Discussion Papers in Economics

No. 02/2019

Do Minimum Wages Improve Self-Rated Health?

Evidence from a Natural Experiment

Lucas Hafner
University of Erlangen-Nürnberg

ISSN 1867-6707

Friedrich-Alexander-Universität Erlangen-Nürnberg
Institute for Economics
https://www.iwf.rw.fau.de/discussion-papers/

Do Minimum Wages Improve Self-Rated Health?

Evidence from a Natural Experiment*

Lucas Hafner

Universität Erlangen-Nürnberg

March 25, 2019

Abstract

In this paper I evaluate a labor market reform in Germany. In particular, I analyze whether the introduction of the general minimum wage in 2015 had an effect on self-rated health and labor market outcomes of individuals who were likely affected by the reform. I exploit the plausibly exogenous variation in hourly wages induced by the natural policy experiment and apply difference-in-difference analysis combined with propensity score matching. I use survey-data combined with administrative records which enables me to control for a vast set of possibly confounding variables. I find on average significant improvements of self-rated health for individuals who are affected by the reform. My analysis indicates, that reduced stress, due to a significant reduction of weekly working hours potentially drives this result.

Keywords: *Minimum Wage, self-rated health, natural experiment.*

*I am grateful for helpful comments and suggestions by Harald Tauchmann. This paper is part of my dissertation "Topics in Health Economics" from 2018 at the FAU Erlangen-Nuremberg. Address for correspondence: Lucas Hafner, Professur für Gesundheitsökonomie, Findelgasse 7/9, 90402 Nürnberg, Germany. Email: lucas.hafner@fau.de.

1 Introduction

Studies that evaluate the effects of minimum wages focus typically on labor market outcomes such as the effects on employment or wage distributions.[1] However, additionally shedding light on non-labor market related outcomes is relevant in order to fully grasp the intended and potentially unintended effects of minimum wage reforms. Accordingly, a growing literature is recently analyzing non-labor market related outcomes such as subjective well-being (Kuroki, 2018) or self-rated health (Lenhart, 2017). Building on this literature, I exploit a natural policy experiment - based on the introduction of the general minimum wage of 8.5 € in Germany in 2015 - and analyze whether this reform had an effect on self-rated health of affected individuals.[2]

Several recent studies examine the effects of minimum wages on different health outcomes for the US and UK. 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 for the US and UK, respectively. Studies analyzing health related effects for the US mostly exploit variation of minimum wage regulations across and within states over time. 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.

A part of the US literature analyzes the relationship between minimum wages and risky health behaviors. Adams et al. (2012) find that higher minimum wages increase alcohol related traffic fatalities among teenagers. This finding is not confirmed by Sabia et al. (2014) who find little evidence for more alcohol consumption or driving under the influence due to increased minimum wages. Wehby et al. (2016) observe higher minimum wages to be associated with increased birth weight which can potentially be attributed to changes in health behaviors such as lower levels of smoking during pregnancy and increased prenatal care. 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.[3] Bullinger (2017) provide evidence that increasing the minimum wage leads to a reduction of teenage births. Results of Pohl et al. (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 and

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

[2] Studies evaluate labor-market related effects of the German minimum wage reform (e.g. employment effects and effects on working hours (Bossler and Gerner, 2016; Bonin et al., 2018). Only a few authors analyze non-market outcomes such as job- or life satisfaction (Bossler and Broszeit, 2017; Pusch and Rehm, 2017; Gülal and Ayaita, 2018).

[3] Evaluating changes in earned income tax credits in the US, Strully et al. (2010) control for state level minimum wage information in their regression analysis and also find a positive association between minimum wages and birth weight.

Ukert (2018) who find that minimum wage increases lower the consumption of fruits and vegetables and raise the probability of being obese.[4]

Furthermore, Andreyeva and 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 my study. Du and 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 et al. (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 et al. (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 my work analyze health effects of the 1999 national minimum wage introduction in the UK (Reeves et al., 2017; Kronenberg et al., 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 et al. (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.

I contribute 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. Comparable to the related literature, I apply regression adjusted difference-in-difference models and additionally use 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 before the reform is implemented, where individuals with hourly wages below 8.5 € are assigned to the treated group while individuals earning hourly wages of at least 8.5 € are assigned to the control group. I use survey data from the German Institute for Employment Research (IAB) combined with administrative records of the Federal Employment Agency ("PASS-ADIAB")[5]. My

[4] Two further studies with different identification strategies provide mixed evidence for the association between minimum wages and body mass index (BMI). Results of Cotti and Tefft (2013) do not provide evidence for an association between minimum wage increases and BMI. Meltzer and Chen (2011) however find a significant effect of rising BMI due to decreasing minimum wages.

[5] The data basis of this Chapter is the factually anonymous PASS survey data linked to administrative data of the IAB (version years 2012-2015). The data was accessed via a guest stay at the Research Data Centre (FDZ) of the Federal Employment Agency at the Institute for Employment Research and subsequently by means of controlled remote data processing at the FDZ. See Antoni et al. (2017) for more details on the data.

estimates indicate significant improvements in self-rated health which is potentially driven by a reduction of weekly working hours.

The remainder of this Chapter is organized as follows. In Section 2, I outline the estimation procedure. I then provide information about the data used, the estimation sample as well as the used covariates in Section 3. I will then present the estimation results in Section 4 and conclude in Section 5.

2 Estimation Procedure

A naive way to evaluate the effect of the minimum wage reform on self-rated health would be to look at differences in self-rated health between treated and control group after the implementation of the reform. This approach does however not consider potential differences between groups before the reform. This problem can be solved by matching treated and control group such "that the expectation of the respective potential outcome does not depend on the treatment status conditional on the covariates" (Lechner et al., 2011, p.189). In other words, if the matching procedure includes all relevant covariates, belonging to the treated or control group is irrelevant for the influence of the reform, i.e. hypothetically receiving the treatment has the same effect for either of both groups. This does however require the inclusion of all possibly confounding variables, which is a rather strong assumption.

In order to identify the causal effect of the German minimum wage reform on self-rated health, I 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 et al. (1997). The general idea of the difference-in-difference framework is to calculate the difference of the average pre- to post-reform changes in self-rated health of individuals who are affected by the reform and the pre- to post-reform changes in self-rated health of those who are not. In order to identify the unbiased average treatment effect on the treated, the so-called parallel trends assumption must hold unconditionally or conditionally on certain covariates (Lechner et al., 2011). This assumption states that - conditional on certain covariates and - in the absence of the minimum wage reform the average difference of self-rated health of the treated and control group would remain constant over time. The difference-in-difference procedure eliminates time-invariant unobserved characteristics. Hence, omitted variable bias is not problematic as long as no relevant time-varying variables are excluded from the model, which determine hourly wages and simulataneously affect the change in self-rated health. If these assumptions hold, any changes in the difference of the average self-rated health between the treated and control group can causally be attributed to the minimum wage reform.

The treated group contains individuals whose hourly wage is below 8.5 €, while the individuals of the control group earn an hourly wage of at least 8.5 €. 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 minimum wage evaluation literature (see for example Bossler and Broszeit (2017); Kronenberg et al. (2017); Arulampalam et al. (2004); Stewart (2004)). My 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, I do not restrict the analyzis to individuals from the treated and control group who actually receive or not receive the minimum wage. I thus identify the intention-to-treat effect, which may differ from the average treatment effect on the treated. Due to measurement error in the hourly wage variable 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. The intention-to-treat effect thus represents a conservative measure of the treatment effect.[6]

A consensus does not seem to exist with respect to the upper hourly wage threshold of the control group. Some authors, such as Stewart (2004) or Reeves et al. (2017) use a very low upper hourly wage threshold of 110% of the minimum wage. Others use higher thresholds: Kronenberg et al. (2017) use 140%, Pusch and Rehm (2017) and Gülal and Ayaita (2018) use around 150%, Lenhart (2017) use about 170%, whereas Bossler and 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. Anyhow, a 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 I opt for an upper hourly wage threshold of 20 € (235% of the minimum wage) and control for differences in covariates across groups via regression adjustment and matching.

In my main specifications I take into account the panel structure of the data and begin with a difference-in-difference estimation including individual and year fixed effects. The crucial common trends assumption must hold either unconditionally or conditionally on covariates. Combining regression adjusted difference-in-difference models with matching probably reduces the risk of violating this assumption. In the next model variant, I therefore augment the basic model by a variety of demographic, socioeconomic and labor market related pre-treatment covariates. Next, I use a matching procedure based on pre-treatment covariates and in some specifications additionally on pre-treatment self-rated health outcomes. By doing so, potential selection into the treated and control group due to different health statuses is accounted for.

Comparable to the procedure of Marcus (2014), I implement three steps to identify

[6] Alternatively, one could limit the treated and control group individuals to those who actually receive or not receive the treatment. I 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). Reeves et al. (2017) attach little worries to the problem that some individuals might be more likely to be exploitet by their employer, as the decision to pay minimum wages is not taken at the individual level but at the company level. Since individuals can select themselves in companies accordingly, the potential problem may arise anyhow.

the treatment effect in the regression models with matching.[7] First, I 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. I exclude 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). Then, similar to the implementation of Heckman et al. (1997) and Marcus (2014) I use kernel matching with a bandwidth of 0.06 to obtain the weights. The idea behind this form of matching – as in any other form of propensity score matching – is to match individuals who are similar with respect to observed covariates, i.e. whose estimated propensity score is close to one another. For this particular variant, all the control individuals, whose propensity score is within the specified bandwidth of a respective treated group member, are assigned a weight depending on their similarity. Controls whose estimated propensity score is closer to the estimated propensity score of the treated are assigned higher weights than controls whose estimated propensity score deviates more.[8] Subsequently, I run the regressions weighted by the obtained weights from matching.[9]

3 Data, Sample and Variables

Data source

I 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 and 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 established in order to study effects of the so called '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 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

[7] I used the user-written Stata command *psmatch2* (Leuven and Sianesi, 2018) to perform the matching.

[8] I also tested other matching algorithms, such as 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.

[9] Whether and how to deal with uncertainty in models with propensity score matching is a disputed debate in the literature (Stuart, 2010). I follow Ho et al. (2007) and do not take it into account for my variance estimations. Evidence suggests, that obtained standard errors are too large, and thus lead to more conservative inference (Stuart, 2010).

et al., 2013).[10] Since I 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 reform.

The survey started in 2006 with about 6000 households in each subsample. A refreshment sample of UBII recipients is added to the UBII sample each year. Additional observations were added to both subsamples in wave five of the survey. Individuals who are born into an existing PASS household are included in the sample and individuals who leave a PASS household are still interviewed after moving out. Every household member who is at least fifteen years old is interviewed via computer-assisted telephone interview or computer-assisted personal interviews (Trappmann et al., 2013). Detailed information about sociodemographic characteristics, economic and social situation, unemployment and benefit receipt, as well as attitudes and behaviors of individuals are included in the survey (Trappmann et al., 2013).

The PASS survey data are linked with individual administrative data[11] - 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 of spells in employment subject to social insurance are documented reliably, as they are relevant for the calculation of pension and unemployment entitlements (Jacobebbinghaus and 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).

My analysis is based on waves 6 to 9 (years 2012-2015) of the PASS. Administrative records linked to the PASS data are only available until the end of 2014 - which is why I can not use administrative data for the year of treatment. However, this is no problem, since the matching procedure, as well as the control variables used in the regressions rely on information from the years 2012 to 2014.

Sample

In the year 2014, 11590 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, the number of individuals reduces to 7567.[12] This excludes individuals who refused linkage or were not registered as unemployed or employed on the date of the interview. I then exclude individuals from the sample who should not

[10]The sample of UBII recipients is drawn directly from administrative registers of UBII recipients, while the general population sample is drawn from a commercial database of residential adresses (microm). For more information about the sampling design see Trappmann et al. (2009).

[11]Linkage of PASS survey data and individual administrative data requires consent of the survey participants. The average consent rate for the years 2006 to 2014 was around 81% (Antoni and Bethmann, 2018, p.5).

[12]I modify an approach introduced by Eberle et al. (2017) to construct cross-sections of administrative records on a day-to-day basis in order to precisely link the survey-data and administrative records.

be affected by the minimum wage reform - such as the self-employed, civil servants, judges, soldiers, individuals on maternity leave or aged 65 or older.

Generally, employers have to pay at least the hourly minimum wage from the first of January in 2015 onwards. However, certain groups were not affected by the minimum wage reform either temporarily or permanently and are therefore also excluded from the analysis. Individuals who are permanently excluded from the minimum wage arrangement are formerly long-term unemployed, apprentices, interns and individuals who are under 18 years old without completed vocational training.[13] Temporary exceptions are of additional relevance, as employees in branches with already existing industry-specific minimum wages or other special legal regulations where not affected by the general minimum wage in 2015. These include workers in the meat industry, hairdressers, workers in agriculture, forestry and horticulture, temporary workers, workers in the textile industry or laundry services, newspaper deliverers, and harvest helpers (Bundesregierung, 2014). The linkage with administrative establishment data offers the advantage of propely identifying and consequently excluding these groups from the sample. After the exclusion of these groups, 6110 individuals remain in the sample.

Next, I exclude individuals for whom there is no information on working hours or wages, despite these individuals being employed. Furthermore, I exclude individuals for who no consistent information about employment status is available in the survey and administrative data. This excludes individuals who claim to be registered as unemployed, although according to administrative records they are employed subject to social insurance. Another form of inconsistency excludes individuals who claim to be only marginally employed, although according to administrative data they are employed in a regular job subject to social insurance. After the exclusion of all inconsistencies and individuals with missing values in the variables, which are necessary to calculate hourly wages, the sample comprises 5255 individuals. After the assignment into treated and control group, which in the main specification restricts the sample to individuals who were in regular employment in the year 2014, the sample consists of 2247 individuals. However, only 1188 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 my main specifications therefore 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.[14] I do not impose restrictions on the employment status in the year of the reform, as I want to capture the total health-effect of the reform - this includes potential employment effects which

[13]see ”Gesetz zur Regelung eines allgemeinen Mindestlohns (Mindestlohngesetz - MiLoG (2014, August 11))”

[14]Due to differences across individuals in regular and marginal employment, I follow Bonin et al. (2018) and conduct separate analyses for individuals in regular and marginal employment. However, due to very low case numbers, the analysis for mainly marginally employed is not reliable. Results are available upon request.

could influence self-rated health.[15]

Variables Used in the Empirical Analysis

My 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*. Based on this I 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 me to consider the panel dimension of the data by estimating ordinary least squares fixed-effects models where unobserved time-invariant individual heterogeneity as well as time-trends are eliminated.[16]

As stated before, my definition of the treated and control group is based on hourly wages in an individual's main job. I obtain hourly wages by dividing the self-reported monthly gross wage by the self-reported average monthly working hours (including overtime).[17] I make use of the actual working hours (as for example Kronenberg et al. (2017); Reeves et al. (2017) or Pusch and Rehm (2017)) 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. In the year 2014, the survey does not contain information about how an employer deals with overtime. This causes one drawback in my approach as it is therefore possible that the calculated hourly wages are smaller than the 'real' hourly wages. This form of measurement error would impose a threat for my identification approach as some individuals who are not affected by the minimum wage reform are assigned to the treated group which possibly attenuates the obtained estimates of the treatment effect. I tackle this potential problem by conducting several robustness checks concerning measurement error.

The covariates in my analysis can be categorized into demographic, socioeconomic and labor market related characteristics of the individuals. Demographic covariates include age, gender, migration background, and region of residence (East or West Germany). Socioeconomic covariates contain years of education, monthly equivalised household income (modified Organisation for Economic Co-operation and Development

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

[16] 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 I also run cross-sectional ordinary least squares and logit regressions. The results are nearly identical to my main specification, however they yield slightly higher standard errors (see Appendix Table 9).

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

(OECD) scale[18] (Hagenaars et al., 1994)), a measure of socioeconomic status (international socio-economic index (ISEI) (Ganzeboom and Treiman, 1996)), marital status (married or not married) and the number of children in the 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.

I use the values of the covariates in the pre-treatment year for the calculation of the propensity scores. In other specifications, I additionally condition the propensity score on all pre-treatment self-rated health outcomes (2012-2014). Gender and migration background are only used in the calculation of the propensity scores, as the effects of time-invariant variables can not be estimated in fixed-effects models. All regression adjusted models include covariates of all pre-treatment years (2012-2014) as well as year-dummies to capture time trends.

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 (column one) as well as the control group before and after propensity score matching, respectively (columns two and three). One way to evaluate matching quality is to look at the mean differences between the treated and control group and applying two sample t-tests, which should be insignificant after matching (Rosenbaum and Rubin, 1985). The stars in Table 1 indicate whether covariates of the matched or unmatched control group differ significantly from the treated group. Another metric to assess the quality of matching is the so called standardized bias in percent (Rosenbaum and Rubin, 1985).[19] Although the metric does not deliver a clear categorization of good or bad matches per se, the empirical literature deems satisfactory matching quality for standardized differences below 3% or 5% (Caliendo and Kopeinig, 2008). 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.

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

[18] By using equivalised household income one takes into account the savings achievable in multi-person households in comparison to single-person households. In multi-person households the real household size is not used as a divisor when calculating per capita income, but a lower number calculated on the basis of the assumed requirements of the persons. The new OECD scale assumes a requirement weight of 1.0 only for the first person in the household (at least 15 years old). All other persons over 15 receive a requirement weight of 0.5; persons up to and including 14 are included in the needs-weighted household size with a weight of 0.3 (Hagenaars et al., 1994).

[19] 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 and Rubin, 1985).

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

	Treated	Controls		Standardized bias %	
		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
Number of observations	264	893	893		

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. * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$

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 East Germany is significantly higher in the treated group than in the control group.[20] 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% for all covariates, I 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% threshold. Furthermore, the t-tests yield no statistically significant mean differences of the covariates after matching. Nevertheless, I include the pre-treatment values of the covariates in some specifications of the regression analysis in order to control for covariate differences as the matching did not perfectly balance

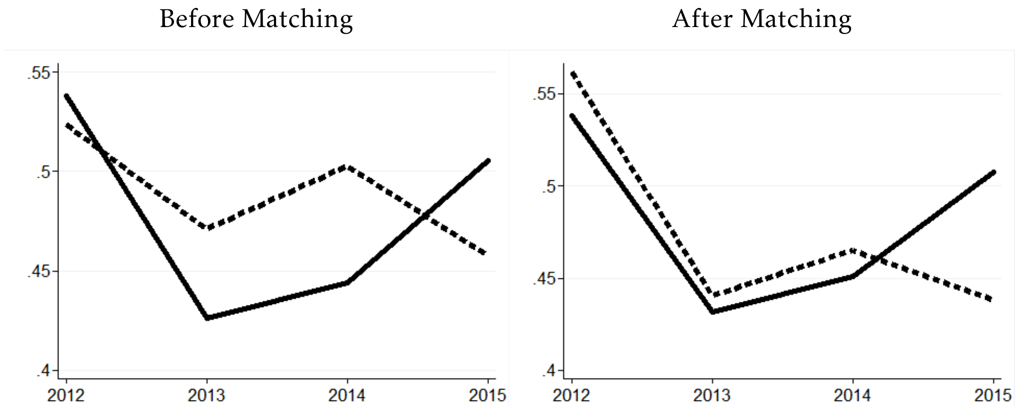
[20] This fact has been used as an identification strategy in German minimum wage evaluation studies before (e.g. (Bonin et al., 2018)).

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 in my 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 subfigure. In a descriptive sense the figures 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, I 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: Both Subfigures illustrate how the share of individuals who rate their health as good or very good developed over time. The left Subfigure illustrates this development for the sample before matching. The right Subfigure 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, respectively.

4 Estimation Results

In this section I will first present 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, I present results of the influence of the minimum wage reform on the working hours and gross wages of the affected individuals.

The structure of all the presented regression tables follows the same principle: 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.

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 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: Standard errors in parantheses; * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$

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 hourly wage an individual earns in reality. If that difference leads to systematic misclassification into either treated or control group, the underlying measurement error may pose a threat for 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 will 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 €. Therefore, I exclude individuals whose calculated hourly wage is near this threshold which is an approach that has been implemented by e.g. Bonin et al. (2018) and Pusch and Rehm (2017) before. In one specification I follow an implementation of

Bonin et al. (2018) and exclude all individuals whose calculated hourly wage is either 5% above or below the minimum wage threshold. In another specifiaction I follow Pusch and Rehm (2017) and exclude all individuals whose hourly wage is between 8.25 € and 8.75 €. If measurement error and thus systematic misclassification is an issue, one would expect an upward deviation of estimated coefficients in these specifications compared to the estimates without the exclusion of individuals close to the threshold.[21] However, a potential upward deviation of coefficients compared to the main specification could not be entirely attributed to the exclusion of 'misclassified individuals'. Another factor which can potentially increase the estimated coefficients is the altered composition of the treated group. Excluding individuals just below the minimum wage threshold will increase the average hourly wage gain in the treated group. This can also result in higher coefficient estimates, if one assumes a dose response relationship between hourly wage changes and self-rated health i.e. the higher the wage gain, the higher the potential impact on self-rated health. This issue will be taken into account in the following subsection, where I vary the lower hourly wage threshold of the control group to deal with potential spillover effects.

Table 3 contains 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 basically identical across my three specifications. Columns (3) to (6) reveal minor, statistically insignificant differences: In the specification where all individuals with hourly wages between 8.25 € and 8.75 € 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 € and 8.925 € 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, measurement error does not seem to bias the estimates of the treatment effect.

[21] Even if the misclassification is not systematic, i.e. individuals around the threshold are assigned to the wrong group randomly, the estimates should be attenuated towards zero, as neither falsely assigning individuals to the control group nor falsely assigning individuals to the treated group should result in higher effect estimates.

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 €						
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 €						
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: Standard errors in parentheses; * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$

Spillovers - Lower Hourly Wage Threshold

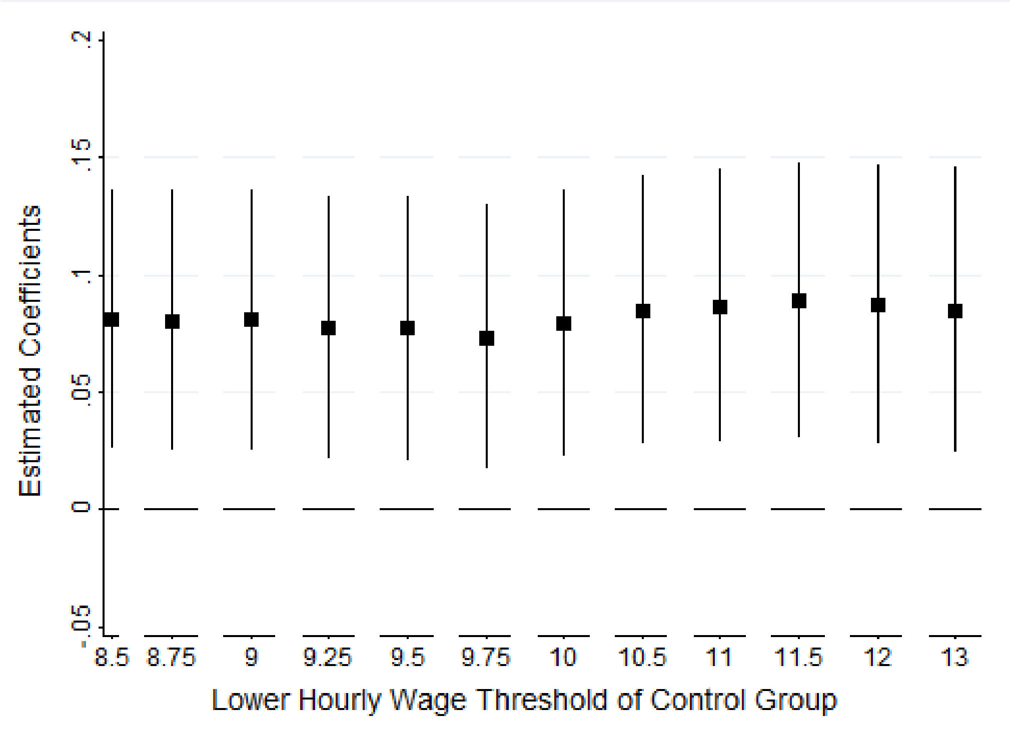
The introduction of minimum wages can also impact hourly wages of individuals who are not directly targeted by a minimum wage reform.[22] Firm wide adjustments of working hours (Neumark et al., 2004) or altered wage negotiations between employers and employees (Dittrich et al., 2014) are possible explanations for this impact. Such spillover effects can be a threat for the identification of the unbiased treatment effect. Similar to the issue concerning 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 and Broszeit (2017) and Bonin et al. (2018) therefore restrict their control groups to individuals whose hourly wage is above 10 €.[23] In order to examine spillover effects I opt for a more granular approach with respect to the lower hourly wage threshold of the control group. Therefore, I run several regressions where I vary the lower hourly wage threshold of the control group while keeping all other factors of my main specification constant. Figure 2 displays estimated coefficients and 90% confidence intervals of the obtained average treatment effects from 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% level.

[22] See for example (Neumark et al., 2004) for an analysis in the US or (Aretz et al., 2013) for an analysis of sectoral minimum wages in Germany.

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

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

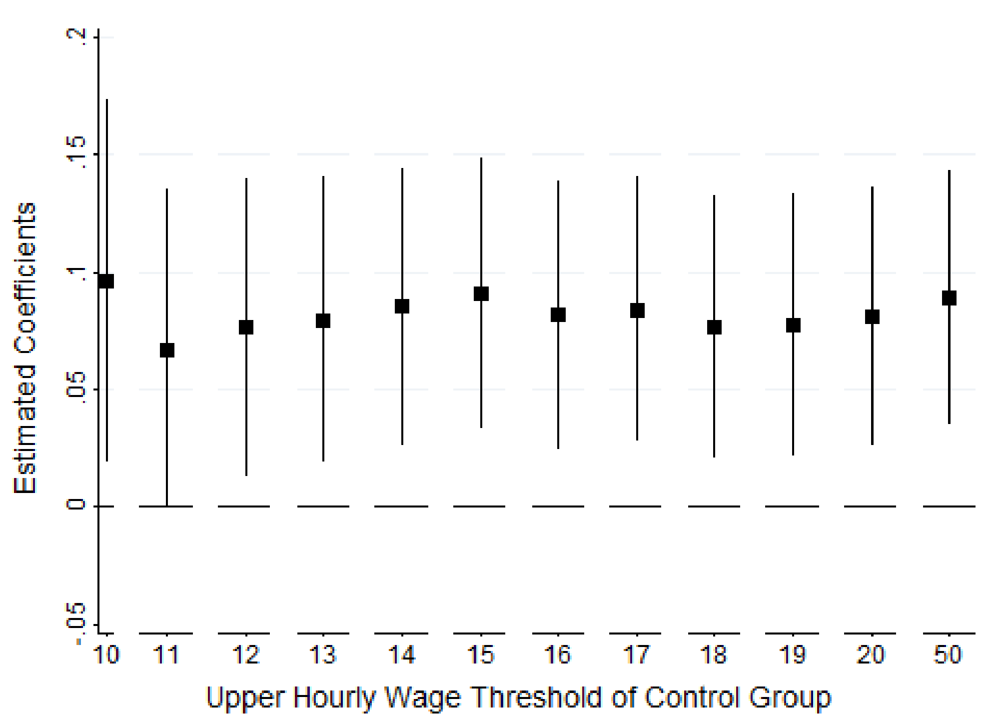


Notes: The Figure displays estimated coefficients and 90% confidence intervals of the average treatment effect on the treated for varying lower hourly wage thresholds of the control group, which are displayed on the x-Axis. The bar with the lower hourly wage of 8.5 € represents the main specification.

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, I conduct a series of regressions. Figure 3 summarizes the estimation results by displaying the estimated treatment effects and 90% confidence intervals of each regression. The results indicate a rather robust treatment effect that ranges from 0.073 to 0.096. One exception (upper hourly wage threshold of control group equals 11 €) yields an insignificant coefficient estimate of 0.063, which has to be taken with a grain of salt, as the number of observations is comparably low in the specifications with small upper hourly wage thresholds. All other specifications return coefficients that are significant at the 10% level.

Figure 3: Upper Hourly Wage Thresholds of the Control Group

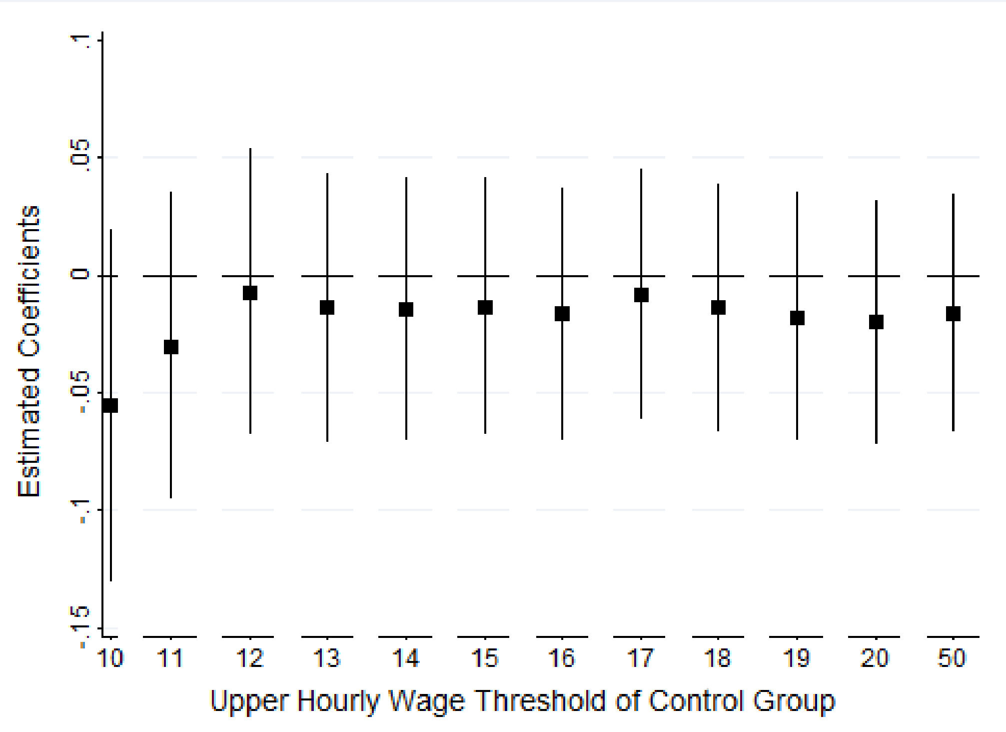


Notes: The Figure displays estimated coefficients and 90% confidence intervals of the average treatment effect on the treated for varying upper hourly wage thresholds of the control group, which are displayed on the x-Axis. The bar with the upper hourly wage of 20 € represents the main specification.

Placebo Reform

Following Lenhart (2017) and Gülal and Ayaita (2018) I also apply a placebo test in which I pretend that the reform took place one year prior to the actual implementation of the minimum wage reform. Years 2012-2014 are considered for this analysis. Keeping all other factors of my main specification constant, this does not yield a significant treatment effect. Additionally applying the previous robustness check with varying upper hourly wage thresholds of the control group does not change this finding: Figure 4 disyplays estimated coefficients and 90% confidence intervals with several upper hourly wage thresholds of the control group. Neither 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% 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.

Placebo Groups

In another robustness check I 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 €, 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 4. Contrary to my 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 4: 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: Standard errors in parentheses; * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$

4.2 Effect on Labor Market Outcomes

In this subsection I 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. I 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%/19%, while it only increased by 8%/4% in the control group. A similar, however slightly lower increase emerged for the gross monthly wage, which grew on average by 20% in the treated group while it increased on average by 6% 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% 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.

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

Regression results are displayed in Table 6. I do not find significant changes in the monthly gross wages and contractual working hours. These results indicate that the contractual hourly wages did on average 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 and 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: Standard errors in parentheses; * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$

5 Discussion and Conclusion

This is the first study to evaluate the effect of the German minimum wage reform on self-rated health. I add to a growing literature of international minimum wage evaluation studies, which analyze health effects in contrast to the majority of earlier studies which focussed primarily on labor market outcomes. This is of particular interest for economists and policy makers as labor market reforms can have consequences that go beyond labor market outcomes.

The applied 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. I 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 € with individuals who are likely not affected by the reform. I use survey-data combined with administrative records which enables me to control for a vast set of possibly confounding variables.

My 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 and Ukert (2018) and Du and 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. My 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.

Unfortunately, I was only able to identify a short-term effect of the minimum wage reform as information on later years is not yet available for the dataset at hand. Future research should analyze whether the German minimum wage introduction has long lasting effects on self-rated health as well as other health measures.

Appendix A1: 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: Standard errors in parentheses; * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$

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: Standard errors in parentheses; * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$

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: 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'.

References

Adams, S., Blackburn, M. L., and Cotti, C. D. (2012). Minimum Wages and Alcohol-Related Traffic Fatalities among Teens. *Review of Economics and Statistics*, 94(3):828–840.

Andreyeva, E. and Ukert, B. (2018). The Impact of the Minimum Wage on Health. *International Journal of Health Economics and Management*, DOI: ”https://doi.org/10.1007/s10754-018-9237-0”:1–39.

Antoni, M. and Bethmann, A. (2018). PASS-ADIAB–Linked Survey and Administrative Data for Research on Unemployment and Poverty. *Journal of Economics and Statistics*, DOI: ”https://doi.org/10.1515/jbnst-2018-0002”.

Antoni, M., Dummert, S., and Trenkle, S. (2017). PASS-Befragungsdaten verknüpft mit administrativen Daten des IAB (PASS-ADIAB) 1975–2015. *FDZ-Datenreport*, 6/2017, Nuremberg.

Antoni, M., Ganzer, A., vom Berge, P., et al. (2016). Sample of Integrated Labour Market Biographies (SIAB) 1975-2014. *FDZ-Datenreport*, 4/2016, Nuremberg.

Aretz, B., Arntz, M., and Gregory, T. (2013). The Minimum Wage Affects Them All: Evidence on Employment Spillovers in the Roofing Sector. *German Economic Review*, 14(3):282–315.

Arulampalam, W., Booth, A. L., and Bryan, M. L. (2004). Training and the New Minimum Wage. *The Economic Journal*, 114(494):C87–C94.

Averett, S. L., Smith, J. K., and Wang, Y. (2017). The Effects of Minimum Wages on the Health of Working Teenagers. *Applied Economics Letters*, 24(16):1127–1130.

Bonin, H., Isphording, I. E., Krause-Pilatus, A., Lichter, A., Pestel, N., Rinne, U., et al. (2018). Auswirkungen des gesetzlichen Mindestlohns auf Beschäftigung, Arbeitszeit und Arbeitslosigkeit. *IZA Research Report*, 83.

Bossler, M. and Broszeit, S. (2017). Do Minimum Wages Increase Job Satisfaction? Micro-Data Evidence from the new German Minimum Wage. *Labour*, 31(4):480–493.

Bossler, M. and Gerner, H.-D. (2016). Employment Effects of the new German Minimum Wage: Evidence from Establishment-Level Micro Data. *IAB-Discussion Paper*, 10.

Bullinger, L. R. (2017). The Effect of Minimum Wages on Adolescent Fertility: A Nationwide Analysis. *American Journal of Public Health*, 107(3):447–452.

Bundesregierung (2014). Gesetzliche Neuregelungen – Das ändert sich mit dem Jahreswechsel (2014, December 22). Retrieved 2018, September 7 from https://www.bundesregierung.de/Content/DE/Artikel/ArtikelNeuregelungen/2015/2014-12-22-neuregelungen-januar.html.

Caliendo, M. and Kopeinig, S. (2008). Some Practical Guidance for the Implementation of Propensity Score Matching. *Journal of Economic Surveys*, 22(1):31–72.

Cotti, C. and Tefft, N. (2013). Fast Food Prices, Obesity, and the Minimum Wage. *Economics & Human Biology*, 11(2):134–147.

Cygan-Rehm, K. and Wunder, C. (2018). Do Working Hours Affect Health? Evidence from Statutory Workweek Regulations in Germany. *Labour Economics*, 53:162 – 171.

Dittrich, M., Knabe, A., and Leipold, K. (2014). Spillover Effects of Minimum Wages in Experimental Wage Negotiations. *CESifo Economic Studies*, 60(4):780–804.

Dorner, M., Heining, J., Jacobebbinghaus, P., and Seth, S. (2010). The Sample of Integrated Labour Market Biographies. *Schmollers Jahrbuch*, 130(4):599–608.

Du, J. and Leigh, J. P. (2017). Effects of Minimum Wages on Absence from Work Due to Illness. *The BE Journal of Economic Analysis & Policy*, 18(1):1–23.

Eberle, J., Schmucker, A., et al. (2017). Creating Cross-Sectional Data and Biographical Variables with the Sample of Integrated Labour Market Biographies 1975-2014: Programming Examples for Stata. *FDZ-Methodenreport*, 06/2017, Nuremberg.

Ganzeboom, H. B. and Treiman, D. J. (1996). Internationally Comparable Measures of Occupational Status for the 1988 International Standard Classification of Occupations. *Social Science Research*, 25(3):201–239.

Gülal, F. and Ayaita, A. (2018). The Impact of Minimum Wages on Well-Being: Evidence from a Quasi-Experiment in Germany. *SOEPpapers on Multidisciplinary Panel Data Research*, 969.

Hagenaars, A., de Vos, K., Zaidi, M., and of the European Communities, S. O. (1994). *Poverty Statistics in the Late 1980s: Research Based on Micro-data*. Luxembourg: Office for Official Publications of the European Communities.

Heckman, J. J., Ichimura, H., and Todd, P. E. (1997). Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme. *The Review of Economic Studies*, 64(4):605–654.

Ho, D. E., Imai, K., King, G., and Stuart, E. A. (2007). Matching as Nonparametric Preprocessing for Reducing Model Dependence in Parametric Causal Inference. *Political Analysis*, 15(3):199–236.

Horn, B. P., Maclean, J. C., and Strain, M. R. (2017). Do Minimum Wage Increases Influence Worker Health? *Economic Inquiry*, 55(4):1986–2007.

Jacobebbinghaus, P. and Seth, S. (2007). The German Integrated Employment Biographies Sample IEBS. *Schmollers Jahrbuch*, 127(2):335–342.

Komro, K. A., Livingston, M. D., Markowitz, S., and Wagenaar, A. C. (2016). The Effect of an Increased Minimum Wage on Infant Mortality and Birth Weight. *American Journal of Public Health*, 106(8):1514–1516.

Kronenberg, C., Jacobs, R., and Zucchelli, E. (2017). The Impact of the UK National Minimum Wage on Mental Health. *SSM-Population Health*, 3:749–755.

Kuroki, M. (2018). Subjective Well-Being and Minimum Wages: Evidence from US States. *Health Economics*, 27(2):e171–e180.

Lechner, M. et al. (2011). The Estimation of Causal Effects by Difference-in-Difference Methods. *Foundations and Trends® in Econometrics*, 4(3):165–224.

Lenhart, O. (2017). Do Higher Minimum Wages Benefit Health? Evidence from the UK. *Journal of Policy Analysis and Management*, 36(4):828–852.

Leuven, E. and Sianesi, B. (2018). PSMATCH2: Stata Module to Perform Full Mahalanobis and Propensity Score Matching, Common Support Graphing, and Covariate Imbalance Testing. version 4.0.1; Available online at "http://ideas.repec.org/c/boc/bocode/s432001.html".

Marcus, J. (2014). Does Job Loss Make You Smoke and Gain Weight? *Economica*, 81(324):626–648.

Meltzer, D. O. and Chen, Z. (2011). The Impact of Minimum Wage Rates on Body Weight in the United States. In Grossmann, M. and Mocan, B. H., editors, *Economic Aspects of Obesity*, pages 17–34. Chicago: University of Chicago Press.

Neumark, D., Schweitzer, M., and Wascher, W. (2004). Minimum Wage Effects Throughout the Wage Distribution. *Journal of Human Resources*, 39(2):425–450.

Neumark, D., Wascher, W. L., et al. (2007). Minimum Wages and Employment. *Foundations and Trends® in Microeconomics*, 3(1–2):1–182.

Pohl, R. V., Clark, K., and Thomas, R. (2017). Minimum Wages and Healthy Diet. DOI: "http://dx.doi.org/10.2139/ssrn.2892894".

Pusch, T. and Rehm, M. (2017). Mindestlohn, Arbeitsqualität und Arbeitszufriedenheit. *WSI-Mitteilungen*, 70(7):491–498.

Reeves, A., McKee, M., Mackenbach, J., Whitehead, M., and Stuckler, D. (2017). Introduction of a National Minimum Wage Reduced Depressive Symptoms in Low-Wage Workers: A Quasi-Natural Experiment in the UK. *Health Economics*, 26(5):639–655.

Rosenbaum, P. R. and Rubin, D. B. (1985). Constructing a Control Group using Multivariate Matched Sampling Methods that Incorporate the Propensity Score. *The American Statistician*, 39(1):33–38.

Sabia, J., Pitts, M. M., and Argys, L. (2014). Do Minimum Wages Really Increase Youth Drinking and Drunk Driving? *FRB Atlanta Working Paper*, No. 2014-20, DOI: "http://dx.doi.org/10.2139/ssrn.2580472".

Schmucker, A., Seth, S., Ludsteck, J., Eberle, J., Ganzer, A., et al. (2016). Establishment History Panel 1975-2014. *FDZ-Datenreport*, 3/2016, Nuremberg.

Stewart, M. B. (2004). The Impact of the Introduction of the UK Minimum Wage on the Employment Probabilities of Low-Wage Workers. *Journal of the European Economic Association*, 2(1):67–97.

Strully, K. W., Rehkopf, D. H., and Xuan, Z. (2010). Effects of Prenatal Poverty on Infant Health: State Earned Income Tax Credits and Birth Weight. *American Sociological Review*, 75(4):534–562.

Stuart, E. A. (2010). Matching Methods for Causal Inference: A Review and a Look Forward. *Statistical Science*, 25:1–21.

Trappmann, M., Beste, J., Bethmann, A., and Müller, G. (2013). The PASS Panel Survey after Six Waves, Die PASS-Panelbefragung nach sechs Wellen. *Journal for Labour Market Research*, 46(4):275–281.

Trappmann, M., Christoph, B., Achatz, J., Wenzig, C., Müller, G., and Gebhardt, D. (2009). Design and Stratification of PASS: a New Panel Study for Research on Long Term Unemployment. *IAB-Discussion Paper*, 5.

Trappmann, M., Gundert, S., Wenzig, C., and Gebhardt, D. (2010). PASS–A Household Panel Survey for Research on Unemployment and Poverty. *Schmollers Jahrbuch*, 130(4):609–622.

Wehby, G., Dave, D., and Kaestner, R. (2016). Effects of the Minimum Wage on Infant Health. *NBER Working Paper*, 22373.