Do Higher Minimum Wages Benefit Health?

Evidence from the UK

Abstract:

This study examines the link between minimum wages and health outcomes by using the introduction of the National Minimum Wage (NMW) in the United Kingdom in 1999 as an exogenous variation of earned income. I test for health effects by using longitudinal data from the British Household Panel Survey for a period of ten years. I find that the NMW significantly improved several measures of health, including self-reported health status and the presence of health conditions. When examining potential mechanisms, I show that changes in health behaviors, leisure expenditures and financial stress can explain the observed improvements in health.

Keywords: Minimum Wage; Health; Mechanisms; United Kingdom.

INTRODUCTION

Previous work has established that low-income families suffer from worse health outcomes than wealthier ones (e.g. Case et al., 2002; Deaton, 2002). The World Health Organization states that "people further down the social ladder usually run at least twice the risk of serious illness and premature death as those near the top" (World Health Organization, 2003). Over the last few years, researchers have started examining the effects of governmental assistance programs on health outcomes (e.g. Hoynes et al., 2011 and 2015; Milligan and Stabile, 2011). A policy tool that is currently receiving much attention by policymakers is the minimum wage. Several developed countries (e.g. USA, Germany and the UK) have been discussing changes to minimum wage policies in recent years. Only a very small number of papers have so far tested for health-related effects of minimum wage policies (Adams et al., 2012; Averett et al., 2016; Horn et al., 2016; Lenhart, 2016; Wehby et al, 2016). By using the introduction of the National Minimum Wage (NMW) in the UK on April 1, 1999, this paper investigates whether this arguably exogenous increase in earned income affected health outcomes of low-wage workers.

By employing difference-in-difference (DD) models to analyze longitudinal data from the British Household Panel Survey (BHPS) for the years 1994-2003, this study examines the effects of the reform on a number of health measures. Specifically, I compare the effects on health status, health conditions and health care usage between workers whose wages most likely increase following the reform and those whose labor income should not be affected by the NMW. While the majority of previous work on minimum wages has looked at the effects on labor market outcomes, this study adds to the very small recent literature that examines the relationship between minimum wages and health-related outcomes. Besides testing for effects on a number of health outcomes, the later part of the analysis furthermore explores potential

mechanisms underlying the relationship between wages and health by examining the role of the reform on health behaviors, leisure activities and financial stress.

This study finds that the implementation of the NMW provided significant health benefits to low-earning individuals who experienced substantial wage increases immediately after the implementation. The findings are consistent across several health outcomes and model specifications and provide evidence for positive health effects of higher minimum wages. Furthermore, I show that the observed health improvements are not driven by changes in hours worked. When examining possible channels for the link between wages and health, I find that workers increase their spending on leisure activities, are less likely to smoke, more likely to be a member in a sports club and less stressed about their financial situations compared to before their wage increases. My results suggest that a combination of these factors can explain the observed health improvements following the NMW introduction.

PREVIOUS LITERATURE

Despite the fact that several studies have previously examined the impact of minimum wages on employment and monetary outcomes, there is still significant controversy regarding whether or not increasing the minimum wage results in positive economic outcomes.[1] While the majority of this controversy exists over findings for the US, previous work on the implementation of the NMW in the UK has shown no significant employment effects (Stewart, 2004; Dickens and Manning, 2004; Connolly and Gregory, 2003) and no effects on hours worked (Connolly and Gregory, 2003). Two studies provide evidence that the NMW substantially affected the overall wage distribution in the UK leading to a reduction in wage inequality, which was one of the

[1] Please see the summary of minimum wage-employment studies by Neumark and Wascher (2007).

proclaimed policy goals of the government (Dickens and Manning, 2004; Dolton et al., 2012). Butcher et al. (2012) and Arulampalam et al. (2004) furthermore show that the introduction of the NMW had spillover effects to workers who previously earned slightly above the new wage floor.

Research interest in examining the relationship between minimum wages and health-related outcomes has grown rapidly in recent years. Using the same data set and examining the same policy change as this study, two recent papers have examined the effects of the NMW on mental health outcomes. Kronenberg et al. (2015) find only small effects, whereas Reeves et al. (2017) show that the reform significantly reduced mental illness. Several recent studies have examined the association between minimum wage and health in the US. Wehby et al. (2016) find that higher minimum wages are associated with increases in birth weight, and note that changes in health behavior (prenatal care, smoking during pregnancy) could serve as mechanisms explaining the health improvements. Averett et al. (2016) and Horn et al. (2016) find only small effects of minimum wages on health, while showing that the effects might differ across population groups. Two previous studies provide mixed evidence when examining the association between minimum wages and Body Mass Index (Meltzer and Chen, 2011; Cotti and Tefft, 2013). Adams et al. (2012) indicate that increased minimum wages are associated with higher rates of fatal traffic accidents among drivers under the legal drinking age. Finally, Lenhart (2016) finds that within-country increases of minimum wages are associated with improved population health outcomes.

Following early work by Case et al. (2002) who find a highly significant positive association between family income and child health in the US, similar results have been found for Canada (Currie and Stabile, 2003), England (Currie et al., 2007; Propper et al., 2007, Adda et al., 2009),

Australia (Khanam et al., 2009) and Germany (Reinhold and Jürges, 2012). Following these finding, the existence of an income gradient in health has been established. More recently, researchers have focused on examining health effects of governmental assistance programs for lower-income families. Studies have shown that policies such as the Women, Infants and Children (WIC) program (Hoynes et al., 2011), the Earned Income Tax Credit (Hoynes et al., 2015; Evans and Garthwaite, 2014) as well as the Canada Child Tax Benefit (Milligan and Stabile, 2011) provide health benefits to vulnerable parts of the population. Fletcher and Wolfe (2014) suggest that further expansions in cash transfer programs can help reduce existing health inequalities. By examining potential health effects of minimum wages, this paper adds to this recently growing area of research.

BACKGROUND ON MINIMUM WAGE IN THE UK

In 1909, Winston Churchill, then President of the Board of Trade, established a Wages Council system in the UK with the goal of protecting the pay of workers in a number of different industries. Despite leading to statutory wage floors in many low-wage sectors of the economy, the system never implemented an economy-wide minimum wage. In 1993, John Major's government decided to abolish the Wage Councils, arguing that the system reduces employment by raising wages.[2] Following a period of six years during which no statutory wage floors existed in any sector of the economy besides agriculture, and soon after Tony Blair was elected as Prime Minister in May 1997, the Low Pay Commission (LPC) was established.[3] Based on the advice of the LPC, a first NMW was introduced on April 1st 1999 in the Minimum Wage Act (Low Pay Commission, 1998). The wage floor was set at £3.60 per hour for adults, £3.20 per hour for

[2] Dickens et al. (1999) provide evidence showing that wage councils had no negative impacts on employment.
[3] The Low Pay Commission is an assembly that consists of nine commissioners which was supposed to serve as an independent body that gives the UK government recommendations about a potential minimum wage.

adults in the first six months of a job with accredited training and £3.00 per hour for those aged 18-21.[4] Besides attempting to improve minimum standards in the workplace, another goal of the reform was to reverse the previous development toward a larger wage inequality in the UK.

Research has shown that the newly introduced NMW substantially impacted the British labor market. The pay of 1.2 million adult jobs increased immediately, which corresponds to 5.4 percent of workers in the UK (Metcalf, 2008). The average pay increase of affected workers has been shown to be between 10-15 percent (Metcalf, 2006). Despite early opposition by the Conservative Party based on the fear of increases in unemployment, the NMW has been widely perceived as extremely successful over the years. The success of this policy has led to the NMW being increased several times since 1999 (Manning, 2013)[5]. Michael Portillo, who was appointed as the new Conservative Leader in 2000, reversed the party's opposition to the NMW, stating that it should not create concerns since "at the modest level at which it has been set by the government… The minimum wage has caused less damage to employment than we feared." (Metcalf, 1999). A number of studies have confirmed that the NMW had no negative effects on labor market outcomes (Stewart, 2004; Metcalf, 2006; Bryan et al., 2013), while reducing lower tail wage inequality (Dolton et al., 2012).

Besides addressing the issue of wage inequality, a secondary goal of the newly elected government was to reduce health inequalities and improve overall population health. However, due to the fact that Blair's cabinet was committed to retain the outgoing Conservative government's expenditure plan, significant changes in healthcare were delayed until the second

[4] Hicks and Allen (1999) provide a better understanding of the value of the NMW by showing average prices of certain goods: a dozen of new laid eggs (£1.57); 16 ounce of beer in a public bar (£1.73); a gallon of petrol (£2.81).
[5] In a poll of political experts by the Institute of Government, the NMW was voted the most successful UK government policy of the past 30 years (Manning, 2013).

term of the Labour government, which is after the period of this study. These changes included increases in NHS inputs and outputs such as staffing services and healthcare activities. This suggests that any observed health improvements as a result of increases in wages should not be driven by changes in health services during the time of the study.

MINIMUM WAGE AND HEALTH

Minimum wages can affect health through several channels. Rather than being driven by one mechanism, it seems more likely that a combination of several factors influences the association between minimum wages and health outcomes. In this section, I discuss four potential pathways.

First, minimum wages can affect health outcomes through changes in health-related behavior. This is consistent with the Grossman model of the demand for health (1972), which states that individuals inherit an initial stock of health that depreciates over time but can be positively influenced through gross investments. These investments in health include factors such as lifestyle, exercise, diet and housing. Assuming that health is a normal good, workers will increase health inputs as a result of wage increases. Hoynes et al. (2015) point out that, despite the fact that the consumption of unhealthy behaviors such as drinking and smoking might increase if they are normal goods, unhealthy behaviors will still decrease if the income elasticity of health is large enough (Hoynes et al., 2015). Thus the effects of minimum wages on health behaviors remain an empirical question. In the later part of this study, I examine whether the implementation of the NMW is associated with changes in smoking, drinking, the likelihood of being a member of a sports club, leisure expenditures and family vacations. While changes in the first three outcomes could potentially directly impact health, changes in leisure expenditures and frequency of family vacation could indirectly affect health by influencing overall levels of utility.

Second, minimum wages could influence health by affecting financial stress and job-related stress as well as the income security of workers, as suggested by Leigh (2013). Early research in the medical literature documents the presence of physiological reactions to stress in the form of heart diseases and problems with the circulatory system (Sterling and Eyer, 1981; Henry, 1982). Reeves et al. (2017) and Horn et al. (2016) provide evidence linking higher minimum wages to improvements in mental health. This study examines the role of financial and job-related stress by estimating the effects of the NMW on five indicators, which equal to one if respondents report to: (1) be in a very difficult financial situation currently; (2) be in a better financial position than one year ago; (3) expect his or her financial situation to worsen over the next year; (4) be satisfied with his or her job; (5) be satisfied with the payment received at his or her job.

Third, given that the initial NMW was set at a relatively low level, observed changes in health could also be the result of other factors unrelated to the increased wages of affected workers. One example of this are increases in overall household income by other members of the household. In order to test for this potential channel, the study provides estimates both including and excluding a measure of household income that subtracts the labor income of the minimum wage worker. If both estimates are similar, this would suggest that the results are robust to possible changes in household income from other members of the household. Furthermore, the analysis includes controls for marital status, household size and the number of children living in the household to account for other potential changes in the worker's environment.

Fourth, higher minimum wages could influence worker's health through its effects on income inequality and relative income. Dickens and Manning (2004) and Dolton et al. (2012) provide evidence that the NMW was successful in reducing wage inequality in the UK, which was one of the main policy goals proclaimed by the government prior to its implementation. Furthermore,

previous work in the field of health economics has shown that income inequality can be linked to health and overall well-being (Wagstaff and van Doorslaer, 2000; Lynch et al., 2004; Macinko et al., 2003; Subramanian and Kawachi, 2004; Wilkinson and Pickett, 2006). The relative income hypothesis suggests that reported levels of well-being depends on how individuals compare their income level to others around them. In the framework of this study, relative income could affect the link between the NMW and health if workers who were earning slightly above the NMW before the policy change and subsequently did not receive any raises, report worse health outcomes due to the fact that other workers received a boost in earned income. While relative income is a potential mechanism underlying the link between minimum wages and health, this channel is not examined in this study.

DATA

This study uses data from waves four to thirteen (1994 to 2003) of the British Household Panel Survey (BHPS), a nationally representative panel survey of private households in Great Britain that started interviewing 10,300 individuals from 5,500 families in 1991.[6] For the waves used in this study, 95 percent of the interviews were conducted in the months September to November, while only 2.4 percent were held in the first three months of the year. Given that the NMW was implemented on April 1st 1999, this provides my analysis with five observations both before and after the policy change. The use of the BHPS provides several advantages for the purpose of this study. Due to it longitudinal nature, the dataset allows accounting for time-invariant unobserved heterogeneity and compositional selection. The potential for measurement error in the self-reported health measure is reduced since each individual's health is only

[6] Taylor (1998) provides a full description of the sampling strategy applied in the initial wave in order to design a nationally representative sample of the British population.

compared to their own prior assessment, while controlling for the fact that each respondent may have their own scales in ranking their health (reference bias). Furthermore, in comparison to the two other commonly used UK datasets with detailed information on earnings (Labor Force Survey and New Earnings Survey), the BHPS also provides information on several health outcomes. Finally, the BHPS gives a complete representation of incomes across the pay distribution since it questions all individuals above 15 years of age who live in the household at the time of the interview.

The main health outcome that this study analyzes is self-reported health status, but it also looks at additional health measures, which could potentially be viewed as more objective. Self-reported health status is categorized from one (=excellent) to five (=very poor) in the BHPS. It has been widely used in previous studies regarding the relationship between income and health (e.g. Case et al., 2002; Currie and Stabile, 2003; Adda et al., 2009). Furthermore, self-reported health has been shown to be a good predictor of other health outcomes, including mortality (Idler and Benyamini, 1997), future health care usage (van Doorslaer et al., 2000) and hospitalizations (Nielsen et al., 2016).

In order to remove concerns about reporting heterogeneity of health status, Johnson et al. (2009) suggests to additionally examine health outcomes which are viewed as more objective (Johnston et al., 2009). This study tests for the effects of the NMW on the presence of 13 types of health conditions, which are reported in the BHPS. In order to further examine the role of wage increases on health, I test for the effects of the NMW on three groups of health conditions: (1) any condition; (2) conditions that workers could treat themselves using their additional labor income to purchase over-the-counter medications; (3) long-term/chronic conditions that should not be affected by having more money in the short-run. Despite the fact that the NHS provides

universal health insurance coverage, issues like quality of care as well as long waiting times were prevalent at the time of the study (Vizard and Obolenskaya, 2013). In order to avoid long waiting times, individuals in the UK can purchase a relatively small number of medications, which are placed on the General Sales List, at pharmacies without any prescription.[7] Finding a decrease in the presence of the second group of conditions after the reform could thus provide additional evidence for effects on health, whereas examining short-run changes in the presence of long-term conditions serves as a falsification test.

Additionally, the study examines whether the policy impacted the frequency of doctor and overnight hospital stays as well as the use of other health services (e.g. physiotherapist, psychotherapist, health visitor at home) within the last 12 months. Observing decreases in these three measures of health care can provide further evidence for improvements in health since healthier people need to see the doctor less often.

Given that individuals in the UK become eligible to receive state pensions at the age of 65, the sample is restricted to workers below the age of 65. A disadvantage of the BHPS is the relatively small sample size. Nevertheless, since the BHPS allows following the same workers over time as well as testing for health effects on workers who are directly affected by the reform, its benefits outweigh the issue of a relatively small sample size.

ECONOMETRIC METHODS

Difference-in-Differences Models

[7] Examples of medications on the General Sales List are painkillers, skin creams, anti-allergy tablets, hearing aids, eye drops as well as non-prescription glasses. Thus, I group the following conditions as potentially treatable by additional income: body pain, skin condition/allergy as well as problems with either hearing or eye sight.

This study employs a difference-in-differences (DD) model to test for the average treatment effects of the reform on treated workers. The model follows the approach used by Stewart and Swaffield (2002), Aralampalam et al. (2004), Kronenberg et al. (2015), and Reeves et al. (2017) and constructs an hourly wage measure for the pre-treatment period by using reported monthly labor income and hours worked per week.[8] The sample is restricted to low-wage workers in this specification. The treatment group consists of hourly paid workers whose wages are below the NMW prior to the policy, whereas hourly paid workers earning between the NMW and £6.00 in the year before the policy change form the control group.[9] This selection into groups provides the analysis with 262 treated workers and 675 workers in the control group. Like the previous papers using this identification approach, this study is unable to use actual self-reported wage information to examine the effects of the policy change since this variable was only introduced to the BHPS after the reform in 1999.

The main DD equation estimated in this study is the following:

$$Y_{it} = \beta_0 + + \delta_{DD} Post_{it}*Treat_{it} + \beta_1 X_{it} + \lambda_1 Area_{it} + \lambda_2 Year_{it} + \lambda_3 Month_{it} + \alpha_i + \varepsilon_{it}, \quad (1)$$

where Y_{it} represents self-reported health status in the main specification; $Treat_{it}$ equals one if an individual belongs to the treatment group; and Post is an indicator for the post-treatment period (after April 1st, 1999)[10]. Since the dependent variable is categorized from 1 (=excellent) to 5 (=very poor), ordered logit estimation is conducted in order to observe impacts of the reform

[8] The BHPS only introduced actual hourly wage information to the survey in 1999. In an additional specification, I use actual hourly wage data to separate individuals into treatment and control group (see section 5.2).
[9] The study accounts for the lower NMW for workers between the ages of 18 and 21.
[10] Less than 3 percent of respondents were interviewed in the first three months of 1999. In the main model, these observations belong to the pre-treatment period. In an additional model, I find that the results remain unchanged when excluding these individuals to control for potential changes in responses due to anticipation of the policy change. These results are not shown, but are available upon request.

across the distribution of health status. For the other health-related outcomes, linear probability models are estimated to test for the effects of the policy on health condition, doctor visits, overnight hospital stays and the use of other health services. In order to examine the effects on labor market outcomes, I also re-estimate equation (1) with monthly personal labor income and hours worked per week as the dependent variables. $\delta_{DD,}$ which represents the effect of the policy change on health outcomes, is the main parameter of interest. X_{it} represents a set of time-varying individual and household characteristics that are controlled for in the analysis. These include marital status, household size, number of children in the household and, in additional specifications, the amount of income by other members of the household. Equation (1) also includes dummy variables for region, year and month of the interview. The inclusion of α_i captures unobserved individual heterogeneity and accounts for potential omitted variable bias.

Additional Models

The previously described main DD analysis uses calculated hourly wages to assign workers into treatment and control group. This selection process does not rule out that some workers in the treatment group are potentially not affected by the NMW implementation due to potential measurement errors in reported wages or due to poor enforcement of the new wage floor.[11] Thus, the main estimates provide intent-to-treat effects. Furthermore, the main DD setup might ignore that the policy change may have raised of some individuals in the control group who earned slightly above the NMW, as suggested by Butcher et al. (2012) and Arulampalam et al. (2004). The presence of these spillover effects would suggest that the main DD estimates are

[11] Metcalf (2006) provides evidence that compliance with the NMW was very good in the early years after its implementation until 2002.

under-estimated. This section introduces seven additional specifications that are estimated to further check for the robustness of the findings from the main model.

First, in order to take into account potential spillover effects, I redefine the treatment group as workers earning up to 1.1 times, 1.2 times and 1.3 times the NMW. Due to the increased sample size for the treatment group in these specifications, the control group includes all other workers who earn up to 200 percent of the NMW. Second, I conduct two types of placebo tests to further increase the credibility of the main results (Bertrand et al., 2004): a) a placebo treatment, which compare the effects of health outcomes between workers between two groups of workers whose wages should not have been affected by the NMW. The treatment group consists of hourly paid individuals earning between 200 to 300 percent of the NMW, while those making more than 300 percent of the NMW form the control group; b) a temporal placebo, which moves the implementation of the NWM one year ahead to April 1st, 1998. Finding no statistically significant results for these models would provide suggestive evidence that the parallel path assumption of the main DD analysis is satisfied for the year 1999.

Third, I test for the effects of wage increases on health when loosening the assumption of a linear relationship between income and health. Abadie (2005) introduced a semiparametric two-step method of capturing average treatment effects for the treated (ATT) for the case that differences in observed characteristics create non-parallel outcome dynamics between the two observed groups, which violates the main assumption of standard DD models. The ATT is given by the following equation:

$$E[Y^1(1) - Y^0(1) \,|D = 1] \;=\; E\left[\frac{Y(1) - Y(0)}{P\,(D = 1)} * \frac{D - P(D = 1\,|X)}{1 - P\,(D = 1\,|X)}\right] \quad , \qquad (2)$$

where Y(1) and Y(0) represent health outcomes before and after the treatment, D is an indicator for belonging to the treatment group, P(D=1) gives the probability of receiving treatment and P(D=1 | X) is the propensity score which equals the probability of treatment, conditional on observed covariates X. The semiparametric estimator is obtained through two steps: (1) estimation of the propensity score and computation of fitted values for the sample; (2) plugging in the fitted values into the sample analogue of equation (2). Abadie (2005) shows that weighted average differences in the outcome of interest can recover estimates for treatment effects on the treated, whereas the weights depend on the propensity score and the same distribution of covariates is imposed for both treatment and control group.

Fourth, I estimate a specification in which the treatment group is defined based on who is actually "treated" rather than on a potential treatment sample. The BHPS added the following question to the survey in 1999: "Has your pay or hourly rate in your current job been increased to bring you up to the National Minimum Wage or has it remained the same?" Individuals who respond with 'yes' are selected into the treatment group. In order to account for potential spillover effects, I furthermore include workers who earned slightly above the NWM before the reform who report having received a raise immediately after the reform in the treatment group. Specifically, those who received a raise while still earning less than £5.00 in 2000 are considered as treated.[12] The control group for this model is comprised of hourly paid workers who did not report experiencing an increase in wages immediately after the policy change. Since the estimates obtained from this specification are closer to the treatment-on-the-treated effects, we would the effects on health to be larger for this specification.

[12] I have repeated the analysis with different thresholds and the results remain unchanged. Due to a relatively large number of missing responses, only 38 percent of the initial treatment group reported receiving a raise after the NMW implementation. Only 22 percent of them mentioned that their raise was a direct result of the policy change.

Fifth, in order to account for potentially different trends between the two groups during period of interest, I re-estimate an alternative DD model based on Mora and Reggio (2015). In their paper, they introduce a DD estimator that identifies policy effects using a fully flexible dynamic specification as well as a number of "parallel growth" assumptions to test for the robustness baseline DD findings. Sixth, I use fixed salary workers who are financially unaffected by the NMW implementation as the control group and compare health outcomes between them and the initial treatment group of low-wage workers who received raises. This specification can provide additional evidence for whether the results of the main specification are robust to the choice of the control group.

Finally, while the NHS provides universal insurance coverage to all individuals in the UK, people additionally have the option to purchase supplemental private coverage. The main estimates showing health improvements from the NMW implementation could be biased if changes to this supplemental coverage occurred that differentially affected members of the treatment and the control group. I estimate two additional specifications to check for this potential concern: 1) I re-estimate equation (1) using an indicator whether respondents have supplemental private insurance as the outcome variable; (2) I re-estimate the main DD model for the effects of the NMW on health status including private coverage as a control variable. Despite private insurance potentially being an endogenous control, this specification can provide evidence whether the main results are driven by shifts in insurance coverage.

Descriptive Statistics

Basic descriptive statistics for the entire sample are given in Table 1. Individuals earn an average monthly labor income of £834.25 report an average health status of 2.12 on a scale from

one (excellent) to five (very poor). Table 2 presents separate summary statistics for the main treatment group of the sample as well as for the main control group of the analysis for the year prior to the NMW implementation (1998). Individuals from both groups are similar regarding most characteristics before the policy change. Given that the assignment into the groups is based on wages prior to the policy change, it is not surprising that average personal monthly labor income of treated people is lower than that of individuals belonging to the control group (£543.59 vs. £876.57). The statistics furthermore show that the share of women is larger in the treatment group, which is consistent with findings by Stewart and Swaffield (2002). 73.2 percent of treated workers and 78.5 percent of workers in the control group report being in excellent or very good health in 1998, respectively (statistically different at 10 percent level). Table 3 shows sample statistics for health conditions in the first year of the study. Panel A provides the share of individuals who report suffering from the 13 health conditions for each group. Treated individuals are 8.5 percentage points more likely to suffer from any health condition, whereas the shares for the two groups are relatively comparable across all conditions.

Figure 1 displays how real hourly wages of workers in the sample changed during the years of the study. Consistent with the selection of treatment and control group, it is observable that individuals in the control group earn higher wages than those in the treatment group. However, Figure 1 shows that the wage gap between the two groups narrowed immediately after the implementation of the NMW, providing suggestive evidence that the reform offers an arguably exogenous increase in wages that allows testing for potential effects on health outcomes. Figure 2 shows changes in the likelihood of respondents from both groups to report excellent health status. While treated workers appear to be substantially less likely to report this top category prior to 1999, the gap again narrows after the NMW introduction. In 2001,

respondent from both group are equally likely to report excellent health. The gap widens again in the last two years of the sample period, but remains smaller than in the pre-treatment period.

RESULTS

Effect of the Policy on Labor Market Outcomes

Before examining the effects of the minimum wage implementation on health outcomes, Table 4 provides evidence for the effects of the policy change on earned income and on hours worked. This can provide evidence for the magnitude of changes in wages experienced by treated workers and for whether workers and employers responded to the NMW implementation by changing the number of hours they work. Panel A shows descriptive statistics for monthly personal labor income earned and weekly hours worked for the year before and after the policy change. Panel B presents two DD estimates for each outcome that are obtained when excluding and including control variables into the model. The results from the model with controls suggest that monthly income of treated workers increased by £44 ($66 using the year 2000 conversion rate), which corresponds to annual pay raises of £528 ($792). The DD estimate for the effects of the NMW on hours worked that the policy change did not affect time spent at work. Both findings for earned income (Metcalf, 2006 and 2008; Butcher, 2005) and hours worked (Connolly and Gregory, 2003) are consistent with previous results in the literature.

Effects of the Policy on Health Status

Table 5 shows the ordered logit results for average treatment effects of the NMW introduction on the health status of affected workers. The estimates show that the NMW

significantly improves self-reported health status of workers.[13] Column (1) of Panel A shows that the policy change increased the likelihood of being in excellent health by 3.04 percentage points ($p<0.05$). Consistent with this, workers are significantly less likely to report being in fair, poor or very poor health after the reform ($p<0.05$). The observed impacts for the various categories of health status correspond to percentage changes of up to 18.7 percent from the pre-treatment period. The magnitude and statistical significance of these findings provide evidence for the presence of positive health effects as a result of an increase in minimum wages.[14] The estimates for other control variables included in the analysis show that they play a very little role in explaining changes in health following the policy change. One explanation for this could be that there are relatively small variation in the observable characteristics within individuals during the sample period. In Panel B, I add a control for income earned by other members of the household. While the estimate suggests that other income has a positive effect on self-reported health, including it in the model does not change the DD estimate for the effect of the NMW on health status.

Additional Health Outcomes

Next, I further investigate the effects of the reform on other health outcomes, which are potentially more objective.[15] Based on the classification of health conditions shown in Table 3, DD estimates for several categories are presented in Table 6. The first column indicates that receiving a raise through the NMW implementation reduces the likelihood of suffering from at least one of the 13 health conditions listed in the BHPS by 3.76 percentage points ($p<0.10$),

[13] Since the share of individuals reporting to be in very poor health is quite small (< 1 percent), the bottom two health responses (poor and very poor) are combined to one outcome leaving the analysis with four health categories.
[14] The results remain consistent when estimating linear models. These results are available upon request.
[15] I find that individuals who are in excellent/very good health are significantly less likely to both suffer from health conditions and to use health services frequently, which confirms that these measures are proxies of overall health.

which corresponds to a 7 percent change compared to the pre-reform period. The estimate for other household income in column (1) implies that higher income earned by other members of the household reduces the likelihood of reporting the presence of a health condition. Column (2) shows that this decline in health conditions is mainly driven by reductions in health conditions that could have potentially been taken care of by purchasing over-the-counter medications (body pain, skin problems/allergy, and hearing or sight issues). This finding suggests that medications for self-treatment are a normal good and changes in consumption can potentially explain health improvements when the budget constraint is relaxed. Column (3) shows that the NMW had no effect on the presence of long-term/chronic health conditions, such as asthma and epilepsy, which require more serious and long-term treatments. While the finding in Table 6 provide suggestive evidence supporting that the NMW implementation improved health, the results should be treated with caution due to the lack of precision in the estimates.

Table 7 presents the effects of the policy change on several measures of health care usage. The estimates in Panel A show that the NMW implementation is associated with a 4.15 percentage point increase in the likelihood of workers having no annual doctor visits ($p<0.05$), which corresponds to a change of 20.2 percent from the pre-treatment period. Treated workers are 2.02 and 2.51 percentage point less likely to see a doctor between three to five and more than five times per year, respectively (both $p<0.05$). Panel B of Table 7 furthermore shows that the policy change reduced the use of any health services by 5.42 percentage points ($p<0.05$). Given that the provision of health care in the UK is mainly financed by taxes with relative low copays for prescription medications, findings of less doctor visits and reduced use of other health services can be regarded as additional evidence for health improvements. Additionally, Panel B shows that the NMW implementation led to a 2.18 percentage point reduction in the likelihood

of staying in a hospital overnight. The lack of significance for this estimate could be due to a combination of the following factors: an inelastic demand for hospital stays, that NHS-provided insurance covers all people in emergencies regardless of income, and the small share of respondents who report to have stayed in a hospital overnight during the period of interest.

Heterogeneous Health Effects

Next, I examine whether the NMW implementation had heterogeneous treatment effects on health status across several subgroups of the sample. Specifically, I test for differences across gender, education level, marital status and age. Panel A of Table 8 shows that the previously observed health improvements of the NMW are almost entirely experienced by male workers who are likely to benefit from the reform ($p<0.01$). A possible explanation for these differences by gender are different income effects of the NMW for men and women. My analysis find that the increase in annual labor income of male workers as a result of the policy change is more than £500 larger than the increase experienced by female workers.

The estimates in Panel B suggest that health benefits of the NMW are larger for treated individuals with higher levels of education, who are 4.81 percentage points more likely to be in excellent health ($p<0.10$). This finding appears surprising since lower-educated individuals seem to be more likely to earn wages below the NMW and thus see their earned income increase following the policy change. However, this assumption does not seem to hold in my sample, where only relatively small differences in the share of workers with at most an O-level education are observable between treatment (61.0 percent) and control group (55.8 percent). While all individuals in the sample receive low wages around the NMW, differences in the types of jobs are observable between respondents with different levels of education. Compared to people with

at most an O-level education, individuals with at least an A-level education are more likely to be in managerial/technical occupations (32.42 vs. 21.91 percent), while less likely to work in partly skilled (24.84 vs. 17.29 percent) and unskilled occupations (3.64 vs. 7.95 percent). Additionally, the observed differences in health effects between the two groups can potentially be explained by differences in hours worked (higher educated individuals work 2.20 hours more per week) and gender (the share of females is 5.1 percent higher for the lower educated sample).

Furthermore, Panel C and D provide suggestive evidence that the effects of the NMW on health are larger for unmarried and younger workers (both $p<0.05$). Similar to the differences by gender, a likely explanation for the different effects by age is the fact that the policy led to slightly larger income increases for individuals below 40 years of age. The findings potentially suggest that changes in health behaviors such as smoking or exercising might provide larger health benefits to younger individuals. Overall, despite potential statistical power limitations in the analysis, the results in Table 8 provide evidence that the NMW had differential effects on health across the population.

MECHANISMS

Table 9 shows the results for the role of health behavior and leisure expenditures as potential channels underlying the link between higher wages and health outcomes. Column (1) shows that the implementation of the NMW reduced smoking by 2.51 percentage points ($p<0.05$), suggesting that smoking might be an inferior good for low-wage workers in the UK. This result is consistent with recent findings by Wehby et al. (2016). Columns (2) and (3) show that the NMW reduced the likelihood with which workers drink at least once per week and increased memberships in sport clubs ($p<0.10$). Both estimates provide additional evidence that changes in

health behaviors can explain the observed health improvements following the policy change to some extent. When examining changes in leisure expenditures, I find that households with treated workers are more likely to spend at least £80 per week on leisure ($p<0.05$) and to go on a family vacation of at least one week ($p<0.10$) after the NMW implementation. Following the assumption that these two activities increases satisfaction, these changes could be viewed as potential channels that impact health by affecting the worker's well-being.

Table 10 presents estimates for the effects of the NMW on financial and job-related stress. Column (1) shows that perceived financial well-being improved significantly for affected workers. Treated individuals are 4.16 percentage points less likely to view their current financial situation as very difficult after the reform ($p<0.05$). Despite being estimating imprecisely, the estimates in Columns (2) and (3) provide additional suggestive evidence for reductions in financial stress by showing that treated workers are more likely to feel that their financial situation has improved compared to the previous year and less likely to expect their financial situation to worsen in the upcoming year, respectively. The final two columns show that the NMW improved overall job satisfaction and satisfaction with the pay being received for workers in the treatment group (6.26 and 5.02 percentage points, respectively; both $p<0.05$).

The findings in Tables 9 and 10 suggest that there is not a single channel through which minimum wages influence health outcomes. It appears that a combination of factors such as health-related behavior, leisure expenditures financial and job-related stress, can be explain the positive link between higher minimum wages and health observed in this study.

ROBUSTNESS CHECKS

Table 11 provides several tests to the robustness of the previously presented main findings of the study. Panel A redefines the groups of the DD analysis in order to account for potential spillover effect of the NMW implementation. Using three different cutoffs for the treatment group (1.1, 1.2 and 1.3 times the NMW), I find that, although slightly smaller in magnitude, the estimates remain consistent to those shown in Table 5. This suggests that the main findings are robust towards potential spillover effects of the policy on workers who earned slightly above the NMW prior to the reform.

Next, I conduct two type of placebo tests to provide additional credibility to the findings of the paper. The first row in Panel B shows DD estimates using a temporal placebo treatment by moving the date of the policy change one year ahead to April 1, 1998. The results indicate slight improvements in health, however, the estimates are statistically insignificant.[16] In the second placebo test, I compare changes in health status between two groups of workers that should not have been affected by the NMW, those earning 200-300 percent of the NMW (treatment group) and those earning above 300 percent of the NMW (control group). Again, the estimates are imprecisely estimated, suggesting that there were no differences in health between the two groups. Both placebo tests provide further robustness to the main DD estimates of the study. Furthermore, the results from the temporal placebo treatment provide suggestive evidence that the parallel path assumption of the baseline DD specification is satisfied.[17]

[16] When using the placebo analyses to conduct the role of changes in health behaviors and leisure expenditures, I find that the estimates for all outcomes shown in Table 9 are smaller in magnitude and statistically insignificant. These results are not shown in the paper, but are available upon request.

[17] Placebo estimates for other health outcomes (health conditions and use of health services) and mechanisms (e.g. smoking and drinking) are also statistically insignificant. These results are not shown in the paper, but are available upon request.

Panel C presents estimates obtained from using selection into treatment and control group based on responses of whether workers experienced a wage increase immediately following the policy change. Compared to the main results of Table 5, these estimates are larger in magnitude, which is consistent with the fact that this specification is able to obtain results that are closer to the treatment-on-the-treated-effects, rather than the intent-to-treat effects. Treated individuals are 5.88 percentage points more likely to report excellent health ($p<0.01$) and 2.15 percentage points less likely to report poor or very poor health ($p<0.01$). In an additional check to the robustness of the group selection of the main analysis, I use financially unaffected salary workers as the control group. The results in Panel D are consistent with the previous findings showing that treated workers report higher levels of health status following their raises ($p<0.05$).

Panel E and F provide estimates for four outcomes using two alternative DD models. First, I test for the effects of the reform by estimating Abadie's (2005) semiparametric DD model, which loosens the assumption of a linear relationship between income and health (Panel E). The results for health status are consistent with the main DD estimates ($p<0.01$), providing suggestive evidence that the impact of the NMW on those treated is similar to the impact on those the policy was intended to impact. One potential explanation for the similarity of the effects on health between the parametric and semi-parametric model is that the control variables in the main model do not significantly affect health status and excluding them would not generate any major bias to the estimates. Thus, whether one controls for them in a parametric or in a semi-parametric way should not be relevant. As shown in Panel E, the semiparametric DD analysis also provides evidence that for reductions in financial stress following the policy change. The negative effect of the NMW on smoking, however, is smaller and imprecisely estimated compared to the main DD analysis.

The estimates obtained by using Mora and Reggio's (2015) alternative DD model that allows testing for the parallel paths assumption are presented in Panel F. The effects on the likelihood of being in excellent or very good health is larger in magnitude than the main DD model. Treated workers are 6.10 percentage points more likely to report the top two categories of health status ($p<0.01$). Furthermore, this alternative DD specification provides evidence for reductions in financial stress ($p<0.10$) and smoking. Despite only being slightly smaller than the main DD effect for smoking in Table 9, the alternative DD result is imprecisely estimated due to increased standard errors. This could suggest that the magnitude and level of significance of the main estimate for the effect of the NMW on smoking ($p<0.05$) could to some extent be explained by a lack of parallel paths for this outcome.

Finally, Table 12 shows estimates for the effects of the NMW on supplemental private insurance coverage and on health status when including a control for private insurance coverage. This analysis can provide evidence whether the main estimates of the study are potentially driven by differential changes in supplemental private insurance coverage between workers in the treatment and the control group. The estimates remove concerns that the main analysis might be biased due to changes in insurance coverage. Panel A shows that treated workers are less likely to have private coverage following the policy change, while the estimate is statistically significant. Furthermore, compared to the estimates in Table 5, the effects on health status are even larger in magnitude when controlling for private coverage ($p<0.01$). An explanation for why supplemental private insurance does not affect the findings of the study could the relatively high costs of the coverage since the sample consists of low-wage workers. Emmerson et al. (2001) show that, between 1995 and 1999, 41.2 percent of people in the richest 10 percent of the

population were privately insured in the UK, whereas only 3.7 percent of people in the bottom 40 percent of the income distribution had private coverage.

DISCUSSION AND CONCLUSION

Although the main goal of the NMW implementation of the NMW by the British Government was to protect the pay of workers and to counter previous trends towards larger income inequality, this paper points out that the reform also provided non-monetary benefits. A complete evaluation minimum wage policy changes should consider evaluating all potential outcomes. Due to the popularity of the NMW, the UK wage floor has been increased several times since its initial introduction in 1999. Most recently, First Secretary of State George Osborne announced the introduction of the National Living Wage (NLW), which would significantly increase wages of low-income workers in the UK (Watt and Stewart, 2015). According to Osborne, the governments' goal is to increase the NLW to 60 percent of median earnings by 2020, while decreasing working-age benefits. Given the findings of this study, potential health benefits of higher wage floors should be considered by policymakers.

Changes to both federal and state minimum wages have been debated intensely in recent decades in the US as well as in other developed and developing countries, which underscores the relevance on studying the effects of such law changes. Only recently, President Obama proposed an increase of the federal minimum wage from $7.25 to $10.10. After the last national elections in Germany in September 2013, the potential introduction of a federal wage floor was one of the first policies to be discussed and to be passed by the new administration. The findings of this study suggest that minimum wage laws are capable of reducing existing health inequalities in society, a result that is desirable by both individuals and governments. The results suggests that

higher minimum wages can impact health outcomes through a number of different channels. Future research should continue to examine the pathways through which minimum wages and other income support programs affect the well-being of low-income individuals.

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Table 1: Descriptive Statistics

Variable	Mean	Std. Dev	Min	Max	N
Age	38.56	12.24	18	64	9,299
Male (%)	0.359	0.480	0	1	9,299
Married (%)	0.586	0.492	0	1	9,299
# Children in HH	0.498	0.852	0	4	9,299
Household Size	3.054	1.168	1	7	9,299
A-Levels (%)	0.126	0.332	0	1	9,299
O-Levels (%)	0.240	0.427	0	1	9,299
Monthly Net Income	£834.25	£503.79	0	£7,256.83	9,299
Health Status (1=excellent, 5=very poor)	2.12	0.84	1	5	9,299
Any Health Condition (%)	0.536	0.500	0	1	4,430
Private Insurance (%)	0.110	0.313	0	1	7,280
Current Smoker (%)	0.313	0.464	0	1	9,299
Completely Satisfied with Job (%)	0.133	0.339	0	1	5,653

Table 2: Descriptive Statistics Pre-Treatment by Groups (1998):

Variables	Treatment Group	Control Group
Income		
Personal Income/Month	£543.59*** (216.69)	£876.57*** (293.38)
Household Income/Month	£2,181.34*** (1,128.30)	£2,414.31*** (1,179.80)
Health:		
% Excellent/Very Good	0.732* (0.444)	0.785* (0.411)
% Poor/Very Poor	0.073 (0.260)	0.048 (0.214)
% Health Condition	0.602** (0.491)	0.517** (0.500)
% Doctor > 5 times last year	0.134 (0.341)	0.127 (0.333)
% Hospital In-Patient last year	0.054 (0.226)	0.061 (0.240)
Education		
% A-Levels	0.146 (0.353)	0.136 (0.343)
% O-Levels	0.195* (0.419)	0.256* (0.437)
% Higher Education	0.255 (0.437)	0.294 (0.456)
Marital Status		
% Married	0.544 (0.499)	0.599 (0.490)
% Divorced	0.046 (0.210)	0.043 (0.204)
% Never Married	0.253 (0.435)	0.208 (0.406)
Age	37.82 (13.03)	37.90 (11.27)
% Male	0.268*** (0.444)	0.392*** (0.488)
# of Children in HH	0.272*** (0.644)	0.592*** (0.909)
Household Size	2.989 (1.132)	3.117 (1.207)
% Private Insurance	0.031*** (0.174)	0.093*** (0.290)
% Saving any	0.398*** (0.491)	0.498*** (0.500)
Observations:	262	675

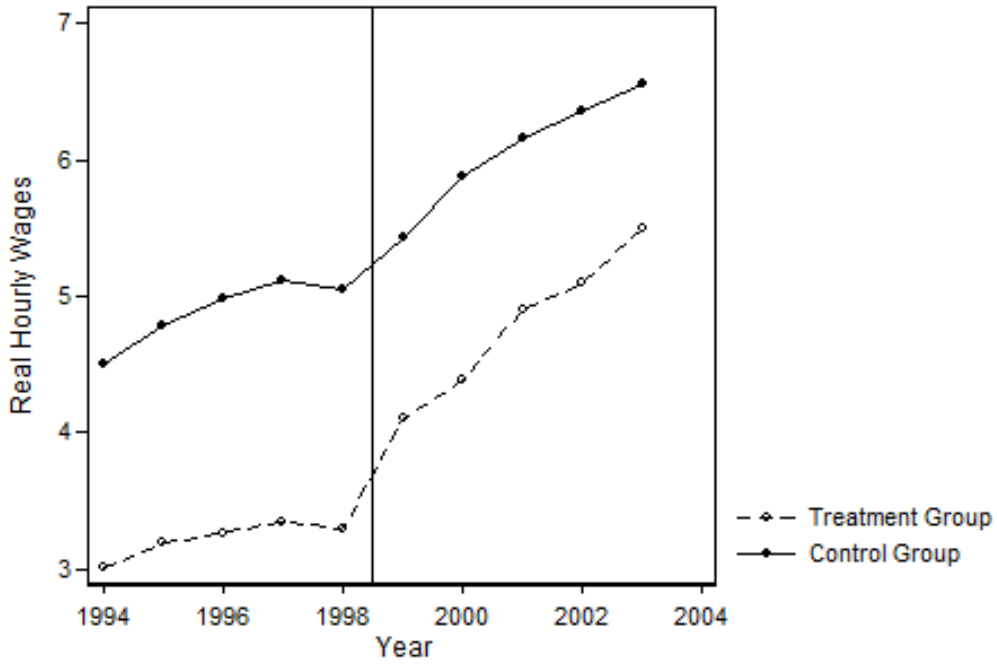
Notes: Standard deviations are shown in parentheses, whereas tests of the null hypothesis whether the statistics for the two groups are the same are indicated by stars. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Descriptive Statistics on Health Conditions Pre-Treatment (1998):

Conditions	Treatment Group	Control Group
Panel A: All Conditions		
Any	0.602** (0.491)	0.517** (0.500)
Body Pain / Problems	0.245 (0.431)	0.202 (0.402)
Migraine	0.130 (0.337)	0.102 (0.302)
Skin / Allergy	0.165 (0.372)	0.139 (0.346)
Asthma / Chest / Breathing	0.111 (0.315)	0.099 (0.298)
Anxiety / Depression	0.061 (0.240)	0.051 (0.220)
Heart / Blood Pressure	0.103 (0.305)	0.073 (0.261)
Hearing	0.038 (0.192)	0.037 (0.190)
Stomach / Liver / Kidney	0.069 (0.254)	0.045 (0.207)
Seeing	0.038*** (0.192)	0.012*** (0.109)
Epilepsy	0.008 (0.087)	0.004 (0.067)
Diabetes	0.015 (0.123)	0.010 (0.102)
Alcohol / Drugs	0.004 (0.062)	0.000 (0.000)
Other	0.046 (0.210)	0.037 (0.190)
Observations	262	675
Panel B: Groups of Conditions	*"Treatable" Conditions*	*Long-Term Conditions*
	Body Pain / Problems	Asthma / Chest / Breathing
	Skin / Allergy	Epilepsy
	Hearing	
	Seeing	
Treatment Group	0.398* (0.491)	0.184 (0.388)
Control Group	0.336 *(0.473)	0.142 (0.349)

Notes: Standard deviations are shown in parentheses, whereas tests of the null hypothesis whether the statistics for the two groups are the same are indicated by stars. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1: Changes in Real Hourly Wages



Notes: The wages shown are deflated to 2000 Pounds using the UK Average Earnings Index. Generated hourly wages are calculated based on the reported number of hours worked per week and the self-reported personal monthly income for the last payment period.

Figure 2: Changes in the Share of Individuals in Excellent Health

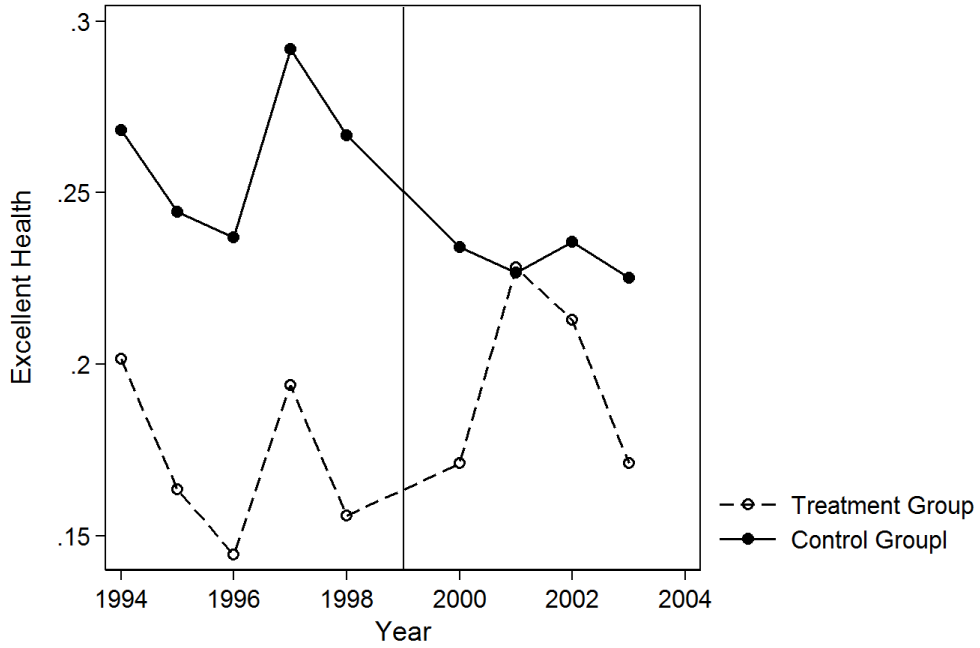


Table 4: The Effects of the Policy on Income and Hours Worked:

	Monthly Income		Hours Worked per Week	
Panel A: Descriptive Statistics	Treatment Group	Control Group	Treatment Group	Control Group
1998	605.68	1,030.86	26.63	32.21
	(338.13)	(590.22)	(12.48)	(11.47)
1999	681.57	1,055.48	26.02	31.97
	(334.06)	(573.68)	(12.30)	(11.78)
Panel B: DD Estimate	50.93***	44.00***	0.21	0.15
	(16.14)	(15.81)	(0.43)	(0.41)
Policy Effect	7.36%	6.35%	0.68%	0.49%
Control Variables	No	Yes	No	Yes
Observations	9,299	9,299	4,469	4,469

Notes: For the descriptive statistics in Panel A, standard deviation are shown in parentheses, while robust standard errors are reported in parentheses for the DD estimates in Panel B. The control variables include information on household size, the number of children and marital status. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: The Effects of the Policy on Health Status

	Marginal Effects			
	Excellent	Very Good	Fair	Poor/Very Poor
Panel A: Baseline				
Post*Treat	0.0304**	0.0049*	-0.0251**	-0.0104***
	(0.0150)	(0.0027)	(0.0124)	(0.0050)
Policy Effect	*17.67%*	*0.89%*	*11.53%*	*18.71%*
HH Size	-0.0027	-0.0004	0.0022	0.0009
	(0.0067)	(0.0010)	(0.0054)	(0.0022)
# of Kids	0.0150	0.0024	-0.0124	-0.0050
	(0.0104)	(0.0017)	(0.0086)	(0.0035)
Divorced	-0.0281	-0.0044	0.0231	0.0094
	(0.0412)	(0.0067)	(0.0340)	(0.0138)
Never Married	0.0191	0.0030	-0.0157	-0.0064
	(0.0219)	(0.0036)	(0.0181)	(0.0074)
Panel B: Additional Control				
Post*Treat	0.0307**	0.0049*	-0.0253**	-0.0103***
	(0.0151)	(0.0027)	(0.0124)	(0.0050)
Policy Effect	*17.85%*	*0.89%*	*11.62%*	*18.69%*
Other Income	0.0164***	0.0026**	-0.0135***	-0.0055***
	(0.0046)	(0.0011)	(0.0038)	(0.0016)
Observations	9,299	9,299	9,299	9,299

Notes: Robust standard errors, clustered by individuals, are shown in parentheses. The excluded category for marital status is married. The models in Panel B include all other control variables listed in Panel A. Other income is a measure for household income that is created by subtracting the respondents' income from the total income of the household. The estimate show the effect of an increase in other income by £1,000. Furthermore, region, year and month dummy variables are included in all models.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: The Effects of the Policy on Health Conditions:

	Types of Health Conditions		
	(1)	(2)	(3)
	Any	Treatable by over-the-counter medications	Long-Term / Chronic
Post*Treat	-0.0376*	-0.0353*	0.0012
	(0.0218)	(0.0206)	(0.0115)
Policy Effect	*7.04%*	*11.17%*	*0.01%*
HH Size	0.0184	0.0216	-0.0097*
	(0.0141)	(0.0137)	(0.0055)
# of Children	-0.0303	-0.0439**	-0.0040
	(0.0199)	(0.0187)	(0.0098)
Divorced	-0.0276	-0.1113**	0.0483
	(0.0733)	(0.0636)	(0.0405)
Never Married	-0.0082	-0.0033	-0.0397
	(0.0589)	(0.0587)	(0.0317)
Other Income	-0.0175**	-0.0086	0.0004
	(0.0078)	(0.0078)	(0.0031)
Observations	4,430	4,430	4,430

Notes: The division of health conditions is based on the categorization in Table 3 and is based on the author's opinion. Robust standard errors, clustered by individuals, are shown in parentheses. The excluded category for marital status is married. Other income is a measure for household income that is created by subtracting the respondents' income from the total income of the household. The estimate show the effect of an increase in other income by £1,000. Furthermore, region, year and month dummy variables are included in all models. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: The Effects of the Policy on Health Care Usage:

Panel A: Doctor Visits	Number of Doctor Visits Last Year			
	None	1-2	3-5	>5
Post*Treat	0.0415**	-0.0037	-0.0202**	-0.0251**
	(0.0206)	(0.0024)	(0.0100)	(0.0124)
Policy Effect	*20.18%*	*0.91%*	*10.95%*	*12.42%*
HH Size	0.0017	0.0002	-0.0008	-0.0010
	(0.0108)	(0.0010)	(0.0053)	(0.0065)
# of Children	0.0039	0.0004	-0.0019	-0.0024
	(0.0128)	(0.0012)	(0.0062)	(0.0077)
Divorced	-0.0930**	-0.0084	0.0452**	0.0562**
	(0.0466)	(0.0056)	(0.0227)	(0.0284)
Never Married	0.0859***	0.0077	-0.0418***	-0.0519***
	(0.0313)	(0.0043)	(0.0153)	(0.0191)
Other Income	-0.0082	-0.0007	0.0040	0.0050
	(0.0075	(0.0008)	(0.0037)	(0.0046)
Observations	4,430	4,430	4,430	4,430
Panel B: Other Health Usage				
	Used any Health Services last year		Hospital In-Patient last year	
Post*Treat	-0.0542**	-0.0536**	-0.0218	-0.0220
	(0.0257)	(0.0258)	(0.0155)	(0.0156)
Policy Effect	*13.53%*	*13.38%*	*38.45%*	*38.80%*
Other Income		-0.0096		0.0043
		(0.0063)		(0.0038)
Observations	4,430	4,430	4,430	4,430

Notes: Robust standard errors are shown in parentheses. The models in Panel B include all other control variables listed in Panel A. Other income is a measure for household income that is created by subtracting the respondents' income from the total income of the household. The estimate show the effect of an increase in other income by £1,000. Furthermore, region, year and month dummy variables are included in all models. Examples of health services asked for in the BHPS are usage of a physiotherapist, psychotherapist, health visitor at home and a hospital consultant. Pregnancies are excluded when examining changes in the likelihood of being a hospital in-patient. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 8: Heterogeneous Effects of the Policy

	Marginal Effects				
	Excellent	Very Good	Fair	Poor/Very Poor	N
Panel A: Gender					
Male	.0990***	-0.0027	-0.0715***	-0.0247***	3,310
	(0.0338)	(0.0069)	(0.0244)	(0.0082)	
Female	0.0051	0.0014	-0.0045	-0.0019	5,989
	(0.0167)	(0.0047)	(0.0149)	(0.0064)	
Panel B: Education					
A-levels or above	0.0481*	0.0074	-0.0392*	-0.0163*	3,841
	(0.0262)	(0.0054)	(0.0215)	(0.0090)	
O-levels or below	0.0117	0.0022	-0.0101	-0.0039	5,160
	(0.0204)	(0.0039)	(0.0175)	(0.0067)	
Panel C: Marital Status					
Married	0.0170	0.0033	-0.0145	-0.0059	5,420
	(0.0201)	(0.0040)	(0.0171)	(0.0069)	
Unmarried	0.0543**	0.0059	-0.0432**	-0.0169**	3,879
	(0.0248)	(0.0044)	(0.0197)	(0.0077)	
Panel D: Age					
Below 40	0.0525**	-0.0005	-0.0378**	-0.0142**	4,902
	(0.0241)	(0.0029)	(0.0173)	(0.0066)	
At least 40	0.0091	0.0035	-0.0088	-0.0038	4,467
	(0.0197)	(0.0075)	(0.0190)	(0.0082)	

Notes: Robust standard errors, clustered by individuals, are shown in parentheses. The models include all other control variables listed in Table 5, including a measure for income of other members of the household. Furthermore, region, year and month dummy variables are included in all models. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: The Effects of the Policy on Health Behavior and Leisure Expenditures:

	Current Smoker	Drink at least once per Week	Member of a Sports Club	Spend > £80 per Week on Leisure	Family Vacation of at least one Week per Year
	(1)	(2)	(3)	(4)	(5)
Post*Treat	-0.0251**	-0.0450	0.0392*	0.0338**	0.0349*
	(0.0118)	(0.0304)	(0.0230)	(0.0159)	(0.0211)
HH Size	0.0069	-0.0228	0.0057	0.0139	0.0054
	(0.0073)	(0.0339)	(0.0132)	(0.0105)	(0.0137)
# of Children	-0.0006	-0.0258	-0.0313	-0.0223**	-0.0042
	(0.0130)	(0.0342)	(0.0227)	(0.0130)	(0.0208)
Divorced	0.0620**	-0.0800	0.1145***	-0.0543	-0.0338
	(0.0218)	(0.1520)	(0.0366)	(0.0426	(0.0745)
Never Married	0.0088	0.1727	-0.0404	0.0408	-0.0575
	(0.0231)	(0.1535)	(0.0590)	(0.0452)	(0.0610)
Other Income	-0.0032	-0.0098	0.0039	0.0104	0.0311***
	(0.0036)	(0.0257)	(0.0061)	(0.0062)	(0.0078)
Observations	9,299	1,762	2,657	4,429	4,429

Notes: Robust standard errors are shown in parentheses. Other income is a measure for household income that is created by subtracting the respondents' income from the total income of the household. The estimate show the effect of an increase in other income by £1,000. Furthermore, region, year and month dummy variables are included in all models. Questions regarding participation in sport clubs are only available in the years 1997, 1999 and 2001, whereas questions about alcohol consumption are only asked in 1998 and 2000. * $p< 0.10$, ** $p< 0.05$, *** $p < 0.01$.

Table 10: The Effects of the Policy on Financial Stress:

	Current financial situation very difficult	Better financial position than 1 year ago	Expect financial situation to worsen next year	Satisfied with current job (overall)	Satisfied with current job (pay)
	(1)	(2)	(3)	(4)	(5)
Post*Treat	-0.0416**	0.0383	-0.0201	0.0626**	0.0502**
	(0.0198)	(0.0283)	(0.0176)	(0.0290)	(0.0246)
HH Size	0.0174	0.0154	0.0039	-0.0168	-0.0071
	(0.0174)	(0.0191)	(0.0101)	(0.0184)	(0.0168)
# of Children	0.0315	-0.0112	-0.0400*	-0.0114	0.0194
	(0.0317)	(0.0235)	(0.0198)	(0.0204)	(0.0211)
Divorced	0.2655***	-0.0588	-0.0135	-0.0112	0.0192
	(0.0865)	(0.1059)	(0.0330)	(0.0837)	(0.0863)
Never Married	0.0324	-0.0357	-0.0402	0.0531	0.0175
	(0.0705)	(0.0807)	(0.0262)	(0.0582)	(0.0598)
Other Income	-0.0259***	0.0378***	0.0020	0.0217	0.0154
	(0.0086)	(0.0096)	(0.0088)	(0.0147)	(0.0099)
Observations	5,643	5,653	5,653	5,653	5,653

Notes: Robust standard errors are shown in parentheses. Other income is a measure for household income that is created by subtracting the respondents' income from the total income of the household. The estimate show the effect of an increase in other income by £1,000. Furthermore, region, year and month dummy variables are included in all models. * $p < 0.10$, ** $p < 0.05$, $^{***}p<0.01$.

Table 11: Robustness Checks

	Marginal Effects				
	Excellent	Very Good	Fair	Poor/Very Poor	N
Panel A: Alternative Treatment Cutoffs		Treat = 1.10 x NMW			
Post*Treat	0.0293**	-0.0031*	-0.0234**	-0.0090**	15,336
	(0.0125)	(0.0016)	(0.0100)	(0.0038)	
		Treat = 1.20 x NMW			
Post*Treat	0.0207**	-0.0021	-0.0165*	-0.0063*	15,385
	(0.0115)	(0.0013)	(0.0091)	(0.0040)	
		Treat = 1.30 x NMW			
Post*Treat	0.0235**	-0.0024*	-0.0187**	-0.0072**	15,455
	(0.0108)	(0.0013)	(0.0086)	(0.0033)	
Panel B: Placebo Tests		Temporal Placebo: Treatment: 4/1/1998			
Post*Treat	0.0230	-0.0037	-0.0190	-0.0077	9.299
	(0.0149)	(0.0026)	(0.0123)	(0.0050)	
		Placebo Treatment: Treat: 2-3 x NMW; Control: >3 x NMW			
Post*Treat	-0.0201	-0.0039	0.0120	0.0041	13,552
	(0.0132)	(0.0027)	(0.0079)	(0.0027)	
Panel C: Alternative Model		Treatment Group: Report Wage Increase in 1999			
Post*Treat	0.0588***	0.0178***	-0.0551***	-0.0215***	3,486
	(0.0175)	(0.0069)	(0.0165)	(0.0067)	
Panel D: Salary Workers		Control Group: Financially Unaffected Salary Workers			
Post*Treat	0.0374**	-0.0020	-0.0270**	-0.0084**	8,925
	(0.0177)	(0.0016)	(0.0128)	(0.0040)	
Panel E: Semiparametric DD	Excellent/Very Good Health	Poor/Very Poor Health	Fin. Situation improved	Current Smoker	
DD Effect	0.0290***	0.0314***	0.0363**	-0.0064	9,299
	(0.0114)	(0.0081)	(0.0154)	(0.0068)	
Panel F: Parallel Trends Test					
DD Effect	0.0610***	-0.0144	0.0473*	-0.0212	9,299
	(0.0197)	(0.0123)	(0.0259)	(0.0171)	

Notes: Robust standard errors, clustered by individuals, are shown in parentheses. The standard errors for the semi-parametric DD estimates are obtained through bootstrapping using 99 replications All models include all the control variables listed in Tables 5, 6, and 7. Furthermore, region, year and month dummy variables are included in all models. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: The Role of Supplemental Private Insurance

	Panel A: Private Coverage	Panel B: Health Status			
		Excellent	*Very Good*	*Fair*	*Poor/Very Poor*
Post*Treat	-0.0305	0.0405***	0.0081**	-0.0338***	-0.0148***
	(0.0203)	(0.0155)	(0.0036)	(0.0129)	(0.0057)
Private Coverage		0.0096	0.0019	-0.008	-0.0035
		(0.0205)	(0.0041)	(0.0171)	(0.0075)
N	7,280	7,280	7,280	7,280	7,280

Notes: Robust standard errors, clustered by individuals, are shown in parentheses. All models include all the control variables listed in Tables 5, 6, and 7.The excluded category for private coverage estimate in Panel B is not having private coverage. Furthermore, region, year and month dummy variables are included in all models. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.