes	yes	yes	yes	yes	yes
Observations	192	192	192	802	802	802
Groups	96	96	96	401	401	401
	G: No Individual Weights			H: Regression Weights		
	p10	p25	Mean	p10	p25	Mean
Bite x 2018	-4.577***	-2.267**	-1.466*	-4.868***	-2.828***	-1.413*
2018	-3.597*	-2.361	-4.486*	-0.782	-0.362	-2.578
Controls	yes	yes	yes	yes	yes	yes
Observations	514	514	514	514	514	514
Groups	257	257	257	257	257	257

Source: SES 2014 and 2018, INKAR; own calculations.
Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The table displays the results of fixed-effects estimations in a difference-in-difference framework with region-fixed effects and robust standard errors. Control variables are included as in Table 4.2 but not reported. In the first column of each panel the dependent variable is the unadjusted wage gap at the 10^{th} percentile of the regional gender-specific wage distribution. Accordingly, in the second (third) column the dependent variable is the gap at the 25^{th} percentile (the mean). The reference year for all estimations is 2014. We adapt them main specification as follows: In Panel A we display the main results from Table 4.2 for comparison. In Panel B employ the minimum wage bite for all employees, irrespective of their gender. In Panel C we use a continuous fraction rather than a binary one. In Panel D we exclude individuals working in sectors with sector-specific minimum wage agreements. Panel E and F do not rely on Labour Market Regions but Planning Regions (E) and Districts (F). In Panel G we do not use individual weights for calculating the bite and the regional wage distribution. In Panel H we weight the regions in the regression using the absolute regional employment as weights.

Appendix Chapter 5

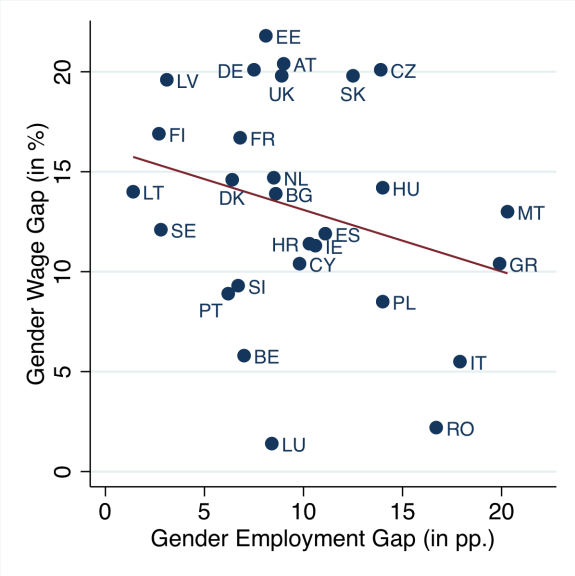
Figure E.1: Unadjusted Gender Wage Gaps in EU-28 Countries in 2008, 2013 and 2018



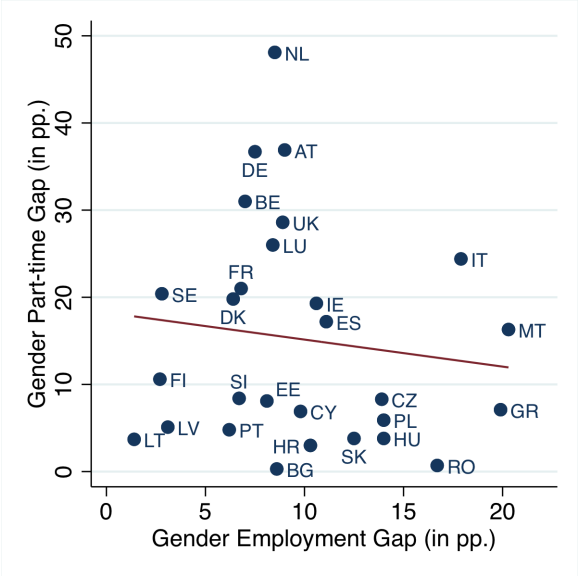
Source: Eurostat (last accessed 16 December 2021).
Note: Data displays the unadjusted wage gap, measured as the difference between average gross hourly earnings of male paid employees and of female paid employees as a percentage of average gross hourly earnings of male paid employees.

Figure E.2: Scatter Plots of Gender Equality Measures in EU-28 Countries in 2018

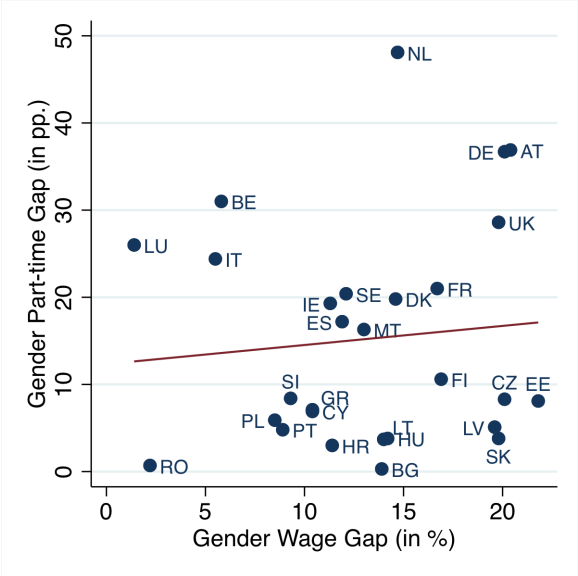
(a) Gender Wage and Employment Gaps



(b) Gender Part-Time and Employment Gaps



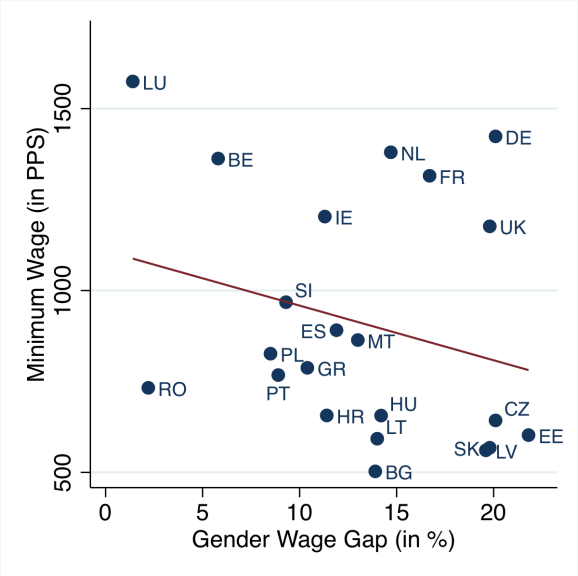
(c) Gender Part-Time and Wage Gaps



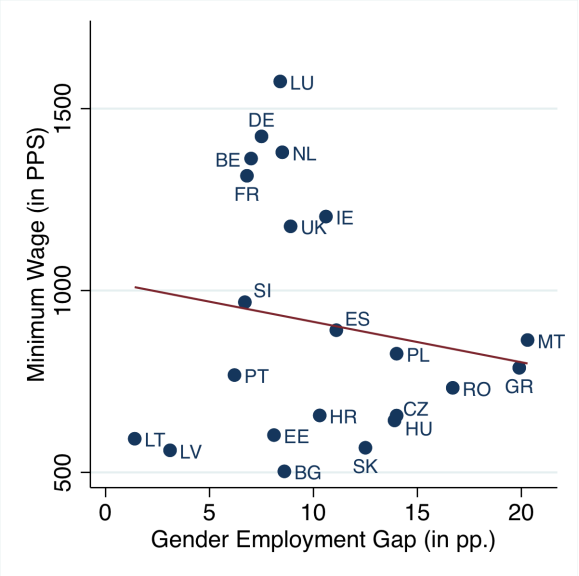
Source: Eurostat (last accessed 16 December 2021).
Note: *Gender Wage Gap*: Data displays the unadjusted wage gap, measured as the difference between average gross hourly earnings of male paid employees and of female paid employees as a percentage of average gross hourly earnings of male paid employees. *Gender Employment Gap:* Data shows the absolute difference between the male and the female employment rates for employees aged 15-64, measured in percentage points (pp). *Gender Part-Time Gap*: It shows the absolute difference between the female and male part-time employment rate (part-time employment as percentage of the total employment) for employees aged 15-64, measured in percentage points (pp).

Figure E.3: Scatter Plots of Minimum Wages with Gender Equality Measures in EU-28 Countries in 2018

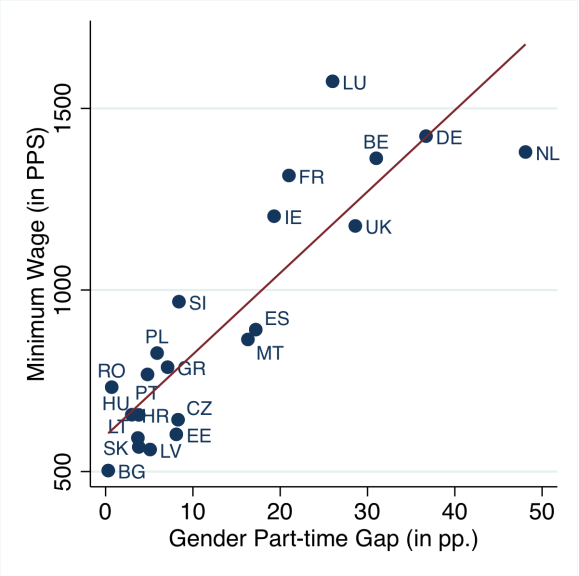
(a) Gender Wage Gap



(b) Gender Employment Gap



(c) Gender Part-Time Gap



Source: Eurostat (last accessed 16 December 2021).
Note: Minimum Wage is measured in Purchasing Power Standards as of January 2018. *Gender Wage Gap*: Data displays the unadjusted wage gap, measured as the difference between average gross hourly earnings of male paid employees and of female paid employees as a percentage of average gross hourly earnings of male paid employees. *Gender Employment Gap:* Data shows the absolute difference between the male and the female employment rates for employees aged 15-64. *Gender Part-Time Gap*: It shows the absolute difference between the female and male part-time employment rate (part-time employment as percentage of the total employment) for employees aged 15-64.

Table E.1: Studies Identifying the Relationship between Minimum Wages and Gender Wage Equality in EU-28 Countries

Author (Year)	Country	Data	Approach	Causal*	Effects found	Group	Female Equality
Bargain *et al.* (2019)	UK, Ireland	LII, BHPS 1998-2001	DR	-	no correlation for UK, neg. correlation for Ireland	A	=/+
Blau and Kahn (2003)	22 countries	ISSP 1985-1994	OLS	-	neg. correlation MW and GWG	A	+
Butcher *et al.* (2012)	UK	NES 1975-2010	panel models	-	pos. wage effect on women larger than on men	A	+
Caliendo and Wittbrodt (2021)	Germany	SES 2010, 2014, 2018	regional DiD	✓	neg. effect on GWG	A	+
Cerejeira *et al.* (2012)	Portugal	QP 1995-2007	DiD	✓	pos. effect on GWG	Y	−
Ferraro *et al.* (2018b)	Estonia	LFS 2001–2014	FE	-	pos. wage effect on women larger than on men	FT	+
Goraus-Tańska and Lewandowski (2019)	10 CEE states	EU-SILC 2003-2012	probit	-	women more affected by non-compliance	FT	−
Grimshaw and Rubery (2013)	16 countries	EU-SILC, OECD, SES, 2000-2010	correlation	-	neg. correlation MW and gender gap in incidence of low pay	FT	+
Huertas *et al.* (2017)	Spain	SES 2002, 2006, 2010	FE	-	neg. correlation MW and GWG	A	+
Matteazzi *et al.* (2018)	11 countries	EU-SILC 2009	correlation	-	neg. correlation MW and GWG	A	+
Majchrowska and Strawinski (2018)	Poland	SES 2006, 2010	decomposition/ counterf. distrib.	-	neg. correlation MW and GWG	A	+
Robinson (2002)	UK	LFS 1995-2000	OLS, QR	-	no correlation	A	=
Robinson (2005)	UK	LFS 1993-2000	regional DiD	✓	neg. effect on GWG	A	+
Schäfer and Gottschall (2015)	25 European countries	EU-SILC 2001-2011	random intercept model	-	neg. correlation MW and GWG	FT	+

Source: Own summary. Note: This table presents studies that examine the relationship between minimum wages and gender wage equality for EU-28 countries. It summarizes key information such as the used methodological approach or the key findings.The last column shows the own interpretation of the association between a minimum wage and female equality. The abbreviations entailed in the table represent:
DiD=Difference-in-Difference, DR=Distribution Regression, FE=Fixed Effects, OLS=Ordinary Least Squares, QR=Quantile Regression
MW=Minimum Wage, GWG= Gender Wage Gap
A=All Employees, FT= Full-time Employees, Y= Young Employees.
* Studies are identified as causal when they estimate a causal relationship on gender wage equality directly.

Table E.2: Studies Identifying the Gender-Specific Relationship between Minimum Wages and Employment

Author (Year)	Country	Data	Approach	Causal	Effects found	Group	Female Equality
Abowd *et al.* (2000)	France	LFS, 1990-1998	DiD	✓	A: neg. effect for both but men more affected	A	+
Aitken *et al.* (2019)	UK	ASHE, 2007-2017	DiD	✓	FT: no effect for both, PT: neg. effect for women	FT/PT	= /−
Baranowska-Rataj and Magda (2015)	Poland	LFS, 2003-2011	DiD	✓	A: neg. effect for both	A (18-29y)	=
Capuano *et al.* (2019)	UK	LFS, 2010-2018	DiD	✓	FT: no effect for both, PT: neg. effect for both	FT/PT	=/=
Dickens *et al.* (2009)	UK	ASHE, LFS 2001-2006	DiD	✓	A: no effect for both	A	=
Dickens *et al.* (2014)	UK	LFS 1999-2009	RD	✓	Y: pos. effect for both	Y	=
Dickens *et al.* (2015)	UK	NES, LFS 1994-2010	DiD	✓	FT: no effect for both, PT: neg. effect for women (men not estimated, possibly same)	FT/PT	= / (=)
Ferraro *et al.* (2018a)	Estonia	LFS, 2013–2016	DiD	✓	FT: no/small neg. effect for both	FT	=
Fialová and Mysíková (2021)	CZ and SK	EU-SILC 2005-2017	FE	✓	FT: no effect for both	FT	=
Kamińska and Lewandowski (2015)	Poland	LFS, 2001-2013	DiD	✓	P: neg. effect for both T: stronger neg. effect for women	P / T	= / −
Kabatek (2015)	Netherlands	admin. data, 2006-2012	employment duration analysis	✓	Y: neg. effect for both	Y	=
López Novella (2018)	Belgium	admin. data, 2010-2015	DiD	✓	Y: men more neg. affected	Y	+
Pestel *et al.* (2020)	Germany	SES, ES, FEA, 2014-2017	regional DiD	✓	A: small neg. effect for both M: larger neg. effect for both	A / M	= / =
Stewart (2002)	UK	NES, LFS 1998-1999	regional DiD	✓	A: no effect for both	A	=
Stewart (2004)	UK	NES, LFS, BHPS 1994-2000	DiD	✓	FT: no effect for both	FT	=

Source: Own summary. Note: This table presents studies that examine gender-specific minimum wage effects on employment for EU-28 countries. It summarizes key information such as the used methodological approach or the key findings. Only significant effects are displayed. The last column shows the own interpretation of the effect on female equality. The abbreviations entailed in the table represent: DiD=Difference-in-Difference, FE=Fixed Effects, RD=Regression Discontinuity A=All Employees, FT=Full-time Employees, M=Marginally Employed, P=Permanently Employed, PT=Part-time Employees, T=Temporarily Employed, Y=Young Employees.

Table E.3: Studies Identifying the Gender-Specific Relationship between Minimum Wages and Working Hours

Author (Year)	Country	Data	Approach	Causal	Effects found	Group	Female Equality
Aitken *et al.* (2019)	UK	ASHE, 2007-2018	DiD	✓	FT: no effect for both PT: no effect for both	FT/PT	=/=
Baranowska-Rataj and Magda (2015)	Poland	LFS, 2003-2011	DiD	✓	A: pos. effect for men, no effect for women	A (18-29y)	–
Burauel *et al.* (2020a)	Germany	SOEP, 2012-2016	DiD	✓	A: neg. effect on women, men unclear	A	unclear
Capuano *et al.* (2019)	UK	LFS, 2010-2018	DiD	✓	FT: no effect for both (volatile neg. effect for men) PT: no effect for both	FT/PT	=/=
Connolly and Gregory (2002)	UK	NES, BHPS, 1994-2001	DiD	✓	FT: no effect for both PT: no effect for both	FT/PT	= /=
Dickens *et al.* (2009)	UK	ASHE, LFS 2001-2006	DiD	✓	A: no effect for both (volatile neg. effect for men)	A	=
Stewart and Swaffield (2008)	UK	NES, LFS 1994-2000	DiD	✓	A: neg. effect for both	A	=

Source: Own summary. Note: This table presents studies that examine gender-specific minimum wage effects on working hours for EU-28 countries. It summarizes key information such as the used methodological approach or the key findings. Only significant effects are displayed. The last column shows the own interpretation of the effect on female equality. The abbreviations entailed in the table represent: DiD=Difference-in-Difference, A=All Employees, FT=Full-Time Employees, PT=Part-time Employees, Y=Young Employees.

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List of Acronyms

ASHE	Annual Survey of Hours and Earnings
BHPS	British Household Panel Survey
CDU	Christian Democratic Union
CEE	Central and Eastern European
CSU	Christian Social Union
DiD	Difference-in-Difference
DiD-E	Establishment-Level Difference-in-Difference
DiD-I	Individual Difference-in-Difference
DiD-R	Regional Difference-in-Difference
ES	Earnings Survey
EU	European Union
EU-SILC	European Union Statistics on Income and Living Conditions
FDP	Free Democratic Party
FEA	Federal Employment Agency
IAB	Institute for Employment Research
IAB-EP	IAB Establishment Panel
IBS	ifo Business Survey
IEB	Integrated Employment Biographies
ISSP	International Social Survey Programme
LFS	Labour Force Survey
LII	Living in Ireland survey

LPP Linked Personnel Panel

MiLoG Mindestlohngesetz

NES New Earnings Survey

NLW National Living Wage

NMW National Minimum Wage

OECD Organisation for Economic Co-operation and Development

PASS Panel Study Labour Market and Social Security

pp percentage points

PPS Purchasing Power Standards

QP Quadros de Pessoal

SES Structure of Earnings Survey

SOEP Socio-economic Panel

SPD Social Democratic Party

RLM Regional Labour Market

UK United Kingdom

US United States of America

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German Summary

Am 1. Januar 2015 wurde in Deutschland ein allgemeiner gesetzlicher Mindestlohn in Höhe von 8,50€ brutto pro Stunde eingeführt. Die Effekte, die sich daraus potenziell für die Beschäftigten sowie für die Wirtschaft als Ganzes ergeben würden, wurden im Vorfeld der Reform stark diskutiert. Diese Dissertation widmet sich den Auswirkungen der Mindestlohneinführung in Deutschland sowie Lohnuntergrenzen im Europäischen Kontext. Damit trägt sie zur nationalen und internationalen Forschung bei. Die wissenschaftliche Debatte über die Effekte von Mindestlöhnen wird schon seit Jahrzehnten geführt und ist dennoch nicht abgeschlossen (Manning, 2021; Neumark, 2019). Insbesondere die Auswirkungen auf die Beschäftigung sind umstritten. So zeigen bisherige Studien aus den Vereinigten Staaten von Amerika uneinheitliche Ergebnisse, die etwa auf Unterschiede in den Identifizierungsmethoden, Datengrundlagen, Ergebnisgrößen, Untersuchungsgegenständen sowie Zeiträumen zurückgeführt werden können. Die meisten Untersuchungen finden jedoch keine oder höchstens geringe negative Beschäftigungseffekte (Doucouliagos and Stanley, 2009; Dube, 2019; Wolfson and Belman, 2019). Ähnliches ist für eine Reihe von Studien aus dem Vereinigten Königreich festzustellen, sowie für Literatur aus anderen Ländern.[1] Es gibt jedoch auch Hinweise auf stärkere negative beziehungsweise unklare Auswirkungen.[2]

Deutschland ist gemessen an seinem Bruttoinlandsprodukt die größte europäische Volkswirtschaft[3]. Daher ergänzt die in dieser Dissertation vorgenommene Analyse des deutschen Mindestlohns die bestehende Literatur um wichtige Einblicke in die Wirkungen einer Lohnuntergrenze, die einerseits auf einem vergleichsweise hohen Niveau eingeführt wurde und anderseits für eine große Anzahl an Beschäftigten bindend war. Die Betrachtung der geschlechtsspez-

[1] Für Literatur zum Vereinigten Königreich siehe beispielsweise Dolton *et al.* (2010, 2012, 2015); Metcalf (2008); Manning (2013, 2021); Stewart (2004). Für andere Länder siehe Broecke *et al.* (2017); Chletsos and Giotis (2015); de Linde Leonard *et al.* (2014); Dolado *et al.* (1996); Harasztosi and Lindner (2019); Jiménez Martínez and Jiménez Martínez (2021).

[2] Siehe etwa Boockmann (2010); Nataraj *et al.* (2014); Neumark and Shirley (2021).

[3] Quelle: Eurostat.

ifische Auswirkungen von Mindestlöhnen innerhalb der Europäischen Union ermöglicht zudem eine Erweiterung des Forschungsblicks auf einen länderübergreifenden Kontext. Darüber hinaus leistet die vorliegende Arbeit einen weiteren Beitrag zur Forschung, in dem sie sich einem zusätzlichen, ebenfalls politikrelevanten Thema widmet: der Ungleichheit von Frauen auf dem Arbeitsmarkt. So wird sowohl der Einfluss des deutschen Mindestlohns auf die geschlechtsspezifische Lohnlücke analysiert, als auch heterogene Mindestlohnauswirkungen auf Männer und Frauen innerhalb der Europäischen Union resümiert.

Diese Arbeit besteht aus vier Hauptkapiteln, die jeweils eigenständige wissenschaftliche Studien darstellen und teilweise bereits in Fachjournalen veröffentlicht wurden. Kapitel 2, 3 und 4 wurden jeweils in Zusammenarbeit mit Koautoren verfasst, das fünfte Kapitel entstand in Einzelautorschaft. Kapitel 2 dieser Arbeit stellt einen wichtigen Beitrag für die Mindestlohnforschung in Deutschland dar, da es die in bisherigen Studien herausgearbeiteten kurzfristigen Effekte der Mindestlohnreform in einem Überblick zusammenfasst. Dabei konzentrieren wir uns auf die Evidenz der ersten drei Jahre nach der Einführung. Da die Reform im Vorfeld kontrovers diskutiert wurde, waren die ersten Schätzungen der Wirkungen von großem wissenschaftlichen sowie politischen Interesse. Zudem ist die Analyse der deutschen Lohnuntergrenze aufgrund ihrer Höhe und des daraus resultierenden hohen Anteils an betroffenen Beschäftigten auch international von Bedeutung. In diesem Kapitel geben wir zunächst einen Überblick über die konkrete Ausgestaltung des deutschen Mindestlohns sowie seine Umsetzung und diskutieren kurz potenziell betroffene Ergebnisgrößen. Darüber hinaus werden die Datenquellen vorgestellt, welche sich am geeignetsten für die Mindestlohnevaluation in Deutschland erwiesen haben und ihre Stärken und Schwächen kurz präsentiert. Schließlich nehmen wir eine systematische Zusammenfassung der bisherigen kausalen Effekte auf eine Vielzahl von Ergebnisgrößen vor, bei der zwischen signifikant positiven oder negativen sowie insignifikanten Wirkungen unterschieden wird. Es zeigt sich, dass die Mindestlohneinführung einen deutlich positiven Effekt auf die Löhne am unteren Ende der Verteilung hatte. Bestimmte Subgruppen haben dabei besonders profitiert, etwa geringfügig Beschäftigte, Frauen sowie Arbeitnehmer:innen mit Migrationshintergrund oder Geringqualifizierte. Allerdings wies kurz nach der Reform noch ein nicht unerheblicher Anteil der Beschäftigungsverhältnisse Löhne unter €8,50 auf. Weiterhin deutet die Evidenz auf geringe negative Beschäftigungseffekte hin, welche durch eine Reduktion von Minijobs getrieben ist. Entgegen der Erwartungen konnten jedoch in der kurzen Frist keine Effekte auf

Armut und allgemeine Ungleichheit gefunden werden. Dies hängt insbesondere mit der Tatsache zusammen, dass Arbeitsstunden reduziert wurden und sich die Stundenlohnerhöhung daher nicht auf die Monatslöhne niederschlug.

Das dritte Kapitel befasst sich eingehender mit den Beschäftigungseffekten der Mindestlohnreform. Es geht der Frage nach, ob die im Vorfeld prognostizierten Arbeitsplatzverluste kurzfristig eingetreten sind und welche Art der Beschäftigung davon gegebenenfalls stärker betroffen war. Zur empirischen Identifikation der Effekte wird in diesem (sowie im vierten Kapitel) ein regionaler Differenzen-von-Differenzen-Ansatz verwendet. Dieser beruht auf der Tatsache, dass deutsche Regionen unterschiedlich stark von der Mindestlohneinführung betroffen waren, da sich die Löhne in den einzelnen Regionen teilweise erheblich unterschieden. Der universell eingeführte Mindestlohn hatte damit nicht überall die gleiche Eingriffstiefe. Darauf aufbauend gehen wir davon aus, dass stärker betroffene Regionen nach der Reform auch größere Mindestlohneffekte aufweisen sollten. Mit Hilfe der Verdienststrukturerhebung 2014 ermitteln wir zwei Indikatoren für die Eingriffstiefe: den *Kaitz-Index*, d.h. das Verhältnis des Mindestlohns zum regionalen Durchschnittslohn und die *Fraction*, d.h. der regionale Anteil der betroffenen Beschäftigten. Auf Basis des Sozio-oekonomischen Panel (SOEP) berechnen wir zudem die regionalen Trends vor der Reform, da unser Ansatz auf der Annahme beruht, dass sich die Löhne ohne die Reform in allen Regionen ähnlich entwickelt hätten. Damit leisten wir ebenfalls wichtige Beiträge zur Literatur. Erstens machen wir deutlich, dass sich künftige Forschung auf der regionalen Ebene in der Tat auf das vergleichsweise kleinere SOEP stützen kann. Zweitens zeigen wir, dass es keine nennenswerten Antizipationseffekte gab, was für zukünftige Differenzen-von-Differenzen-Methoden von Bedeutung ist. In unserer Analyse schätzen wir die Auswirkungen auf reguläre Beschäftigung (Teil- und Vollzeit) sowie Minijobs (letztere entweder als Haupt- oder Zusatztätigkeit) in den ersten sechs Monaten nach der Reform sowie auf die Gesamtbeschäftigung, die wir als Summe aus beidem definieren. Unsere Ergebnisse deuten darauf hin, dass der Mindestlohn die Gesamtbeschäftigung leicht reduziert hat, was im Wesentlichen auf einen Rückgang von Minijobs zurückzuführen ist. So ist die Gesamtbeschäftigung um etwa 0,4 Prozent bzw. etwa 140.000 Arbeitsplätze gesunken, geringfügige Beschäftigung reduzierte sich um etwa 3 Prozent bzw. etwa 190.000 Arbeitsplätze. Während diese Punktschätzungen mit Vorsicht zu betrachten sind, ist die Richtung der Effekte eindeutig und die Ergebnisse robust gegenüber einer Reihe von Sensitivitätstests. Betrachtungen über den Verbleib der geringfügigen Beschäftigung weisen jedoch darauf hin,

dass diese aufgrund der durch die Lohnerhöhung bedingten Überschreitung der Minijob-Grenze zu große Teilen in Teilzeitbeschäftigung umgewandelt wurden.

Das vierte Kapitel schließt methodisch an das vorige an. Seine Motivation ergibt sich aus der Beobachtung, dass Frauen unter den Niedriglohnempfänger:innen häufig überrepräsentiert sind. Ein Mindestlohn könnte sich daher auf die Lohnungleichheit zwischen den Geschlechtern auswirken, nämlich dann, wenn die Löhne der Frauen im Vergleich zu denen der Männer überproportional steigen. Zwar wurde dieser Zusammenhang bereits oft diskutiert, jedoch gibt es mindestens innerhalb Europas kaum kausale Belege dafür. Zudem kommt auch hier der Fakt zum Tragen, dass die Mindestlohneinführung durch die Eingriffstiefe und Menge an betroffenen Beschäftigten besonders forschungsrelevant ist. Die primäre Forschungsfrage in diesem Kapitel ist daher, ob der Mindestlohn tatsächlich zu einer Verringerung der geschlechterspezifischen Lohnlücke geführt hat. Dazu identifizieren wir die Effekte auf die Lohnlücke am 10. und 25. Perzentil sowie beim Mittelwert der zugrundeliegenden geschlechtsspezifischen Lohnverteilungen. Auch hier wird der regionale Differenzen-von-Differenzen-Ansatz sowie Daten der Verdienststrukturerhebung verwendet, hier jedoch im wesentlichen aus 2014 und 2018. Unsere Ergebnisse zeigen, dass – verglichen mit Regionen mit niedriger Eingriffstiefe – die geschlechtsspezifische Lohnlücke am 10. Perzentil für mindestlohnberechtigte Beschäftigte in Regionen mit hoher Eingriffstiefe um 4,6 Prozentpunkte gesunken ist. Wir schätzen, dass dies eine Reduktion um 32% im Vergleich zu 2014 bedeutet. Die Ergebnisse halten einer Reihe von Sensitivitätstests stand. Am 25. Perzentil und am Mittelwert sind die Auswirkungen geringer und nicht gleichermaßen robust.

Das fünfte Kapitel schließt inhaltlich an das vorangegangene an, indem es den geschlechterspezifischen Fokus auf die Mindestlohneffekte beibehält. Im Vergleich zum Rest der Dissertation ist es jedoch weniger auf Deutschland ausgerichtet, sondern weitet den Blick auf andere Länder der Europäischen Union (EU). Zwar ist die Gleichstellung von Frauen eines der Gründungsprinzipien der EU, allerdings sind die Mitgliedstaaten hinsichtlich der Erfüllung dieses Ziels sehr heterogen. Gemäß der für das vorangegangene Kapitel dargelegten Überlegungen, könnten Frauen potenziell besonders von einem Mindestlohn profitieren. Dementsprechend argumentiert auch die EU (2021), dass durch die Förderung von 'angemessenen' Mindestlöhnen innerhalb der EU auch die geschlechtsspezifische Lohnlücke verringert werden könnte. Eine entsprechende Grundlage für das Voranbringen solcher Lohnuntergrenzen wurde bereits gelegt (Europäische Kommission, 2020). Die Tatsache, dass Frauen potenziell

häufiger von Lohnerhöhungen profitieren, könnte jedoch auch bedeuten, dass sie dadurch auch öfter von Arbeitsplatzverlusten oder Arbeitszeitverkürzungen betroffen sind. Dieses Kapitel resümiert daher einerseits vorhandene Evidenz aus EU-Staaten, die sich auf den Zusammenhang zwischen Lohnuntergrenzen und der geschlechtsspezifischen Lohnlücke bezieht. Darüber hinaus enthält es eine systematische Zusammenfassung von Studien, die den Einfluss von Mindestlöhnen auf Beschäftigungsverluste oder Arbeitszeitveränderungen untersuchen, von denen insbesondere Frauen betroffen sind. Es zeigt sich, dass es sowohl hinsichtlich der Relevanz des Mindestlohns als auch der Gleichstellung der Geschlechter auf dem Arbeitsmarkt erhebliche Unterschiede zwischen den Mitgliedsstaaten gibt. Außerdem sind die einzelnen Dimensionen der Ungleichheit im Ländervergleich miteinander verknüpft, wobei ein Zusammenhang zwischen höheren Lohngefällen und höheren Beschäftigungsquoten von Frauen besteht. In Bezug auf den Mindestlohn zeigen sich Hinweise, dass höhere Lohnuntergrenzen mit einer geringeren geschlechtsspezifischen Lohnlücke verbunden sind. Hinsichtlich der Beschäftigung zeigt sich zwar nicht, dass Frauen per se größere Beschäftigungsverluste erleiden als Männer. Allerdings zeigen Studien, dass sich der Mindestlohn hier besonders auf Teilzeitbeschäftigte auswirkt. Da diese meist zu großen Teilen weiblich sind, scheinen Frauen doch öfter von Arbeitsplatzverlusten betroffen zu sein. Es ist daher nicht auszuschließen, dass der negative Zusammenhang zwischen dem Mindestlohn und dem geschlechtsspezifischen Lohngefälle mit den Arbeitsplatzverlusten dieser schlechter bezahlten Teilzeitbeschäftigten zusammenhängt. Diese spezifische Form der Arbeit sollte daher im Zusammenhang mit dem Mindestlohn besondere Beachtung finden. Von Anpassungen der Arbeitszeit waren Frauen jedoch offenbar nicht häufiger betroffen als Männer.

Insgesamt beschäftigt sich die vorliegende Dissertation mit Mindestlohneffekten in Deutschland und Europa. Dabei gibt sie einen allgemeinen Überblick über die Mindestlohneinführung in Deutschland und präsentiert detailliertere Analysen der spezifischen Auswirkungen der Lohnuntergrenze. Es hat sich gezeigt, dass der Mindestlohn die Löhne von Geringverdiener:innen deutlich erhöht hat, wenngleich er in der kurzen Frist nicht immer eingehalten wurde. Es wurden geringe Auswirkungen auf die Beschäftigung festgestellt, diese sind jedoch wesentlich auf eine Reduktion der geringfügigen Beschäftigung zurückzuführen. Erhoffte negative Effekte auf Armut konnten nicht festgestellt werden, was hauptsächlich mit der Verringerung der Arbeitszeit und dementsprechend stagnierenden Monatsverdiensten begründet werden kann. Dagegen hat sich das Lohngefälle zwischen den Geschlechtern durch

den Mindestlohn reduziert, insbesondere bei den Geringverdiener:innen. Ein Zusammenhang zwischen beiden Maßen konnte bereits für andere EU-Mitgliedsstaaten konstatiert werden. Allerdings deutet die bisherige empirische Evidenz auch darauf hin, dass (häufig weibliche) Teilzeitbeschäftigte durch Lohnuntergrenzen häufiger von Arbeitsplatzverlusten betroffen sind. Was Mindestlöhne für die Gleichstellung der Geschlechter auf dem Arbeitsmarkt als Ganzes bedeuten, muss also noch weiter erforscht werden.

Diese Seite enthält persönliche Daten und wurde aus der Arbeit entfernt.