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# The Minimum Wage Effect on Low Wage Workers

## Employment, Life Satisfaction, and Populism Effects from the 2015 German Minimum Wage


Omar Mansour


Master's Thesis supervised by Professor Pierre Cahuc

Second member of the Jury: Professor Ghazala Azmat

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Abstract

The thesis's aim is to study the impact of the minimum wage implemented in Germany in 2015 on real wage growth, employment, life satisfaction and populist voting. The former two outcomes can have great ramifications on real wage growth, which in turn influences the latter outcomes. Thus, the research question is **How did the minimum wage, introduced in Germany in 2015, affect employment, life satisfaction and populist support among low wage workers?** Data from the German Socioeconomic Panel (SOEP) is used to conduct an event design by splitting the wage distribution of low wage workers into wage bins and evaluating the effects over time using a baseline as a control. The estimated effects are an increase in real wage employment, no significant effect on employment, nor populist voting, nor life satisfaction. It appears that while wage inequality was improved for low wage workers, this did not translate into improvements in terms of life satisfaction nor populist party voting. The policy recommendations based on that is that the minimum wage is likely not sufficient on its own to reverse the course of declining life satisfaction and increase in populist voting due to increasing distrust and economic insecurity.



Key words

Minimum Wage; Employment; Populism; Life Satisfaction; Wages; Income Inequality

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## Main Contributions

This thesis contributes to several strands of work in areas of economics and policy. The research into the effect of employment adds to the plethora of research into how minimum wages affect employment and how might they affect it differently. Namely, it adds onto the use of an event design that splits existing workers into wage bins and compares their outcome over time, so as to separate workers who originally earned below the minimum wage from those who earn much higher than that. With the finding of a positive wage effect and no dis-employment effect, this thesis shows that minimum wages can be effective tools to reduce the wage inequality that may arise from macroeconomic and institutional changes.

This thesis also contributes to the research on how life satisfaction and happiness is affected by wage inequality. The results show that while there were no discernible effects on the overall life satisfaction nor income satisfaction, but there were positive effects on job satisfaction. This is likely connected to the finding that workers reallocated to better and larger firms (Dustmann, Lindner, Schönberg, Umkehrer, & vom Berge, 2020). This highlights the potential from intersectionality between different areas or outcomes rather than just focusing on employment and income outcomes.

This thesis also adds the ongoing debate regarding the more prominent theory in explaining the effects of the rise in Populism. While there were no clear effects on the support towards far right and far left party, there did appear to be some other "side effects" to the minimum wage for other groups in the population besides low-wage workers

Lastly, this thesis's main contribution is the use of several dimensions to evaluate the minimum wage as a policy. This is vital as a too narrow view for example on one dimension may ignore the other effects. This use of several dimensions also allows for expanding onto other policy contexts as seen in the policy recommendations given at the very end.

# i. Introduction

Proposals for increasing the minimum wage, up to possibly $15 an hour from $7.25, have been discussed as a cornerstone of the Biden administration's economic package aimed at stimulating the economy after a pandemic-induced recession (Politi, 2021). Proponents argue that the minimum wage hike will induce more productivity by motivating workers and improving their wellbeing, while downplaying the potential negative employment effect. Oppositions on the other hand, are wary that the minimum wage would increase costs on the already burdened businesses, leading to a reduction in employment. Increasing the minimum wage, while historically a hotly debated topic, has been garnering support over the past few years. This trend, often coupled with concerns about income inequality, may very well be driven by the pattern of wage growth over the past two decades in developed economies. For example, the United States and Germany have exhibited a pattern of stagnant wage growth particularly at the bottom of the wage distribution, even though both have enjoyed better employment prospects compared to other countries (Krüger, Schönberg, & Schreiner, Productivity Growth, Wage Growth and Unions, 2018). The slower wage growth at the lower end of the distribution essentially leads to a rising wage inequality, thereby providing merit for increasing the minimum wage, in particular as unemployment continues to fall.

The German government introduced a federal minimum wage of €8.5 per hour starting from January 2015. The minimum wage affects a substantial portion of the distribution with about 15% of workers earning an hourly wage below that amount six months before the minimum wage came into effect (Dustmann, Lindner, Schönberg, Umkehrer, & vom Berge, 2020). In this thesis, the employment effects for low wage workers are examined alongside the effects of the minimum wage on other social and political outcomes, namely life satisfaction and support towards populist parties. The employment outcome provides the traditional economic approach to evaluate the effects of the minimum wage, while the other outcomes allow for investigating the potential political, social and wellbeing effects that may have arisen. Therefore, the key contribution of this thesis is to connect the economic effects with effects of both social and political outcomes to give a more comprehensive evaluation of the minimum wage policy enacted in 2015. Based on that the research question of this thesis is:

**How did the minimum wage, introduced in Germany in 2015, affect employment, life satisfaction and populist support among low wage workers?**

This research question is investigated by looking at the effects on real wage growth, employment, life satisfaction along with job and income satisfaction, and populist party support. The analysis is based on an event study design as constructed by Dustmann et al. (2020) and Cengiz et al. (2019), where workers are split into wage bins across the distributions with an evaluation of the effects per wage bin. The effect of the minimum wage is based on the effect on those in the wage bin below the minimum wage prior to its introduction. By applying the event study design to the data from the German Socio-Economic Panel (SOEP), a positive effect on real wage growth is found for workers initially earning below the minimum wage without a negative effect on employment. This creates an improvement in wage inequality, but it does not materialise into less populist voting or better life satisfaction. The positive effects were limited to improvements to job satisfaction, which could be explained by reallocating to better firms (Dustmann, Lindner, Schönberg, Umkehrer, & vom Berge, 2020).

The thesis contributes to the strand of literature discussing the effect of the minimum wage, more specifically it involves an application of the use of wage bins, which allows for an assessment on the workers affected rather than an overall assessment (Cengiz, Dube, Linder, & Zipperer, 2019). It namely contributes to the literature surrounding the German minimum wage, in particular with a comparison to Dustmann et al. (2020) due to the methods used being very similar. The findings in this thesis were in line with it, finding positive real wage growth effects and no unemployment effects, thus contributing to a drop in wage inequality. Furthermore, the thesis contributes to the area of research surrounding the relationship between wage inequality and self-reported or subjective wellbeing by investigating the minimum wage effect on life satisfaction. While no such link was established in this thesis unlike in others studying Germany (Lipps & Oesch, 2018). Lastly, the thesis contributes to the discussion regarding the drivers of populist support by analysing a potential economic effect on populist party support. While Algan et al. (2017) find clear effects from economic crises, this thesis looks at economic improvements by looking at the minimum wage. No strong effects were found implying that it could be that the minimum wage does not create a big enough change or that the cultural backlash position (Inglehart & Norris, 2016) is more prominent in explaining the rise in populism.

The thesis is split into 7 more sections. In the next section, the relevant literature and state of knowledge regarding the German economy, the effects of minimum wage on employment and wages, and the effects of wage inequality on social and political attitudes are discussed. This is followed by a section on the data collected from SOEP that explains the hourly wage proxy that was developed along with the cleaning of the data. Afterwards, the methodology section contains the models used to analyse the data and obtain the results. Furthermore, three analysis sections, each containing its own discussion subsection, contain the relevant results with the discussion subsection containing the acceptance/rejection of the hypothesis. At last, the conclusion is presented along with the limitations and policy implications.

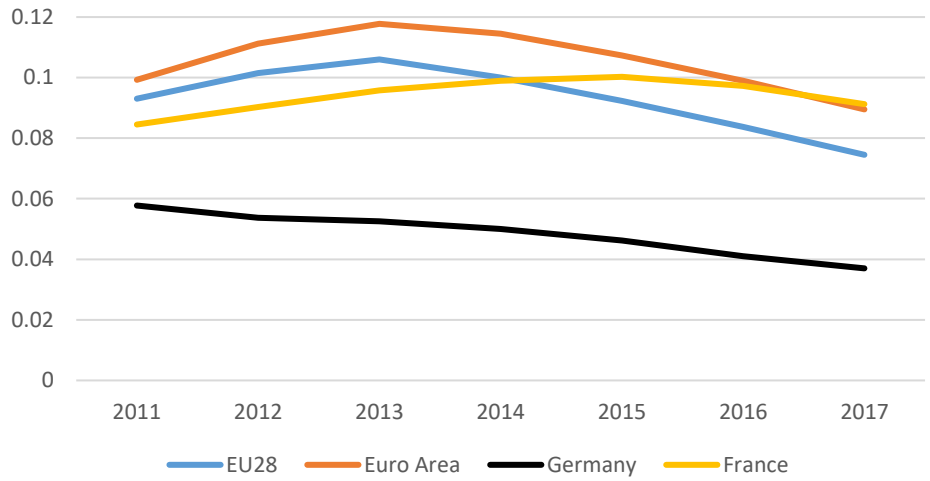
## ii. State of Knowledge

*The German Economy: Wage Growth and Economic Recovery*

In the aftermath of the Great Recession, countries diverged in their recovery patterns with Germany making a strong recovery, other EU countries lagged behind as can be seen in Figure 1 (Eurostat, the statistical office of the European Union, 2021). However, in Germany wage growth did not grow in tandem with the growth in productivity before the Great Recession (Krüger, Schönberg, & Schreiner, Productivity Growth, Wage Growth and Unions, 2018). In comparison to France, wage growth was much slower especially at the bottom of the wage distribution, while labour productivity has increased relatively similarly up until the Great Recession, only for the divergence in patterns to start subsiding after the Great Recession (Krüger, Schönberg, & Schreiner, Productivity Growth, Wage Growth and, 2018). This phenomenon has been attributed to several factors including technological change (Dustmann,

Ludsteck, & Schönberg, Revisiting the German Wage Structure, 2009), an increase in the influence of personal characteristics such as education as well as matching of high skilled workers with high-paying firms (Card, Heining, & Kline, 2013), and a drop in collective bargaining power driven by a drop in union membership in the beginning of the 21st century

**Figure 1. Evolution of Unemployment Rates across EU countries 2011-2017**




(Antonczyk, Fitzenberger, & Sommerfeld, 2010).

The combination of a recent economic recession, rising wage inequality and declining union power thus led the German government to introduce a first-ever minimum wage of €8.5 per hour effective from the 1st of January 2015 across the entire country. The minimum wage affected almost 15 percent of workers, cutting deep into the wage distribution, albeit not as much as in France (Dustmann, Lindner, Schönberg, Umkehrer, & vom Berge, 2020). Exemptions to the minimum wage included those under 18 years old, apprentices, interns, volunteers, the long-term workers. Temporarily, workers in the hairdressing, meat, and agriculture and forestry industries were also exempted and allowed to keep their collectively bargained union wages until the end of 2016. The minimum wage was set to increase to €8.84 per hour in 2017 and subsequently €9.19 per hour.

*Minimum Wage & Employment*

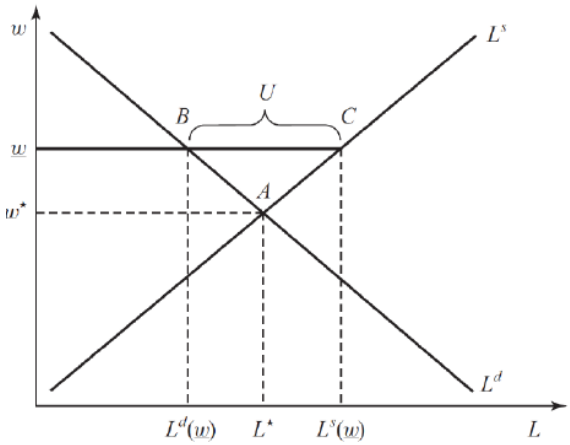
The minimum wage's effect on employment has been a controversial topic, rarely with a consensus on its employment effect even when looking at the same policy setting. In particular, the standard theoretical model, the competitive labour market, would imply a drop in employment due to an introduction of the minimum wage (Boeri & van Ours, 2013). As seen in Figure 2 panel (a), the result of introducing a minimum wage higher than the equilibrium wage at point A, will lead to a drop in employment to point B and an increase in unemployment equal to the difference between labour supply at C and labour demand at B. The key assumption in this case is that neither workers nor firms have market power, implying that they are **wage takers** (i.e., they have no influence over the wages). A violation of that assumption implies different consequences due to the minimum wage. Specifically, if the firms have monopsony power, they have (some) wage-setting power, making them **wage setters** (Boeri & van Ours, 2013). In that scenario, firms employ less workers (at C) than would be efficient (at A) and pay them less. Introducing a minimum wage, as seen in panel (b), would lead the firm to increase the wages,

necessitating an increase in hiring to match the increase in marginal cost with an increase in marginal value. Since it employs a lower number of workers than is efficient, there is an increased marginal value from hiring more workers, leading the firm to reduce its rents and employ more people. This pattern exists until the firm is hiring at the competitive level point A, beyond which a higher minimum wage leads to a similar effect as discussed in the competitive model.

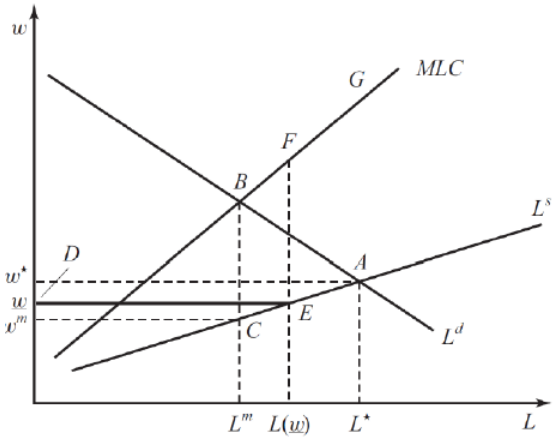
**Figure 2. Responses to the minimum wage by labour market model**

*Adapted from Boeri & van Ours (2013)*

Panel (a) Competitive Model




Panel(b) Monopsonistic Model




Extensive research is done to determine which effect is apparent, whenever a minimum wage is introduced or increased. Giuliano (2013) exploits geographic variation in initial wage levels to investigate the effect of a US federal minimum wage increase in 1996. Her findings are consistent with the competitive model, as she finds negative (albeit insignificant) effects due to the minimum wage on overall employment. However, she also finds that the increase led to a rise in employment for teenagers and provides evidence of a non-competitive teenager labour market. Her findings also highlight the potential for strong heterogeneity across different subgroups in the population, which is particularly relevant when the minimum wage affects a large population such as those implemented nationwide or federally. Furthermore, the Hungarian minimum wage increase between 2001 and 2002 was found to have a strong positive effect on wage growth with a heterogenous effect across sectors in terms of how firms adjusted (Harasztosi & Lindner, 2019). In sectors where there is less competition with foreign firms, who were unaffected by the minimum wage rise, firms were able to adjust by increasing their prices without losing their competitive edge leading to smaller negative shocks to output and employment. On the other hand, firms competing with foreign firms were found experienced a loss of output due to raising prices, leading to a negative effect on employment. Similarly, the minimum wage was found to affect restaurants in the Bay Area differently, with lower-rated restaurants more likely to exit the market due to a minimum wage increase (Luca & Luca, 2019).

The heterogeneity discussed above was used as motivation to employ a different approach to researching the minimum wage. Rather than just relying on geographic variation (particularly difference-in-difference estimates by comparing US states), Cengiz et al. (2019) go a step further and split the wage distribution into wage bins, assessing the minimum wage effect using Difference-in-Difference estimates for each wage bin. They then focus on low-wage workers by comparing at the jobs lost due in the wage bins below the minimum wage and

the excess jobs created right above the minimum wage. They find that for 138 state-level minimum wage changes in the US between 1976 and 2016, employment of low-wage workers was unaffected over 5 years after the minimum wage change. Moreover, they infer that the lack of job loss for existing low-wage workers provides evidence against the substitution of low-skilled workers with high-skilled new workers, who they also show by restricting the sample to lower education levels as a proxy for low skills. This approach was also used to investigate city minimum wages in the US, with estimates showing increased wages at the bottom of the distribution leading mostly to an increase in prices rather than a decrease in employment (Dube & Lindner, 2021).

The German minimum wage has also been investigated using Cengiz et al.'s (2019) methodology. The increase in wage growth was again not found to lead to a drop in employment for low-wage workers employed before the minimum wage, with the wage growth mainly translating to reallocation to better-paying and larger firms (Dustmann, Lindner, Schönberg, Umkehrer, & vom Berge, 2020). This was also confirmed by using the geographic variation in how hard the minimum wage affects a district, with employment prospects of those unemployed also not being negatively affected. This contradicts earlier findings, where a difference-in-difference framework based on geographic variation in how hard the minimum wage bites the regional labour market showed negative employment effects with a higher fraction of affected workers in the region (Caliendo, Fedorets, Preuss, Schröder, & Wittbrodt, 2018). However, it is also noted that this is mainly driven by a drop in marginal employment with no specific inferences drawn about regular employment (full-time or part-time). Furthermore, it is also possible that workers shifted to regular employment from marginal employment, which was not possible to investigate using this design.

Based on the literature discussed above as well as the access to a sample individual employment spells, an event study similar to that implemented by Dustmann et al., (2020) will be implemented to investigate the wage growth and employment effects arising from the introduction of the minimum wage. The competitive labour market model is used to construct the following hypotheses:

*Hypothesis 1a: The minimum wage growth led to an increase in real wage growth for low-wage workers earning below the minimum wage prior to its introduction.*

*Hypothesis 1b: The minimum wage leads to lower employment for low-wage workers earning below the minimum wage prior to its introduction.*

The expectation based on the literature discussed above would be primarily a large heterogeneity in effects. Furthermore, the research conducted using the same methodology would imply a wage growth increase without negative employment effects (Dustmann, Lindner, Schönberg, Umkehrer, & vom Berge, 2020), leading to Hypothesis 1a being accepted, while Hypothesis 1b being rejected. The evaluation of both hypotheses and how they're measured is discussed later on in the Employment Analysis section.

*Life Satisfaction, Populism & The Rise of Inequality*

Economic motivations may not be the only motivation or benefit behind the introduction of a minimum wage. As discussed earlier, wage growth stagnated in Germany over the course of the 1990s and 2000s in the run-up to the Great Recession (Antonczyk, Fitzenberger, &

Sommerfeld, 2010). This likely does not only have economic consequences in terms of employment but also social and political consequences for the working class that is left behind as the higher classes in the economy make gains. Two primary outcomes are documented and discussed below: a drop in life satisfaction and the rise of populism, both driven in part due to the wage growth inequality creating discontent in the lower part of the wage distribution.

The link between wage inequality and life satisfaction has also been reported elsewhere. Alesina et al. (2004) find that individuals had a lower chance of reporting being happy in both the US and Europe when inequality is high, an effect that seemingly is stronger in Europe amongst the poor and those on the left part of the spectrum. The link seemingly builds on the feeling of employment insecurity, where the adverse effect on life satisfaction increases with less generous labour market policies (Carr & Chung, 2014). This also extends to the level of public good spending as well as the quality of the country's institutions, where the variation within the country in happiness was found to decrease with better quality and more spending, albeit high levels of happiness seem to be less likely, supposedly due to the brunt of taxation (Clark, Flèche, & Senik, 2015).

In Germany, wage stagnation and loss of bargaining power with the fall of unions seems to have led to a fall in life satisfaction. In a comparison with Switzerland, self-reported life satisfaction is found to have increased more for the working class in Germany than for the upper-middle class, as the economy started to recover in the early 2000s (Lipps & Oesch, 2018). The gap between both classes was found to be associated with unemployment risks and outcome in Germany, whereas in Switzerland the class gap did not follow a time trend and was half as large as in Germany. Hence, it would seem that the introduction of the minimum wage would work towards not only improving the economic outcomes, but also life satisfaction and happiness by increasing wage growth for the low wage workers. Based on that the following hypotheses is formed:

*Hypothesis 2a: The minimum wage leads to a higher self-reported life satisfaction for low-wage workers earning below the minimum wage prior to its introduction.*

*Hypothesis 2b: The minimum wage leads to a higher self-reported job satisfaction for low-wage workers earning below the minimum wage prior to its introduction.*

*Hypothesis 2c: The minimum wage leads to a higher self-reported income satisfaction for low-wage workers earning below the minimum wage prior to its introduction.*

For workers who were not displaced from work due to the minimum wage, which appears to be the general case (Dustmann, Lindner, Schönberg, Umkehrer, & vom Berge, 2020), the increase in wage growth would increase satisfaction by alleviating job and income security concerns. However, an opposite effect would be found if the wage growth was coupled with large substantial negative employment effects, as employment uncertainty would increase for low-wage workers. Similarly, job and income satisfaction would fall if workers were laid off due to the minimum wage, but it would rise if they stayed employed and experience an increase in wages.

Another factor affected by the rising wage inequality as well as the Great Recession is trust. The negative effect of wage inequality on life satisfaction above was found to often be driven by a rise in distrust (Delhey & Dragolov, 2014). This while influences life satisfaction,

may also have an impact on the political landscape as well. By using the share of the construction sector in the regional economy as an instrument, the effect of the financial crisis in terms increased unemployment was estimated (Algan, Guriev, Papaioannou, & Passari, 2017). The results showed increased support for populist parties as well as increased distrust towards political institutions. As Guriev (2018) argues, there needs to be structural reforms to the labour market and towards social policies that address the issue of trust and revert the rise in populism. While these reforms would normally be costly in the short run, leading to a further decrease in trust. However, in the case of Germany, there was a strong recovery in the period leading up to the minimum wage, meaning that its introduction can potentially reduce the support for populist parties, which have not been in government for decades. Nevertheless, a randomised experiment into the French elections showed that even when presented with facts, populist voters seemingly update their factual knowledge without changing their voting intentions (Barrera, Guriev, Henry, & Zhuravskaya, 2020). Instead, it was found that immigration partly had to do with this effect.

In the context of Germany, this would imply that an increase in the minimum wage should reduce the voting intentions or support for populist parties. This should especially be the case, since populist parties (namely Die Linke, Alternative für Deutschland) were in the opposition during the introduction of the minimum wage, with the main establishment parties the SPD and CDU/CSU being in the governing coalition. The SPD in particular had included the introduction of a minimum wage as part of its manifesto. Based on that, the following hypotheses are generated:

*Hypothesis 3a: The minimum wage leads to lower support for far-right parties Alternative für Deutschland and NPD for low-wage workers earning below the minimum wage prior to its introduction.*

*Hypothesis 3b: The minimum wage leads to lower support for far-left party Die Linke for low-wage workers earning below the minimum wage prior to its introduction.*

*Hypothesis 3c: The minimum wage leads to more support for centre-left party SPD for low-wage workers earning below the minimum wage prior to its introduction.*

*Hypothesis 3d: The minimum wage leads to more support for centre-right alliance of CDU and CSU for low-wage workers earning below the minimum wage prior to its introduction.*

Indeed, this will likely experience a heterogenous effect depending on job insecurity, initial party support, party support intensity and social class. The hypotheses are based on the notion that economic drivers play an integral role in support for populist parties, disputed by the cultural backlash view, which is beyond the scope of the thesis.

## iii. Data

The analysis implemented in this thesis is based on data from the German Socio-Economic Panel (SOEP), designed by the German Institute for Economic Research (DIW Berlin) (Goebel, et al., 2018). The database contains information on social, income and demographic variables that allow for measuring outcomes in different fields of social sciences (SOEP, 2019). The data is collected from 18,682 private households residing information

including both adults and children (who are dropped from the analysis). The selection is done using multi-stage random samples and regional clustering, with SOEP interviewers using questionnaires to collect individual data from all members above 12 years old and household data from the head of the household (SOEP, 2019). The data spans from 1984 until 2018 and was published in November 2019.

The relevant variables include the imputed yearly wages, imputed annual hours of work, as well as employment status and experience, along with occupations and industry. Demographic controls are also included, along with self-reported measures on life satisfaction, job satisfaction, trust and trust towards migrants (collected only for 2013 and 2018), concern about a range of issues (finances, economic growth, environment, jobs, crime, …etc). Party support is reported as party affiliation with party intensity and interest towards politics included as well. In the next subsections the construction of different variables is described, otherwise the variables were used as was found in the SOEP database. Income data was based on data collected in Cross National Equivalent File (CNEF), while employment data was used across CNEF, the Person-Related Status and Generated Variables (PGEN) file and the self-reported answers of respondents available in the Longitudinal file (PL). As the data in CNEF is reported for the earlier year (Grabka, 2020), the analysis is restricted to the period 2011-2017. The following subsections describe the construction of several important variables used in the analysis.

*Construction of Paid Weekly Hours and Employment Status*

Paid weekly hours were constructed using the imputed annual hours worked in CNEF and the self-reported overtime hours and paid overtime hours worked in the month prior to the survey. As in Dustmann et al. (2020), weekly working hours are calculated by multiplying annual hours by the 5 potential weekly working days and dividing it by the 250 potential working days in a year. When the weekly no. of working days are reported, these are used instead of 5 potential weekly working days. To eliminate unpaid working hours, which can bias the hourly wage downwards, the paid weekly working hours are constructed by reducing the annual hours with the difference between total overtime hours and paid overtime hours multiplied by 12. This is followed by the same calculation as described above to obtain the weekly hours. Where this data is missing or inaccurate (for example, paid overtime hours are greater than overtime hours), the weekly working hours are used without the adjustment.

The employment data in CNEF was based on the imputed labour hours, which in turn are based on reported working hours. Full-Time employees were determined to be those who were detected as such in the CNEF and had self-reported to be full-time employed. Similarly, part-time employees were detected as such in the CNEF and had self-reported to be part-time employed. Marginally employed workers were ones reported as part-time in CNEF but self-reported as marginal workers or were working mini jobs. Unemployed individuals were those reported as not working in the CNEF but had self-reported receiving unemployment benefits/assistance, reported as unemployed or reported spending months earning unemployment benefits. The remainder of not working is assigned to non-workers. Retirees were those who reported being retired or reported receiving pensions in the CNEF. Self-employed individuals were those who reported as such or earned any self-employed income.

Corrections were made to the above employment concept to exclude those who were not working but had imputed annual working hours, those reporting to be vocational trainees, those under 18, those reporting zero annual labour hours but positive labour earnings, those with paid week hours greater than 60 hours, along with those who were collected as employed but had no job earnings or had zero full-time or part-time job experience over the past year. The experience variable is adjusted for the differing month of interview by dividing the difference from the last observation with the no. of months in between their collection to get an approximate annual figure. Those with that figure equalling zero for both full-time and part-time but positive for unemployment were assigned as unemployed if they were recorded as employed or non-working.

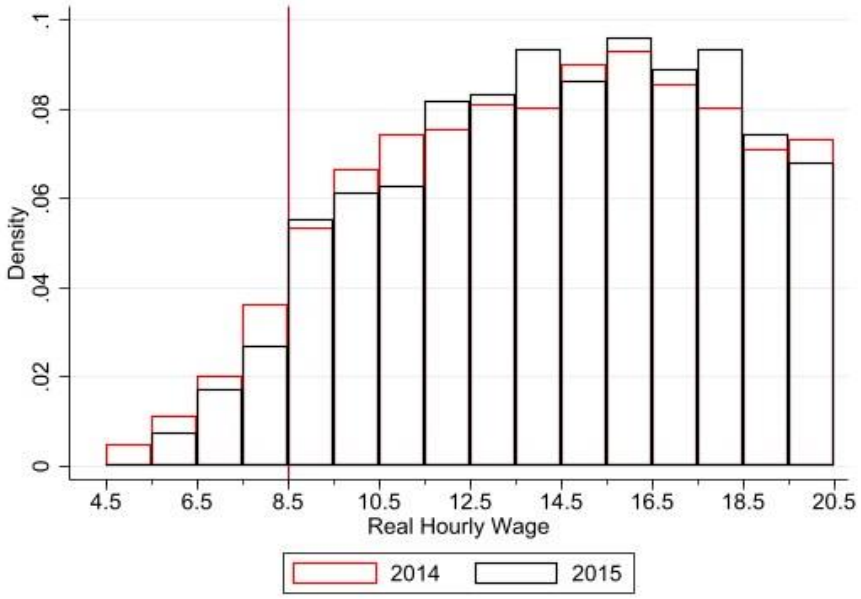
*Construction of Real Hourly Wage and Wage Bins*

Data on hourly wage has not actively collected in Germany since 2015 (Dustmann, Lindner, Schönberg, Umkehrer, & vom Berge, 2020). This necessitates a construction of a proxied hourly wage based on labour earnings and annual hours worked. The nominal hourly wage is constructed as the annual labour income divided by the annual paid hours, estimated as the paid week hours multiplied by 250 potential working days and divided by the reported weekdays worked (5 if not reported). For individuals with employment experience indicating they worked less than a year, the adjusted labour experience described above is multiplied by the no. of annual hours. The observations are dropped, if income from primary employment is zero or missing, income from self-employment is positive, income was imputed rather than reported, paid weekly hours were less than 30 for full-time employees or greater than 30 for part-time employees. Furthermore, the individuals working in industries that were exempt from the minimum wage were dropped (Dustmann, Lindner, Schönberg, Umkehrer, & vom Berge, 2020). Those industries had collective bargaining agreements that were allowed to run until the end of 2016. Afterwards, there remained 61 observations below €4.50 per hour across the entire time period, which were dropped due to missing data across several other dimensions such as paid overtime as well as some the controls described later on in the methodology.

Finally, to generate the real hourly wage, the nominal hourly wage is multiplied by a 100 and divided by the CPI measure included in the CNEF, where the base year is 2015 (CPI = 100). The motivation behind this is to assess the real wage growth as well as include the year 2017 in the sample, where the minimum wage was increased to €8.84 per hour but in real terms using the CPI measure would be a very small increase to €8.67 approximately. This hike was intended to account for inflation and given its magnitude will not generate a discernible effect especially given the small sample size. Three wage bins are constructed: $€4.5 \leq w < €8.5$, $8.5 \leq w < 12.5$ and $12.5 \leq w \leq 20.5$. This leads to a focus on low-wage employees and allows large enough wage bins to have a sufficient no. of observations to obtain precise estimates with as minimal standard error as possible. There are overall 33,409 observations with real hourly wage data spanning between 2011 and 2017, however, this is an upper bound given that the methodology requires that individuals are repeated across several years, and the models contain a large set of controls that may be missing for some observations. In practice, the former tends to limit the no. of observations the most. Overall, while estimates are not expected to be as precise as those based on administrative data (Dustmann, Lindner, Schönberg, Umkehrer, & vom Berge, 2020), the proxied hourly wage seems to perform well as seen in the histogram below, showing the hourly wage distributions between 2014 (in red) and 2015 (in black). Those

still earning under the minimum wage after 2015 may represent either measurement error or potentially non-compliance, which was highlighted as a possibility in this context (Caliendo, Fedorets, Preuss, Schröder, & Wittbrodt, 2018).

**Figure 1 Histogram showing the distribution of the real hourly wages for 2014 and 2015**




**Notes:** Figure 1 shows the distribution of real hourly wages for the years 2014 (in red) and 2015 (in black). The hourly wage is proxied by dividing the annual earnings with an estimate of the paid annual hours based on the paid weekly hours. The hourly wages are calculated for those who were full-time employed and part-time employed. The distribution also excludes those who are exempted from the minimum wage including those younger than 18 years old, interns, apprentices, vocational trainees, long-term unemployed, as well as those with collective bargaining agreements that were allowed to run until 2016 (Dustmann, Lindner, Schönberg, Umkehrer, & vom Berge, 2020).

## iv. Methodology

Two models are constructed to compare the evolution of outcomes (e.g., wage growth) for each of the three wage bins over time, similar to the method implemented by both Dustmann et al. (2020) and Cengiz et al. (2019). The first model uses tracks the outcome of worker i between periods t and t-1 on and indicator variable T equal to 1 if the worker falls in wage bin w in time t-1. As such for wage growth the model would be:

$$\Delta y_{i_{w(t-1)}t} = \delta_{w_{(t-1)}t} T_{i_{w(t-1)}t} + \beta X_{i,t-1} + e_{it}$$

Where δ measures the average wage growth between year t and year t-1 of workers in wage bin w in the baseline period (t-1), conditional on a vector of individual characteristics measured at the baseline as well as some time-variant controls. The second model is identical to that of Dustmann et al. (2020) in construction, in that it is based on a two-year difference (t and t-2) rather than a one-year difference. The use of both models allows for robustness and comparison of results, with the model above measuring an extra placebo period as well as an extra lag period.

The two models are exposed to several sources of bias that need to be controlled for. First of all, mean reversion, where wage growth for workers in the lowest wage bin is likely to be higher than that of workers in the middle and high wage bins, as they edge towards the mean (Dustmann, Lindner, Schönberg, Umkehrer, & vom Berge, 2020). Secondly, there may be macroeconomic trends that speed up growth of wages due to economic growth, especially with Germany on an economic recovery path as described above. To control for both of those, the effect δ for the years 2012 vs 2011 for the one-year difference model and 2013 vs 2011 for the two-year difference model will be used as a baseline for all the other effects leading to the following models:

$$\Delta y_{i_{w(t-1)t}} = \gamma_{w(11)12} + \delta_{w_{(t-1)}t} T_{i_{w(t-1)t}} + \beta X_{i,t-1} + e_{it}$$

$$\Delta y_{i_{w(t-2)t}} = \gamma_{w(11)13} + \delta_{w_{t-2}t} T_{i_{w(t-2)t}} + \beta X_{i,t-2} + e_{it}$$

This effectively implies that the effect on a wage bin over years t and (t-1) is estimated relative to the 2012 vs 2011 effect, while the effect on a wage bin over years t and (t-2) is estimated relative to the 2013 vs 2011 effect. The underlying assumption here is that for each wage bin, mean reversion and macroeconomic trends that drive wage growth, or any other outcomes are stable over time. This can be tested by looking at the pre-policy effects 2013 vs 2012 and 2014 vs 2013 for the one-year difference model, and at 2014 vs 2012 for the two-year difference model.

The effect estimated above may still be violated by a macroeconomic or other trend that changes over time. The effect of the minimum wage on the highest wage bin [12.5,20.5] should be close to zero, as even in the presence of substitution between low-skilled minimum wage workers and high-skilled workers, they tend to have a small portion of labour costs, making the effects limited for firms (Cengiz et al., 2019; Dustmann et al., 2020). Thus, a difference-in-difference estimator can be calculated, where the effects above are calculated relative to the largest wage bin. The underlying assumption will be that this trend that evolves over time affects wage bins in the same way. This was shown to produce robust results that were did not violate the parallel trends assumption (Dustmann, Ludsteck, & Schönberg, Revisiting the German Wage Structure, 2009), nevertheless, will be discussed later in the thesis.

All models include baseline controls for age, state interacted with the type of residence area (commercial, residential, rural area), immigration status (native, immigrant, foreigner), nationality, education, gender, age, firm size, industry, occupation, class of occupation (Lipps & Oesch, 2018), years with firm and initial employment status (full-time or part-time). Time-variant controls added included a change in state, firm or industry, as these could bias the result if they affect the wage growth. They're generally insignificant but help with the precision of the estimates along with the log of real state GDP per capita change by lowering the standard errors without changing the point estimates.

## v. Employment and Wage Growth Analysis

*One-Year Difference Model*

To assess hypothesis 1a, the real wage growth calculated as the change in wage divided by the baseline real hourly wage was regressed using the one-year difference model described earlier. The results can be seen in panel (a) in table 1. There appears to be a 64.2%-point increase

over the baseline, implying a massive increase for the wage bin under the minimum wage as soon as it was implemented. While the effect is positive and significant, it appears to be somewhat imprecisely estimated given the size of the standard error. Other wage bins did not have any significant effects after the minimum wage was implemented. The lagged effects and placebo effects appear to be insignificant, with the latter's insignificance giving confidence that there's no bias in the results. The strong positive effect is persistent even when using the difference-in-difference estimator, giving confidence in its robustness.

Several measures were used to estimate the employment effect and evaluate the effect on employment. In panel (b) the probability of remaining employed is estimated by running the model using the dummy of being employed at time t. There appears to be an insignificant effect on those treated by the minimum wage, although those earning above prior to its introduction seem to have a significant decrease in the probability of being employed. However, the placebo effects for 2013 vs 2012 for both of those wage bins seem to imply the presence of a violation in the parallel trends. When using the difference-in-difference estimator, the placebo effects are all insignificant, including the one for the treated wage bin [4.5,8.5), which was originally insignificant as well. The difference-in-difference estimators imply that employment probability did not change for both the wage bin right under the minimum wage and the minimum wage right over, with very weakly significant effects, positive for the treated and negative for the spill over wage bin. This is under the assumption that the minimum wage does not affect the employment probability as discussed in the methodology section, and that the effects in column (3) capture the parallel trend violation that is common across all wage bins.

To measure a change in employment intensity, the change in full-time equivalent (FTE) is assessed. FTE is assigned 1.0 for full-time workers, 0.5 for part-time workers, 0.2 for marginal workers, 0 for the unemployed and non-participants (Dustmann, Lindner, Schönberg, Umkehrer, & vom Berge, 2020). Similar to the earlier result, there is a violation of the parallel trends assumption, as the placebo for 2013 vs 2012 is significant for the untreated wage bins. Using the same assumption regarding the difference-in-difference estimator, it appears that the FTE did not change for either wage bin, implying that employment was largely unaffected. This is also shown in the biased estimates, which even though were negative for the spill over wage bin [8.5,12.5) and untreated one [12.5,20.5), were very small and did not imply a substantial reduction in employment intensity.

In panel (d) the probability of being unemployed is proxied using a dummy that takes the value one if a worker draws unemployment benefit or assistance. The effects are insignificant except for the untreated wage bin, which also seems to have some bias due to the placebo being significant. The difference-in-difference estimators imply generally insignificant effects, except for the spill over wage bin, which has a significant reduction in the probability of drawing unemployment by 3.2%-points.

**Table 1 Employment and Wage Growth Results based on the one-year difference model**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Changes Relative to (2011 vs 2012) | | | Difference-in-Difference | |
| Wage bin in (t-2) | [4.5,8.5) | [8.5,12.5) | [12.5,20.5) | (1) minus (3) | (2) minus (3) |
| **Panel (a): Hourly Wage Growth** | | | | | |
| 2015 vs 2014 (Min Wage Effect) | 0.642** | 0.043 | 0.007 | 0.635** | 0.037 |
| | (0.2858) | (0.0294) | (0.0183) | (0.2857) | (0.0327) |
| 2016 vs 2015 (Lagged Effect 1) | 0.106 | 0.018 | -0.018 | 0.124 | 0.036 |
| | (0.0775) | (0.0241) | (0.0184) | (0.0786) | (0.0280) |
| 2017 vs 2016 (Lagged Effect 2) | 0.294 | 0.055 | -0.019 | 0.313 | 0.074* |
| | (0.2091) | (0.0372) | (0.0227) | (0.2107) | (0.0390) |
| 2014 vs 2013 (Placebo Effect 1) | 0.013 | 0.006 | -0.021 | 0.034 | 0.028 |
| | (0.0702) | (0.0280) | (0.0227) | (0.0722) | (0.0313) |
| 2013 vs 2012 (Placebo Effect 2) | 0.110 | 0.070* | 0.006 | 0.104 | 0.064 |
| | (0.0836) | (0.0383) | (0.0252) | (0.0890) | (0.0453) |
| 2012 vs 2011 (Baseline) | 0.245 | 0.134 | 0.057 | | |
| **Panel (b): Employed (1 if employed)** | | | | | |
| 2015 vs 2014 (Min Wage Effect) | 0.030 | -0.037*** | -0.015*** | 0.045* | -0.022* |
| | (0.0265) | (0.0104) | (0.0054) | (0.0270) | (0.0115) |
| 2016 vs 2015 (Lagged Effect 1) | 0.070*** | 0.000 | 0.018*** | 0.053* | -0.018 |
| | (0.0270) | (0.0100) | (0.0062) | (0.0275) | (0.0111) |
| 2017 vs 2016 (Lagged Effect 2) | -0.036 | -0.039*** | -0.021*** | -0.015 | -0.018* |
| | (0.0344) | (0.0098) | (0.0063) | (0.0344) | (0.0105) |
| 2014 vs 2013 (Placebo Effect 1) | 0.023 | -0.001 | 0.008 | 0.015 | -0.010 |
| | (0.0298) | (0.0107) | (0.0069) | (0.0301) | (0.0116) |
| 2013 vs 2012 (Placebo Effect 2) | -0.014 | -0.033*** | -0.025*** | 0.011 | -0.008 |
| | (0.0286) | (0.0090) | (0.0054) | (0.0291) | (0.1031) |
| 2012 vs 2011 (Baseline) | 0.939 | 0.985 | 0.982 | | |
| **Panel (c): Change in Full-Time Equivalent** | | | | | |
| 2015 vs 2014 (Min Wage Effect) | -0.001 | -0.031*** | -0.009 | 0.009 | -0.022* |
| | (0.0273) | (0.0097) | (0.0057) | (0.0279) | (0.0110) |
| 2016 vs 2015 (Lagged Effect 1) | 0.053* | 0.007 | 0.017*** | 0.036 | -0.010 |
| | (0.0283) | (0.0093) | (0.0059) | (0.0289) | (0.0104) |
| 2017 vs 2016 (Lagged Effect 2) | -0.062* | -0.034*** | -0.017*** | -0.045 | -0.017 |
| | (0.0361) | (0.0096) | (0.0063) | (0.0362) | (0.0103) |
| 2014 vs 2013 (Placebo Effect 1) | 0.024 | -0.003 | 0.009 | 0.015 | -0.006 |
| | (0.0294) | (0.0101) | (0.0066) | (0.0298) | (0.0110) |
| 2013 vs 2012 (Placebo Effect 2) | -0.013 | -0.026*** | -0.023*** | 0.011 | -0.003 |
| | (0.0284) | (0.0088) | (0.0053) | (0.0289) | (0.0102) |
| 2012 vs 2011 (Baseline) | -0.075 | -0.023 | -0.020 | | |
| **Panel (d): Drawing Unemployment Benefits/Assistance** | | | | | |
| 2015 vs 2014 (Min Wage Effect) | -0.020 | -0.015 | 0.017*** | -0.036 | -0.032** |
| | (0.0363) | (0.0151) | (0.0066) | (0.0368) | (0.0161) |
| 2016 vs 2015 (Lagged Effect 1) | -0.016 | -0.003 | 0.001 | -0.017 | -0.004 |
| | (0.0383) | (0.0152) | (0.0067) | (0.0385) | (0.0157) |

| | | | | | |
|---|---|---|---|---|---|
| 2017 vs 2016 (Lagged Effect 2) | 0.038 | 0.008 | 0.012 | 0.027 | -0.004 |
| | (0.0429) | (0.0156) | (0.0076) | (0.0340) | (0.0160) |
| 2014 vs 2013 (Placebo Effect 1) | 0.023 | -0.013 | 0.002 | 0.021 | -0.015 |
| | (0.0375) | (0.0152) | (0.0078) | (0.0375) | (0.0156) |
| 2013 vs 2012 (Placebo Effect 2) | 0.058 | 0.009 | 0.018*** | 0.040 | -0.009 |
| | (0.0372) | (0.0144) | (0.0059) | (0.0376) | (0.0155) |
| 2012 vs 2011 (Baseline) | 0.091 | 0.053 | 0.022 | | |

**Notes:** In panel (a), the excess hourly wage growth is reported for the years 2013 vs 2012, 2014 vs 2013, 2015 vs 2014, 2016 vs 2015 and 2017 vs 2016 relative to the baseline wage growth for the years 2012 vs 2011. It is reported for each of the wage bins based on the baseline year (t-1): the treated wage bin [4.5,8.5) in column 1, the spill over wage bin [8.5,12.5) in column 2, and the control/unaffected wage bin [12.5,20.5) in column 3. In column 4, the difference-in-difference estimates are reported for the treated wage bin, calculated as the difference in the effect for each year between the treated wage bin and the control wage bin. In column 5, the difference-in-difference estimates are reported in a similar fashion to those in column 4, also relative to the control wage bin. The effects in panel (a) are restricted to individuals employed in both years reported. In panel (b), the estimates are reported for the outcome that an individual is employed (i.e., a dummy variable taking one if an individual is employed and zero otherwise). In panel (c), the difference in full-time equivalents (fte) is used, with full-time work corresponding to 1.0, part-time to 0.5, marginal to 0.2 and 0 if the individual is unemployed or not participating (Dustmann, Lindner, Schönberg, Umkehrer, & vom Berge, 2020). In panel (d) the estimates are reported for a dummy variable that takes the value one if an individual draws unemployment benefits or assistance. All outcomes are estimated using controls at the baseline level as well as time-variant controls for firm changes, industry changes, state changes and log of change in a state's real GDP per capita. Robust standard errors are used. ***p<0.01, **p<0.05, *p<0.010

*Two-Year Difference Model*

The real wage growth is regressed using the two-year difference model described earlier. The results can be seen in panel (a) in table 2. The 2015 vs 2013 effect seems to imply a significant positive increase of 26.4%-points over the baseline for the wage bin earning under the minimum wage prior to its introduction. The effect is positive but insignificant and lower when using the 2016 vs 2014 change at 15.4%-points. Again, both effects are imprecisely estimated, but seem to hold even after using the difference-in-difference estimator, which produces a significant increase 26.4%-points for 2015 vs 2013 and an insignificant increase of 13.2%-points for 2016 vs 2014. The other effects were generally insignificant, with some weakly significant effects that do not hold after using the difference-in-difference estimator. More importantly, the placebo effect 2014 vs 2012 is insignificant across all wage bins, giving confidence in the results.

Panel (b) shows the effect on employment probability, as was the case earlier. All effects are insignificant, implying that employment prospects were not affected this time around. This is robust to even using the difference-in-difference estimators, with only some weakly significant coefficients becoming insignificant. The placebo effect this time is also insignificant, which gives confidence that estimates are not biased by a common trend. This was also the case for the estimates in panel (c) for the effect on FTE

In panel (d) the probability of being unemployed appears to decrease for those in the treated wage bin by 6.4%-points for 2015 vs 2013 difference and 7.2%-points for 2016 vs 2014. These effects are only weakly significant (at 10% level), but they are persistent even after using the difference-in-difference estimator, with very minimal change in magnitude.

**Table 2 Employment and Wage Growth Results based on the two-year difference model**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Changes Relative to (2011 vs 2013) | | | Difference-in-Difference | |
| Wage bin in (t-2) | [4.5,8.5) | [8.5,12.5) | [12.5,20.5) | (1) minus (3) | (2) minus (3) |

| | | | | | |
|---|---|---|---|---|---|
| **Panel (a): Hourly Wage Growth** | | | | | |
| 2015 vs 2013 (Min Wage Effect 1) | 0.264** | 0.061 | 0.000 | 0.264** | 0.061 |
| | (0.1265) | (0.0497) | (0.0207) | (0.1257) | (0.0542) |
| 2016 vs 2014 (Min Wage Effect 2) | 0.154 | 0.096* | 0.022 | 0.132 | 0.074 |
| | (0.1000) | (0.0530) | (0.0223) | (0.1049) | (0.0610) |
| 2017 vs 2015 (Lagged Effect) | 0.164* | 0.041 | 0.021 | 0.143 | 0.020 |
| | (0.0936) | (0.0400) | (0.0285) | (0.0986) | (0.0458) |
| 2014 vs 2012 (Placebo) | -0.018 | -0.026 | -0.003 | -0.014 | -0.022 |
| | (0.0744) | (0.0319) | (0.0211) | (0.0758) | (0.0365) |
| 2013 vs 2011 (Baseline) | 0.314 | 0.168 | 0.067 | | |
| **Panel (b): Remaining Employed** | | | | | |
| 2015 vs 2013 (Min Wage Effect 1) | 0.043 | 0.020 | 0.009 | 0.034 | 0.011 |
| | (0.0304) | (0.0126) | (0.0079) | (0.0308) | (0.0131) |
| 2016 vs 2014 (Min Wage Effect 2) | 0.017 | 0.021* | 0.013* | 0.004 | 0.007 |
| | (0.0350) | (0.0124) | (0.0072) | (0.0356) | (0.0134) |
| 2017 vs 2015 (Lagged Effect) | 0.042 | 0.012 | 0.011 | 0.031 | 0.001 |
| | (0.0333) | (0.0129) | (0.0087) | (0.0334) | (0.0131) |
| 2014 vs 2012 (Placebo) | 0.038 | 0.015 | 0.001 | 0.037 | 0.014 |
| | (0.0311) | (0.0123) | (0.0078) | (0.0312) | (0.0131) |
| 2013 vs 2011 (Baseline) | 0.929 | 0.962 | 0.971 | | |
| **Panel (c): Change in Full-Time Equivalent** | | | | | |
| 2015 vs 2013 (Min Wage Effect 1) | 0.037 | 0.018 | 0.009 | 0.028 | 0.009 |
| | (0.0309) | (0.0123) | (0.0081) | (0.0313) | (0.0131) |
| 2016 vs 2014 (Min Wage Effect 2) | 0.007 | 0.020 | 0.014* | 0.008 | 0.005 |
| | (0.0328) | (0.0120) | (0.0075) | (0.0335) | (0.0133) |
| 2017 vs 2015 (Lagged Effect) | 0.056* | 0.006 | 0.008 | 0.048 | -0.001 |
| | (0.0325) | (0.0128) | (0.0088) | (0.0329) | (0.0132) |
| 2014 vs 2012 (Placebo) | 0.037 | 0.014 | -0.002 | 0.039 | 0.016 |
| | (0.0313) | (0.0124) | (0.0080) | (0.0315) | (0.0133) |
| 2013 vs 2011 (Baseline) | -0.095 | -0.043 | -0.030 | | |
| **Panel (d): Drawing Unemployment Benefits/Assistance** | | | | | |
| 2015 vs 2013 (Min Wage Effect 1) | -0.064* | 0.004 | 0.004 | -0.068* | -0.008 |
| | (0.0378) | (0.0157) | (0.0079) | (0.0380) | (0.0156) |
| 2016 vs 2014 (Min Wage Effect 2) | -0.072* | -0.019 | 0.000 | -0.073* | -0.020 |
| | (0.0381) | (0.0151) | (0.0074) | (0.0388) | (0.0156) |
| 2017 vs 2015 (Lagged Effect) | -0.058 | -0.003 | -0.002 | -0.057 | -0.001 |
| | (0.0402) | (0.0159) | (0.0089) | (0.0403) | (0.0156) |
| 2014 vs 2012 (Placebo) | 0.021 | -0.0138 | 0.008 | 0.014 | -0.021 |
| | (0.0424) | (0.0148) | (0.0078) | (0.0424) | (0.0147) |
| 2013 vs 2011 (Baseline) | 0.112 | 0.052 | 0.023 | | |

**Notes:** In panel (a), the excess hourly wage growth is reported for the years 2014 vs 2012, 2015 vs 2013, 2016 vs 2014, and 2017 vs 2015 relative to the baseline wage growth for the years 2013 vs 2011. It is reported for each of the wage bins in based on the baseline year (t-2): the treated wage bin [4.5,8.5) in column 1, the spill over wage bin [8.5,12.5) in column 2, and the control/untreated wage bin [12.5,20.5) in column 3. In column 4, the difference-in-difference estimates are reported for the treated wage bin, calculated as the difference in the effect for each year between the treated wage bin and the control wage bin. In column 5, the difference-in-difference estimates are reported in a similar fashion to those in column 4, also relative to the control wage bin. The effects in panel (a) are restricted to individuals employed in both years reported. In panel (b), the estimates are reported for the outcome that an individual is employed (i.e., a dummy variable taking one if an individual is

employed and zero otherwise). In panel (c), the difference in full-time equivalents (fte) is used, with full-time work corresponding to 1.0, part-time to 0.5, marginal to 0.2 and 0 if the individual is unemployed or not participating (Dustmann, Lindner, Schönberg, Umkehrer, & vom Berge, 2020). In panel (d) the estimates are reported for a dummy variable that takes the value one if an individual draws unemployment benefits or assistance. All outcomes are estimated using controls at the baseline level as well as time-variant controls for firm changes, industry changes, state changes and log of change in a state's real GDP per capita. Robust standard errors are used. ***p<0.01, **p<0.05, *p<0.010

*Discussion*

The effects reported using both models show that the wage growth significantly increased relative to the baseline for the wage bin under the minimum wage after the minimum wage was introduced. The effects are substantial and imply that the real wage growth increased greatly right after the minimum wage, as seen with the one-year difference of 2015 vs 2014. Furthermore, they were consistently robust and while the magnitude varied between both models due to imprecision, the effect remained significantly positive with the placebo effects being insignificant. The positive wage growth is in line with Dustmann et al.'s (2020) findings as well as the standard theory, where the minimum wage increases the wage for those who remain employed. Hypothesis 1a stating that *the minimum wage growth led to an increase in real wage growth for low-wage workers earning below the minimum wage prior to its introduction* is thus accepted.

The wage growth effect does not seem to be accompanied with a drop in employment prospects. This is shown in both models, although the one-year difference model did have some indications of being somewhat biased, the difference-in-difference seem robust as the two-year difference model seems to also predict insignificant changes to employment. This was across all outcomes, with even the two-year model robustly showing that the minimum wage may have lightly reduced the probability of drawing unemployment benefits or assistance. The confirmation across separate outcomes, where the probability of employment does not fall, FTE does not fall and the probability of drawing unemployment does not increase, implies that there were no negative employment effects for employed workers. This is also in line with Dustmann et al.'s (2020) findings, but it does contradict the competitive labour market model. As there is no measure in this sample for firm power, there's not enough evidence to point to the presence of a monopsonistic model. Therefore, Hypothesis 1(b) stating that *the minimum wage leads to lower employment for low-wage workers earning below the minimum wage prior to its introduction* is rejected. The combination of both of these effects implies that the minimum wage reduced wage inequality by increasing the real wage growth of low-wage workers without hampering their employment.

## vi. Life Satisfaction Analysis

The drop in wage inequality has great consequences for the life satisfaction and support of populist parties. It should imply an increase in the former and a decrease in the latter. However, this will need to be formally measured to assess the effects. For Life Satisfaction, the measures used will be the self-reported Life Satisfaction, Job Satisfaction and Income Satisfaction. The outcome will be a dummy that the relevant satisfaction measure is above the median (8 for Life Satisfaction, 7 for the others). The analysis is again run using the same model to test Hypothesis 2, with additional controls for the baseline satisfaction measure being predicted, baseline levels of concerns about finances, economic growth, crime, the

environment, health and job security. trust and trust towards Migrants were only measured in 2013 in the sample, hence they're used as fixed effects. These allow for controlling for potential parallel trends violations that may vary across wage bins, namely the migrant crisis, with that captured by controlling for trust towards migrants. The controls from the employment analysis are retained.

*One-Year Difference Model*

To assess hypothesis 2a, a dummy for whether the life satisfaction of an individual is at the median of 8 or higher (until 10) is regressed on the one-year difference model and the results are presented in table 3 panel (a). The minimum wage seemingly does not affect the life satisfaction of those earning below the minimum wage in the year prior. In fact, it only seems to affect the two other wage bins positively with a significant probability increase of 8.3%-points for the [8.5,12.5) wage bin and 7.2%-points for the [12.5,20.5). However, this result involved one of the placebos being significant, meaning that there is a potential violation of common trends. Using the difference-in-difference estimator in this case has different implications, as in the literature discussed it seems that life satisfaction decreases with inequality. Hence, the introduction of a minimum wage may affect the highest wage bin, even if slightly. The interpretation of the difference-in-difference estimator thus becomes the effect of the minimum wage on the life satisfaction gap between the wage bins right below and right above the minimum wage, and the highest wage bin [12.5,20.5). The minimum wage does not appear to influence the gap in life satisfaction between any of the wage bins, with all the difference-in-difference effects being insignificant.

To assess hypothesis 2b, a dummy for whether job satisfaction of an individual is at the median of 7 or higher (until 10) is regressed on the one-year difference model and the results are presented in panel (b). There only appears to be a weakly significant lagged effect (2016 vs 2015) for those earning above the minimum wage, with an increase of 5.7%-points in the probability of being at median job satisfaction or higher. This effect, however, appears to be significant in that it reduces the gap between the second wage bin [8.5,12.5) and the highest wage bin between the years 2016 vs 2015, with the probability increasing 7.2%-points relative to the highest wage bin, thereby closing the gap in job satisfaction. This effect appears to be unbiased as the placebo effects are all insignificant.

To assess hypothesis 2c, a dummy for whether income satisfaction of an individual is at the median of 7 or higher (until 10) is regressed on the one-year difference model and the results are presented in panel (c). There appear to be no significant effects on neither the probability of attaining a median income satisfaction or above, nor on the gap in the probability with the highest wage bin. These insignificant effects appear to be unbiased given that the placebo effects were also insignificant.

**Table 3 Life, Job and Income Satisfaction Results based on the one-year difference model**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Changes Relative to (2011 vs 2012) | | | Difference-in-Difference | |
| Wage bin in (t-2) | [4.5,8.5) | [8.5,12.5) | [12.5,20.5) | (1) minus (3) | (2) minus (3) |
| **Panel (a): Median Life Satisfaction or Above** | | | | | |
| 2015 vs 2014 (Min Wage Effect) | 0.034 | 0.083*** | 0.072*** | -0.038 | 0.012 |
| | (0.0687) | (0.0304) | (0.0196) | (0.0557) | (0.0355) |

| | | | | | |
|---|---|---|---|---|---|
| 2016 vs 2015 (Lagged Effect 1) | 0.037 | 0.043 | 0.029 | 0.040 | 0.014 |
| | (0.0591) | (0.0289) | (0.0189) | (0.0576) | (0.0327) |
| 2017 vs 2016 (Lagged Effect 2) | -0.002 | 0.044 | (0.003) | 0.034 | 0.041 |
| | (0.04420) | (0.0313) | (0.0216) | (0.0607) | (0.0339) |
| 2014 vs 2013 (Placebo Effect 1) | 0.058 | 0.031 | 0.021 | 0.038 | 0.011 |
| | (0.0542) | (0.0299) | (0.0211) | (0.0560) | (0.0326) |
| 2013 vs 2012 (Placebo Effect 2) | 0.047 | 0.64** | 0.027 | 0.021 | 0.037 |
| | (0.0537) | (0.0283) | (0.0180) | (0.0564) | (0.0333) |
| 2012 vs 2011 (Baseline) | 0.475 | 0.473 | 0.497 | | |
| **Panel (b): Median Job Satisfaction or Above** | | | | | |
| 2015 vs 2014 (Min Wage Effect) | 0.032 | 0.017 | 0.002 | 0.030 | 0.016 |
| | (0.0558) | (0.0311) | (0.0192) | (0.0586) | (0.0357) |
| 2016 vs 2015 (Lagged Effect 1) | -0.033 | 0.057* | -0.015 | -0.018 | 0.072** |
| | (0.0564) | (0.0294) | (0.0185) | (0.0582) | (0.0328) |
| 2017 vs 2016 (Lagged Effect 2) | -0.015 | 0.023 | -0.021 | 0.006 | 0.044 |
| | (0.0570) | (0.0320) | (0.0212) | (0.0584) | (0.0341) |
| 2014 vs 2013 (Placebo Effect 1) | 0.014 | 0.031 | 0.011 | 0.002 | 0.020 |
| | (0.0536) | (0.0315) | (0.0206) | (0.0552) | (0.0338) |
| 2013 vs 2012 (Placebo Effect 2) | -0.006 | 0.014 | 0.008 | -0.014 | 0.006 |
| | (0.0524) | (0.0298) | (0.0174) | (0.0550) | (0.0343) |
| 2012 vs 2011 (Baseline) | 0.635 | 0.633 | 0.681 | | |
| **Panel (c): Median Income Satisfaction or Above** | | | | | |
| 2015 vs 2014 (Min Wage Effect) | 0.029 | 0.018 | 0.032 | -0.003 | -0.051 |
| | (0.0563) | (0.032) | (0.0195) | (0.0592) | (0.0363) |
| 2016 vs 2015 (Lagged Effect 1) | -0.022 | -0.024 | 0.020 | -0.042 | -0.044 |
| | (0.0544) | (0.0296) | (0.0187) | (0.0566) | (0.0331) |
| 2017 vs 2016 (Lagged Effect 2) | 0.044 | -0.051 | -0.016 | 0.060 | -0.035 |
| | (0.0616) | (0.0322) | (0.0215) | (0.0633) | (0.0347) |
| 2014 vs 2013 (Placebo Effect 1) | 0.036 | -0.011 | 0.016 | 0.020 | -0.026 |
| | (0.0534) | (0.0314) | (0.0209) | (0.0552) | (0.0339) |
| 2013 vs 2012 (Placebo Effect 2) | -0.057 | -0.015 | 0.002 | -0.060 | -0.017 |
| | (0.0517) | (0.029) | (0.0177) | (0.0544) | (0.0341) |
| 2012 vs 2011 (Baseline) | 0.461 | 0.538 | 0.581 | | |

**Notes:** In panel (a), a dummy for life satisfaction being equal to the median of 8 or above is regressed with the effects reported for the years 2013 vs 2012, 2014 vs 2013, 2015 vs 2014, 2016 vs 2015, and 2017 vs 2016 relative to the baseline for the years 2013 vs 2012. It is reported for each of the wage bins based on the baseline year (t-1): the treated wage bin [4.5,8.5) in column 1, the spill over wage bin [8.5,12.5) in column 2, and the control/untreated wage bin [12.5,20.5) in column 3. In column 4, the difference-in-difference estimates are reported for the treated wage bin, calculated as the difference in the effect for each year between the treated wage bin and the control wage bin. In column 5, the difference-in-difference estimates are reported in a similar fashion to those in column 4, also relative to the control wage bin. In panel (b), the same analysis is done with the estimates for the outcome that an individual has median job satisfaction of 7 or above being reported. In panel (c), the same analysis is done with the estimates for the outcome that an individual has median income satisfaction of 7 or above being reported. All outcomes are estimated using controls at the baseline level as well as time-variant controls for firm changes, industry changes, state changes and log of change in a state's real GDP per capita. Robust standard errors are used. ***p<0.01, **p<0.05, *p<0.010

*Two-Year Difference Model*

The dummy for having median life satisfaction of median 8 or higher is regressed on the two-year difference model and the results are presented in table 4 panel (a). The estimated effect for 2015 vs 2013 shows that for the wage bin below the minimum wage there was a weakly significant drop in life satisfaction. However, this is accompanied by a significant placebo effect, meaning that there is potentially some bias causing this effect. This seems to not be resolved by looking at the difference-in-difference estimators, thus these effects are not taken into account for assessing hypothesis 2(a).

The dummy for having median job satisfaction of median 7 or higher is regressed on the two-year difference model and the results are presented in table 4 panel (b). The estimates all point towards an insignificant effect, with the exception of a weakly positive significant effect of 4.1%-points for the highest wage bin. Furthermore, the difference-in-difference estimators do not point to that effect causing a significant change in the gap in the probability of job satisfaction being at least at the median between the highest wage bins and the other two wage bins. These results appear to be unbiased as the placebo estimates were all insignificant.

The dummy for having median income satisfaction of median 7 or higher is regressed on the two-year difference model and the results are presented in table 4 panel (c). The minimum wage seems to have led to an increase of 12.3%-points for the lowest wage bin, but the effect is only weakly significant. All other effects were insignificant with the exception of the difference-in-difference estimator of 2017 vs 2015, which increases significantly by 15.5%-points for the wage bin under the minimum wage. This implies that the gap between the highest and lowest wage bins in the probability of the job satisfaction being at least at the median seemingly decreased, with the lowest wage bin making relative gains on the highest one.

**Table 4 Life, Job and Income Satisfaction Results based on the two-year difference model**

| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** |
|---|---|---|---|---|---|
| | Changes Relative to (2011 vs 2013) | | | Difference-in-Difference | |
| Wage bin in (t-2) | [4.5,8.5) | [8.5,12.5) | [12.5,20.5) | (1) minus (3) | (2) minus (3) |
| **Panel (a): Median Life Satisfaction or Above** | | | | | |
| 2015 vs 2013 (Min Wage Effect 1) | -0.099* | 0.010 | 0.031 | -0.130*** | -0.021 |
| | (0.0590) | (0.0338) | (0.0247) | (0.0612) | (0.0367) |
| 2016 vs 2014 (Min Wage Effect 2) | 0.010 | 0.035 | 0.008 | 0.001 | 0.027 |
| | (0.0613) | (0.0333) | (0.0237) | (0.0644) | (0.0380) |
| 2017 vs 2015 (Lagged Effect) | -0.048 | -0.025 | -0.036 | -0.012 | 0.011 |
| | (0.0668) | (0.0352) | (0.0276) | (0.0680) | (0.0370) |
| 2014 vs 2012 (Placebo) | -0.132** | 0.000 | -0.007 | -0.125** | 0.007 |
| | (0.0601) | (0.0331) | (0.0245) | (0.0622) | (0.0364) |
| 2013 vs 2011 (Baseline) | 0.577 | 0.517 | 0.529 | | |
| **Panel (b): Median Job Satisfaction or Above** | | | | | |
| 2015 vs 2013 (Min Wage Effect 1) | -0.028 | -0.004 | 0.041* | -0.068 | -0.045 |
| | (0.0627) | (0.0337) | (0.0242) | (0.0648) | (0.0364) |
| 2016 vs 2014 (Min Wage Effect 2) | 0.064 | 0.035 | 0.014 | 0.051 | 0.021 |
| | (0.0592) | (0.0337) | (0.0229) | (0.0622) | (0.0378) |
| 2017 vs 2015 (Lagged Effect) | 0.017 | 0.011 | 0.002 | 0.015 | 0.009 |

| | | | | | |
|---|---|---|---|---|---|
| | (0.0652) | (0.0351) | (0.0268) | (0.0664) | (0.0362) |
| 2014 vs 2012 (Placebo) | -0.048 | 0.047 | 0.028 | -0.076 | 0.018 |
| | (0.0619) | (0.0366) | (0.0241) | (0.0641) | (0.0364) |
| 2013 vs 2011 (Baseline) | 0.679 | 0.655 | 0.654 | | |
| **Panel (c): Median Income Satisfaction or Above** | | | | | |
| 2015 vs 2013 (Min Wage Effect 1) | 0.052 | 0.012 | 0.010 | 0.043 | 0.002 |
| | (0.0618) | (0.0356) | (0.0242) | (0.0636) | (0.0381) |
| 2016 vs 2014 (Min Wage Effect 2) | 0.123* | 0.008 | 0.012 | 0.111 | -0.004 |
| | (0.0652) | (0.0343) | (0.0231) | (0.0680) | (0.0384) |
| 2017 vs 2015 (Lagged Effect) | 0.117 | -0.042 | -0.034 | 0.151** | -0.008 |
| | (0.0715) | (0.0361) | (0.0271) | (0.0722) | (0.0375) |
| 2014 vs 2012 (Placebo) | 0.045 | 0.001 | -0.018 | 0.064 | 0.019 |
| | (0.0627) | (0.0341) | (0.0242) | (0.0645) | (0.0369) |
| 2013 vs 2011 (Baseline) | 0.471 | 0.550 | 0.605 | | |

**Notes:** In panel (a), a dummy for life satisfaction being equal to the median of 8 or above is regressed with the effects reported for the years 2014 vs 2012, 2015 vs 2013, 2016 vs 2014, and 2017 vs 2015 relative to the baseline for the years 2013 vs 2011. It is reported for each of the wage bins based on the baseline year (t-2): the treated wage bin [4.5,8.5) in column 1, the spill over wage bin [8.5,12.5) in column 2, and the control/untreated wage bin [12.5,20.5) in column 3. In column 4, the difference-in-difference estimates are reported for the treated wage bin, calculated as the difference in the effect for each year between the treated wage bin and the control wage bin. In column 5, the difference-in-difference estimates are reported in a similar fashion to those in column 4, also relative to the control wage bin. In panel (b), the same analysis is done with the estimates for the outcome that an individual has median job satisfaction of 7 or above being reported. In panel (c), the same analysis is done with the estimates for the outcome that an individual has median income satisfaction of 7 or above being reported. All outcomes are estimated using controls at the baseline level as well as time-variant controls for firm changes, industry changes, state changes and log of change in a state's real GDP per capita. Robust standard errors are used. ***p<0.01, **p<0.05, *p<0.010

*Discussion*

For assessing hypothesis 2(a), the two-year difference model cannot be used, as the placebo effect is significant for both the regular estimate and the difference-in-difference estimate. The one-year-difference model also has this issue, but it does not extend to the difference-in-difference estimator, which in this context measures how the gap between the probability of a median or above life satisfaction is affected. There appears to be no change due to the insignificant effect, implying that the minimum wage did not affect the life satisfaction. This also seems to be the case when seeing that the estimates for the effect that might be biased are insignificant or weakly significant. Hypothesis 2(a) stating that *the minimum wage leads to a higher self-reported life satisfaction for low-wage workers earning below the minimum wage prior to its introduction* is thus rejected

Median or above job satisfaction probability appears to have changed very slightly. The one-year difference model points to a weakly positive significant effect for the 2016 vs 2015 difference in the wage bin right above the minimum wage. The corresponding difference-in-difference estimate is also positive but highly significant implying that the gap in the probability of attaining a median or above job satisfaction is decreased. Given the wage growth effects discussed above, the implication would be that some members of the wage bin under the minimum wage are now part of this wage bin. This would imply that those workers only had a gain in job satisfaction after they had spent a year earning a higher wage. The two-year difference model points to positive effect for the 2016-2014 difference for that lower wage bin,

but they are not significant, though it is comparable in magnitude. Additionally, the highest wage bin does appear to have a weakly significant increase in the probability of median or above job satisfaction in the year 2015 vs 2013, this change however, does not involve a significant effect for the difference-in-difference estimator, meaning the gap in probabilities is unchanged. Given that the pay for the spill over wage bin does not increase, nor do employment prospects change, the most plausible scenario seems that the arrival of previous members of the lower wage bins led to an increase in the probability of having a median or above satisfaction. This would be a delayed effect as those workers would perhaps only feel obtain the gains from their new better-paid employment in. Thus, hypothesis (2b) stating that *Hypothesis 2b: The minimum wage leads to a higher self-reported job satisfaction for low-wage workers earning below the minimum wage prior to its introduction* is accepted.

Median or above income satisfaction probability appears to have not changed at all. There was a positive effect for the 2016 vs 2014 difference for the wage bin below the minimum wage in the two-year difference model, but it was only weakly significant. The only significant effect was the difference-in-difference for the difference 2017 vs 2015 for the low wage bin. This implies that the gap in income satisfaction probability shrunk between the wage bin lower than the minimum wage and the highest wage bin. Potentially, this implies that those who were working for non-compliant firms, eventually managed to get the minimum wage, leading to an increase in the income satisfaction. However, significant wage growth increases are not noted for 2017 in either model, so there's nothing to show that this increase was due to noncompliance. Given that there's limited evidence of change and the only alternative scenario involves non-compliance with the wage, Hypothesis 2(c) stating that *the minimum wage leads to a higher self-reported income satisfaction for low-wage workers earning below the minimum wage prior to its introduction*

## vii. Populist Voting Analysis

The drop in wage inequality observed in the employment analysis seemingly has had mixed effects on life satisfaction. With some increased satisfaction in jobs, this might alleviate some of the concern and insecurity regarding jobs. Combined with an increase in real wage growth without an extra risk of unemployment, the results so far should point to the hypotheses being accepted. To test the hypotheses, the party affiliation is used, where dummies for each party wing is generated. Those who are affiliated with the Afd, NDP or Die Rechte, receive a 1 for the far-right dummy, while those affiliated with Die Linke receive a value of 1 for the far-left dummy. Similarly, dummies for being affiliated with the SPD, who had the minimum wage as part of their platform and were the junior coalition partner, as well as the CDU/CSU alliance were generated and tested using the one-year difference and two-year difference models. Additional controls include the baseline interaction of the affiliated party with the intensity of party support, while the baseline life satisfaction was removed. The difference-in-difference again reports the changes relative to the highest wage bin rather than the overall change with the assumption of no minimum wage effect for the highest wage bin.

*One-Year Difference Model*

The dummy for being affiliated with far-right parties is regressed on the one-year difference model and the results are presented in table 5 panel (a). The minimum wage appears to have decreased the probability of being affiliated with a far-right party, but the effect is

insignificant. There are also positive lagged weakly significant effects of 3.6%-points for 2016 vs 2015 and 3.7%-points for 2017 vs 2016 at the wage bin just above the minimum wage. At the same time, the only significant effect due to the minimum wage appears to be an increase of 2.0%-points for the highest wage bin in 2016 vs 2015. Nevertheless, the difference-in-difference estimators are all insignificant, implying that there had been no relative changes between wage bins. These effects appear to be unbiased as the placebo effects are insignificant.

The dummy for being affiliated with the far-left party is regressed on the one-year difference model and the results are presented in table 5 panel (b). The minimum wage appears to have significantly decreased the probability of being affiliated with the far-left party Die Linke in the difference between 2016 vs 2015 for the treated wage bin by 19.3%-points. The effect is the same as a difference-in-difference estimator, given that there was no change in the probability of being affiliated with the far left for the highest wage bin over those years. All the other effects are insignificant, including the placebos, which gives confidence that the effects are not biased.

The dummy for being affiliated with the centre-left party SPD is regressed on the one-year difference model and the results are presented in table 5 panel (c). The minimum wage appears to have decreased the probability of being affiliated with the centre-left party SPD for the two higher wage bins in the difference between 2016 vs 2015. The effect, however, is possible biased, due to the placebo for the highest wage bin being also significant and negative. The difference-in-difference estimator fortunately has an insignificant placebo, and while there seems to be a sizeable increase in relative support for the SPD for the lowest wage bin in 2016 vs 2015, this was not significant along with the rest of the difference-in-difference estimates.

The dummy for being affiliated with the centre-right alliance of CDU and CSU is regressed on the one-year difference model and the results are presented in table 5 panel (d). The minimum wage appears to not have made any changes besides an insignificant positive gain in probability to be affiliated with CDU/CSU. Moreover, the difference-in-difference estimates appear to be exposed to bias due to one of the placebo effects being significant and negative.

**Table 5 Party Affiliation Results based on the one-year difference model**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Changes Relative to (2011 vs 2012) | | | Difference-in-Difference | |
| Wage bin in (t-2) | [4.5,8.5) | [8.5,12.5) | [12.5,20.5) | (1) minus (3) | (2) minus (3) |
| **Panel (a): Support Far Right** | | | | | |
| 2015 vs 2014 (Min Wage Effect) | -0.005 | 0.036 | 0.004 | -0.008 | 0.032 |
| | (0.0155) | (0.0233) | (0.0062) | (0.0167) | (0.0317) |
| 2016 vs 2015 (Lagged Effect 1) | 0.105 | 0.036* | 0.020** | 0.085 | 0.016 |
| | (0.0888) | (0.0200) | (0.0091) | (0.0889) | (0.0217) |
| 2017 vs 2016 (Lagged Effect 2) | -0.002 | 0.037* | 0.014 | -0.016 | 0.023 |
| | (0.0229) | (0.0201) | (0.0098) | (0.0233) | (0.0203) |
| 2014 vs 2013 (Placebo Effect 1) | 0.044 | 0.016 | 0.009 | 0.036 | 0.007 |
| | (0.0473) | (0.0152) | (0.0081) | (0.0479) | (0.0154) |
| 2013 vs 2012 (Placebo Effect 2) | 0.008 | -0.011 | -0.007 | 0.014 | -0.004 |
| | (0.0132) | (0.0098) | (0.0059) | (0.0148) | (0.0106) |
| 2012 vs 2011 (Baseline) | 0.040 | 0.028 | 0.035 | | |

| | | | | | |
|---|---|---|---|---|---|
| **Panel (b): Support Far Left** | | | | | |
| 2015 vs 2014 (Min Wage Effect) | -0.068 | 0.009 | -0.002 | -0.067 | 0.011 |
| | (0.0655) | (0.0134) | (0.0118) | (0.0652) | (0.0164) |
| 2016 vs 2015 (Lagged Effect 1) | -0.193** | 0.014 | 0.000 | -0.194** | 0.014 |
| | (0.0951) | (0.0220) | (0.0110) | (0.0947) | (0.222) |
| 2017 vs 2016 (Lagged Effect 2) | 0.045 | 0.012 | 0.014 | 0.030 | -0.003 |
| | (0.1030) | (0.0235) | (0.0144) | (0.1020) | (0.0244) |
| 2014 vs 2013 (Placebo Effect 1) | -0.073 | -0.013 | 0.012 | -0.085 | -0.024 |
| | (0.0670) | (0.0232) | (0.0134) | (0.0655) | (0.0238) |
| 2013 vs 2012 (Placebo Effect 2) | -0.117 | 0.006 | 0.000 | -0.117 | 0.006 |
| | (0.0822) | (0.0160) | (0.0096) | (0.0828) | (0.0184) |
| 2012 vs 2011 (Baseline) | 0.155 | 0.084 | 0.079 | | |
| **Panel (c): Support SPD** | | | | | |
| 2015 vs 2014 (Min Wage Effect) | -0.045 | -0.038 | -0.013 | -0.033 | -0.025 |
| | (0.0343) | (0.0296) | (0.0178) | (0.0385) | (0.0335) |
| 2016 vs 2015 (Lagged Effect 1) | 0.066 | -0.056* | -0.047** | 0.113 | -0.010 |
| | (0.0687) | (0.0328) | (0.0190) | (0.0707) | (0.0354) |
| 2017 vs 2016 (Lagged Effect 2) | -0.006 | 0.004 | -0.023 | 0.017 | 0.027 |
| | (0.0388) | (0.0335) | (0.0219) | (0.0402) | (0.0362) |
| 2014 vs 2013 (Placebo Effect 1) | -0.019 | -0.004 | -0.018 | -0.001 | 0.014 |
| | (0.0423) | (0.0324) | (0.0191) | (0.0439) | (0.0338) |
| 2013 vs 2012 (Placebo Effect 2) | -0.046 | -0.007 | -0.048*** | 0.002 | 0.041 |
| | (0.0470) | (0.0261) | (0.0164) | (0.0499) | (0.0301) |
| 2012 vs 2011 (Baseline) | 0.328 | 0.343 | 0.343 | | |
| **Panel (d): Support CDU+CSU** | | | | | |
| 2015 vs 2014 (Min Wage Effect) | 0.034 | -0.025 | 0.007 | 0.027 | -0.032 |
| | (0.0532) | (0.0328) | (0.0137) | (0.0553) | (0.0347) |
| 2016 vs 2015 (Lagged Effect 1) | -0.037 | -0.034 | -0.013 | -0.024 | -0.021 |
| | (0.0889) | (0.0259) | (0.0145) | (0.0903) | (0.0285) |
| 2017 vs 2016 (Lagged Effect 2) | 0.005 | -0.040 | 0.003 | 0.002 | -0.043 |
| | (0.0550) | (0.0264) | (0.0151) | (0.0567) | (0.0274) |
| 2014 vs 2013 (Placebo Effect 1) | 0.003 | -0.014 | 0.003 | 0.000 | -0.017 |
| | (0.0686) | (0.0229) | (0.0152) | (0.0707) | (0.0247) |
| 2013 vs 2012 (Placebo Effect 2) | 0.102 | -0.036 | 0.018 | 0.084 | -0.053** |
| | (0.0647) | (0.0240) | (0.0122) | (0.0667) | (0.0260) |
| 2012 vs 2011 (Baseline) | 0.313 | 0.383 | 0.367 | | |

**Notes:** In panel (a), a dummy for being affiliated with far-right parties is regressed with the effects reported for the years 2013 vs 2012, 2014 vs 2013, 2015 vs 2014, 2016 vs 2015, and 2017 vs 2016 relative to the baseline for the years 2013 vs 2012. It is reported for each of the wage bins based on the baseline year (t-1): the treated wage bin [4.5,8.5) in column 1, the spill over wage bin [8.5,12.5) in column 2, and the control/untreated wage bin [12.5,20.5) in column 3. In column 4, the difference-in-difference estimates are reported for the treated wage bin, calculated as the difference in the effect for each year between the treated wage bin and the control wage bin. In column 5, the difference-in-difference estimates are reported in a similar fashion to those in column 4, also relative to the control wage bin. In panel (b), the same analysis is done with the estimates for the outcome that an individual is affiliated with the far-left party Die Linke. In panel (c), the same analysis is done with the estimates for the dummy of being affiliated with the centre-left party SPD being reported. In panel (d) the analysis is done with the estimates for the dummy of being affiliated with the centre-right alliance CDU/CSU being reported. All outcomes are estimated using controls at the baseline level as well as time-variant controls for firm changes, industry changes, state changes and log of change in a state's real GDP per capita. Robust standard errors are used. ***p<0.01, **p<0.05, *p<0.010

*Two-Year Difference Model*

The dummy for being affiliated with far-right parties is regressed on the two-year difference model and the results are presented in table 6 panel (a). The minimum wage appears to have increased the probability of being affiliated with a far-right party for the two higher wage bins. However, the placebo effect for the higher wage bin is also significant so there appears to be potentially some bias there. The difference-in-difference estimates had insignificant placebos, giving confidence that unparallel trends were not an issue. It shows that in the minimum wage seemingly increased support for far-right parties for the high wage bin relative to the one right above the minimum wage by 3.2%-points over 2015 vs 2013. However, this seems to be cancelled out by a weakly significant gain from the wage bin right above the minimum wage of 4.6%-points.

The dummy for being affiliated with the far-left party is regressed on the two-year difference model and the results are presented in table 6 panel (b). The minimum wage appears to have significantly increased the probability of being affiliated with the far-left party Die Linke in the difference between 2017 vs 2015 for the wage bin right above the minimum wage by 7.4%-points. The effect is even stronger for the difference-in-difference estimator, with a relative increase of 8.3%-points. All the other effects are insignificant, with only one placebo effect being weakly significant, giving confidence that the effects are not biased.

The dummy for being affiliated with the centre-left party SPD is regressed on the two-year difference model and the results are presented in table 6 panel (c). The minimum wage appears to have only weakly increased the probability of being affiliated with the centre-left party SPD for the wage bin right above the minimum wage over 2015 vs 2013 by 7.6%-points. The weakly significant effect appears robust, as it stronger in magnitude at 8.8%-points, when measured relative to the largest wage bin as a difference-in-difference estimator. However, it remains to be weakly significant. All the other effects are insignificant, including the placebos, which gives confidence that there is no bias in the results arising from unparallel trends.

The dummy for being affiliated with the centre-right alliance of CDU and CSU is regressed on the two-year difference model and the results are presented in table 6 panel (d). The minimum wage appears to have reduced the probability of affiliation for the wage bin right over the minimum wage in 2016 vs 2014 by 9.3%-points. However, one of the placebo effects was also significant, thus shedding doubt on whether there is bias or not in the estimates. The difference-in-difference estimators do not appear to have that problem, where the effect is still present with a relative reduction of 11.5%-points for the same wage bin in 2016 vs 2014. The placebo is only weakly significant, in addition to the effect appearing to be somewhat robust.

**Table 6 Party Affiliation Results based on the two-year difference model**

| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** |
|---|---|---|---|---|---|
| | Changes Relative to (2011 vs 2013) | | | Difference-in-Difference | |
| Wage bin in (t-2) | [4.5,8.5) | [8.5,12.5) | [12.5,20.5) | (1) minus (3) | (2) minus (3) |
| **Panel (a): Support Far Right** | | | | | |
| 2015 vs 2013 (Min Wage Effect 1) | 0.043 | -0.009 | 0.022*** | 0.021 | -0.032** |
| | (0.0512) | (0.0129) | (0.0082) | (0.0524) | (0.0148) |
| 2016 vs 2014 (Min Wage Effect 2) | 0.041 | 0.065** | 0.019** | 0.023 | 0.046* |
| | (0.0303) | (0.0260) | (0.0085) | (0.0327) | (0.0270) |
| 2017 vs 2015 (Lagged Effect) | 0.150 | 0.005 | 0.028** | 0.122 | -0.023 |

| | | | | | |
|---|---|---|---|---|---|
| | (0.1201) | (0.0216) | (0.0115) | (0.1199) | (0.0244) |
| 2014 vs 2012 (Placebo) | 0.023 | 0.016 | 0.025** | -0.002 | -0.010 |
| | (0.0199) | (0.0166) | (0.0119) | (0.0210) | (0.0166) |
| 2013 vs 2011 (Baseline) | 0.018 | 0.030 | 0.019 | | |
| **Panel (b): Support Far Left** | | | | | |
| 2015 vs 2013 (Min Wage Effect 1) | -0.044 | 0.007 | -0.007 | -0.037 | 0.015 |
| | (0.0536) | (0.0266) | (0.0143) | (0.0545) | (0.0297) |
| 2016 vs 2014 (Min Wage Effect 2) | -0.051 | 0.016 | -0.016 | -0.036 | 0.032 |
| | (0.0537) | (0.0295) | (0.0160) | (0.0557) | (0.0329) |
| 2017 vs 2015 (Lagged Effect) | -0.141 | 0.074*** | -0.009 | -0.132 | 0.083** |
| | (0.0875) | (0.0323) | (0.0192) | (0.0888) | (0.0350) |
| 2014 vs 2012 (Placebo) | -0.111 | 0.048* | 0.002 | -0.112 | 0.047* |
| | (0.0720) | (0.0275) | (0.0169) | (0.0733) | (0.0282) |
| 2013 vs 2011 (Baseline) | 0.145 | 0.060 | 0.097 | | |
| **Panel (c): Support SPD** | | | | | |
| 2015 vs 2013 (Min Wage Effect 1) | 0.023 | 0.076* | -0.011 | 0.034 | 0.088* |
| | (0.0686) | (0.0418) | (0.0194) | (0.0716) | (0.0455) |
| 2016 vs 2014 (Min Wage Effect 2) | -0.037 | -0.006 | -0.036 | -0.001 | 0.030 |
| | (0.0506) | (0.0375) | (0.0229) | (0.0557) | (0.0413) |
| 2017 vs 2015 (Lagged Effect) | 0.046 | 0.000 | -0.026 | 0.072 | 0.026 |
| | (0.0841) | (0.0365) | (0.0243) | (0.0862) | (0.0398) |
| 2014 vs 2012 (Placebo) | -0.013 | -0.019 | -0.027 | 0.015 | 0.008 |
| | (0.0581) | (0.0397) | (0.0230) | (0.0600) | (0.0409) |
| 2013 vs 2011 (Baseline) | 0.373 | 0.317 | 0.337 | | |
| **Panel (d): Support CDU+CSU** | | | | | |
| 2015 vs 2013 (Min Wage Effect 1) | -0.026 | -0.031 | 0.021 | -0.047 | -0.052 |
| | (0.0560) | (0.0300) | (0.0170) | (0.0593) | (0.0341) |
| 2016 vs 2014 (Min Wage Effect 2) | 0.019 | -0.093** | 0.022 | -0.003 | -0.115*** |
| | (0.0379) | (0.0370) | (0.0203) | (0.0440) | (0.0404) |
| 2017 vs 2015 (Lagged Effect) | -0.115 | -0.013 | 0.020 | -0.135 | -0.034 |
| | (0.1115) | (0.0310) | (0.0200) | (0.1132) | (0.0337) |
| 2014 vs 2012 (Placebo) | 0.009 | -0.016 | 0.040** | -0.032 | -0.056* |
| | (0.0447) | (0.0316) | (0.0201) | (0.0477) | (0.0331) |
| 2013 vs 2011 (Baseline) | 0.336 | 0.377 | 0.346 | | |

**Notes:** In panel (a), a dummy for being affiliated with far-right parties is regressed with the effects reported for the years 2014 vs 2012, 2015 vs 2013, 2016 vs 2014 and 2017 vs 2015 relative to the baseline for the years 2013 vs 2011. It is reported for each of the wage bins based on the baseline year (t-2): the treated wage bin [4.5,8.5) in column 1, the spill over wage bin [8.5,12.5) in column 2, and the control/untreated wage bin [12.5,20.5) in column 3. In column 4, the difference-in-difference estimates are reported for the treated wage bin, calculated as the difference in the effect for each year between the treated wage bin and the control wage bin. In column 5, the difference-in-difference estimates are reported in a similar fashion to those in column 4, also relative to the control wage bin. In panel (b), the same analysis is done with the estimates for the outcome that an individual is affiliated with the far-left party Die Linke. In panel (c), the same analysis is done with the estimates for the dummy of being affiliated with the centre-left party SPD being reported. In panel (d) the analysis is done with the estimates for the dummy of being affiliated with the centre-right alliance CDU/CSU being reported. All outcomes are estimated using controls at the baseline level as well as time-variant controls for firm changes, industry changes, state changes and log of change in a state's real GDP per capita. Robust standard errors are used. ***p<0.01, **p<0.05, *p<0.010

*Discussion*

Both models show that far-right support seemingly increased for the higher two wage bins. The one-year difference model showed that this increased in support generally came a year or two after the minimum wage implementation, and it was more significant for the highest wage bin. The two-year difference model shows similar trends with the difference-in-difference estimator implying that there was a relative increase in probability of affiliation for the highest wage bin relative to the one just above the minimum wage, with a less significant effect coming later on that worked in the opposite direction between the two. The consistent finding however, is seemingly that there was no significant effect on the workers that were in the wage bin under the minimum wage. Hypothesis 3(a) stating that *the minimum wage leads to lower support for far-right parties Alternative für Deutschland and NPD for low-wage workers earning below the minimum wage prior to its introduction* is thus rejected.

Both models show different patterns when it comes to the far-left support seemingly. The one-year difference model showed that there was a significant lagged decrease in 2016 vs 2015 for the wage bin under the minimum wage. On the other hand, the two-year difference model showed that there was a significant lagged increase between 2017 vs 2015 for the wage bin right above the minimum wage. The two results together seemingly indicate that workers who had their wage grow a year after the minimum wage was introduced shifted towards the wage bin right above the minimum wage, thereby increasing the probability that this wage bin does vote for the far left. Alternatively, it is possible that the two work completely separately. Workers left behind after the reform (i.e., earn less than the minimum wage in 2015) are disenchanted with the left wing and shift to a different party, while those who benefit from the minimum wages become more empowered to pursue further left-wing policies that will deliver more income to them as well as address inequalities. Either way, hypothesis 3(b) stating that *the minimum wage leads to lower support for far-left party Die Linke for low-wage workers earning below the minimum wage prior to its introduction* is rejected. The caveat in this case is that low-wage workers who potentially no longer support Die Linke, choose to do so due to non-compliance rather than the minimum wage itself.

Both models provide conflicting results with regards to the probability of affiliation with the centre-left. The one-year difference model shows that the probability of affiliation with the SPD decreased in the two higher wage bins due to the minimum wage, with insignificant difference-in-difference estimators used due to the placebo effects being significant and signalling a potential bias. On the other hand, the two-year difference model points to a weakly significant increase in the probability of being affiliated with the centre-left, which appears to be robust to the difference-in-difference estimate, remaining both positive and weakly significant. However, given that this effect was only weakly significant, and the difference-in-difference estimate for the one-year difference model shows insignificant effects, it would appear that this effect is at best very small. Furthermore, given that the effects on the lower wage bin for both models were insignificant, Hypothesis 3(c) stating that *the minimum wage leads to more support for centre-left party SPD for low-wage workers earning below the minimum wage prior to its introduction* is rejected.

Both models follow a similar pattern when it comes to the probability of affiliation with the alliance of CDU and CSU. The one-year difference model showed insignificant effects across all wage bins and all time periods, with the exception of one placebo effect being violated for

the Difference-in-Difference estimates. Similarly, the two-year difference model only showed a negative effect due to the minimum wage for the wage bin immediately higher than the minimum wage between 2016 and 2014, though it might be biased as shown by the placebo effect that was significant. This effect was robust nonetheless, with the difference-in-difference estimate indicating that the probability of affiliation fell for that wage bin relative to the highest wage bin. Regardless, the common aspect was that there were no significant effects for the lower wage bin. Therefore, hypothesis 3 (d) stating that *the minimum wage leads to more support for centre-right alliance of CDU and CSU for low-wage workers earning below the minimum wage prior to its introduction* is rejected.

## viii. Conclusion

This thesis set out trying to investigate the effects of the minimum wage beyond just employment and wage growth. The research question was thus constructed as following:

**How did the minimum wage, introduced in Germany in 2015, affect employment, life satisfaction and populist support among low-wage workers?**

To answer this question, hypotheses regarding the effect of the minimum wage on real wage growth, employment, life satisfaction, job satisfaction, income satisfaction, and party affiliation were constructed. Using the SOEP data, an event study design was constructed based on Cengiz et al. (2019) and similar to the one used to study employment effects by Dustmann et al. (2020). Two models were used: a one-year difference model and a two-year difference model, with the latter being the one used by Dustmann et al. (2020). Firstly, the minimum wage was found to increase the real wage growth of workers earning below the minimum wage without negatively impacting their employment. This signalled an improvement in wage inequality, as the higher wage combined with the lack of unemployment point towards low-wage workers receiving more income without losing their jobs. The consequences of that should've been an increase in satisfaction and a drop in support of populist parties, as discussed in the literature. However, these effects seemingly did not materialise. Both life and income satisfaction did not rise, and support for populist parties as well as establishment parties did not change. Only job satisfaction seemed to follow the expected pattern by experiencing positive effects.

Hence, the minimum wage introduced in Germany in 2015 seemed to have been mostly confined in its effects on its targeted population, low-wage workers, to employment. In fact, even job satisfaction may be a product of the employment effects. One of the mechanisms through which employment did not decrease while wages increased in Germany at the time of the minimum wage, was the upgrading to better-paying and larger firms (Dustmann, Lindner, Schönberg, Umkehrer, & vom Berge, 2020). This finding seems to go somewhat hand-in-hand with the idea that job satisfaction increased, while life and income satisfaction remained unchanged. It would imply that the increase in satisfaction would be driven by working for a perceivably better firm. In addition, the effect is detected one year after the introduction of the minimum wage, which could show that some of these benefits take time to materialise (and supposably the better firm). Lastly, the minimum wage may have created a host of side-effects onto other wage bins or groups in society, namely potentially an increase in populist voting for either far right or left in the wage bin right above the minimum wage.

Several factors could explain why life satisfaction and populist voting do not follow the patterns laid out in the literature discussed above. Firstly, the effects measured were until two years after the initial introduction of the minimum wage. For both outcomes, the models estimated were linear, when the relationship could be non-linear. However, this is unlikely to have worked, since a host of controls and interactions were used, and non-linear specifications produced large prediction errors with those with large standard errors. In addition, it is possible that these effects require a much larger shift in labour market policy than simply an introduction of the minimum wage to produce the necessary effects. In fact, making the necessary changes simply to shift back the pattern of wage inequality in itself will likely require a host of policies rather than just one (Guriev, 2018). Nevertheless, it is also possible that for example economic events do not influence populist voting as much. Similar to how some populist voters acquired facts but insisted on their voting intentions (Barrera, Guriev, Henry, & Zhuravskaya, 2020), as it could be that the low wage workers who support populist parties, still insist on supporting them even when presented with the fact that their real wages grew due to the minimum wage. Lastly, it is possible -albeit highly unlikely - that economic factors are strong determinants on their own of overall life satisfaction and populist voting. An example of that is the notion of *cultural backlash*, which argues that the rise in populism is a response to progressive responses.

The research methodology while was generally robust had some downfalls. Firstly, the hourly wage is a proxy and is as such subject to measurement error. As Caliendo et al. (2019) highlights, measurement error is especially more likely with the SOEP dataset, especially since it relies on self-reported data. However, the results seem to be in line in terms of direction with the ones Dustmann et al. (2020) found, even though he used administrative data, which is much more comprehensive. This leads into the next point, which was the small sample size. As the sample size relies on presence over multiple time periods, this involved a huge loss of observations when running the models, even though there were over 33 thousand observations. This created larger standard errors, and thus less precise estimates, which can explain why the estimates were different in magnitude from Dustmann et al.'s (2020). This also put a constraint over the type of models that can be run, particularly logistic models could not be run due to the sample size given the no. of controls. Furthermore, the assumption of the difference-in-difference estimator in the employment analysis is strong, with the assumption that the higher wage bin not being affected being particularly strict. Nevertheless, with the benefit of two models and the relaxing of this assumption later on in the other analyses, it does not pose a huge problem.

The findings in this thesis open up avenues for future research. In particular, it shows the relative robustness of using the wage bin method introduced recently, as it allows tracing employment outcomes to the relevant workers (Cengiz, Dube, Linder, & Zipperer, 2019), as well as potentially testing other outcomes. Furthermore, while due to sample size limitations there were limits on being able to study heterogeneous effects, it would be investigating any potential heterogeneity in the satisfaction and populist support effects. In particular, looking at both migrant groups as well as social classes (Lipps & Oesch, 2018), can point to potential explanations to how inequalities led to certain social and political outcomes. Moreover, the use of labour market policies to simply evaluate employment and wages can be a bit restrictive, when the use of these policies to investigate other outcomes such as life satisfaction, trust and happiness can be an area of great added value. Lastly, the use of an alternative method or sample can point to different results, for example the geographic variation (Caliendo, Fedorets, Preuss,

Schröder, & Wittbrodt, 2018), can be used to investigate how different voting patterns or any other attitudes arose. The nature of using a survey involves the need to replicate across different samples, due to the data on attitudes being generally collected in these forms as well as the fact that the small samples may not necessarily produce a representative account of the effects.

There are also a lot of policy implications in the context of minimum wages and labour market policies in general. The case of Germany does show that with rising output, it does not necessarily need to unemployment, but in fact can lead to more redistribution. Furthermore, it is necessary for governments to balance out the drop in bargaining power workers face due to the fall in unionisation, any other labour market mechanisms, or macroeconomic trend (e.g., globalisation) such as what happened in Germany and the US (Antonczyk, Fitzenberger, & Sommerfeld, 2010).

In the context of the pandemic, there will need to be more involvement in social policies as well as labour market policies to regain trust. In particular the European governments had to declare lockdowns, since they were beneficial in the long run for the medical system, but these tended to have short-term negative impacts on trust (Guriev, 2018). Regaining the trust of the citizens will require a special effort, especially with the newly acquired funds from the EU potentially creating future political tension. Effectively without strong communication, the government might find itself stuck between investing in long-term options or overspending on social policies that are not proper solutions. Lastly, the pandemic has most likely exacerbated wage inequality yet again. A focus on policies such as the minimum wage to re-boost the wage growth of low-wage workers is going to be to avoid a second rise in wage inequality accompanied by the adverse side effects including mistrust, populism and a drop in life satisfaction.

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# Title

Omar, Mansour

Abstract

The thesis's aim is to study the impact of the minimum wage implemented in Germany in 2015 on real wage growth, employment, life satisfaction and populist voting. The former two outcomes can have great ramifications on real wage growth, which in turn influences the latter outcomes. Thus, the research question is **How did the minimum wage, introduced in Germany in 2015, affect employment, life satisfaction and populist support among low wage workers?** Data from the German Socioeconomic Panel (SOEP) is used to conduct an event design by splitting the wage distribution of low wage workers into wage bins and evaluating the effects over time using a baseline as a control. The estimated effects are an increase in real wage employment, no significant effect on employment, nor populist voting, nor life satisfaction. It appears that while wage inequality was improved for low wage workers, this did not translate into improvements in terms of life satisfaction nor populist party voting. The policy recommendations based on that is that the minimum wage is likely not sufficient on its own to reverse the course of declining life satisfaction and increase in populist voting due to increasing distrust and economic insecurity.

Key words

Minimum Wage; Employment; Populism; Life Satisfaction; Wages; Income Inequality