

Caliendo, Marco; Schröder, Carsten; Wittbrodt, Linda

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

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

**Marco Caliendo**

*University of Potsdam, IZA, DIW and IAB*

**Carsten Schröder**

*SOEP at DIW and Free University Berlin*

**Linda Wittbrodt**

*University of Potsdam*

DECEMBER 2018

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## ABSTRACT

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

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.

**JEL Classification:** J22, J23, J31, J38

**Keywords:** minimum wage, evaluation, earnings, working hours, employment

**Corresponding author:**

Marco Caliendo  
University of Potsdam  
Chair of Empirical Economics  
August-Bebel-Str. 89  
14482 Potsdam  
Germany  
E-mail: [caliendo@uni-potsdam.de](mailto:caliendo@uni-potsdam.de)

# 1 Introduction

In January 2015, Germany introduced a major labour market intervention: For the first time, a nationwide statutory minimum wage was implemented.<sup>1</sup> 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.<sup>2</sup> 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, 2018), 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

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<sup>1</sup>Previously, individual sector specific minimum wage agreements existed for specific industries.

<sup>2</sup>Different data sources and estimation strategies lead to differing numbers; see Amlinger *et al.* (2016); Brenke (2014); Destatis (2016); 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.*, 2014; 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).<sup>3</sup>

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.<sup>4</sup> 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

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

<sup>4</sup>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.*, 2016a), facility cleaning services (Bosch *et al.*, 2011b), care sector (Boockmann *et al.*, 2016b), painters and varnishers (Boockmann *et al.*, 2016c), 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 focuses on the minimum wage legislation and possible impact channels for various outcomes. Section 3 discusses the information content of existing data sources, implementation and measurement issues, as well as descriptive evidence. Section 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 5 sums up, discussing the results and outlining avenues for future research.

## 2 Minimum Wage Legislation, Potential Effects and Outcome Variables of Interest

### 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.<sup>5</sup> 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).<sup>6</sup> 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,<sup>7</sup> 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,

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<sup>5</sup>MiLoG, <https://www.gesetze-im-internet.de/milog/>, last accessed on December 14, 2017.

<sup>6</sup>For an overview of the sectors see WSI Tarifarchiv.

<sup>7</sup>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.<sup>8</sup>

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.

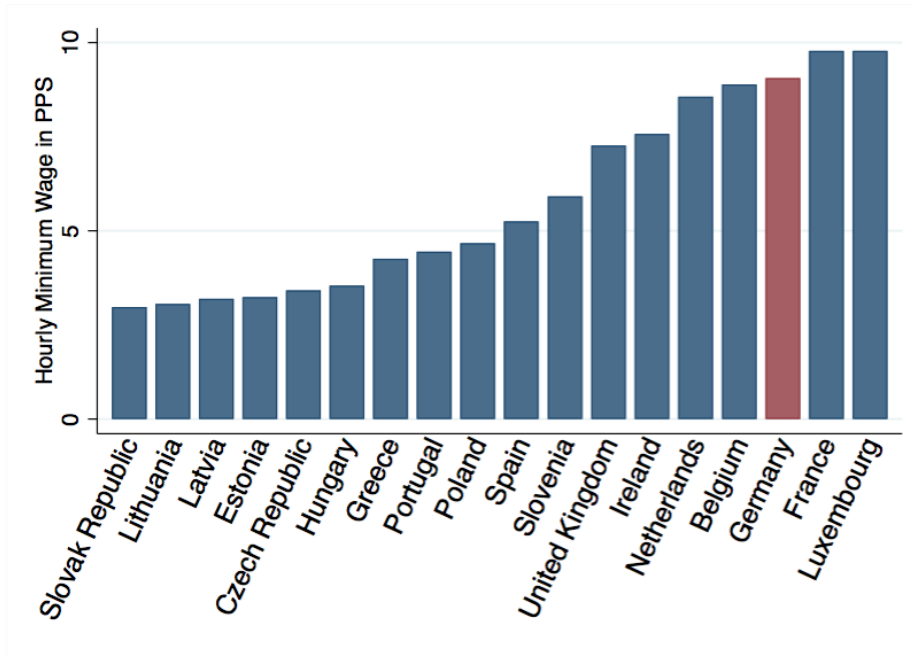
The German minimum wage was introduced at a comparatively high level, despite the fact that there was no experience with a nationwide wage floor that reached beyond sectoral minimum wages. At the time of its introduction, it ranked among the highest wage floors among European countries when accounting for purchasing power (see Figure 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.<sup>9</sup> For instance, the German Federal Statistical Office estimates that 10.7 percent of employees are affected, which amounts to 4 million workers (see Table 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, 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 shows

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<sup>8</sup>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](http://www.zoll.de/DE/Fachthemen/Arbeit/Mindestarbeitsbedingungen/Mindestlohn-AEntG-Lohnuntergrenze-AUeG/Berechnung-Zahlung-Mindestlohns/berechnung-zahlung-mindestlohns_node.html).

<sup>9</sup>See Amlinger *et al.* (2016); Brenke (2014); Destatis (2016); Falck *et al.* (2013); Kalina and Weinkopf (2014); Lesch *et al.* (2014).

Figure 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 €.

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 4).

## 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 1: Minimum Wage Beneficiaries in 2014

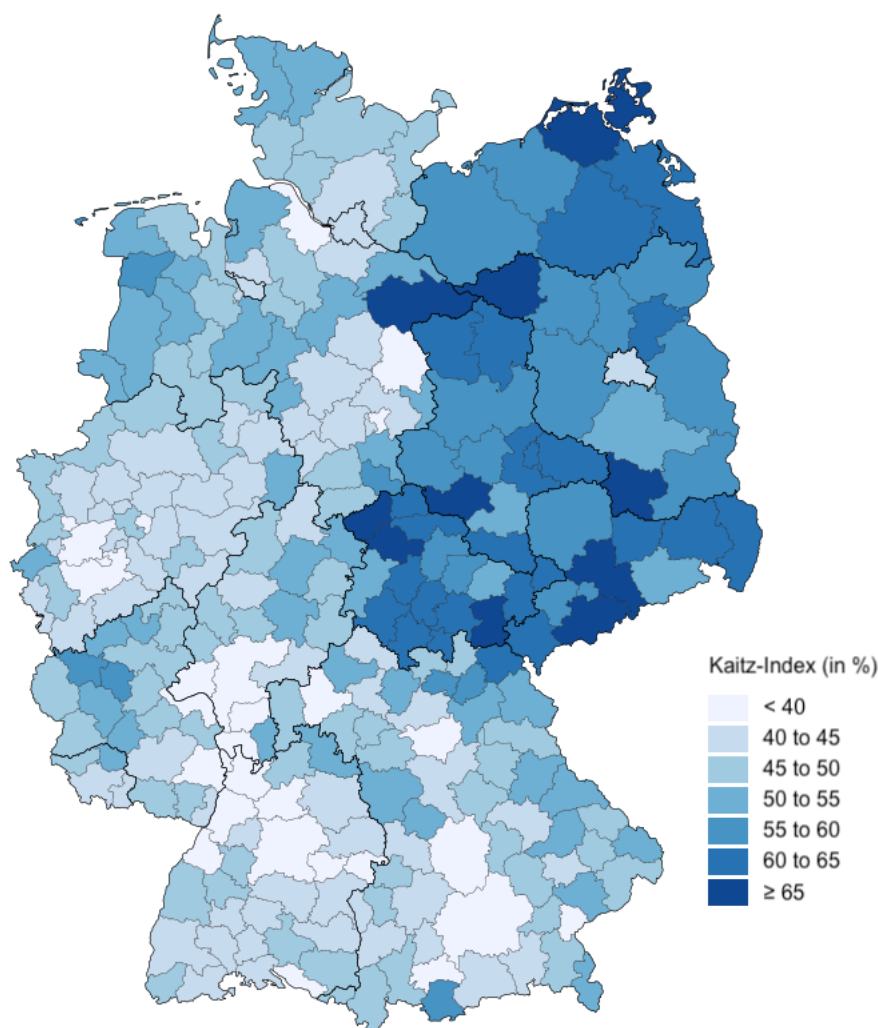
	Absolute (in mio.)	Share (in %) of	
		employed	affected
Employed	37.4	100	-
Wage <€8.50	5.5	14.7	-
Wage <€8.50 and eligible thereof	4.0	10.7	100
West-German residents	2.9	7.8	72.9
East-German residents	1.1	2.9	27.1
Full-time employment	0.9	2.4	22.4
Part-time employment	0.9	2.4	22.4
Mini-jobs	2.2	5.9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

Source: Destatis (2016). 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: 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).

## 3 Data Sources, Implementation Issues and Descriptive Evidence

### 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 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 Federal Employment Agency (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 1). Moreover, civil servants and self-employed are not included in the data.

Table 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 information, 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).<sup>10</sup>

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.*, 2018). 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.<sup>11</sup> 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.

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<sup>10</sup>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.

<sup>11</sup>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).

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.

### **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 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). 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).<sup>12</sup> 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-

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<sup>12</sup>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.<sup>13</sup> 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

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<sup>13</sup>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.

### 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, 2016a, 2018a, for more extensive overviews), while the causal evidence is discussed in Section 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. Groll, 2016; vom Berge *et al.*, 2016, 2018).

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).

## 4 Causal Evidence

### 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 DiD identification (DiD-R) and the second as the individual DiD (DiD-I). The firm level strategy is referred to as establishment DiD (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). 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 3).

Table 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 <i>et al.</i> (2018) ◇ Bossler and Gerner (2016) □ Burauel <i>et al.</i> (2018) ▽ Caliendo <i>et al.</i> (2018b) ◇ Caliendo <i>et al.</i> (2017) ◇
Overall Employment	Bonin <i>et al.</i> (2018) ◇□ Bossler and Gerner (2016) □ Bossler <i>et al.</i> (2018) □ Caliendo <i>et al.</i> (2018a) ◇ Schmitz (2017) ◇	Ahlfeldt <i>et al.</i> (2018) ◇ Garloff (2018) ⊙ Link (2018) □	
Regular Employment	Caliendo <i>et al.</i> (2018a) ◇ Schmitz (2017) ◇	Bonin <i>et al.</i> (2018) □◇	Garloff (2018) ⊙ Holtemöller and Pohle (2017) ⊙
Marginal Employment	Bonin <i>et al.</i> (2018) □◇ Caliendo <i>et al.</i> (2018a) ◇ Garloff (2018) ⊙ Holtemöller and Pohle (2017) ⊙ Schmitz (2017) ◇		
Unemployment	Ahlfeldt <i>et al.</i> (2018) ◇	Bonin <i>et al.</i> (2018) ◇ Garloff (2018) ⊙	
Self-employment		Bossler and Hohendanner (2016) □ Bossler <i>et al.</i> (2018) □	
Hours	Bonin <i>et al.</i> (2018) ▽ Bossler and Gerner (2016) □ Caliendo <i>et al.</i> (2017) ◇ Caliendo <i>et al.</i> (2018b) ◇		
Monthly Earnings		Caliendo <i>et al.</i> (2017) ◇ Caliendo <i>et al.</i> (2018b) ◇	
Benefit Recipients	Schmitz (2017) ◇	Bruckmeier and Becker (2018) ▽	
Poverty		Bruckmeier and Becker (2018) ▽	
Prices			Link (2018) □
Reservation Wage			Fedorets <i>et al.</i> (2018) ◇
Training	Bellmann <i>et al.</i> (2017) □ Bossler <i>et al.</i> (2018) □		
Internships		Bossler and Wegmann (2018) ◇□	
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.

## 4.2 Hourly Wages

The causal literature is unanimous about positive effects on gross hourly wages.<sup>14</sup> 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 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 4.4).

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<sup>14</sup>See Ahlfeldt *et al.* (2018); Bossler and Gerner (2016); Burauel *et al.* (2018); Caliendo *et al.* (2017, 2018b).



### 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 of studies estimating the causal impact of the minimum wage on employment, with most identifying small negative or zero employment effects.<sup>15</sup>

Both Bossler and Gerner (2016) and Bossler *et al.* (2018) employ an establishment level DiD with the IEB-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-wage 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.

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<sup>15</sup>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 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 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.

#### 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.<sup>16</sup> 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.

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<sup>16</sup>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.<sup>17</sup> 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 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.

#### 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 exceeds that of their unaffected counterparts by

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<sup>17</sup>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 corresponds 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 (2018) 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, Güral 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.



## 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 benefitted 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.<sup>18</sup> 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.

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<sup>18</sup>See also Bossler and Möller (2018) and Neumark (2018).

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## A Appendix

Table A.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 <i>et al.</i> (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 <i>et al.</i> (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 <i>et al.</i> (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

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No.	Author (Year)	Main Outcome	Main Method/ Identification	Main Data Source	Time Frame	Main Findings
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)
6	Bossler <i>et al.</i> (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 (2018)	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’

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No.	Author (Year)	Main Outcome	Main Method/ Identification	Main Data Source	Time Frame	Main Findings
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 <i>et al.</i> (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 <i>et al.</i> (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
12	Caliendo <i>et al.</i> (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 <i>et al.</i> (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

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No.	Author (Year)	Main Outcome	Main Method/ Identification	Main Data Source	Time Frame	Main Findings
14	Fedorets <i>et al.</i> (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
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%)

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No.	Author (Year)	Main Outcome	Main Method/ Identification	Main Data Source	Time Frame	Main Findings
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.