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The spill-over effects of Germany's first federal minimum wage

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Abstract

This paper investigates whether individuals who earn marginally above a minimum wage are experiencing spill-over effects as a result of its implementation, adjustment and existence. It focuses on Germany's federal statutory minimum wage regime that was introduces in 2015 and later adjusted in 2017, through the experiences of individuals who earned 100% to 110% of the new wage floor in 2014. Using data from the German Socio-Economic Panel (SOEP), this paper utilizes a difference-in-differences approach by exploiting an exogenous regional variation in treatment intensity between East- and West Germany. The main findings include that the minimum wage has negative spill-over effects on the hourly wages throughout the post-treatment period. The paper does not find any spill-over effects on employment probabilities or total labour earnings or monthly hours worked. Furthermore, it does not investigate which channels the spill-over effects operate through, but rather focuses on investigating their existence. These results imply that a minimum wage reform may have an unforeseen negative impact on this group of individuals, a lesson for policy makers that means to introduce similar schemes.

Keywords: minimum wage, spill-over effects, hourly wage

Table of Contents

1 The Minimum Wage Act

Germany’s first ever federal minimum wage reform (the so called Mindestlohngesetz or MiLoG) marks an exception from the traditional union led collective wage bargaining process. The MiLoG is a result of a compromise between the ruling coalition that emerged after the German parliamentary elections of September 2013 and was later approved by the German parliament as the Minimum Wage Act in July 2014. The MiLoG was introduced to combat unreasonably low wages and stabilize the social security system. The reform went into effect on the first of January 2015, providing a gross hourly minimum wage of 8.50 EUR. To follow the development of negotiated wages, the amount of the minimum wage was adjusted to 8.84 EUR per hour as of the first of January 2017 and further adjustments are planned in the future (Federal Ministry of Labour and Social Affairs, 2017).

The MiLoG is applicable to everyone working in Germany, with some exceptions. Shortly stated these exceptions are minors, trainees, volunteers, the long-term unemployed and some interns who are engaged in study related or shorter internships. For the long-term unemployed, workers who were unemployed for 1 year or longer, the minimum wage does not come into effect until after the first six months of employment. There were initially some exceptions based on previous collectively bargained as well, most notably for newspaper deliveries and seasonal workers for whom the minimum wage did not go into full effect until the first of January 2018 (Spielberger & Schilling, 2014).

The MiLoG has decreased the amount of people that had contractual wages lower than the floor of 8.50 EUR per hour from 2.8 million workers before the implementation, to 1.8 workers in early 2016 (Bureauel, 2017).

2 Previous Literature

2.1 The effects of minimum wages

The effects of implementing, or in any way adjusting, a minimum wage scheme is a highly researched issue in labour economics. Its implications on the labour market and the economy as a whole is widely debated as policy makers of different ideological stand points make different claims of its impact. Economic theory only brings us so far when trying to understand its effects considering that different schools of thought may reach different conclusions. These perspectives are ranging from a classical standpoint where an increase in the factor cost of labour will reduce its usage, hence reducing employment (Hamermesh, 1986), to models of imperfect competition including the aspect of a monopsony. In some models including the concept of monopsony, a minimum wage may actually increase the usage of labour as a factor in production, and therefore employment (Alan, 2011). Since it is hard to rely solely on economic theory when trying to understand the effects of such a government intervention into the labour market, it is crucial to conduct applied research.

The effects of a minimum wage implementation, or adjustment, are often measured in its effects on wages and employment. Such as Rybczynski and Sen (2018) who finds a negative relationship between minimum wages and employment in Canada, or Jardim et al. (2018) who similarly finds a negative relationship between minimum wages and employment as well as total income in Seattle. Contrarily, Card and Krueger (1994) finds no or a positive relationship between minimum wages and employment in the American fast food industry. Moreover, David Card (1992) found no negative relationship between minimum wages and employment among teenagers in California's retail industry. The same result is found by Boockmann et al. (2013) who examine the effects of previous sectoral minimum wages in the German electrical sector.

David Card (1992b) uses a ground-breaking research design to measure the effects of the federal minimum wage increase of 1990 for teenagers in the United States. The idea was to use the regional differences in treatment intensity as a natural experiment. The regional treatment intensity, or the "bite" of the reform, is defined as the fraction of individuals in a state who initially earn less than the new minimum wage regime. Using this research design, he found that the reform had no consequences for teenage employment or school enrolment.

David Neumark et al. (2004) aim at gaining a better understanding of the total effects of minimum wages by measuring the effects on different points of the wage distribution. They argue that firms and employers may apply different methods of adjustment depending on who they employ. They argue further that some of these methods potentially could lead to a decrease in working hours, employment and subsequently income opportunities for workers who are not directly affected by the minimum wage. This is a group that they find to often be ignored in minimum wage research. Methodologically they use monthly CPS survey data between 1979 and 1997 to examine the two-year responses to an increase in minimum wages. Responses are compared to the average response of that wage category. They find that while hourly income on average increases for income groups earning up to 150% of the minimum wage, employment decreases for those with hourly earnings between 100% and 150% of the minimum wage. At the same time, hours worked, and total labour earnings decrease for those individuals who before the minimum wage increase had hourly earnings between 100% and 110% of the minimum wage. Their results imply that the minimum wage has both negative and positive effects on those individuals who before its implementation had hourly earnings at or marginally above the new minimum wage. Similar results are found by Lopresti and Mumford (2016) who find that a large minimum wage increase can have positive spill-over effects on the wages of workers with wages higher than the new minimum.

Aretz et al. (2013) likewise find a negative relationship between minimum wages and employment throughout the wage distribution. They investigate the effects of minimum wages in the German roofing sector, which had a sectoral minimum wage before the implementation of the MiLoG, by comparing it to the plumbing sector which did not have a sectoral minimum wage. They argue that these spill-over effects may be due to capital/labour substitution and/or scale effects. These mechanisms imply that employers decrease its relative use of labour in its production or produces less, as a response to an increase in labour costs. This reduction in labour usage would not only affect those workers who gained an increased wage, but also those individuals who did not as they may be working similar jobs. Indeed, other studies such as Lordan and Neumark (2018) find that a minimum wage have negative employment effects on low-skilled workers in easily automatable professions.

Examining this literature, the effects of minimum wages seem to be varying and depending on circumstances related to the individual case: when, why and in which intensity it is implemented as well as which methods that is used to assess its consequences.

2.2 Exploring the Minimum Wage Act

The German MiLoG reform is a major German labour market policy and as such its effects on different aspects of German society is being explored by a wide range of researchers. These in turn have differing objectives, such as Boll et al. (2015) who study its potential effects on the gender pay gap, Fedorets et al. (2018) who study its impact on German’s reservation wages or Gülal and Ayaita (2018) who study its effects on the perceived well-being of its receivers. In common for these researchers is that they use survey data from the German Socio Economic Panel, the SOEP, the same database as I will use in this paper.

Caliendo et al. (2017) also study the effects of the MiLoG on German society, but they do so in a way that is of more interest to this paper. These authors investigate the effects of the MiLoG on three labour market outcomes for those workers who are entitled to it, throughout the wage distribution. Their main finding is an above average positive effect on hourly wages for those at the bottom of the wage distribution. However, they do not find any change in monthly contractual earnings for the same group, which is explained by a decrease in contractual hours worked. They use a mix of data from the German Socio Economic panel (the SOEP) and the Structure of Earnings Survey (SES) to implement a method inspired by that of David Card (1992b).

2.3 Research objective

The current literature is far from a monolith, results may differ based on a variety of circumstances. This is also true when looking at a specific group such as those with earnings marginally above a new minimum wage. Investigating this often overlooked group is important for understanding the total effect of a minimum wage regime, as well as for assessing its success and fairness. I therefore find it relevant to research whether the negative spill-over effects for individuals marginally above a minimum wage, found by Neumark et al. (2004) and Aretz et al. (2013), also can be found in the setting of the MiLoG. Additionally, the MiLoG is a recent and newly introduced reform which has not yet been fully explored by researchers. So far, the literature on how and if different groups in the German wage distributions are heterogeneously affected is limited to a few works. With this paper I aim to add to the literature on the total effects of the German minimum wage implementation as well as the growing literature on the impact of minimum wages. I will do so by focusing on a group that is often overlooked; those with hourly wages marginally higher than the implemented minimum wage prior to its implementation. The continuation of this paper will first provide an explanation and discussion of my chosen methodology, followed by descriptive statistics and a presentation of my results as well as robustness checks, before concluding.

3 Methodology

My chosen method is influenced by that of Caliendo et al. (2017) who in turn is influenced by the work of Card (1992b). I will use similar concepts, research design and data but look at a different part of the population during a different time period.

3.1 Data

I will be using survey data from the German Socio Economic Panel, SOEP, up until wave v34. More specifically I will be using the "SOEP-core" which contains a large number of longitudinal micro variables collected from around 30,000 individuals and 11,000 households annually during 1984-2017. The SOEP is a database collected and run by the German Institute for Economic Research (German Institute for Economic Research, 2019). Even though survey data is far from perfect with multiple risks of measurement errors, this data is perhaps the best available when researching the effects of the new German minimum wage. It has therefore been used by multiple researchers with different research objectives, as mentioned above. I will discuss the risks of measurement errors in survey data more extensively in a later section of this paper.

3.2 Hourly wage

The SOEP-core has not collected data on hourly wages prior to its 2017 questionnaire. Calculating this measurement is therefore a first challenge of this paper. The SOEP-core does however include consecutive data for monthly income and weekly hours worked. This allows me to calculate an individual's hourly income. Because the SOEP-core includes measurements for both contractual and actual hours worked there are two possible measurements of hourly wage: actual- and contractual hourly wage. The same is noted by Caliendo et al. (2017) who choose to include both measurements, partly to be able to account for changes in overtime as an adjustment channel. In this paper I will only be using actual hourly wages from primary employment as this is the most policy relevant indicator.

3.3 Sample

My sample will consist of every individual of working age, 16-65, earning marginally above the minimum wage before its implementation, meaning those with hourly wages within a certain range during 2014. I define this range with the help of Neumark et al. (2004). Their results indicated that individuals with hourly incomes of up to 150% of the minimum wage experienced an increase in hourly income but a decrease in employment. Furthermore, those individuals with hourly wages of 100% to 150% of the minimum wage suffered a decrease in employment, while individuals with hourly wages of 100% to 110% of the minimum wage prior to its implementation also experienced a drop in total income and hours worked. Considering these results there are two relevant ranges for me to investigate, those with hourly wages in 2014 of in between 100% and 150% or 100% and 110% of the MiLoG. Both measurements offer pros and cons. While a range of 10% is arguably more "marginal" and consequently more in line with the question that I wish to answer, the 10% range also risks making the sample too small. A too small sample could increase potential bias and the variance of my sample mean, such that it would be difficult to draw relevant conclusions on causality as potential underfitting and large standard errors may cloud the results. The 50% range on the other hand would include a larger sample size, which potentially could lower this bias as well as the standard errors. The 50% range may however at the same time potentially introduce another form of bias as the sample within the range is now more heterogeneous. After some consideration I decide to opt for a smaller sample of between 100% to 110% of the MiLoG. More specifically I choose to include those who in 2014 had hourly incomes from primary employment of in between 8.5 and 9.35 EUR per hour. I choose this range as these individuals are closer to the MiLoG and as such more representative of the marginal worker. It is also not clear whether I would lower or replace my potential bias if I increased my sample size.

The sample will furthermore consist of observations from the year of 2012 up until 2017. 2017 is selected because the SOEP wave v.34 that I am using contains data up until this point and thus allows me to fully use the available data. The three years of 2015, 2016 and 2017 is different in the evolution of the MiLoG and may therefore tell different and interesting stories. 2015 is the year of its implementation and may therefore show me the immediate short-term impact, while 2017 could potentially show the immediate effects of an adjustment. 2016 on the other hand is an "in between year", it will be interesting to see how and if employers adjust during this year, perhaps still adjusting to the 2015 implementation or anticipating the 2017 raise. I elect to use the year of 2012 because of symmetry, three years after as well as three years before the reform.

The sample is constant throughout the time period, meaning that the sample only consists of the same individuals as defined above. There are however some cases of attrition as individuals exit the defined age-gap or stopped filling out the SOEP questionnaire during the 2014-2017 period. This does however mean that the sample is not entirely balanced, as some individuals are not represented during all years.

Lastly, I will define a trimmed version of my sample as those who had a constant employment during the post-MiLoG years 2015, 2016 and 2017. When looking at some of my effects of interest, I wish to measure the effects on individuals who maintain its employment. I will therefore create a trimmed sample by removing those individuals who became unemployed during 2015 to 2017 from my sample. It will be clearly stated in the paper when I am using the trimmed version of the sample.

3.4 Outcomes

I define the outcomes, inspired by Neumark et al. (2004), as hourly wage from primary employment, monthly total earnings and employment as defined by both actual hours worked and employment probability. The latter is defined as a binary variable while the other three are continuous variables. When looking at the effects on hourly wages and hours worked per month, I am interested in seeing the effects on the individuals who maintain their employment. I will therefore use the trimmed version of the sample when running these regressions. I hope to catch employment effects through the employment status and total labour income variables.

3.5 Difference-in-differences estimator

To analyse the potential spill-over effects of the MiLoG I have opted to use the difference-in-differences (DiD) estimator. The DiD estimator is well suited to do so as it solves two key endogeneity problems that occurs when one does not have access to randomized treatment- and control groups, as well as when dealing with panel data. These are that potential group specific characteristics and time specific effects may bias the result.

This technique, its theory and assumptions are described by Angrist & Pischke in their book *Mostly Harmless Econometrics: An Empiricist's Companion*, from 2009. In the simplest DiD framework the researcher has two groups: one treatment group who has been treated and one control group that has not been treated. The researcher also needs pre- and post-treatment data for both groups such that the periods corresponds to one another. At this point the DiD estimator is basically a comparison of four means, before and after treatment for both the treatment- and the control group. First, find the first difference changes within the groups separately by subtracting the pre- from the post-treatment mean. Second, subtract the resulting first difference of the control group from the corresponding first difference of the treatment group. This resulting value is the treatment effect. The DiD framework allows the researcher to avoid the endogeneity problems mentioned above as group specific effects are cancelled out in the first step of the DiD process, while time specific effects are cancelled out in the second step.

This simple DiD framework can also be used in a regression model through an equation such as this.

$$Y_{igt} = \beta_0 + \beta_1 * T_g + \beta_2 * d_t + \beta_3 * (T_g * d_t) + \varepsilon_{igt}$$

In this regression T_g and d_t are treatment group- and time dummies, respectively. β_1 captures the difference between the groups before treatment while β_2 captures the time effects. The most important part of this equation is the interaction between the treatment and time dummies as β_3 captures the treatment effect. Moreover, Y_{igt} is the outcome variable, β_0 is a constant which captures a baseline average and ε_{igt} is an error term.

The DiD framework requires one key assumption to be fulfilled in order to generate unbiased results, the parallel trends assumption. Intuitively the parallel trends assumption can be summarized as: The treatment- and control groups need to have common trends before treatment, such that their outcomes would develop parallelly in the absence of any treatment. This assumption will be discussed at length later in this paper.

3.6 Research design

Like Caliendo et al. (2017) and Card (1992b) I will be using a model that utilizes an exogenous variation in regional treatment intensity. Similar methods have also been used by authors such as Dolton et al. (2012) for a comparable reform in the United Kingdom. The federal nature of the MiLoG means that all individuals are treated at the same time, I will therefore use the regional intensity of treatment as my treatment variable, to use the terminology of the typical DiD framework. To do so I first construct a "bite" variable defined as the ratio of workers earning less than the minimum wage before its implementation. This is a measurement used to quantify to which extent a region and its inhabitants is exposed to the new minimum wage, the treatment intensity in the region. The bite variable will show if there are spill-over effects by determining whether the proportion of individuals earning less than the MiLoG in a region affects the outcomes. This regional variation needs to be exogenous and should not be affected by the implementation of the MiLoG. I will therefore, like Caliendo et al. (2017), choose the year 2013 to calculate the regional bite to avoid anticipation effects as well as the potential endogeneity it may bring with it.

I will be using East- and West Germany as my regions. These are defined as the regions who together in 1990 reunified to form what is today the Federal Republic of Germany. My choice of regions is a contrast to Caliendo et al. (2017) who used the 96 planning regions of Germany. Using two regions instead of 96 of course comes with some drawbacks. One major drawback being that I will have less regional variation which in turn may lead to higher standard errors and bias, as this also could increase the variance of my sample mean and potential underfitting. The most important reason for my choice of using the larger but fewer two regions is that I expect my sample to be much smaller. My sample will only include those with hourly wages in 2014 of in between 8.50 to 9.35 EUR, while Caliendo et al. (2017) use the entire population that is entitled to the MiLoG. Granted, Caliendo et al. (2017) does trim this sample, but they should still capture a larger amount of the 30,000 people who answer the survey. With a larger sample they can divide the country into smaller region with more ease. As my sample is smaller, only including 819 individuals in the full sample (see the *Descriptive Statistics* section), dividing Germany into too many regions may also bias the returns if there are too few observations per region. In fact, Caliendo et al. (2017) only uses 92 of the 96 planning regions because some included too few observations. I hope to circumvent this by using larger regions and calculating the regional bite accordingly.

Moreover, the exogeneity of the bite variable can be debated, especially when using regions as large as East- and West Germany. The risk, of course, is that the ratio of workers with a low wage is endogenous to the overall economic status, history and development of the region in question. East- and West Germany are reasonably expected to perform differently because of the country's history. Caliendo et al. (2017), influenced by Dolton et al. (2015), use a two-period lagged GDP per capita measurement in order to control for such regional effects. I will do something similar by adding a regional fixed effect to my regression. Apart from the regional fixed effects, I will include individual fixed effects as a way of controlling for unobserved individual characteristics such as ability or motivation. Furthermore, demographics will be added to my regression to control for gender-, marriage-, origin- and age fixed effects.

The equation that I will use is specified below.

$$A_{irt}^{dv} = c + \gamma * M_t^{2015} + \alpha * M_t^{2016} + \beta * M_t^{2017} + \delta * Bite_r^{2013} + \theta * (M_t^{2015} * Bite_r^{2013}) + \mu * (M_t^{2016} * Bite_r^{2013}) + \rho * (M_t^{2017} * Bite_r^{2013}) + \omega * X_{irt} + \varepsilon_{irt}$$

Where A_{irt}^{dv} is equal to any of the four dependent variables; hourly wage from primary employment, total labour earnings, actual hours worked or employment status. The M_t^{year} variables are year dummies which are equal to one if an observation is within the corresponding year. Furthermore, X_{irt} is a vector of fixed effects including age, gender, if the individual were born in Germany and marital status, as well as a variable counting the number of children under 16 years of age within the individual's household. The X_{irt} vector will also house individual- and regional fixed effects. Lastly, ε_{irt} is the error term and c is a constant.

The most important parts of the above equation are the interaction terms ($M_t^{year} * Bite_r^{2013}$). These interactions capture the treatment effect of the MiLoG's implementation and adjustment through θ, μ and ρ. These are the variables that will be most important to look at in order to determine the existence of any causal spill-over effects on my sample. Meanwhile, γ, α and β will show the effect of time, while δ is interpreted as the difference between the two regions before treatment. ω captures effects from the included demographics and fixed effects but will not be important for any later analysis, neither will c which captures a baseline average.

I have elected to not use the logarithms of my dependent variables, as seen in the above specifications. This is diverging from the work of researchers such as Caliendo et al. (2017), Card (1992b) and Rattenhuber (2014). My reasoning for diverging from the works of these researchers is mainly that I have a narrow sample in which I expect my individuals and observations to be similar. Hence, I do not expect to have a significant problem with outliers that may potentially introduce noise and thus I do not have the need to squeeze my observations together.

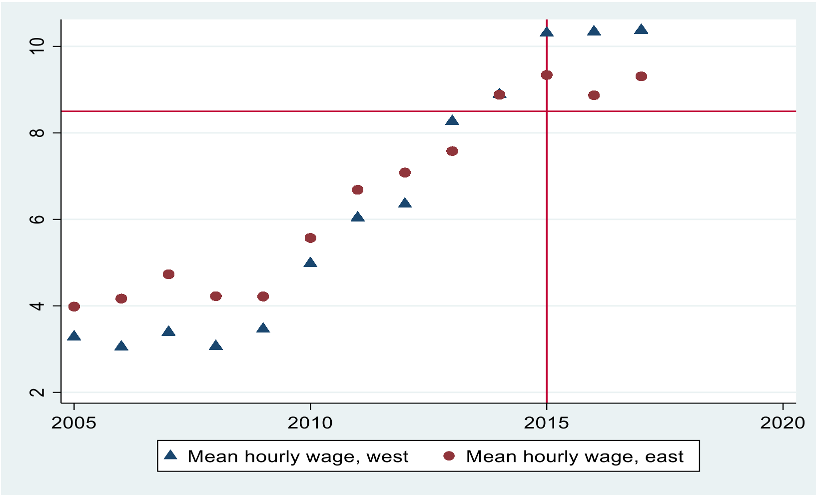
A final note on this research design is that it possesses both limitations and possibilities. The design investigates the existence of spill-over effects as well as its potential trends over time. It will however not calculate percentage changes or study the potential channels through which potential spill-over effects operate. While these are also important fields of research, they are not a part of the scope of this paper. Not part of this paper is also general equilibrium effects as it focuses on finding the partial equilibrium effects for a selected sample.

3.7 Satisfying the parallel trend assumption

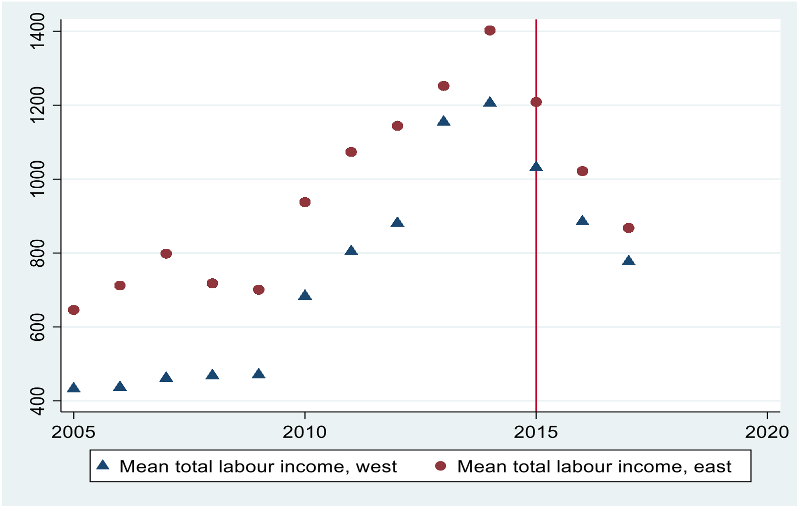
An important part of any DiD approach is to be able to support the so-called common trends assumption. With this assumption, you assume that a treated individual would have similar trends as an untreated individual, in the absence of treatment (Angrist & Pischke, 2009). Theoretically a researcher has two groups who ideally resemble each other as much as possible, one treatment- and one control group. As mentioned previously, this is not possible due to the federal nature of the MiLoG where both East- and West Germany are treated. I overcome this by using the differences in regional treatment intensity. This means that East- and West Germany are comparable to my treatment- and control group, again using the terminology of a simple DiD setup. As such I need to satisfy the common trends assumption using East- and West Germany. I will in this section prove the common trends assumption graphically by plotting yearly means of the outcome variables for both regions, if the assumption holds, trends should be similar up until the reform is implemented. If the trends are common prior to the implementation, it is probable that they would have remained that way in the absence of the MiLoG.

Graphs one through four depicts a graphical test of the parallel trends assumption. Noteworthy is that graphs one and three only include my trimmed sample while tables two and four include the full untrimmed sample. This is again, because I want to measure the effects on hourly wage and monthly hours worked on those that remained employed. The graphs show the evolution of the samples from 2005 to 2017 as shown by the yearly means of the four dependent variables. 2005 is arbitrarily selected to show medium to long trends. Key at this stage is to do a visual comparison of the trends between the regions to see if they did have common trends prior to the implementation of the minimum wage. Indeed, from looking at the trends I gather that they, while not identical, seem to be remarkably similar. Especially graphs two and three seem to show near identical trends. Meanwhile tables one and four suggest that West Germany has slightly steeper trends during the period, while still being strikingly comparable. Based on this graphical evidence I argue that the common trends assumption is fulfilled.

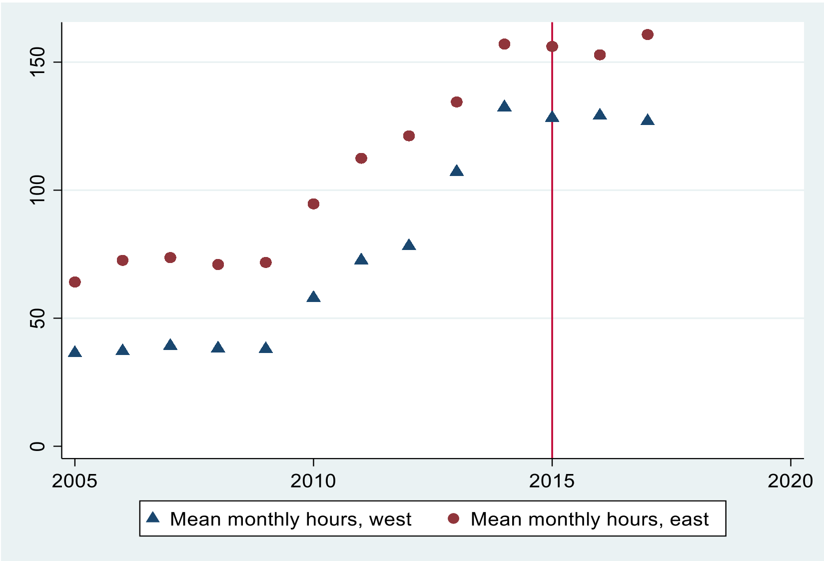
Moreover, in order to find any treatment effect trends should diverge post-treatment. Graphs one through fours also depict the post-treatment evolution of the means. The four graphs suggest that this change is very similar to individuals of both regions, the relative changes in trends are therefore difficult to distinguish graphically. Considering that the regional variety is only based on two regions, and that the bite variable of these two are similar, 0.3324 compared to 0.2131 (see the *Descriptive Statistics* section), these small movements are expected.



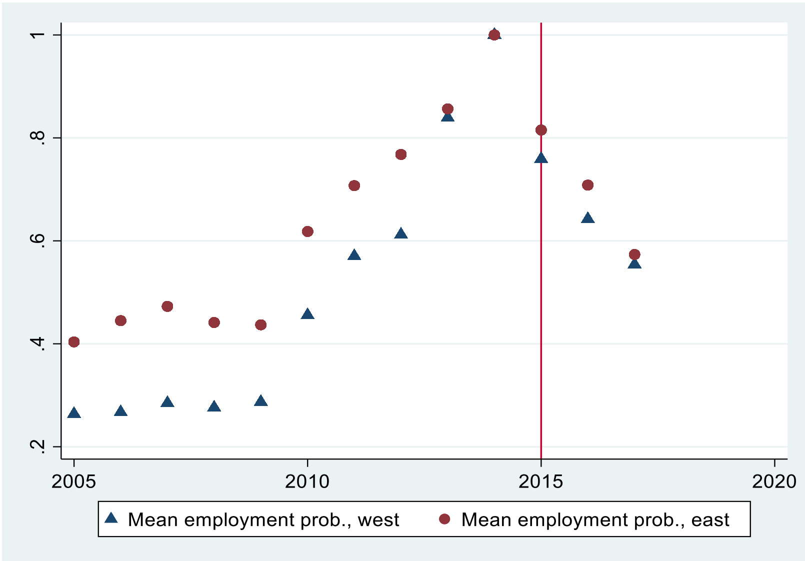
Graph 1: Development of mean hourly wages, 2005 -2017, trimmed sample



Graph 2: Development of mean total labour income, 2005 -2017



Graph 3: Development of mean hours worked, 2005 -2017, trimmed sample



Graph 4: Development of mean employment probabilities, 2005 -2017

4 Descriptive Statistics

Using the previously mentioned restrictions for my full untrimmed sample, I end up with a sample of 819 individuals and a total of 4845 observations. Of these 819 individuals, 596 (73%) live in west Germany while the remaining 223 (27%) lives in East Germany. What may be an issue is the limited number of individuals in my sample, as discussed previously. That is however an inherent risk when looking at such a small group as those individuals who are marginally above the new wage floor. In formulating my ambition in such a manner, I am limiting myself to a small sample. More on this in the *Methodology* and *Robustness* sections of this paper.

The two different bite variables are as described earlier defined as the ratio of people in each region who earns less than the minimum wage of 8.5 EUR per hour during 2013. Using the SOEP data I find that the bite variable equals 0.3324 in East Germany and 0.2137 in West Germany. These numbers will be inserted into my equations as the regional bite variable and remains constant over time. The higher bite variable for East Germany indicates that this is the region that experiences the treatment most intensely.

Table one depicts the above statistics, as well as the means, minimum- and maximum values of all my included variables throughout the period of 2012 to 2017. It therefore depicts multiple values from the same individuals at different points in time which explains why, for example, the mean of the hourly wage is well below the MiLoG's wage floor at 7.257 EUR. Note that descriptive statistics for my trimmed sample, defined as those individuals with constant post-treatment employment, is presented in table A1 of the appendix. Values are similar, such that the analysis of table one is valid for both.

Table 1: Descriptive statistics, untrimmed sample.

Variable	Obs	Mean	Std. Dev.	Min	Max
Monthly hours worked	4,845	102.7755	80.06813	0	320
Hourly wage	4,845	7.257187	5.792113	0	89.5
Employment probability	4,845	.7504644	.4327889	0	1
Total monthly labour earnings	4,845	1034.658	940.1988	0	10300
Regional bite	4,845	.2460148	.0528403	.2137	.3324
Gender (1=man)	4,845	.4111455	.4920923	0	1
Born in Germany (1=yes)	4,845	.7145511	.4516746	0	1
East Germany (1=yes)	4,845	.2722394	.4451584	0	1
Married (1=yes)	4,845	.4738906	.4993694	0	1
Age	4,845	42.21858	11.89683	16	65
Number of kids below 16 in household	4,845	1.085449	1.035958	0	8

Moreover, table one explains what kind of individuals are part of the sample. A majority of observations are collected from German born individuals, there are more women than men, the biggest group of individuals lives in the less treatment intensive West Germany, the majority is married and the average individual lives in a household with one child under the age of 16. On average, the age of the sample individual is 42. It is an age when employment is expected to be the main activity and focus of the individual, as opposed to younger individuals who may have a stronger tendency to study and therefore systematically earn and work less.

Continuing the graphical analysis of graphs one through four and looking at graphs A1 thorugh A4 of the appendix, I note that the post MiLoG trends diverge more when looking at monthly hours worked and hourly wage. While East German workers have had a small increase in monthly hours worked, West German workers in the sample has seen a slight decrease. The opposite is true for hourly wages which plateaued in the West and saw an initial decrease in the East. These comparisons of means are in themselves interesting, but there seems to be an on average negative correlation between hourly wages and hours worked, an interesting observation from an economics standpoint as it is what classical theories of supply and demand would have predicted. This development seems to be correlated with the implementation of the MiLoG, after a five-year period of strong positive development since 2010. The same is true for employment probabilities and total labour earnings which seem to have experienced sharp drops post-treatment. Workers of both regions have a lower probability of being employed and have lower total earnings. This comparison of means does in this sense seem to suggest similar conclusions as Neumark et al. (2004); workers who receive higher wages because of a raised minimum wage earns less in total as the demand for labour diminishes. This pattern is particularly true in West Germany were workers earn slightly more per hour but work and earn less. In East Germany however, the yearly means suggest that the sample worker earns less but works more hours, if it is employed. The negative correlation between hours worked and employment probabilities imply that fewer workers are working more. Considering that hourly wages and hours worked seem to have plateaued, the decrease in average total income is then probably driven by increased unemployment, which also arguably is feasible for West Germany.

Whether there exists any causality is however not clear as this is just a comparison of means. The same goes for any causality between these four labour market indicators and the MiLoG. The panel nature of my data suggests that it suffers from endogeneity, most notably due to time- and group fixed effects, which may be driving the correlation observed above. Using the DiD strategy solves these endogeneity problems, as explained in the DiD section, and helps me isolate any potential causation in outcomes. At the same time the inclusion of regional- and individual fixed effects isolates the effects of the MiLoG from other effects, such as regional policies or differences in motivation. As such the results of the statistical analysis may differ from the picture that is painted in graphs one through four and described above.

5 Results

The following section will first discuss how to interpret my results, before presenting them in tables two and three. The results will then be described and discussed.

The interpretations of the variables below are important to keep in mind. Most important are the year*Bite interaction variables, but interesting are also the year dummies and the pure Bite variable. The coefficient in front of the Bite variable tells us the difference in average outcomes before treatment effects between the two regions. It can also be described as how much an outcome would have changed, on average, if an individual moved from West Germany to East Germany, in the pre-treatment period. The coefficients in front of the interaction terms is the main treatment variables as they measure the effects of living in a region with a higher treatment intensity during the years of treatment. A positive value suggests an increase with treatment intensity such that an individual in East Germany on average will be relatively better off, while a negative value indicates the opposite. Any significant value would however lend evidence to the existence of spill-over effects. Lastly the coefficients for the included yearly dummies reflects yearly specific effects and should mirror my earlier comparison of post-treatment means in graphs one through four.

In common for all four dependent variables is that they all have an R-squared of roughly 10%-20% and that the coefficients of the interaction term only changes slightly between different specifications. This is however not as important as the effects of my year*Bite variable where, in many of the cases, the results are encumbered by large standard errors. These large standard errors may reflect my methodology in which I choose to utilize a small sample knowing that this could increase the variance of my sample mean and risk large standard errors. The same is true as I narrowed down the regions that I would use to two, East and West Germany. At the same time, as discussed above, this was a conscious choice. Moreover, I can only reject the null hypothesis in a few cases, the null hypothesis being that regional variation in treatment intensity caused workers who are marginally above the MiLoG to be affected differently. This may be a result of the high standard error but could also be suggesting that there were in fact no causality between regional variation in treatment intensity and outcomes. In this context the results tell us that the visible decline and stagnation of means in graphs one through four are not primarily a result of the MiLoG in combination with regional bite, but something else that the model does not capture. Perhaps the MiLoG affected the sample equally in a way that is independent of the treatment intensity, for example.

Table two depicts the results for hourly wages and monthly hours worked for my trimmed sample. The first three columns depict the results for hourly wages. All variables of interest are significant. The bite as well as interaction terms are negative while yearly dummies are positive. This means that while there exists a common positive yearly trend, the wages of workers in the more intensely treated east experiences a relative decrease, compared to wages of workers in the west. This relative decrease is caused by the higher treatment intensity of East Germany. The negative value on the lone bite variable means that the average wages were lower in the less intensely treated East Germany before treatment, which is also visible in graph one. The results of these columns support the idea that there were spill-over effects on those individuals who are earning marginally more than the minimum wage at the time of its implementation. The actual size of the coefficients must be seen in the broader positive trend of the yearly fixed effects. This makes the actual loss in hourly wages smaller than what these coefficients would suggest. Moreover, as the treatment effect can be interpreted as a relative development from comparing East- and West Germany, the evolution of the year*Bite variable reflects what is seen in graph one. Most notable the largest negative treatment effect from the 2016*Bite variable, which can be observed in graph one as a weak increase in mean wages for West Germany and a sharp decrease for East Germany.

Table two also depicts the results of the statistical analysis on number of hours worked per month, in columns four through six. The interaction terms are not significant and since these have varying signs and substantial standard errors, I do not dare to draw any conclusions from these. The lone bite variable and yearly dummies are however significant. These results capture what we see in graph three, a weak overall positive yearly effect and a pre-treatment value that is larger in East Germany. These results do not however support causality between the actual working hours of my sample and the MiLoG through regional treatment intensity.

Table 2: Regression results for hourly wages and monthly hours worked, trimmed sample. Demographics include dummies for age, gender, marriage and if the individual is born in Germany as well as a variable for number of kids under 16 in the household.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES		Hourly wage			Monthly hours worked	
2015*Bite	-7.805*	-7.399*	-7.805*	-35.42	-32.33	-35.42
	(4.221)	(4.203)	(4.221)	(54.04)	(53.97)	(54.04)
2016*Bite	-11.80***	-11.39**	-11.80***	-57.74	-54.57	-57.74
	(4.519)	(4.500)	(4.519)	(57.85)	(57.78)	(57.85)
2017*Bite	-8.472*	-8.433*	-8.472*	10.97	11.26	10.97
	(4.949)	(4.928)	(4.949)	(63.36)	(63.27)	(63.36)
2015	3.909***	3.827***	3.909***	28.11**	27.49**	28.11**
	(1.070)	(1.065)	(1.070)	(13.70)	(13.68)	(13.70)
2016	4.848***	4.763***	4.848***	33.36**	32.70**	33.36**
	(1.145)	(1.141)	(1.145)	(14.66)	(14.64)	(14.66)
2017	4.023***	4.053***	4.023***	14.59	14.82	14.59
	(1.248)	(1.242)	(1.248)	(15.97)	(15.95)	(15.97)
Bite	-5.524***	-5.964***		188.1***	184.8***	
	(1.980)	(1.973)		(25.35)	(25.33)	
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effects		Yes			Yes	
Regional fixed effects			Yes			Yes
Observations	4,045	4,045	4,045	4,045	4,045	4,045
R-squared	0.105	0.113	0.105	0.215	0.217	0.215

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3: Regression results for total labour earnings and employment probabilities, untrimmed sample. Demographics include dummies for age, gender, marriage and if the individual is born in Germany as well as a variable for number of kids under 16 in the household.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES		Total labour earnings			Employment probability	
2015*Bite	77.84	93.06	77.84	0.0886	0.0898	0.0886
	(665.5)	(663.4)	(665.5)	(0.300)	(0.300)	(0.300)
2016*Bite	-443.5	-439.5	-443.5	0.0668	0.0671	0.0668
	(670.0)	(667.9)	(670.0)	(0.302)	(0.302)	(0.302)
2017*Bite	-748.0	-742.3	-748.0	-0.314	-0.314	-0.314
	(675.9)	(673.8)	(675.9)	(0.305)	(0.305)	(0.305)
2015	-88.67	-89.18	-88.67	-0.0832	-0.0833	-0.0832
	(167.7)	(167.1)	(167.7)	(0.0756)	(0.0756)	(0.0756)
2016	-81.75	-79.40	-81.75	-0.163**	-0.163**	-0.163**
	(168.6)	(168.0)	(168.6)	(0.0760)	(0.0760)	(0.0760)
2017	-111.9	-109.2	-111.9	-0.153**	-0.152**	-0.153**
	(170.1)	(169.5)	(170.1)	(0.0767)	(0.0767)	(0.0767)
Bite	505.0	435.0		0.222	0.216	
	(338.9)	(338.0)		(0.153)	(0.153)	
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effects		Yes			Yes	
Regional fixed effects			Yes			Yes
Observations	4,845	4,845	4,845	4,845	4,845	4,845
R-squared	0.167	0.172	0.167	0.201	0.202	0.201

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table three depicts the results of the statistical analysis for total labour earnings and employment probabilities. The results for total labour earnings are in columns one through three, no results are significant. Moreover, several coefficients are smaller than their standard errors. This makes it impossible to draw any kind of conclusion based on the coefficients or signs of the variables. What is striking is that even the yearly dummies are insignificant, as these variables are expected to confirm the yearly trend in means found in graph two. These results do not support any causality between a decrease in total monthly labour earnings and the MiLoG through the variation in treatment intensity. A very similar analysis can be made for columns four through six of table three, which depicts the regression results for employment probabilities. The only difference being that the yearly dummies support the negative trend found in graph four.

To summarize, the results of my statistical analysis suggests that there exists a causal link between the difference in regional treatment intensity of the MiLoG and a wage reduction for my trimmed sample. The results are a stark contrast to the results of Neumark et al. (2004) who estimate that the same group should experience an increase in hourly wages and decreases in the remaining three variables, where I found no effect. Which specification I use when running the regressions does not matter. The significant effects on wages are consistently negative during the post-treatment period 2015 to 2017, with the biggest relative decline being in 2016 and the smallest in 2015. This is interesting, as it suggests that my sample were consistently held back in their wage development, even when they remained employed. The effects were not only observed when the MiLoG was introduced or adjusted in 2015 and 2017, but also during 2016. The results suggest that the wage development of this group of individuals was anchored as a result of the MiLoG and that this effect increased with treatment intensity. Furthermore, the results imply the existence of negative spill-over effects on hourly wages.

6 Robustness

In order to prove the validity and robustness of my findings, I will use this section of the paper to test my methodology by conducting two different tests. I will also use this section for some discussions of the model and data.

6.1 Placebo testing

A placebo regression is a way of validating the parallel trends assumption, and therefore my model. While the graphical analysis in graphs one through fours is one way of proving this assumption, another way is through a placebo regression. A placebo regression can also be used to confirm that the effect did not come before its supposed cause.

The control groups should have common trends leading up to the reform and indeed in the absence of the reform. My chosen method uses an exogenous variation in treatment intensity to measure potential spill-over effects of a treatment. To do so I run a DiD regression including time dummies for the post-treatment period of 2015, 2016 and 2017 that interact with the regional bite from 2013. This setup is based on the idea that the development of outcomes was similar before the treatment and that treatment occurred. As such, the interaction terms between years and bite should not be able to explain any variation in outcomes if there is no treatment. Considering this, the idea of the placebo regression is to use this same research design but for a period without the reform. To do this I will use data from 2008 to 2013. The sample is collected and trimmed in the same way as before with the distinction of being the individuals who earn between 8.5 and 9.35 EUR in 2010. Likewise, the bite variable is calculated using data from 2009. The dependent variables and the explanatory variables remain the same as before. The placebo regression then looks as follows.

$$A_{irt}^{dv} = c + \gamma * M_t^{2011} + \alpha * M_t^{2012} + \beta * M_t^{2013} + \delta * Bite_r^{2009} + \theta * (M_t^{2011} * Bite_r^{2009}) + \mu * (M_t^{2012} * Bite_r^{2009}) + \rho * (M_t^{2013} * Bite_r^{2009}) + \delta * X_{irt} + \varepsilon_{irt}$$

The results are presented in tables three and four in a similar way as the main results. The interpretations of the variables are also the same. Most important is the results for hourly wages as I in my main regressions could find evidence of a causal link between this outcome and the implementation of the MiLoG through my interaction terms. I would need to place serious doubts in my methodology if I would find similar results in my placebo regressions. Continuingly, I could no longer claim those results as evidence of any causation, as my model would be defective. Similarly, when observing the results of the placebo regressions for monthly hours worked, total earnings and employment probability, I do not wish to find any significant results. This is likewise because my main regression found no causality through the interaction terms. If then my placebo regression would do so, I again would need to place serious doubt in my methodology. In other words, to verify the robustness of my model, I do not wish to find any significance for the interaction terms.

Inspecting tables four and five, I note that none of the interaction variables are significant in this new specification of my model. Consistently for all four dependent variables and throughout all three specifications, I am unable to reject the null hypothesis of the year*Bite variable even at a 10% confidence level. This is good news for the robustness and validity of my previous findings, model and coming conclusions.

Noteworthy as well is the low R-squared values of the regressions on employment probability and hourly wage, even though I include individual fixed effects and demographics. The values are significantly smaller than those of the regressions for monthly hours worked and total labour earnings. These results suggest that my fixed effects may have missed something vital in determining hourly wages and employment probabilities.

Table 4: Results of placebo regressions for hourly wages and monthly hours worked, trimmed sample. Demographics include dummies for age, gender, marriage and if the individual is born in Germany as well as a variable for number of kids under 16 in the household.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES		Hourly wage			Monthly hours worked	
2011*Bite	1.868	1.609	1.868	5.605	4.025	5.605
	(3.767)	(3.762)	(3.767)	(40.44)	(40.44)	(40.44)
2012*Bite	-0.255	-0.463	-0.255	-34.36	-35.63	-34.36
	(3.879)	(3.874)	(3.879)	(41.65)	(41.64)	(41.65)
2013*Bite	-1.872	-2.242	-1.872	14.30	12.04	14.30
	(4.050)	(4.045)	(4.050)	(43.48)	(43.48)	(43.48)
2011	0.826	0.987	0.826	3.677	4.662	3.677
	(1.094)	(1.093)	(1.094)	(11.75)	(11.75)	(11.75)
2012	1.038	1.206	1.038	17.62	18.64	17.62
	(1.127)	(1.126)	(1.127)	(12.10)	(12.10)	(12.10)
2013	1.553	1.780	1.553	3.187	4.575	3.187
	(1.172)	(1.172)	(1.172)	(12.58)	(12.60)	(12.58)
Bite	-8.250***	-7.820***		76.58***	79.21***	
	(1.721)	(1.724)		(18.48)	(18.53)	
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effects		Yes			Yes	
Regional fixed effects			Yes			Yes
Observations	3,593	3,593	3,593	3,593	3,593	3,593
R-squared	0.047	0.050	0.047	0.208	0.209	0.208

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Result of placebo regressions for total labour earnings and employment probabilities, untrimmed sample. Demographics include dummies for age, gender, marriage and if the individual is born in Germany as well as a variable for number of kids under 16 in the household.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES		Total labour earnings			Employment probability	
2011*Bite	197.1	155.4	197.1	0.193	0.195	0.193
	(549.8)	(547.6)	(549.8)	(0.215)	(0.215)	(0.215)
2012*Bite	-29.47	-96.11	-29.47	0.104	0.107	0.104
	(567.2)	(564.9)	(567.2)	(0.222)	(0.222)	(0.222)
2013*Bite	-702.5	-779.3	-702.5	-0.205	-0.202	-0.205
	(583.2)	(580.9)	(583.2)	(0.228)	(0.229)	(0.228)
2011	-124.9	-91.14	-124.9	-0.131**	-0.133**	-0.131**
	(159.2)	(158.6)	(159.2)	(0.0624)	(0.0624)	(0.0624)
2012	-52.93	-6.520	-52.93	-0.0911	-0.0932	-0.0911
	(164.4)	(163.9)	(164.4)	(0.0644)	(0.0645)	(0.0644)
2013	131.8	183.3	131.8	-0.0179	-0.0203	-0.0179
	(169.2)	(168.8)	(169.2)	(0.0663)	(0.0664)	(0.0663)
Bite	-682.9**	-566.4**		-0.105	-0.110	
	(265.5)	(265.1)		(0.104)	(0.104)	
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effects		Yes			Yes	
Regional fixed effects			Yes			Yes
Observations	3,967	3,967	3,967	3,967	3,967	3,967
R-squared	0.142	0.150	0.142	0.042	0.042	0.042

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

6.2 Confirming previous results

A second and further way for me to test the robustness of my methodology of choice is to apply it to different circumstances. My methodology has much in common with the previous work of Caliendo et al. (2017). As such, a fitting test for whether my methodology is correctly specified is for me to try and confirm their result, using my methodology. If I can reproduce similar results as they have, I can be more certain of my own results. I will in this section give a brief reminder of the work of Caliendo et al. (2017) and discuss how I will conduct this test, as well as discuss how my approach will differ from theirs. Finally, I will present the results and discuss my interpretations.

Caliendo et al. (2017) looked at the short-term effects of the MiLoG in 2015 by measuring its effects on three labour market outcomes for all individuals who were eligible for the MiLoG, throughout the income distribution. To do so they used a DiD approach similar to mine, though only including observations for 2014 and 2015. Their main results, and the result that I will be trying to recreate, is a positive effect on wages, negative effect on monthly hours worked and no effects on total income for the lowest quintile of the wage distribution. They come to the conclusion that MiLoG led to an increase in equality. I will use a version of my methodology that is slightly adapted to measure the effects that I wish to confirm. The method is still distinct from that of Caliendo et al. (2017) which means that some variation is expected, I am however hoping to see similar effects.

Firstly, I am not using the same data as Caliendo et al. (2017). While I am only using data from the SOEP-core data, they are also using a different database called Structure of Earnings Survey (SES), as well as a second datafile containing regional data from the SOEP. This regional data is how they divide individuals by the 96 planning regions of Germany and is not available for me to use. I will therefore still use East- and West Germany to calculate the variation in regional treatment intensity.

Secondly, I will use a sample of those individuals who in 2014 earned less than 8.5 EUR per hour to capture the results found for the quintile with lowest income in 2013. This quintile had an average actual wage of 7.35 EUR per hour. They used two samples, one with all individuals eligible to the minimum wage and one with the same individuals given that they had an income during both 2014 and 2015. I will only be using a sample resembling the latter of these two, but with harsher restrictions on consecutive pay data as I am using three post-treatment years instead of only one.

Thirdly, Caliendo et al. (2017) are using two specifications, one base without demographics or individual fixed effects and one with. The results are differing between them, but only in size as they have the same sign. I will therefore only use the latter, a baseline plus personal fixed effects and demographics. I will be using the same demographics as I am in the main regressions of this paper, as these are the same as Caliendo et al. (2017) are using, with one exception. I will be using an indicator variable for if an individual is born in Germany, where Caliendo et al. (2017) are using an indicator for whether the individual has German citizenship. I do not believe this trade will influence the results in any meaningful way.

Finally, for me to confirm their results and conclusion of converging equality, I would need to find treatment effects suggesting that the same effects would happen for my sample. Inspecting table six and my interaction terms, I do find evidence suggesting the existence of such effects. I find positive wage effects and negative effects for hours worked. At the same time, I find no significant effect on total earnings. The way the model is set up indicates that the effect is amplified in poorer and more treatment intensive regions, confirming some of the conclusions on equality that Caliendo et al. (2017) makes. Most notably that individuals living in the poorest regions would see a relatively accelerated wage increase as well as a decline in hours worked.

What is moreover worth mentioning with regards to the results from table six is that the coefficient for the 2015*Bite variable is not significant, indicating that the regional bite did not have an impact on individuals wage increases in 2015. This result does not mean that wages did not increase during 2015, they did of course as this was the year of the MiLoG's implementation. It did however not change according to the regional bite. Probably instead suggesting that the wages were raised in a similar way in both East- and West Germany, irregardless of regional bite. This is not impossible to imagine as all individuals of the sample was brought up to the new common level of the federal MiLoG during 2015, while they during 2016 experienced more regionally based wage development. This also implies that the federal adjustment of 2017 did not affect the sample in the same common way as the initial implementation.

Table 6: Attempt at confirmation of results by Caliendo et al. (2017). Demographics include dummies for age, gender, marriage and if the individual is born in Germany as well as a variable for number of kids under 16 in the household.

VARIABLES	(1) Actual hourly wage	(2) Total labour earnings	(3) Actual monthly hours worked
2015*Bite	-0.937	-322.7	-78.99**
	(2.530)	(365.8)	(31.39)
2016*Bite	7.064***	340.7	-61.13*
	(2.568)	(371.3)	(31.86)
2017*Bite	5.418**	487.4	-34.77
	(2.618)	(378.6)	(32.49)
2015	0.469	117.9	14.05*
	(0.630)	(91.09)	(7.817)
2016	-2.068***	-145.1	-6.246
	(0.638)	(92.27)	(7.918)
2017	-2.364***	-273.2***	-23.70***
	(0.649)	(93.88)	(8.057)
Bite	-5.147***	660.2***	246.4***
	(1.315)	(190.2)	(16.32)
Demographics	Yes	Yes	Yes
Personal fixed effects	Yes	Yes	Yes
Observations	13,572	13,572	13,572
R-squared	0.165	0.167	0.235

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

6.3 Standard errors

In my main regressions I have opted to not discuss or address clustering of the standard errors, even though the panel nature of my data implies that I will have effects on the individual or group level that may affect my standard errors. These effects could lead to incorrectly specified standard errors and most importantly misleading p-values. For me to properly validate my findings and their robustness, I will also perform the regressions with standard errors that are clustered at the individual level. The results can be seen in tables A2 and A3 of the appendix. Observing these two tables and comparing them to tables two and three, the main regressions, I find no substantial difference which indicates that my main findings in any way are flawed because I did not adjust the standard errors.

6.4 Validity of findings

The question of internal validity, whether the research design is robust enough such that the results can be trusted, is a key discussion that I have briefly touched upon in various parts of this paper. Whenever I have presented an addition to my methodology, I have had a brief discussion of their possible consequences and why I find them worthwhile. Two prominent examples of this is my choice to utilize a small sample as well as my choice to only let regional variation vary between two different regions, East- and West Germany. My reasonings behind selecting these options are clearly defined in the *Methodology* section. I do still believe that the right choices were made, but unfortunately, I have no way of testing what would happen if I for example would have used regions that were defined in another way. A quick recap of my previous discussions on the subject will remind the reader that a larger pool of regional variety, using more than two regions, potentially could have produced results with less bias and lower standard errors. However, the narrowness of my selected sample may have introduced another form of bias as the number of observations per region would have been small if I had utilized a larger pool of regions. The small size of the sample, as also discussed above, is a result of the ambition of this paper to investigate the potential effects on a narrowly defined part of the population. In this light, the choice of sample and regions is highly reasonable and arguably an asset for the internal validity of the finding.

Moreover, graphs one though four also suggest that there were no obvious anticipation effects or Ashenfelter-dips clouding the results. Such effects would make the DiD estimator inconsistent, the lack thereof is therefore supporting the validity of my findings.

A final note on the panel data from SOEP that I have used in my research is that it is based on questionnaires that are voluntarily filled out by individuals. This does unfortunately mean that the data is weighted down by attrition, missing- and incorrectly specified answers. The latter is especially important considering that individuals who to a large extent do not work jobs with fixed working hours or pay are expected to accurately remember and report these. Since I am looking at individuals at the bottom of the wage distribution, the risk of such misspecification is high. Fortunately, the SOEP has as one of its functions to prepare the data they collect. This means that they have already labelled answers that are unlikely, inconsistent or impossible, such as having labour incomes while being unemployed or working 168 hours per week (German Institute for Economic Research, 2019). I have also been consistent in my own preparations of the data, in order to avoid systematic misspecifications. The inclusion of demographics as well as regional and individual fixed effects may also capture parts of any remaining systematic misspecification. There is otherwise unfortunately no way of testing whether the data from the questionnaires of SOEP is encumbered with any systematic burdens.

The external validity of my results can be interpreted in two ways, whether the results are transferable to other parts of the German income distribution or whether the experiences are applicable to other countries. Considering the former, external validity in the German wage distribution, the results themselves are not transferable. There is however a lesson to be learnt in that the MiLoG may have heterogenous effects on different parts of the wage distribution. As for whether the experiences of contemporary Germany are applicable to other nations, I would again argue that there are indeed some lessons to be learnt from this paper. While the underlying data and actual results would not be applicable to a different country with different laws, labour markets, wage structures and social institutions, its final conclusions could. The fact that different minimum wage regimes work under different circumstances is probably part of the reason why Neumark et al. (2004) found effects that differ greatly from those that I have found. They were investigating effects of another minimum wages, in another country and time. While this is true, the final and key lesson from the work of this paper is internationally salvageable. This lesson is that there may be unintended spill-over effects on this often overlooked group of workers. Any policy maker wishing to promote equality by means of minimum wages should keep this in mind.

7 Conclusion

The implementation of the MiLoG in 2015 was unique for Germany who previously did not have a federal statutory minimum wage. Together with the fact that the SOEP has a tradition of collecting quality micro data through questionnaires, the MiLoG presents a rare opportunity to investigate its consequences on an often overlooked part of the wage distribution. Namely those individuals with hourly earnings of in between 100% and 110% of the new federal wage floor before its implementation.

My statistical analysis suggests that the hourly wages of these workers are experiencing negative spill-over effects caused by the implementation and later adjustment of the MiLoG. The statistical analysis did however not indicate any causation between the varying treatment intensity of the MiLoG and total labour earnings, employment probabilities and monthly hours worked. A comparison of means does however suggest a correlation between the MiLoG and a decline in employment probabilities and total labour earnings, as well as a plateau in monthly hours worked. Regardless if the negative trends are correlating with the MiLoG or if there is measurable causation, the results of this paper indicate that policy makers need to also consider the impact on these individuals when building minimum wages. The German government implemented the reform in order to lift those with the lowest wages, a goal that at least partly succeeded according to the results of Caliendo et al. (2017). This move did, however, negatively affect those individuals who were marginally not included in that group. In this sense the government has moved towards greater equality by lifting the group with the lowest incomes closer to the general population, but also managed to adversely affect other low wage workers. These two wage groups on the lower end of the wage distribution seem to have been concentrated around a predetermined wage floor. At the same time, workers from the sample in the more intensely treated East Germany had a lower wage in 2014 and suffered a negative relative spill-over effect. In this way the government may also have unwillingly contributed to solidifying income disparities between the East and the West.

Whether these spill-over effects are due to an anchoring in wage development or a general decrease in demand for low skilled labour through phenomenon such as automatization, is not clear. Considering that the statistical analysis could only find results for the hourly wage, it is tempting to believe that employers are anchoring wages to a new government approved low. Perhaps it is harder for these individuals to argue that their wages are unfair when the government has set a wage floor below their current wages. Taking the general trend into consideration however, a new angle is exposed. As mentioned earlier in the descriptive statistics section, the post-MiLoG trend seems to be that employment is decreasing while the average weekly hours worked has plateaued for this group of individuals. Assuming that the individuals of my sample supply labour that is similar in skill level as those individuals below the wage floor, one might instead be inclined to believe that the lower wages are due to a decline in demand of low skilled labour. This could be the case since the costs to employ such labour has on average seen an exogenous increase due to the MiLoG. In this scenario, the causation could potentially work through channels described by Aretz et al. (2013) or Lordan and Neumark (2018). One other piece of evidence that is indicating that this may be the case, is the results this paper produced when confirming the results of Caliendo et al. (2017). These results say, among other things, that individuals who earned less than the MiLoG in 2014 experienced a drop in average monthly hours worked as a result of the MiLoG. Continuing this line of thought, one could start a discussion around whether policy makers can accept these adverse consequences or if there needs to be other labour market programs to stop these individuals from being adversely affected. Whether these are the channels through which causality operates is however unclear and is an excellent topic for future research. These issues, uncertainties and policies are unfortunately not covered by this paper, but the potential for further discussion exists.

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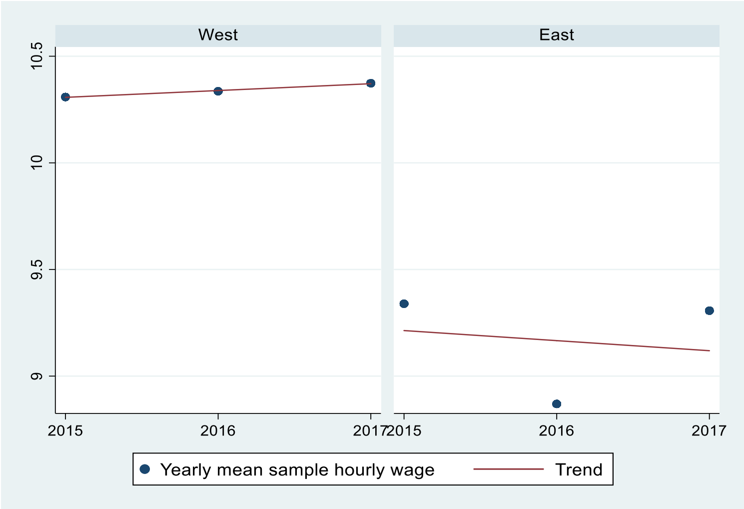
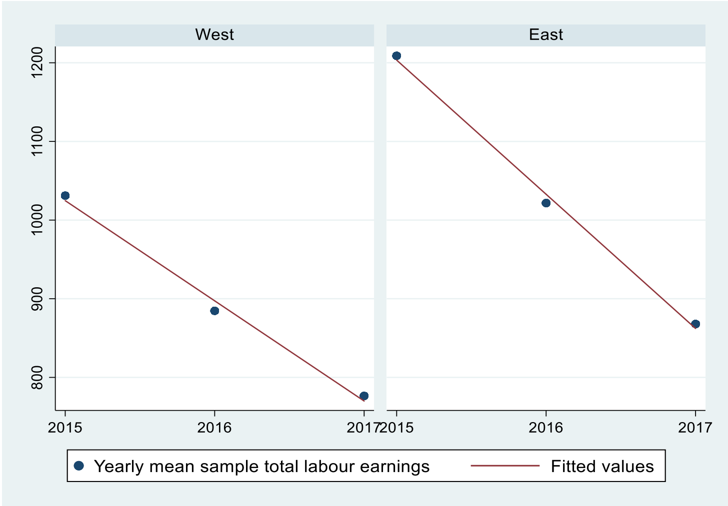
Appendix

Table A1: Descriptive statistics, trimmed sample.

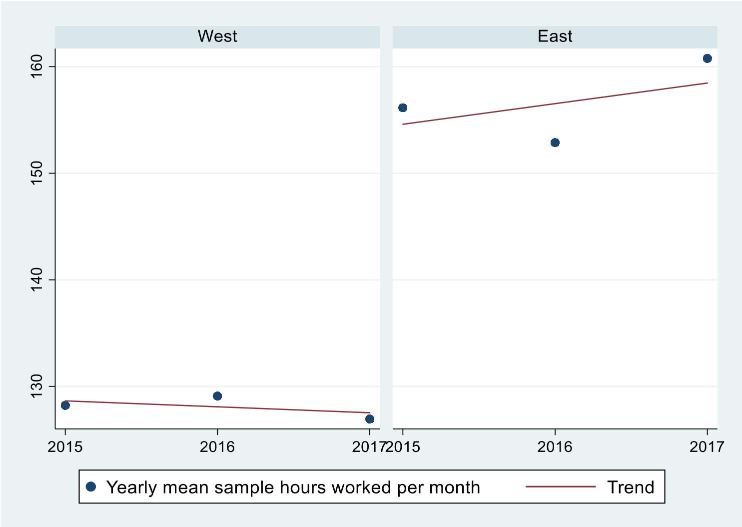
Variable	Obs	Mean	Std. Dev.	Min	Max
Monthly hours worked	4,045	123.1019	71.94648	0	320
Hourly wage	4,045	8.692477	5.263646	0	89.5
Employment probability	4,045	.8988875	.3015149	0	1
Total labour earnings	4,045	1238.349	898.4585	0	10300
Regional bite	4,045	.246713	.0531929	.2137	.3324
Gender (1=man)	4,045	.4123609	.4923203	0	1
Born in Germany (1=yes)	4,045	.7206428	.4487388	0	1
East Germany (1=yes)	4,045	.2781211	.4481288	0	1
Married (1=yes)	4,045	.5290482	.4992172	0	1
Age	4,045	42.30878	11.95064	16	65
Number if kids below 16 in household	4,045	1.194314	1.027715	0	8



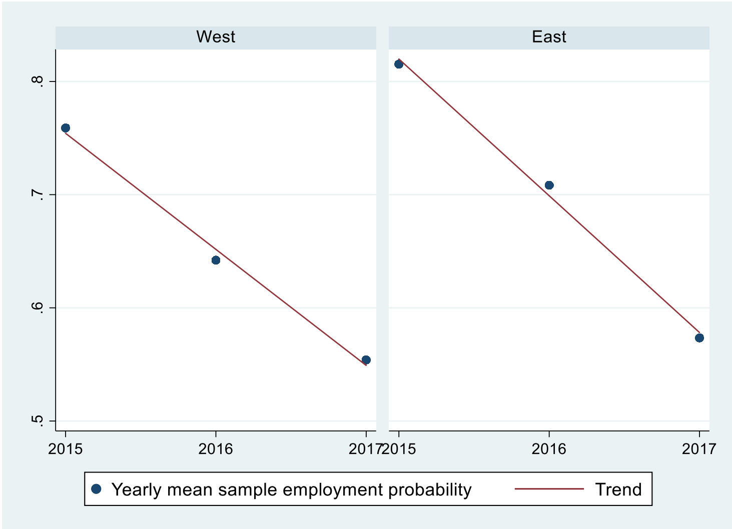
Graph A1: Development of hourly wages, by region, trimmed sample.



Graph A2: Development of total labour earnings, by region.



Graph A3: Development of hours worked, by region, trimmed sample.



Graph A4: Development of employment probability, by region.

Table A2: Regression results for hourly wage and monthly hours worked, trimmed sample, with controls for clustered standard errors on individual level. Demographics include dummies for age, gender, marriage and if the individual is born in Germany as well as a variable for number of kids under 16 in the household.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES		Hourly wage			Monthly hours worked	
2015*Bite	-7.805*	-7.399*	-7.805*	-35.42	-32.33	-35.42
	(4.186)	(4.171)	(4.186)	(37.37)	(37.36)	(37.37)
2016*Bite	-11.80***	-11.39***	-11.80***	-57.74	-54.57	-57.74
	(3.717)	(3.713)	(3.717)	(42.26)	(42.20)	(42.26)
2017*Bite	-8.472**	-8.433**	-8.472**	10.97	11.26	10.97
	(3.972)	(3.972)	(3.972)	(45.02)	(45.01)	(45.02)
2015	3.909***	3.827***	3.909***	28.11***	27.49***	28.11***
	(1.120)	(1.117)	(1.120)	(9.629)	(9.633)	(9.629)
2016	4.848***	4.763***	4.848***	33.36***	32.70***	33.36***
	(0.965)	(0.962)	(0.965)	(10.84)	(10.82)	(10.84)
2017	4.023***	4.053***	4.023***	14.59	14.82	14.59
	(1.062)	(1.063)	(1.062)	(11.61)	(11.62)	(11.61)
Bite	-5.524***	-5.964***		188.1***	184.8***	
	(1.968)	(2.000)		(34.48)	(34.52)	
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Personal fixed effects		Yes			Yes	
Regional fixed effects			Yes			Yes
Observations	4,045	4,045	4,045	4,045	4,045	4,045
R-squared	0.105	0.113	0.105	0.215	0.217	0.215

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A3: Regression results for total labour income and employment probabilities, untrimmed sample, with controls for clustered standard errors on individual level. Demographics include dummies for age, gender, marriage and if the individual is born in Germany as well as a variable for number of kids under 16 in the household.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES		Total labour earnings			Employment probability	
2015*Bite	77.84	93.06	77.84	0.0886	0.0898	0.0886
	(492.3)	(492.2)	(492.3)	(0.260)	(0.260)	(0.260)
2016*Bite	-443.5	-439.5	-443.5	0.0668	0.0671	0.0668
	(523.7)	(524.9)	(523.7)	(0.281)	(0.282)	(0.281)
2017*Bite	-748.0	-742.3	-748.0	-0.314	-0.314	-0.314
	(557.4)	(559.3)	(557.4)	(0.313)	(0.313)	(0.313)
2015	-88.67	-89.18	-88.67	-0.0832	-0.0833	-0.0832
	(127.6)	(127.6)	(127.6)	(0.0667)	(0.0667)	(0.0667)
2016	-81.75	-79.40	-81.75	-0.163**	-0.163**	-0.163**
	(135.1)	(135.5)	(135.1)	(0.0718)	(0.0718)	(0.0718)
2017	-111.9	-109.2	-111.9	-0.153**	-0.152**	-0.153**
	(140.9)	(141.4)	(140.9)	(0.0773)	(0.0774)	(0.0773)
Bite	505.0	435.0		0.222	0.216	
	(414.0)	(415.0)		(0.157)	(0.156)	
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Personal fixed effects		Yes			Yes	
Regional fixed effects			Yes			Yes
Observations	4,845	4,845	4,845	4,845	4,845	4,845
R-squared	0.167	0.172	0.167	0.201	0.202	0.201

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1