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Do Minimum Wages Improve Self-Rated Health? Evidence from a Natural Experiment

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Contents

Abstract

In this paper, we analyze whether the introduction of the general minimum wage in Germany in 2015 had an effect on workers' self-rated health. To study this question, we use survey-data linked to administrative employment records and apply difference-in-difference regressions combined with propensity score matching. This approach enables us to control for a vast set of possibly confounding variables. We find on average significant improvements of self-rated health for individuals who are affected by the reform. Our results indicate, that a significant reduction of weekly working hours potentially drives this result.

Zusammenfassung

Wir untersuchen, ob die Einführung des allgemeinverbindlichen Mindestlohns in Deutschland in 2015 einen Effekt auf die selbsteingeschätzte Gesundheit der Arbeitnehmer hatte. Um diese Frage zu beantworten nutzen wir Befragungsdaten, die mit administrativen Beschäftigungsdaten verknüpft wurden. Wir führen Differenzen-in-Differenzen Schätzungen mit Propensity-Score Matching durch, was uns erlaubt eine Vielzahl von potentiellen Störgrößen zu berücksichtigen. Wir finden heraus, dass die Mindestlohneinführung eine signifikante Verbesserung der selbsteingeschätzten Gesundheit zur Folge hatte. Unsere Ergebnisse deuten darauf hin, dass dieser Effekt auf sinkende Wochenarbeitszeit zurückzuführen ist.

JEL classification: I10, I18, J38
Keywords: Keywords Minimum Wage, self-rated health, natural experiment

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1 Introduction

In 2015, the German labor market experienced the introduction of a statutory, economy-wide minimum wage of 8.5 Euro. Prior to 2015, minimum wages had only been implemented in certain industries. According to Bellmann et al. (2015), around 12 percent of all establishments employed at least one worker with a wage below the minimum wage in the year prior to the reform.

So far, studies that evaluate the effects of minimum wages typically focus on labor market outcomes such as the effects on employment or the wage distribution.[1] However, research on the effects of minimum wages on non-labor market outcomes like health is only recently on the rise and far from being conclusive (see among others Reeves et al., 2017; Kuroki, 2018). One reason for this might be that there are plenty of possible channels that could explain how minimum wages could affect health – as Leigh/Leigh/Du (2019) discuss. On the one hand, the minimum wage can be interpreted as a positive income shock that leads to higher consumption of healthcare goods and services (if these are normal goods). On the other hand, if an increase in the minimum wage has dis-employment effects, consumption of healthcare goods and services could also decrease. Moreover, if working hours are reduced in response to higher wages, this could have psychosocial or stress-induced effects on workers' health. Since these theoretical implications are a priori ambiguous, it is ultimately an empirical question whether and in which way the minimum wage affects workers' health. In this study, we contribute to the growing literature of empirical articles that focus on non-labor market outcomes by examining the effects of the introduction of the German minimum wage on workers' self-rated health. To the best of our knowledge, this paper is the first to study the introduction of the German minimum wage as a natural experiment in the health context.[2] We use survey data from the German Institute for Employment Research (IAB) combined with high-quality administrative records of the Federal Employment Agency ("PASS-ADIAB")[3] and apply regression adjusted difference-in-difference models to identify the health effect of the minimum wage reform. We apply propensity score matching in order to make treated and controls more comparable on a vast set of characteristics. Treated and controls are categorized according to their hourly wages in the year prior to the reform, where individuals with hourly wages below 8.5 Euro are assigned to the treated group while individuals earning hourly wages of at least 8.5 Euro are assigned to the control group. Our estimates indicate that the introduction of the minimum wage caused significant improvements in self-rated health. This result holds across a variety of robustness checks. Furthermore, we find that the health improvements are potentially driven by a reduction of weekly working hours, which is consistent with previous studies (see among others Bossler/Gerner, 2016).

Our paper is related to several recent studies that examine the effects of minimum wages on different health outcomes. Some of the literature from the US analyze the relationship between minimum wages and risky health behaviors. Adams/Blackburn/Cotti (2012) and

[1] See e.g. Neumark/Wascher et al. (2007) for a review of studies.

[2] For studies that evaluate labor-market related effects of the German minimum wage reform see for example Bossler/Gerner (2016), Bonin et al. (2018), and Bossler et al. (2019). Only a few authors analyze non-market outcomes such as job- or life satisfaction (Bossler/Broszeit, 2017; Pusch/Rehm, 2017; Gülal/Ayaita, 2018).

[3] See Antoni/Dummert/Trenkle (2017) for details.

Sabia/Pitts/Argys (2014) find that higher minimum wages increase alcohol related traffic fatalities among teenagers. Wehby/Dave/Kaestner (2016) observe higher minimum wages to be associated with increased birth weight. Komro et al. (2016) identify a positive effect of minimum wages on birth weight as well as a decrease in postneonatal (28—364 days after birth) mortality. Bullinger (2017) provide evidence that increasing the minimum wage leads to a reduction of teenage births. Results of Pohl/Clark/Thomas (2017) suggest that increases in the minimum wage have a modest but positive effect on fruits and vegetables consumption. This finding is however not supported by Andreyeva/Ukert (2018) who find that minimum wage increases lower the consumption of fruits and vegetables and raise the probability of being obese.

Furthermore, Andreyeva/Ukert (2018) find minimum wage increases to be positively associated with health care access and self-rated health, which is the outcome of main interest in our study. Du/Leigh (2017) provide evidence for a negative association between minimum wages and absence of work due to illness. This is possibly driven by health changes, as they also detect significant improvements in self-rated health after minimum wage increases. Horn/Maclean/Strain (2017) analyze whether increased minimum wages improve self-rated health of workers. Their results do not suggest that this is the case. On the contrary, their estimates even suggest a deterioration of self-rated health for unemployed male workers. Averett/Smith/Wang (2017) obtain heterogenous self-rated health effects of minimum wage increases among teenagers of different ethnicities. For those actually experiencing an increase in earnings only white women rated their health better, while white men and hispanic women did on average not significantly alter their self-rated health.

Besides the growing literature from the US, studies similar to our work analyze health effects of the 1999 national minimum wage introduction in the UK (Reeves et al., 2017; Kronenberg/Jacobs/Zucchelli, 2017; Lenhart, 2017). Reeves et al. (2017) find significant improvements of mental health after the minimum wage introduction, which is potentially driven by a reduction of financial strain. Estimates of Kronenberg/Jacobs/Zucchelli (2017) do not support these results as they do not provide evidence for mental health improvements of affected workers while using the same data. Lenhart (2017) finds significant improvements of self-rated health and other measures of health.

Most of the authors apply difference-in-difference models to identify the effects of interest. However, definitions of treated and control groups usually differ across studies. Studies analyzing health related effects for the US mostly exploit variation of the minimum wage across or within states. Usually, the treated group consists of individuals who reside in states with changes in minimum wage regulations while control group members reside in states without changes in minimum wage regulations. In contrast, this study as well as studies analyzing the UK minimum wage reform use individual hourly wages to define treated and controls. Individuals in the treated group earn hourly wages below the minimum wage prior to the reform, whereas control group members earn hourly wages of at least the minimum wage prior to the reform. Our paper contributes to this literature by examining the effect of the German minimum wage reform on self-rated health of affected individuals. Due to mixed findings of the previous literature, limited external validity caused by institutional differences between US, UK and German labor and healthcare markets, the analysis of health-effects

encountered after the German reform is a relevant extension of existing studies.

The remainder of this paper is organized as follows. Section 2 outlines the estimation procedure. Next, we provide information about the data used, the estimation sample as well as the used covariates in Section 3. Section 4 presents the estimation results and Section 5 concludes.

2 Estimation Procedure

In order to identify the effect of the German minimum wage reform on self-rated health, we rely on regression adjusted difference-in-difference models as well as a combination of matching and regression adjusted difference-in-difference models as suggested by Heckman/Ichimura/Todd (1997).

The treated group contains individuals whose hourly wage is below 8.5 Euro, while the individuals of the control group earn an hourly wage of at least 8.5 Euro. The underlying idea behind this categorization is that individuals in the treated group should be affected by the minimum wage reform, while individuals in the control group should not be affected. This is a common approach in the literature that evaluates minimum wage reforms (see among others Bossler/Broszeit, 2017; Kronenberg/Jacobs/Zucchelli, 2017; Arulampalam/ Booth/Bryan, 2004; Stewart, 2004). Our definition of the treated and control group is solely based on an individual's hourly wage in his or her main job at the time of the interview in the year prior to the reform. Thereby, we do not restrict the analysis to individuals from the treated and control group who actually receive or not receive the minimum wage. Thus, we identify the intention-to-treat effect, which may differ from the average treatment effect on the treated. Due to potential measurement error in hourly wages or due to a lack of compliance on behalf of the employers, there may be individuals who do not receive the minimum wage, although they are in the treated group (Lenhart, 2017). Both, non-compliance and measurement error would probably attenuate the effect towards zero. Hence, we think that the intention-to-treat effect represents a conservative measure of the treatment effect.[4]

There seems to be no clear-cut consensus with respect to which upper hourly wage threshold should be chosen in order to define the control group. Some authors, such as Stewart (2004) or Reeves et al. (2017) use a very low upper hourly wage threshold of 110 percent of the minimum wage. Others use higher thresholds: Kronenberg/Jacobs/Zucchelli (2017) use 140 percent, Pusch/Rehm (2017) and Gülal/Ayaita (2018) use around 150 percent, Lenhart (2017) use about 170 percent, whereas Bossler/Broszeit (2017) use no upper threshold at all. The main purpose of a narrowly defined hourly wage band of the control group is to ensure comparability of the treated and control group, which seems plausible for individuals whose hourly wages are very close. Any small hourly wage band has the drawback of lower case numbers in the remaining control group. Therefore, in order to obtain more observations in the control group we chose an upper hourly wage threshold of 20 Euro

[4] Alternatively, one could limit the treated and control group individuals to those who actually receive or not receive the treatment. We abstain from doing so to avoid selection bias that might occur "if those who remain below the minimum wage are more susceptible to worsening health" (Reeves et al., 2017: p.20).

(235 percent of the minimum wage), but additionally control for differences in observable characteristics across groups via regression adjustment and matching.

Our main specifications take into account the panel structure of the data by including both individual and year fixed effects in the difference-in-difference estimation. The common trends assumption must hold either unconditionally or conditionally on the covariates. Combining regression adjusted difference-in-difference models with matching probably reduces the risk of violating this assumption. In a refined approach, we therefore augment the basic model by a variety of demographic, socioeconomic and labor market related pre-treatment covariates. Next, we use a matching procedure based on pre-treatment covariates and in some specifications additionally on pre-treatment self-rated health outcomes. We think that this approach reduces the potential self-selection into the treated and control group of workers that differ in their health status.

Comparable to the procedure of Marcus (2014), we implement three steps to identify the treatment effect in the regression models with matching. First, we run a probit model to estimate the propensity score, i.e. the probability of receiving the treatment conditional on the covariates, which as mentioned above include demographic, socioeconomic and labor market related pre-treatment covariates in one specification, and additionally pre-treatment self-rated health outcomes in another specification. Individuals from the treated group, whose propensity score is above/below the maximal/minimal propensity score in the control group (i.e. the common support restriction) are exluded. Next, similar to the implementation of Heckman/Ichimura/Todd (1997) and Marcus (2014) we perform kernel matching with a bandwidth of 0.06 to obtain the weights. We tried other matching algorithms, e.g. nearest-neighbor matching with and without caliper and varying numbers of neighbors. However, Kernel-Matching performed best in terms of establishing covariate balance between treated and controls. Subsequently, we run weighted regressions using the obtained weights from matching.[5]

3 Data, Sample and Variables

Data source

We use the PASS-ADIAB dataset, which links survey data from the German panel study 'Labour Market and Social Security' (PASS) with administrative data from the Federal Employment Agency (Antoni/Bethmann, 2018). The PASS is a longitudinal survey of households in Germany, conducted annually by the Institute for Employment Research (Trappmann et al., 2010). It was originally established to study effects of the largest labor market reforms in Germany – the 'Hartz-reforms'. One essential part of these reforms was the introduction of unemployment benefit II (UBII), which is a means-tested benefit scheme providing financial assistance for households with insufficient income. Accordingly, the

[5] We are aware of the lively discussion about how to deal with uncertainty in models with propensity score matching (see among others Stuart, 2010). We follow Ho et al. (2007) and do not take it into account for the variance estimations. Evidence suggests, that obtained standard errors are too large, and thus lead to more conservative inference.

PASS consists of two subsamples: One subsample represents households in which at least one person receives UBII. The other subsample includes the general population of Germany in which households with low socioeconomic status are oversampled (Trappmann et al., 2013). Since we analyze effects of the minimum wage reform, oversampling of low-income households is an advantage of the data as it provides comparably high case numbers of individuals who are most likely to be affected by the minimum wage introduction.

For individuals, who aggreed, the PASS survey data can be linked to individual administrative data - so called "Integrated Employment Biographies" (IEB) from the records of the Federal Employment Agency (see Dorner et al. (2010) for an overview of the IEB). These data stem from mandatory social security notifications by employers as well as from the Federal Employment Agency. Information like start and end dates (with daily precision) of spells in employment subject to social insurance are documented reliably, as they are relevant for the calculation of pension and unemployment entitlements (Jacobebbinghaus/Seth, 2007; Antoni et al., 2016). In addition to the individual administrative data, the dataset contains administrative establishment data from the "Betriebs-Historik Panel" which provides information about e.g. firm size or economic branch (see Schmucker et al. (2016) for information on the establishment data).

Sample

Our analysis is based on waves 6 to 9 (years 2012-2015) of the PASS. In the year 2014, 11,590 individuals were interviewed in the personal questionnaire of the PASS. Since not all individuals have a record in the administrative data at the time of the survey interview (such as civil servants, self-employmed etc.), the number of individuals reduces to 7,567. This includes individuals who refused linkage or were not registered as unemployed or employed on the date of the interview.[6]

Generally, employers have to pay at least the hourly minimum wage from the first of January in 2015 onward. However, certain worker groups are exempted from the minimum wage either temporarily or permanently and are therefore excluded from the analysis. Permanently excluded are long-term unemployed, apprentices, interns, and individuals aged below 18 years without completed vocational training.[7] Temporary exceptions are employees in branches with already existing industry-specific minimum wages or other special legal regulations. The linkage with administrative establishment data offers the advantage of properly identifying and consequently excluding these groups from the sample. After the exclusion of these groups, 6,110 individuals remain in the sample.

Next, we exclude individuals with no valid information on working hours, wages, or employment status. After the exclusion of all inconsistencies and individuals with missing values in the variables, which are necessary to calculate hourly wages, the sample comprises 5,255 individuals. After the assignment into treated and control group, which in the main

[6] We use parts of Eberle/Schmucker et al. (2017) to properly link the survey data to the administrative records.

[7] See "Gesetz zur Regelung eines allgemeinen Mindestlohns (Mindestlohngesetz - MiLoG (2014, August 11))".

specification restricts the sample to individuals who were in regular employment in the year 2014, the sample consists of 2,247 individuals. However, only 1,188 individuals are present in the data for the entire period from 2012 to 2015. Consequently, the remaining balanced sample consists of 277 treated and 911 untreated individuals.

The sample in the main specifications comprises individuals who are working either full or part-time employed under social security contributions in the year 2014 and are potentially affected by the minimum wage reform. We do not impose restrictions on the employment status in the year of the reform, as we want to capture the total health-effect of the reform - this includes potential employment effects which could influence self-rated health.[8]

Variables Used in the Empirical Analysis

The main outcome variable is the answer to the question: 'How would you describe your general health status in the last four weeks?', where the five possible answer categories range from *very good* to *bad*. We create a binary outcome variable that takes on the value one if the individual claims to be in *very good* or *good* health, while the value zero represents *satisfactory*, *poor* and *bad* self-rated health. Dichotomizing the ordinal variable enables to consider the panel dimension of the data by estimating fixed-effects models where unobserved time-invariant individual heterogeneity as well as time-trends are eliminated. In order to show the robustness of the estimates with respect to (i) the cross-sectional identification of the effect and (ii) the non-linearity of the dependent variable we additionally perform cross-sectional and logit regressions. These results are very similar to the ones in our main specification, however they yield slightly higher standard errors (see Table 9 in Appendix A).

As stated before, the definition of the treated and control group is based on hourly wages in an individual's main job. We calculate hourly wages by dividing the self-reported monthly gross wage by the self-reported average monthly working hours (including overtime).[9] We use the actual working hours since overtime is subject to the minimum wage regulation just like contractual working hours and must therefore be compensated financially or in terms of time.

The covariates in our analysis can be categorized into demographic, socioeconomic and labor market related worker characteristics. Demographic covariates include age, gender, migration background, and region of residence. Socioeconomic covariates contain years of education, monthly equivalised household income (modified OECD scale as in Hagenaars et al., 1994), a measure of socioecopppnomic status (international socio-economic index (ISEI) as in Ganzeboom/Treiman, 1996), marital status, and the number of children in the

[8] Studies with a similar identification strategy restrict the sample to individuals who are employed in both 2014 and 2015 see (Gülal/Ayaita, 2018) or even in the same job (Pusch/Rehm, 2017) in order to disentangle the effect of the reform from the effects of gaining employment or changing jobs. In a robustness check, we also estimate models with a sample, where only individuals who are employed both in 2014 and 2015 are taken into account. This does not alter our findings (see Table 9 in Appendix A).

[9] Monthly working hours equal weekly working hours multiplied by the average number of weeks in a month ($52/12$).

household. Linking survey data with administrative records also allows controlling for labor market related characteristics, including information about the total number of days in regular employment, number of days in the current job as well as total days with social benefit receipts, and the firm size of an individual's current employer.[10]

Descriptive Statistics and Matching Results

Table 1 shows the mean values of the covariates in the year 2014 and pre-reform self-rated health outcomes for the treated group as well as the control group before and after propensity score matching, respectively (see Table 7 and 8 in Appendix A for more detailed statistics on our covariates). Column two and three show the mean differences between the treated and control group. Columns four and five of Table 1 display the standardized differences between the treated and the control group before and after propensity score matching, respectively.[11] Although the metric does not deliver a clear categorization of good or bad matches, the empirical literature views satisfactory matching quality for standardized differences below 3 or 5 percent (Caliendo/Kopeinig, 2008).

The average standardized bias before matching (37.69) indicates rather large covariate differences between the treated and control group, which are reduced substantially by the matching approach (mean standardized bias after matching of 4.39). Before matching, significant differences between the treated and control group are found especially among the labor market related variables. Individuals in the treated group have spent on average fewer days in employment and more days receiving social benefits, work in companies with less employees and work more frequently on a part-time basis. Socioeconomic variables are also significantly different before matching. Individuals in the control group have on average a higher household income as well as a higher socioeconomic status and are more likely to be married. Furthermore, participants in the control group have on average a higher level of education. Among the demographic variables it is noticeable that the proportion of people living in Eastern Germany is significantly higher in the treated group than in the control group. The proportion of women in the treated group is higher than in the control group.

Although the standardized bias after matching is not below the mentioned 5 percent for all covariates, we still regard the matching result as a success with respect to the previously existing large differences. Except for the part-time variable, the mean deviations after matching are close to the 5 percent threshold. Furthermore, the t-tests yield no statistically significant mean differences of the covariates after matching. Nevertheless, we include the pre-treatment values of the covariates in some specifications of the regression analysis

[10] We use the values of the covariates in the pre-treatment year for the calculation of the propensity scores. In other specifications, we additionally condition the propensity score on all pre-treatment self-rated health outcomes (2012-2014). All regression adjusted models include covariates of all pre-treatment years (2012-2014) as well as year-dummies to capture time trends.

[11] The standardized bias in percent $[\frac{100(\overline{x_t}-\overline{x_c})}{\sqrt{0.5(Var(x_t)+Var(x_c))}}]$ represents the mean difference of the treated and control group for each covariate $(\overline{x_t}-\overline{x_c})$ as a percentage of the square root of the average of the sample variance (Rosenbaum/Rubin, 1985).

Table 1: Pre-Treatment Means of Treated, Unmatched Controls and Matched Controls

		Controls		Standardized bias %	
	Treated	Unmatched	Matched	Unmatched	Matched
Demographic					
Age	45.36	44.59	46.03	7.63	6.71
Female	0.65	0.56***	0.65	19.71	1.12
Migrant	0.19	0.21	0.17	5	5.69
East	0.62	0.31***	0.62	65.81	0.11
Socioeconomic					
Years of Education	11.37	12.39***	11.29	47.6	3.39
Household Income	1135.73	1651.87***	1168.79	79.29	5.08
Socioeconomic Index	34.05	40.86***	35	54.3	7.58
Married	0.48	0.55**	0.46	14.45	3.3
Number of Children	0.92	0.84	0.88	7.36	3.21
Labor Market Related					
Days in employment	4871.35	6036.92***	4878.44	41.4	0.25
Days in Current Job	1550.95	2543.58***	1453.3	47.3	4.65
Days Social Benefits	2292.19	985.55***	2377.48	85.76	5.6
Firm-size	159.86	414.9***	193.99	29.55	3.95
Part-Time	0.47	0.36***	0.52	22.49	10.84
Past Self-Rated Health					
SRH_{2014}	0.45	0.5	0.47	10.2	2.85
SRH_{2013}	0.43	0.47	0.44	7.96	1.75
SRH_{2012}	0.54	0.52	0.56	3.43	4.77
Avg. Standardized bias %				37.69	4.39

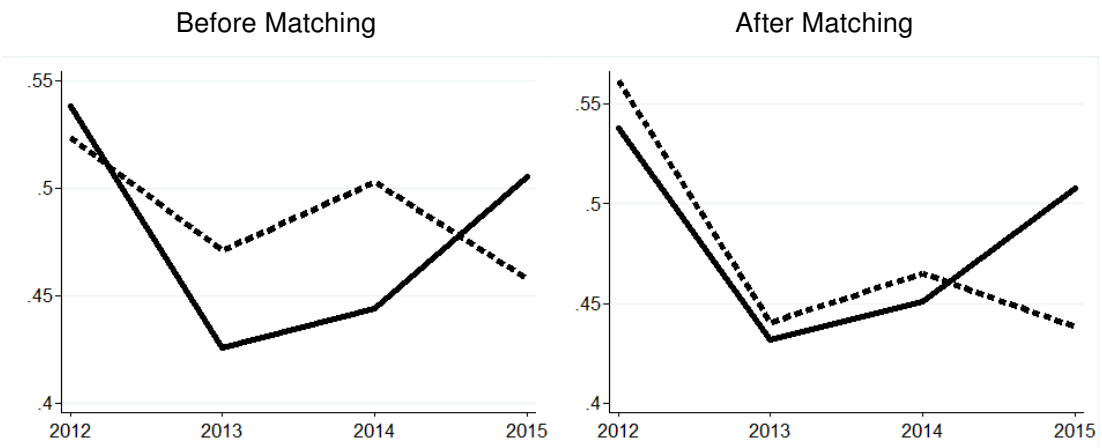
Notes: Stars indicate p-values of two-sided t-tests, testing whether there is a statistically significant difference between the treated and unmatched or matched controls, respectively. * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Data source: PASS-ADIAB.

in order to control for covariate differences as the matching did not perfectly balance the covariates of the control and treated group.

Figure 1 displays the development of the share of individuals in good or very good health for the treated (solid lines) and control group (dashed lines) for the years 2012 to 2015 before and after propensity score matching, respectively. The identifying assumption of our estimation approach is that the share of individuals who rate their health as good or very good would have developed similarly if the minimum wage reform had not taken place. In order for this assumption to hold, the lines should be parallel before the intervention.

Before matching, the lines are clearly not parallel between 2012 and 2013, however between 2013 and 2014, the development across treated and control group appears to follow a similar pattern. After matching on all covariates as well as past self-rated health, the parallel trend assumption does not seem to be violated, as the lines are fairly parallel in the right panel. Both panels reveal an increase of self-rated health for the treated group after the reform. In order to allow for a more causal interpretation of the influence of the minimum wage reform on self-rated health, we continue with the presentation of the regression results in the following section.

Figure 1: Share of Individuals with Good or Very Good Self-Rated Health



Notes: The Figure illustrates how the share of individuals who rate their health as good or very good developed over time. The left panel illustrates this development for the sample before matching. The right panel shows this development for the weighted sample after propensity score matching including past self-rated health outcomes and the other covariates. Dashed (Solid) lines represent individuals from the control (treated) group. Data source: PASS-ADIAB.

4 Estimation Results

This section first presents the regression results, examining the impact of the minimum wage reform on self-rated health of affected individuals. This analysis is supplemented by a series of placebo tests and robustness checks that investigate the influence of measurement errors and spillover effects. At the end of this section, we present results of the influence of the minimum wage reform on the working hours and gross wages of the affected individuals.

4.1 Effect on Self-Rated Health

Table 2 summarizes the regression results of the main specifications. The estimated treatment effect remains fairly stable across the different model specifications. Neither the inclusion of covariates nor the use of matching have a noticeable effect on the coefficient. The magnitude of the effect implies that the introduction of the universal minimum wage has on average increased the treated individuals' probability of assessing their health as good or very good by 8 to 9 percentage points.

Table 2: Average Treatment Effect of the Minimum Wage Reform on Self-Rated Health

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effect	0.08**	0.08**	0.09**	0.09**	0.08**	0.09**
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Number of obs.	4752	4445	4628	4434	4628	4434
Control Variables		✓		✓		✓
PS-Matching with:						
control variables			✓	✓	✓	✓
past self-rated health					✓	✓

Notes: The first two columns contain the estimation results for the specifications without matching. Columns three and four contain the estimation results of the models with matching on the characteristics of the covariates from the year before the reform. Columns five and six report the estimation results with matching where in addition to the characteristics of the covariates from the year prior to the reform the values of self-rated health from years prior to the reform are included. The even columns contain the estimates controlling for the covariates, the odd columns contain the estimation results for the models without controlling for the covariates. Standard errors in parantheses; * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Data source: PASS-ADIAB.

Measurement Error in the Hourly Wage Measure

As mentioned before, the reported actual working hours and thus the calculated hourly wage may differ from the true hourly wage an individual earns. If that difference leads to systematic misclassification into either treated or control group, the underlying measurement error may pose a threat to the identification of the unbiased treatment effect. Systematic misclassification would probably lead to an underestimation of the true effect of the minimum wage introduction as individuals who receive an hourly wage above the minimum wage threshold are falsely assigned to the treated group, even though they would probably not be directly affected by the reform.

The probability of falsely assigning individuals to the treated or control group should be higher the closer the calculated hourly wage is to the minimum wage threshold of 8.5 Euro. Therefore, following Bonin et al. (2018) and Pusch/Rehm (2017), we exclude individuals whose calculated hourly wage is near this threshold. In one specification we exclude all individuals whose calculated hourly wage is either 5 percent above or below the minimum wage threshold. In another specification we exclude all individuals whose hourly wage is between 8.25 Euro and 8.75 Euro. If measurement error and thus systematic misclassification was an issue, we would expect an upward deviation of estimated coefficients in these specifications compared to the estimates without the exclusion of individuals close to the threshold.

Table 3 shows the regression results of the main specification as well as the regression results of both specifications which are intended to reduce the potential measurement error problem. It is noticeable that the results of both additional specifications are very similar to the regression results of the main specifications. Columns (1) and (2), i.e. the regressions without matching, show that the estimated coefficients are virtually identical across the three specifications. Columns (3) to (6) reveal minor differences: In the specification where all individuals with hourly wages between 8.25 Euro and 8.75 Euro are excluded, the coefficients in the estimations with matching are slightly lower if covariates are included in

the regressions (columns (4) and (6)). If covariates are not included, the coefficient remains unchanged in the first matching variant (column 3) while it is slightly higher in the second matching variant (column 5). If all individuals whose hourly wages lie between 8.095 Euro and 8.925 Euro are excluded, the estimated coefficients in the regressions with matching increase slightly more if covariates are not included. The estimated coefficients are identical to the main specifications, if covariates are included in the matching regressions. Based on these regression results, we conclude that measurement error does not seem to bias the estimates of the treatment effect.

Table 3: Exclusion of Hourly Wages Close to the Minimum Wage Threshold

	(1)	(2)	(3)	(4)	(5)	(6)
Main specifications						
Treatment effect	0.08**	0.08**	0.09**	0.09**	0.08**	0.09**
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Number of obs.	4752	4445	4628	4434	4628	4434
Excluding hourly wages between 8.25 and 8.75 Euro						
Treatment effect	0.08**	0.08**	0.09**	0.08**	0.09**	0.08**
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Number of obs.	4532	4252	4420	4242	4420	4242
Excluding hourly wages between 8.075 and 8.925 Euro						
Treatment effect	0.08**	0.08**	0.10**	0.09**	0.10**	0.09**
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Number of obs.	4344	4089	4244	4079	4244	4079
Control Variables		✓		✓		✓
PS-Matching with:						
control variables			✓	✓	✓	✓
past self-rated health					✓	✓

Notes: The first two columns contain the estimation results for the specifications without matching. Columns three and four contain the estimation results of the models with matching on the characteristics of the covariates from the year before the reform. Columns five and six report the estimation results with matching where in addition to the characteristics of the covariates from the year prior to the reform the values of self-rated health from years prior to the reform are included. The even columns contain the estimates controlling for the covariates, the odd columns contain the estimation results for the models without controlling for the covariates. Standard errors in parantheses; * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Data source: PASS-ADIAB.

Placebo Reform

Following Lenhart (2017) and Gülal/Ayaita (2018) we also apply placebo tests in which we pretend that the reform took place one year prior to the actual minimum wage reform. The years 2012-2014 are considered for this analysis. Table 4 summarizes the regression results of the placebo reform. Keeping all other factors of the main specification constant, the placebo reform does not induce significant effects on self-rated health. We take this as support for our main result.

Table 4: Average Treatment Effect of the Minimum Wage Reform on Self-Rated Health - Placebo Reform

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effect	-0.03	-0.02	0.00	0.00	-0.02	-0.02
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Number of obs.	4503	4261	4371	4250	4371	4250
Control Variables		✓		✓		✓
PS-Matching with:						
control variables			✓	✓	✓	✓
past self-rated health					✓	✓

Notes: The first two columns contain the estimation results for the specifications without matching. Columns three and four contain the estimation results of the models with matching on the characteristics of the covariates from the year before the placebo-reform. Columns five and six report the estimation results with matching where in addition to the characteristics of the covariates from the year prior to the placebo-reform the values of self-rated health from years prior to the placebo-reform are included. The even columns contain the estimates controlling for the covariates, the odd columns contain the estimation results for the models without controlling for the covariates. Standard errors in parantheses; * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Data source: PASS-ADIAB.

Additional Robustness Checks

We perform additional robustness checks that are presented in Appendix B: (i) In order to rule out that our results are driven by spillover effects, we estimate our presented model over a grid of lower hourly wage thresholds that define the control group. (ii) We estimate the model over a grid of upper hourly wage thresholds that define the control group to avoid artifacts of the chosen upper hourly wage threshold on the estimated treatment effect. (iii) We combine the second additional robustness exercise, where we vary the upper hourly wage threshold, with a placebo reform. Hence, we estimate a placebo reform, that took place in 2014 and estimate the effects of this placebo reform over a grid of upper hourly wage thresholds. (iv) we build placebo groups, where we construct a treatment group with hourly wages almost twice as high as the true minimum wage.
All these additional checks support our main result of a significant positive minimum wage effect on self-rated health, while our placebo tests yield no significant treatment effects.

4.2 Effect on Labor Market Outcomes

In this subsection we present effects of the minimum wage introduction on working hours and wages of individuals who are still employed at the date of interview in 2015. We start with a descriptive summary in Table 5, which displays pre- and post reform means for the treated and control group before matching. The relative change of hourly wages was considerably higher in the treated group. On average the actual/contractual hourly wage in the treated group increased by 27 percent/19 percent, while it only increased by 8 percent/4 percent in the control group. A similar, however slightly lower increase emerged for the gross monthly wage, which grew on average by 20 percent in the treated group while it increased on average by 6 percent in the control group. The average contractual working hours for both the treated and control group as well as the actual working hours for the

control group have hardly changed at all. By contrast, the actual weekly working hours in the treated group have decreased considerably by 6 percent from approximately 39 hours in 2014 to around 37 hours in 2015.

Table 5: Average Working Hours and Wages of the Treated and Control Group

	Treated		Controls	
	2014	2015	2014	2015
Actual Hourly Wage	6.93	8.78	13.43	14.57
Contractual Hourly Wage	7.96	9.49	14.82	15.44
Monthly Gross Wage	1166.39	1402.87	2191.91	2322.55
Contractual Working Hours	34.06	33.92	34.47	34.76
Actual Working Hours	39.26	37.07	37.65	37.73
Number of Obs.	277	252	911	889

Notes: More descriptive statistics of the displayed variables are shown in Table 8 of the Appendix. Data source: PASS-ADIAB.

In order to go beyond this descriptive analysis, we run regression adjusted difference-in-difference models combined with matching. Therefore, we change the outcome variable and use the respective labor market outcomes instead of self-rated health.

We do not find significant changes in the monthly gross wages and contractual working hours, as Table 6 shows. These results indicate that the contractual hourly wages on average did not change significantly after the minimum wage reform. However, the analysis yields a significant increase of the actual hourly wage which is caused by a significant decline of actual working hours. The magnitude of this effect is comparable to the descriptive results obtained from Table 5. The regression results suggest a decrease of the actual weekly working hours of two to three hours for the treated individuals.

A reduction of weekly working hours might be an explanation for improved self-rated health as individuals of the treated group have to work less in order to earn a comparable salary as before the minimum wage reform. Cygan-Rehm/Wunder (2018) report that evidence on the effect of working hours on self-rated health is ambiguous and that the majority of previous studies do not take into account the endogenity of working hours. In contrast, the authors exploit statutory workweek regulations in the German public sector and provide causal evidence that longer working hours worsen self-rated health.

Table 6: Average Treatment Effect of the Minimum Wage Reform on Working Hours and Wages

	(1)	(2)	(3)	(4)	(5)	(6)
Contractual Hourly Wage						
Treatment effect	0.16	0.24	0.42	0.43	0.47	0.49
	(0.29)	(0.30)	(0.41)	(0.41)	(0.42)	(0.42)
Actual Hourly Wage						
Treatment effect	0.31	0.42	0.93**	0.95**	0.97**	1.00**
	(0.55)	(0.58)	(0.43)	(0.43)	(0.43)	(0.43)
Monthly Gross Wage						
Treatment effect	3.77	18.75	66.76	66.62	71.23	71.89
	(38.68)	(40.03)	(65.78)	(65.12)	(66.16)	(65.66)
Contractual Working Hours						
Treatment effect	-0.30	-0.29	-0.20	-0.31	-0.23	-0.33
	(0.24)	(0.24)	(0.36)	(0.35)	(0.36)	(0.35)
Actual Working Hours						
Treatment effect	-2.08***	-2.24***	-2.81***	-2.95***	-2.77***	-2.91***
	(0.36)	(0.37)	(0.59)	(0.58)	(0.59)	(0.58)
N	4532	4400	4419	4389	4419	4389
Control Variables		✓		✓		✓
PS-Matching with:						
control variables			✓	✓	✓	✓
past self-rated health					✓	✓

Notes: The first two columns contain the estimation results for the specifications without matching. Columns three and four contain the estimation results of the models with matching on the characteristics of the covariates from the year before the placebo-reform. Columns five and six report the estimation results with matching where in addition to the characteristics of the covariates from the year prior to the placebo-reform the values of self-rated health from years prior to the placebo-reform are included. The even columns contain the estimates controlling for the covariates, the odd columns contain the estimation results for the models without controlling for the covariates. Standard errors in parentheses; * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Data source: PASS-ADIAB.

5 Discussion and Conclusion

This is the first study to evaluate the effect of a large minimum wage reform in Germany on self-rated health. Studying health effects is of particular interest for economists and policy makers because labor market reforms can have consequences that go beyond labor market outcomes.

Our estimation procedure uses exogenous variation in hourly wages induced by the German minimum wage introduction on the first of January 2015. This natural policy experiment enables the conduction of a difference-in-difference analysis combined with propensity score matching. We compare self-rated health changes of individuals who are most likely affected by the minimum wage reform as their hourly wage prior to the reform was below the hourly minimum wage of 8.5 Euro with individuals who are likely not affected by the reform. We use survey-data combined with administrative records which enables us to control for a vast set of possibly confounding variables.

Our results suggest that the minimum wage introduction leads to a significant improvement of self-rated health of affected individuals, which is in line with several previous studies (Lenhart (2017), Andreyeva/Ukert (2018) and Du/Leigh (2017)). Quantitatively, the increasing hourly

wages increased the probability of rating one's health as good or very good on average by eight to nine percentage points. This effect is robust with respect to several robustness checks concerning measurement error, spillover effects and placebo tests. Our results also suggest that the reform did not significantly increase monthly earnings. However, it significantly reduced the weekly working hours of affected individuals which could be a channel of the observed improvements of self-rated health.

One limitation of our study is that we are only able to identify a short-term effect of the minimum wage reform as information on later years is not available in our dataset. Analyzing the long-run effects of the German minimum wage thus remains an interesting topic for future research.

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A Additional Tables

Table 7: Detailed Descriptive Statistics of Covariates of Treated and Control Group in the Year Prior to the Reform

	No. of obs.	Mean	Std. Dev.	Median	Min	Max
Treated Group						
Age	277	45.23	10.14	46	22	63
Female	277	0.65	0.48	1	0	1
Migrant	272	0.20	0.40	0	0	1
East	277	0.62	0.49	1	0	1
Years of Education	277	11.37	1.82	11.50	7	18
Household Income	276	1140.46	447.24	1038.50	92	4800
Socioeconomic Index	271	33.97	12.02	32	16	69
Married	276	0.47	0.50	0	0	1
Number of Children	277	0.90	1.07	1	0	6
Days in Employment	277	4831.75	2501.05	4642	255	12730
Days in Current Job	277	1540.56	1546.14	1010	5	7988
Days Social Benefits	277	2316.14	1751.43	1911	0	8491
Firm-size	276	161.25	495.67	37.50	1	6100
Part-Time	277	0.48	0.50	0	0	1
Control Group						
Age	911	44.51	10.04	46	20	63
Female	911	0.55	0.50	1	0	1
Migrant	900	0.21	0.41	0	0	1
East	911	0.31	0.46	0	0	1
Years of Education	911	12.37	2.44	11.50	7	21
Household Income	909	1650.63	804.65	1573	384	16667
Socioeconomic Index	907	40.81	12.96	38	16	88
Married	911	0.55	0.50	1	0	1
Number of Children	911	0.84	1	1	0	6
Days in Employment	911	5995.22	3078.18	5939	225	14310
Days in Current Job	911	2517.84	2519.36	1623	4	13962
Days Social Benefits	911	992.81	1283.31	496	0	10584
Firm-size	910	410.31	1104.73	84	1	11018
Part-Time	911	0.35	0.48	0	0	1

Notes: Data source: PASS-ADIAB.

Table 8: Detailed Descriptive Statistics of Working Hours and Wages of Treated and Control Group

	No. of obs.	Mean	Std. Dev.	Median	Min	Max
Treated Group						
Before the Reform (2014)						
Contractual Hourly Wage	277	7.96	1.69	7.96	1.62	13.14
Actual Hourly Wage	277	6.93	1.25	7.21	1.62	8.48
Contractual Weekly Working Hours	277	34.06	8.8	38	10	72
Actual Weekly Working Hours	277	39.26	11.47	40	12	80
Monthly Gross Wage	277	1166.39	373.76	1200	350	2200
After the Reform (2015)						
Contractual Hourly Wage	252	9.49	5.17	8.7	2.88	84.87
Actual Hourly Wage	252	8.78	5.21	8.31	2.88	84.87
Contractual Weekly Working Hours	252	33.92	8.64	35.5	12	72
Actual Weekly Working Hours	252	37.07	10.19	40	15	72
Monthly Gross Wage	252	1402.87	948.97	1400	250	14710
Control Group						
Before the Reform (2014)						
Contractual Hourly Wage	911	14.82	5.29	14.57	3.27	109.62
Actual Hourly Wage	911	13.43	3.14	13.26	8.5	20
Contractual Weekly Working Hours	911	34.47	8.02	38.5	8	60
Actual Weekly Working Hours	911	37.65	9.58	40	10	80
Monthly Gross Wage	911	2191.91	753.02	2200	420	5000
After the Reform (2015)						
Contractual Hourly Wage	889	15.44	4.32	15	4.66	43.08
Actual Hourly Wage	889	14.57	8.48	13.85	4.08	191.54
Contractual Weekly Working Hours	889	34.76	7.78	38.5	9	60
Actual Weekly Working Hours	889	37.73	9.25	40	4	70
Monthly Gross Wage	889	2322.55	823.44	2300	450	5400

Notes: Data source: PASS-ADIAB.

Table 9: Average treatment effect of the minimum wage reform on self-rated health

	(1)	(2)	(3)	(4)	(5)	(6)
Main Specifications						
Treatment effect	0.08**	0.08**	0.09**	0.09**	0.08**	0.09**
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Number of obs.	4752	4445	4628	4434	4628	4434
Cross-sectional OLS						
Treatment effect	0.08*	0.08**	0.09	0.10*	0.08	0.10*
	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Number of obs.	4752	4445	4628	4434	4628	4434
Cross-sectional Logit						
Est. Coef.	0.31*	0.35**	0.34	0.41*	0.34	0.40*
	(0.16)	(0.17)	(0.21)	(0.22)	(0.21)	(0.22)
Avg. marg. Eff.†	0.08*	0.08**	0.09	0.10*	0.08	0.10*
	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Number of obs.	4752	4445	4628	4434	4628	4434
Only individuals who are still employed in 2015						
Treatment effect	0.08**	0.07**	0.08**	0.08**	0.08*	0.08*
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Number of obs.	4532	4400	4419	4389	4419	4389
Control Variables		✓		✓		✓
PS-Matching with:						
control variables			✓	✓	✓	✓
past self-rated health					✓	✓

Notes: The first two columns contain the estimation results for the specifications without matching. Columns three and four contain the estimation results of the models with matching on the characteristics of the covariates from the year before the placebo-reform. Columns five and six report the estimation results with matching where in addition to the characteristics of the covariates from the year prior to the placebo-reform the values of self-rated health from years prior to the placebo-reform are included. The even columns contain the estimates controlling for the covariates, the odd columns contain the estimation results for the models without controlling for the covariates. Standard errors in parantheses; * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$; †Sample average of individual marginal effects of being treated on the probability of rating one's health 'good' or 'very good'. Data source: PASS-ADIAB.

B Additional Robustness Checks

B.1 Spillovers - Lower Hourly Wage Threshold

The introduction of minimum wages can also impact hourly wages of workers who are not directly targeted by a minimum wage reform.[12] Firm-wide adjustments of working hours (Neumark/Schweitzer/Wascher, 2004) or altered wage negotiations between employers and employees (Dittrich/Knabe/Leipold, 2014) are possible explanations for this impact. Such spillover effects can be a threat for the identification of the unbiased treatment effect. Similar to the issues of measurement error, the probability of spillovers should be higher, the closer the hourly wage is to the minimum wage threshold. In robustness checks both Bossler/Broszeit (2017) and Bonin et al. (2018) therefore restrict their control groups to individuals whose hourly wage is above 10 Euro.[13] In order to examine spillover effects

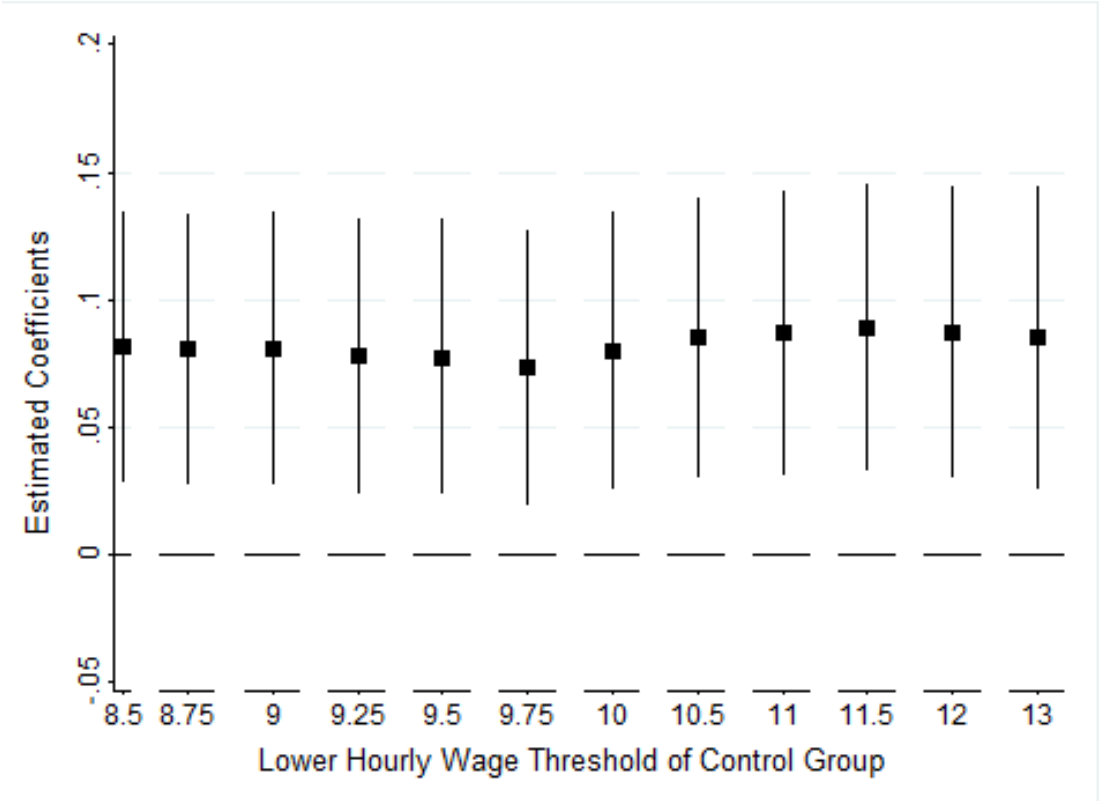
[12] See among others (Neumark/Schweitzer/Wascher, 2004) for an analysis in the US or (Aretz/Arntz/Gregory, 2013) for an analysis of sectoral minimum wages in Germany.

[13] While Bossler/Broszeit (2017) do not restrict the upper hourly wage threshold of the control group, Bonin et al. (2018) restrict it to 11.5 Euro.

we opt for a more granular approach with respect to the lower hourly wage threshold of the control group. Therefore, we run several regressions where we vary the lower hourly wage threshold of the control group while keeping all other factors in the main specification constant. Figure 2 displays the estimated coefficients and 90 percent confidence intervals of the obtained average treatment effects from the regression adjusted difference-in-difference models without matching.

Spillover effects do not seem to play a significant role, as the estimated coefficients across all lower hourly wage thresholds appear to stay on a constant level. The estimated treatment effects range from 0.07 to 0.09 and are all statistically significant on the 5 percent level.

Figure 2: Spillover Effects - Lower Hourly Wage Thresholds of Control Group



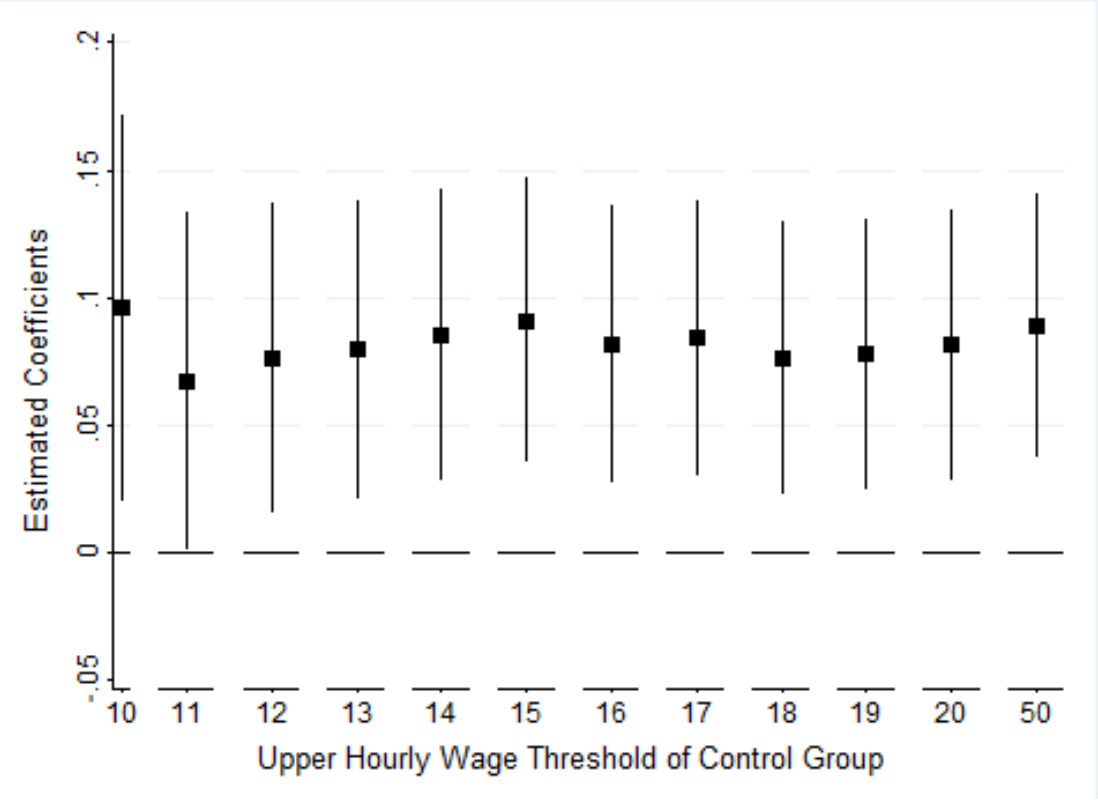
Notes: The Figure displays the estimated coefficients and 90 percent confidence intervals of the average treatment effect on the treated for various lower hourly wage thresholds of the control group, which are displayed on the x-axis. The bar with the lower hourly wage of 8.5 Euro represents the main specification. Data source: PASS-ADIAB.

B.2 Upper Hourly Wage Threshold

As explained earlier, the upper hourly wage threshold of the control group varies considerably in related studies that rely on a similar identification strategy. In order to test the robustness of the estimation results with respect to the choice of the upper hourly wage threshold, we conduct a series of regressions. Figure 3 displays the estimated treatment effects and 90 percent confidence intervals of each regression. The results indicate a rather robust treatment effect that ranges from 0.073 to 0.096. All but one specifications return coefficients that are significant at least at the 10 percent level. The one exception is the regression with the upper hourly wage threshold of 11 Euro. This might be driven by the small sample size,

as the number of observations in the control group decreases with lower upper thresholds.

Figure 3: Upper Hourly Wage Thresholds of the Control Group

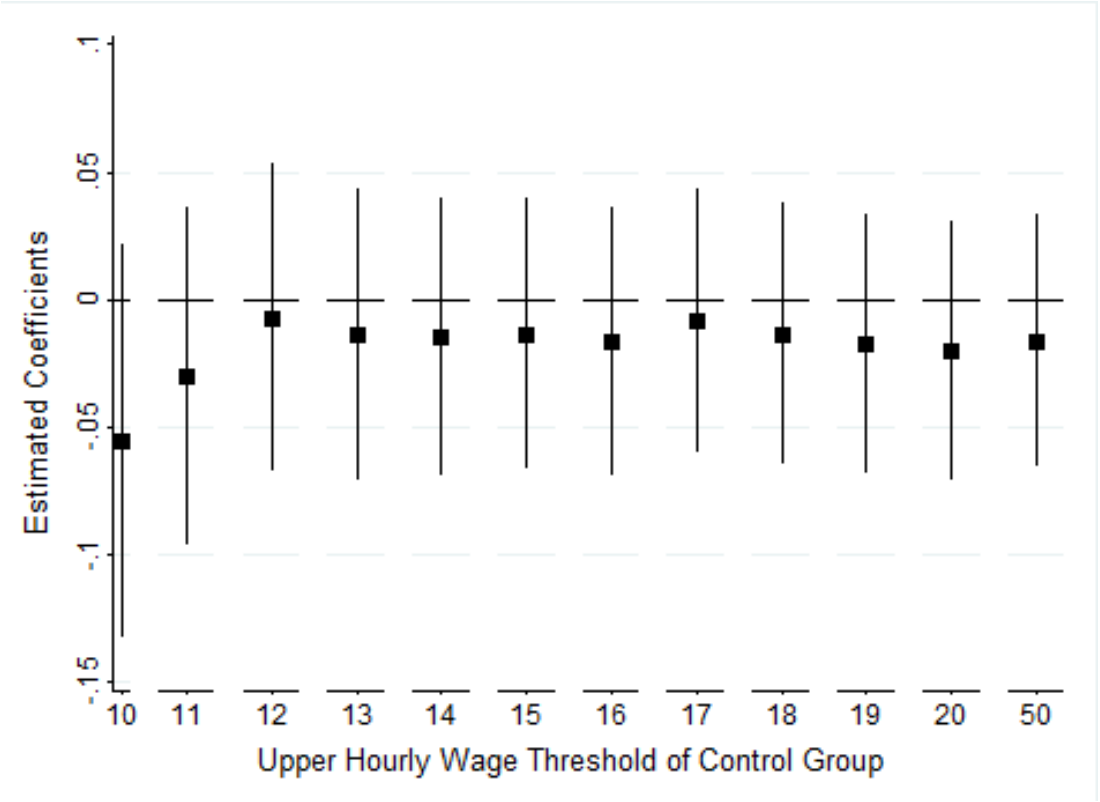


Notes: The Figure displays the estimated coefficients and 90 percent confidence intervals of the average treatment effect on the treated for various upper hourly wage thresholds of the control group, which are displayed on the x-axis. The bar with the upper hourly wage of 20 Euro represents the main specification. Data source: PASS-ADIAB.

B.3 Placebo Reform and Upper Hopurly Wage Threshold

Additionally applying the placebo reform presented above with varying upper hourly wage thresholds of the control group does not change this finding: Figure 4 displays the estimated coefficients and 90 percent confidence intervals with several upper hourly wage thresholds of the control group. None of the implemented specifications yield a significant treatment effect for the placebo reform.

Figure 4: Placebo Timing of the Reform



Notes: The Figure displays estimated coefficients and 90 percent confidence intervals of the average treatment effect on the treated for varying upper hourly wage thresholds of the control group, which are displayed on the x-Axis. Data source: PASS-ADIAB.

B.4 Placebo Groups

In another robustness check we change the composition of treated and control group to the extent that the reform should not affect either of them as hourly wages in both groups are considerably above the minimum wage threshold. In this specification the treated group is made up of individuals whose hourly wage is between 13 and 17 Euro, whereas the control group members earn between 17 and 50 per hour. This form of robustness check follows an approach of Lenhart (2017), who also implements specifications with placebo groups. The regression results for the placebo group specifications are displayed in table 10. Contrary to our main specification, none of the placebo group specifications yield a significant treatment effect. Obtained coefficients in the regressions without matching are very close to zero. In the variants with matching, the point estimates are slightly higher, however none is of statistical significance.

Table 10: Average Treatment Effect of the Minimum Wage Reform on Self-Rated Health for Placebo Groups

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effect	0.01	0.00	0.04	0.04	0.03	0.03
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Number of obs.	3104	2990	3016	2982	3016	2982
Control Variables		✓		✓		✓
PS-Matching with:						
control variables			✓	✓	✓	✓
past self-rated health					✓	✓

Notes: The first two columns contain the estimation results for the specifications without matching. Columns three and four contain the estimation results of the models with matching on the characteristics of the covariates from the year before the placebo-reform. Columns five and six report the estimation results with matching where in addition to the characteristics of the covariates from the year prior to the placebo-reform the values of self-rated health from years prior to the placebo-reform are included. The even columns contain the estimates controlling for the covariates, the odd columns contain the estimation results for the models without controlling for the covariates. Standard errors in parentheses; * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Data source: PASS-ADIAB.

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