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Article

The impact of the UK National Minimum Wage on mental health

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ABSTRACT

Despite an emerging literature, there is still sparse and mixed evidence on the wider societal benefits of Minimum Wage policies, including their effects on mental health. Furthermore, causal evidence on the relationship between earnings and mental health is limited. We focus on low-wage earners, who are at higher risk of psychological distress, and exploit the quasi-experiment provided by the introduction of the UK National Minimum Wage (NMW) to identify the causal impact of wage increases on mental health. We employ difference-in-differences models and find that the introduction of the UK NMW had no effect on mental health. Our estimates do not appear to support earlier findings which indicate that minimum wages affect mental health of low-wage earners. A series of robustness checks accounting for measurement error, as well as treatment and control group composition, confirm our main results. Overall, our findings suggest that policies aimed at improving the mental health of low-wage earners should either consider the non-wage characteristics of employment or potentially larger wage increases.

1. Introduction

Mental health problems affect around 18% of the working-age population in England (van Stolk, Hofman, Hafner & Janta, 2014) and cost the UK economy around £105 billion every year, arising mainly from treatment costs, lost productivity and forgone income (Centre for Mental Health, 2010). Furthermore, treatment costs for mental illness alone represent around 13% of the overall NHS budget (Layard et al., 2012). Given the magnitude of the costs caused by mental illness, it is important to understand its determinants better, especially those relating to the labour market.

A recent OECD (2012) report argues that mental illness is a key issue for labour markets. The reason being that while physical health issues mainly affect the elderly, mental ill-health tends to be concentrated among people of working age. Hence, economic and policy considerations might substantially differ for people with mental versus physical illness (Layard, 2015).

In line with this, the relationship between employment and mental health has received growing attention in the literature (Baert, De Visschere, Schoors & Omey, 2014; Greve & Nielsen, 2013; Paul & Moser, 2009; Tefft, 2012). Previous studies indicate that they appear to influence each other. More specifically, people with low mental health are less likely to be in paid employment (Marwaha & Johnson, 2004; Rinaldi, Montibeller & Perkins, 2011) and conversely, individuals who have been unemployed are disproportionally affected by mental health problems (Diette, Goldsmith, Hamilton & Darity, 2012; Paul & Moser, 2009).

Evidence on the relationship between earnings and mental health is still sparse, especially among individuals at the bottom end of the income distribution. Within the economic literature, a number of studies have tried to disentangle the relationship between wealth and mental health (Apouey & Clark, 2015; Ásgeirsdóttir, Corman, Noonan, Ólafsdóttir & Reichman, 2014; Askitas & Zimmermann, 2011; Cesarini, Lindqvist, Östling & Wallace, 2016; Lindahl, 2005; McInerney, Mellor & Nicholas, 2013). These studies often employ either lottery winnings or the Great Recession of 2008 as exogenous shocks to an individual's wealth (e.g. savings or ownership of property, shares and life insurance). Overall, they find small but positive effects of increased wealth on mental health. While these studies focus on the effect of wealth on mental health, they appear to pay little attention to individuals at the bottom 20% of the income distribution who are twice as likely to experience mental illness compared to individuals with

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average incomes (Meltzer et al., 2002). Furthermore, although wages constitute the core element of income for low-earning individuals, there is limited evidence on the causal effect of wages on mental health.[1]

In this paper, we explore whether wage increases causally improve mental health among low-wage earners. This is important information for policy makers when considering changes to the minimum wage. We exploit the policy experiment provided by the introduction of the 1999 National Minimum Wage (NMW) and employ quasi-experimental methods on data from the British Household Panel Survey (BHPS) to identify the impact of wage increases on mental health.

The UK NMW was a significant policy change introduced in April 1999 aimed at raising the wages of around two million workers with estimated average wage increases of nearly 30% (Low Pay Commission, 1998). Prior to the NMW, the Trade Board Act of 1909 required wage councils to set minimum wages for different industries and these were in place until 1993 while between 1993-1999 there was no legal wage floor in the UK (Metcalf, 1999). Since access to mental health care is associated with income, even in countries with universal health care such as England (White, Gutacker, Jacobs & Mason, 2014), and the minimum wage needed to live a healthy life in the UK has been found to lie above the NMW (Morris, Donkin, Wonderling, Wilkinson & Dowler, 2000), the effect of the NMW on mental health should be non-negligible.

We estimate a series of difference-in-differences (DiD) models, including panel data fixed effects specifications and define mental health using the General Health Questionnaire (GHQ), a psychometrically validated tool. Our findings do not appear to show statistically significant causal effects of the NMW on mental health. Robustness checks, including alternative definitions of treatment and control groups based on the previous economic literature concerning the employment effects of the UK NMW, appear to confirm our main results. We discuss potential explanations for our findings.

The paper offers three main contributions to the literature. First, this paper explores the causal impact of wage increases on mental health, focusing specifically on low-wage earners. Secondly, we provide new evidence on the potential unintended consequences of an important policy (the NMW) on mental health, a particularly relevant outcome for the labour market. In doing so, we contribute directly to the emerging literature on the health effects of minimum wage policies. These studies are mainly US based and still present mixed results. In addition, by employing the UK NMW as a policy experiment, we revisit the only UK causal evidence currently available on the relationship between wages and mental health (Reeves, McKee, Mackenbach, Whitehead & Stuckler, 2016). Our study differs from Reeves et al. (2016) in that we employ alternative definitions of treatment and control groups. Finally, we contribute to the broader literature on the effects of socioeconomic status on mental health by focusing on changes in wages.

2. Data

The data is drawn from waves 7 to 9 (29th of August 1997 to 30th of April 2000) of the British Household Panel Survey (BHPS) with the NMW being introduced in April 1999, that is between waves 8 and 9. The BHPS is a longitudinal dataset, which is representative of the whole of Great Britain. The survey includes rich information on individual and household socioeconomic as well as health related characteristics such as physical and mental health, work, education, wages, income and wealth (Lynn, 2006).

Since our main objective is to estimate the impact of the introduction of the NMW on mental health, our sample does not include individuals who did not qualify for the NMW. This comprises individuals in specific occupations such as the armed forces, the self-employed, as well as retirees.[2] Moreover, since the minimum wage only applied to adults, we drop observations for individuals younger than 18 years old.

2.1. Mental health

We define mental health using the self-reported General Health Questionnaire (GHQ), a validated screening tool for psychiatric illness that is widely used in both mental health research and the economics literature (Apouey & Clark, 2015; Goldberg & Williams, 1988; Hauck & Rice, 2004). The BHPS includes the reduced 12-item version of the GHQ which is based on the following items: concentration; sleep loss due to worry; perception of role; capability in decision making; whether constantly under strain; perception of problems in overcoming difficulties; enjoyment of day-to-day activities; ability to face problems; loss of confidence; self-worth; general happiness; and whether suffering from depression or unhappiness. Respondents score each individual item from 0 to 3 with 0 being the best score. These 12 scores are then aggregated into a scale ranging from 0 to 36 which is *increasing in ill-health* i.e. lower scores correspond to lower mental health (Goldberg & Williams, 1988). Goldberg et al. (1997) show that the 12-item version of the GHQ has a sensitivity (correctly identifying individuals with mental health) of 83.7% and a specificity (suitably identifying the proportion of individuals with mental health) of 79.0%. Furthermore, gender, age and education do not appear to affect the validity of the GHQ (Goldberg et al., 1997). The GHQ also appears to be robust to re-test effects[3] (Pevalin, 2000).

2.2. Covariates

A number of variables were added to our models to control for further observable differences between individuals in treatment and control groups. These variables were chosen for their relevance to mental health, based on previous literature. These include: age; age-squared; gender (Madden, 2010); whether the individual works part-time; region (via the Nomenclature of Units for Territorial Statistics, NUTS); [4] occupation defined as primary, secondary and tertiary sector according to the International Standard Classification of Occupations (ISCO 88);[5] whether the individual works in a small (1-49), medium (50-499) or large (> 500) firm; the season of the year (Tefft, 2012); and whether the individual has a permanent contract (Carrieri, Novi, Jacobs & Robone, 2014) as well as length of employment spells (i.e. the number of days in current employment) (Paul & Moser, 2009). Appendix A provides detailed definitions of each variable (Table A1).

3. Estimation strategy

Our empirical strategy exploits the policy experiment provided by

[1] There is a large literature on the determinants of happiness, which often employs the General Health Questionnaire (GHQ) – as used in this paper – as the main outcome measure, see for example the work by Blanchflower and Oswald (2008); Gardner and Oswald (2007); Oswald and Powdthavee (2008) as well as Dolan, Peasgood, and White (2008).

[2] It might be important to note that the self-employed might present a different relationship between wages and mental health compared to wage workers. We refer to Rietveld, van Kippersluis, and Thurik (2014) for a comprehensive study on self-employment and mental health. We also acknowledge that retirees and individuals in the armed forces might have systematically different levels of mental health compared to the regular workforce. This might be due to several factors including age (retired) and work-environment (armed forces).

[3] Re-tests effects refer to the concept of repeatability that using the same measure (GHQ) on the same person, under the same conditions within a short-time frame multiple times should produce only small variation.

[4] NUTS is a hierarchical system used to identify subdivisions of EU member countries.

[5] ISCO groups all occupations into a hierarchical system which has 10 groups at its highest level (Legislators, senior officials and managers, Professionals, Technicians and associate professionals, Clerks, Service workers and shop and market sales workers, Craft and related trades workers, Plant and machine operators and assemblers, Elementary occupations and the armed forces).

the introduction of the NMW and involves the estimation of a series of difference-in-differences (DiD) models. DiD is an established econometric method of ex-post policy evaluation (Angrist & Pischke, 2008). In this case, DiD focuses on the difference in outcomes (GHQ) in pre- and post-treatment periods between treated and control groups. Our estimation strategy, including the definitions of control and treated groups, follows well-established papers on the effects of the NMW on employment outcomes (Arulampalam, Booth & Bryan, 2004; Stewart, 2004b). The aim of this estimation strategy is to identify the average treatment effect on the treated (ATT), in this case the change in GHQ scores driven by the wage variation due to the NMW. This approach assumes that changes in mental health between treated and control groups would have developed in a similar way had the NMW not been introduced (i.e. the standard common trend assumption). Our basic DiD model is:

$$Y_{it} = \beta_0 + \beta_1 T_i + \beta_2 A_t + \beta_3 T_i A_t + \beta'_4 X_{it} + \gamma_i + \mu_{it} \quad (1)$$

Our main outcome of interest, $Y_{it,}$ is the GHQ Likert scale score of individual i at time t. T_{it} is a binary indicator that identifies individuals in the treatment group (i.e. individuals affected by the NMW) and A_t is an indicator for the post-treatment period. β_3 (the interaction between treatment group indicator and post-treatment period) is the parameter of interest as it provides an estimate of the treatment effect. X_{it} represents a vector of individual and job characteristics known to affect mental health e.g. age, gender, region of residence, workplace size, industry sector etc., γ_i is an unobserved time-invariant individual-specific effect and μ_{it} the idiosyncratic error term clustered at the individual level (Bertrand, Duflo & Mullainathan, 2004).

While we account for a number of observable factors, there might still be other important unobservable individual characteristics such as genetic predisposition to mental illness. If present and not accounted for, these unobservable factors could potentially bias our estimates. For this reason and to further explore the role of time-invariant unobserved heterogeneity, we present estimates from both pooled OLS models and fixed effects specifications. Importantly, since a potential effect of the minimum wage on mental health would be a "second-order" (indirect) effect, we also explore "first-order" (direct) effects on wages and employment. This is to ensure that the minimum wage had the expected impacts on labour outcomes.

3.1. Identification of treatment and control groups

We employ two alternative definitions of treatment and control groups. The first set of treatment and control groups is based on information on an individual's hourly wage. We follow Arulampalam et al. (2004) and derive the individual's hourly wage as:

$$\frac{\frac{12}{52} usual\ gross\ pay\ per\ month}{usual\ standard\ weekly\ hours + 1.5 \times (usual\ paid\ over\ time\ weekly\ hours)} \quad (2)$$

The numerator presents the usual gross pay per month transformed into weekly pay. The denominator is the sum of the usual standard weekly hours and paid overtime increased by 50%. Given that both numerator and denominator are measured on a weekly basis, this ratio produces individual hourly pay rates. The derived wages were deflated to 1998 values using Office for National Statistics Consumer Price Indices[6] and variations to the calculation of hourly wages were tested. An individual is included in this first *treatment group* if his or her wage in wave 8 of the BHPS (before the introduction of the NMW) was below the minimum wage and therefore would have needed to increase to minimum wage levels between waves 8 and 9. The amount of the NMW depends on an individual's age: the adult rate was £3.60/hour; while the 18-20 year old rate was £3.00/h; and a "development rate" for workers older than 21 participating in approved training programs was fixed as £3.20/h.[7]

The *control group* was defined using individuals whose wages fell between the NMW and 140% of the NMW before the introduction of the NMW. The threshold of 140% (which is tested in our sensitivity analyses) was used to build a group of individuals whose wages were just above the minimum wage but were not affected by the NMW.[8] It is important to note that using derived wage data to identify treatment and control groups assumes both no measurement errors and that the self-reported information closely reflects the actual per hour rates.[9]

The second set of treatment and control groups is based on a special NMW question added to wave 9 of the BHPS (Stewart & Swaffield, 2002). According to this definition, the *treatment group* includes individuals who replied "yes" to the question: "*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?*". Conversely, the *control group* comprises individuals replying "no" to this question.

It should be noted that the use of this question might present some limitations. First, the question is asked of everyone who met the eligibility criteria for the NMW. Therefore, some individuals in the control group (not eligible for the NMW) may have large hourly wages. In order to ensure that the control group was as similar as possible to the treatment group, we restrict the highest wage of the control group to £7.20 per hour (or twice the NMW rate). Secondly, the question was only asked of individuals who did not change jobs between 1st September 1999 and 30th April 2000. This implies that the question is asked post-treatment (after the introduction of the NMW). Hence, we cannot exclude the possibility that replies to this question might be affected by changes in mental health that occurred during the course of the introduction of the NMW. An additional consideration is that this question was only asked in wave 9, therefore to be included in our analysis, individuals needed to be present throughout waves 7 to 9.

Table 1 reports the questions used to build treatment and control groups while Table 2 shows the number of observations per treatment and control group.

Appendix A provides descriptive statistics showing statistically significant differences between mean values of variables in treated and control groups before the introduction of the NMW (Table A2). Table A3 provides a comparison of the treatment/control group definitions used in this paper.

3.2. Robustness checks

In order to further check the validity of our main results, we provide a wide range of robustness checks. Given that females usually report worse mental health (Madden, 2010), we present separate analyses by gender. In addition, we provide separate estimates for part-time workers as these disproportionally represent individuals receiving the NMW.[10] Further checks relate to measurement error (limiting the sample to those who can produce a payslip for the interviewer); testing variations in control groups, including merging the two definitions of treated and control groups (self-reported and wage-based); estimating our models on a balanced sample; including only individuals who experience no job change; single person

[6] Available at http://www.ons.gov.uk/timeseriestool.

[7] Within the BHPS we could only identify individuals affected by the first two rates. However, previous research suggests that nobody in the BHPS might have been actually affected by the development rate (Arulampalam et al., 2004).

[8] We choose a 140% threshold in order to increase the sample size. This implies hourly rates between £3.60-£5.04 and £3.00-£4.20 for control and treated groups, respectively. We have also estimated our DiD models with different thresholds (e.g. 110 and, 120%). Results are available upon request and 100%-130% as well as 120%-140% are reported in Table 5.

[9] Stewart and Swaffield (2002) discuss reasons for potential measurement error in reported wages and hours, including overtime pay and presenting of payslips to the interviewer. In any case, only systematic measurement error would affect our estimation strategy. We further discuss and account for misreporting in our sensitivity checks.

[10] On the other hand, it could be argued that those working part-time often do so, because their partner earns more. This is tested by focusing on one-person households.

Table 1
Questions used to define treatment and control groups.

Wage-based identifier	
Usual gross pay per month	Usual monthly wage or salary payment before tax and other deductions in current main job for employees.
Total weekly hours	"Thinking about your (main) job, how many hours, excluding overtime and meal breaks, are you expected to work in a normal week?"
Usual paid overtime weekly hours	Number of hours overtime the individual works in a usual week
Self-reported identifier	
"Has your pay or hourly rate in your current job been increased to bring you up to the National Minimum Wage introduced in April 1999?"	Potential answers: Yes, No, Don't know, Refused

Notes: questions presented under the label wage-based identifier refer to those used to build the wage-based definition of treatment and control groups. The question reported under the label self-reported identifier was employed to build an alternative self-reported definition of treatment and control groups.

Table 2
Summary of treatment and control group definitions.

Definition	Selection variable	Treatment Group	N	Control Group	N
Wage-based group	Derived wage in wave 8 (September 1998- March 1999)	Wage < NMW	515	NMW < Wage < 1.4 NMW	981
Self-identified group	Answer to: "Has your pay or hourly rate in your current job been increased to bring you up to the National Minimum Wage introduced in April 1999?" Potential answers: Yes, No, Don't know, Refused	Yes	683	No	2,950

households; excluding any overtime adjustment; adding time trends; reducing the sample to those with low baseline mental health; and separately analysing each GHQ-item.

As part of verifying our results we attempt to replicate the results of Reeves et al. (2016) using their treatment and control groups. Appendix D also presents an alternative way of identifying individuals in the treated group. This is obtained by applying a treatment intensity estimator based on the computation of a wage gap between the actual wage and the minimum wage applicable.

4. Results

Fig. 1 and Fig. 2 show trends of our measure of mental health (GHQ) over time, separately by treated and control groups for both wage-based and self-reported definitions, respectively. For the wage-based measure (Fig. 1), the GHQ increases slightly from 1997-98 (wave 7) to 1998-99 (wave 8) in both treatment and control groups. The mean GHQ score then declines from 1998-99 to 1999-00 (wave 9). Fig. 2 presents the same graph for self-reported treatment and control groups. According to this figure, both treatment and control groups appear to have increased their mean GHQ scores from 1997-98 to 1998-99 and present a slight decrease

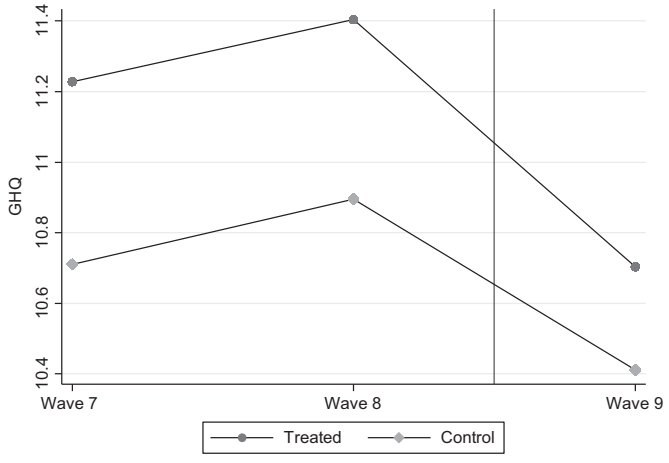

Fig. 1. GHQ over time split by treated and control (Wage based measure). Note: The NMW was introduced between waves 8 and 9. The dots show the mean GHQ for waves 7-9 separately for individuals in the treatment and the control group.

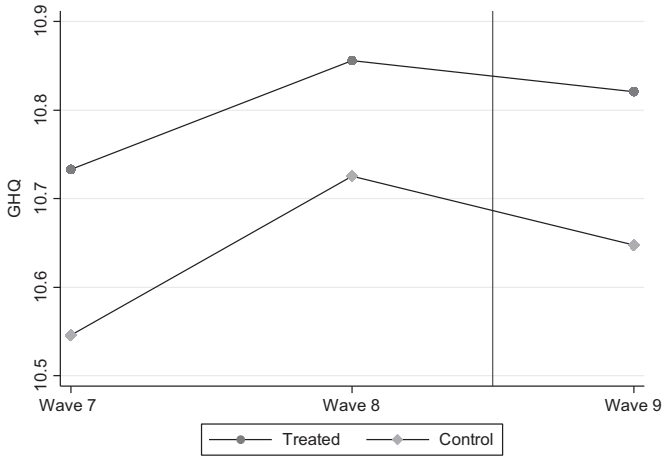

Fig. 2. GHQ over time split by treated and control (Self-reported measure). Note: The NMW was introduced between waves 8 and 9. The dots show the mean GHQ for waves 7-9 separately for individuals in the treatment and the control group.

between 1998-99 and 1999-00. Since higher GHQ scores correspond to higher levels of mental health, this implies that the NMW might have increased the mental health of individuals in these two groups. These graphs also appear to display broadly similar GHQ trends before the introduction of the policy.

Table 3 to Table 5 presents results of DiD models concerning the effects of the NMW on the GHQ. Each table separately reports results based on wage-based and self-reported definitions of treated and control groups. The first column of each table includes treatment effects for DiD models estimated *without* control variables, while the second column includes models *with* control variables. The upper parts of Table 3 and Table 4 include DiD models estimated using standard OLS, while the lower parts present fixed effects specifications. Table 5 shows robustness checks estimated using OLS models.

Table 3 reports that the estimated treatment effect of the introduction of the NMW on mental health is negative, not statistically significant and around 0.20 GHQ points on the Likert scale for an OLS model without covariates (wages-based definition of treatment and controls). The corresponding estimated coefficient for the alternative definition of treatment and controls is also not statistically significant, though positive and very small at around 0.01 GHQ points. Treatment effects obtained via OLS

Table 3
Main models for the effect of the NMW on GHQ.

OLS	Wage based		Self-reported	
	DiD	DiD with covariates	DiD	DiD with covariates
DiD Coeff.	-0.20	-0.41	0.01	0.02
95% C.I.	(-1.37 - 0.97)	(-1.57 - 0.74)	(-0.80 - 0.81)	(-0.78 - 0.82)
Observations	1,457	1,457	3,529	3,529
Fixed effects	Wage based		Self-reported	
DiD Coeff.	0.32	0.25	-0.14	-0.09
95% C.I.	(-0.81 - 1.45)	(-0.89 - 1.39)	(-0.98 - 0.70)	(-0.92 - 0.75)
Observations	1,457	1,457	3,529	3,529

Notes: 95% confidence intervals in parentheses, *significant at 10%, **significant at 5%, ***significant at 1%. This table reports average treatment effects on the treated (ATT) obtained from panel data difference-in-difference (DiD) models. The outcome variable is the GHQ12 on the 0-36 scale. The table is split vertically by whether wage information or self-reported information was used to identify treated and control groups. The table is split horizontally by whether OLS or fixed effects models were used to estimate the effect.

Table 4
Main models for the effect of the NMW on GHQ for females.

	Wage based		Self-reported	
	DiD	DiD with covariates	DiD	DiD with covariates
OLS				
DiD Coeff.	0.29	0.13	-0.31	-0.28
95% C.I.	(-1.41 - 1.99)	(-1.51 - 1.76)	(-1.33 - 0.70)	(-1.30 - 0.75)
Observations	717	717	1,804	1,804
Fixed effects	Wage based		Self-reported	
DiD Coeff.	0.52	0.43	-0.40	-0.32
95% C.I.	(-1.08 - 2.11)	(-1.25 - 2.12)	(-1.48 - 0.68)	(-1.39 - 0.76)
Observations	717	717	1,804	1,804

Notes: 95% confidence intervals in parentheses, *significant at 10%, **significant at 5%, ***significant at 1%. This table reports average treatment effects on the treated (ATT) obtained from panel data difference-in-difference (DiD) models. The outcome variable is the GHQ12 on the 0-36 scale. The table is split vertically by whether wage information or self-reported information was used to identify treated and control groups. The table is split horizontally by whether OLS or fixed effects models were used to estimate the effect.

models with covariates are similar and still not statistically significant. Fixed effects specifications in Table 3 appear to present analogous results. Noticeably, the signs of the estimated effects switch from positive to negative when moving from OLS to FE estimates. Yet, it should be born in mind that all estimated treatment effects are not statistically different from zero. Full results of the OLS models from Table 3 with all covariates can be found in Table A4 in Appendix A.

Table 4 shows results for the female sub-sample and reports positive treatment effects of between 0.13 and 0.29 GHQ points for the wage-based definition and negative effects of between -0.28 and -0.31 for the self-reported definition using OLS with and without covariates. FE models produce treatment effects of between 0.43 and 0.52 and -0.32 and -0.40 for models with and without covariates, respectively. However, none of these estimates are statistically significant. Overall, this appears to imply that the introduction of the NMW had no statistically significant effect on mental health. In addition, the direction of the effect does not appear to be clearly defined.

Results in Appendix B (Table A5 and Table A6) suggest wages were substantially affected by the NMW and employment was also positively affected (under the self-reported definition of treatment and control groups) suggesting the NMW had the intended effect on wages and no negative effect on employment, which in turn might have affected mental health.

4.1. Robustness checks

Table 5 includes results from our robustness checks. Here, we examine the sensitivity of our results to the wage-based definition of treated and control groups used in our main models. To do this, we employ further control groups for the wage-based measure using individuals with wages 100-130% and 120-140% of the NMW. In all of these specifications, estimates of treatment effects appear to be positive but not statistically

Table 5
Robustness checks using OLS.

	Wage based		Self-reported	
	DiD	DiD with covariates	DiD	DiD with covariates
Interviewer has seen payslip of interviewee				
DiD Coeff.	-0.71	-1.04	-0.60	-0.89
95% C.I.	(-2.91 - 1.50)	(-3.21 - 1.13)	(-2.32 - 1.12)	(-2.60 - 0.82)
Observations	411	411	990	990
Part-time workers only				
DiD Coeff.	-0.40	-0.64	0.43	0.37
95% C.I.	(-3.03 - 2.22)	(-3.18 - 1.90)	(-0.94 - 1.80)	(-1.04 - 1.78)
Observations	280	280	688	688
Control group including only individuals with a stable wage				
DiD Coeff.	-2.40	-2.30		
95% C.I.	(-7.87 - 3.06)	(-7.44 - 2.83)		
Observations	540	540		
Control group equal > NWM & < = 130%				
DiD Coeff.	-0.16	-0.42		
95% C.I.	(-1.38 - 1.07)	(-1.63 - 0.78)		
Observations	1,217	1,217		
No job change				
DiD Coeff.	-0.20	-0.41		
95% C.I.	(-1.37 - 0.97)	(-1.57 - 0.74)		
Observations	1,457	1,457		
Control Group 120-140% of NMW				
DiD Coeff.	-0.30	-0.38		
95% C.I.	(-1.60 - 1.00)	(-1.68 - 0.92)		
Observations	1,003	1,003		
No overtime adjustment				
DiD Coeff.	0.50	0.26		
95% C.I.	(-0.82 - 1.83)	(-1.03 - 1.55)		
Observations	1,198	1,198		
Only one person households				
DiD Coeff.	-2.25	-1.15	0.45	0.06
95% C.I.	(-6.93 - 2.42)	(-5.37 - 3.07)	(-3.33 - 4.24)	(-4.03 - 4.15)
Observations	95	95	282	282
Balanced sample				
DiD Coeff.	-0.15	-0.29	-0.01	0.01
95% C.I.	(-1.40 - 1.10)	(-1.54 - 0.96)	(-0.88 - 0.85)	(-0.86 - 0.87)
Observations	1,238	1,238	2,809	2,807
Combination of both treatment/control group definitions				
DiD Coeff.		1.22	1.13	
95% C.I.		(-1.36 - 3.80)	(-1.38 - 3.64)	
Observations		592	592	
Added time-trend and squared time trend				
DiD Coeff.	-0.20	-0.41	0.01	0.06
95% C.I.	(-1.37 - 0.97)	(-1.57 - 0.75)	(-0.80 - 0.81)	(-0.74 - 0.86)
Observations	1,457	1,457	3,529	3,529

(continued on next page)

Table 5 *(continued)*

	Wage based		Self-reported	
	DiD	DiD with covariates	DiD	DiD with covariates
Restricted to those with low baseline GHQ (GHQ > 18) in wave 8				
DiD Coeff.	-0.39	-1.37	-2.25	-2.09
95% C.I.	(-5.09 – 4.30)	(-2.27 – 2.68)	(-6.79 – 2.28)	(-6.85 – 2.66)
Observations	154	154	228	228

Notes: 95% confidence intervals in parentheses, *significant at 10%, **significant at 5%, ***significant at 1%. This table reports average treatment effects on the treated (ATT) obtained from panel data difference-in-difference (DiD) models. The outcome variable is the GHQ12 on the 0-36 scale. The table is split vertically by whether wage information or self-reported information was used to identify treated and control groups. The type of robustness check that was undertaken splits the table horizontally.

significant, confirming previous results. Furthermore, to mitigate potential measurement error in self-reported wages used to build the alternative definition of treatment and control groups, we estimate our models on a sample of individuals who were asked to produce payslips. The treatment effects are -0.73 and -0.62 on the GHQ Likert scale for the wage-based and self-reported definitions respectively. We also estimate separate models for part-time workers as they might have been more frequently affected by the NMW compared to full-time workers (Manning & Petrongolo, 2008). The resulting treatment effects are -0.46 and 0.47 (GHQ Likert scale) for wage-based and self-reported definitions and both are not statistically significant.

In order to account for wage increases that might have offset the minimum wage, we estimate a model in which the control group is reduced to those individuals who had the same wage throughout the study period and also find no significant effects. This model can only be estimated for the wage-based definition and the corresponding treatment effect is -2.43 GHQ scores. In addition, to explore potential issues related to second earners within the same household, we tested our DiD models on a sample of one-person households. Invariably, treatment effects for all other robustness checks are still not significantly different from zero. Finally, we also present robustness checks for the subscales of the GHQ in Table 6. Throughout all robustness checks, we do not identify any statistically significant relationship between the introduction of the NMW and mental health.

Further robustness checks in Appendix C present an attempt to replicate results of Reeves et al. (2016) (Table A8 and Table A9). Using their treatment and control group definitions, our results differ from theirs and show no statistically significant effects of the introduction of the NMW on GHQ. Appendix D (Table A10) shows estimates obtained from the treatment intensity estimator. The corresponding estimated effects are still not statistically significant.

5. Discussion and conclusions

We exploit the policy experiment provided by the introduction of the UK national minimum wage and explore the causal impact of wage increases on mental health among low-wage workers. We employ data drawn from the BHPS and estimate a series of difference-in-difference models. We find only limited and not statistically significant effects of the NMW on mental health. Our main findings appear to be confirmed by several robustness checks and alternative definitions of treatment and control groups.

Given the current debate around the living wage and the effects of mental health on employment and productivity, exploring the effects of the UK NMW on mental health is particularly relevant in the UK as well as internationally. Accordingly, several countries have recently introduced minimum wages such as China (Chinese Ministry of Labour and Social Security, 2004), Hong Kong (Legislative Council of Hong Kong, 2010) and Germany (CDU, CSU & SPD, 2013). Furthermore, while evidence shows that mental illness appears to be concentrated among the low-wage earners

Table 6
Effects of the introduction of the NMW on GHQ subscales.

OLS with control variables						
Item	1	2	3	4	5	6
Wage-based						
Coeff.	-0.09	0.00	-0.04	-0.07	0.03	-0.06
95% C.I.	(-0.20 - 0.03)	(-0.17 - 0.17)	(-0.17 - 0.09)	(-0.18 - 0.04)	(-0.14 - 0.19)	(-0.22 - 0.10)
N	1,464	1,464	1,464	1,464	1,466	1,467
Self-reported						
Coeff.	-0.03	0.00	-0.01	-0.07*	0.03	0.02
95% C.I.	(-0.12 - 0.05)	(-0.11 - 0.12)	(-0.09 - 0.07)	(-0.15 - 0.00)	(-0.08 - 0.15)	(-0.08 - 0.13)
N	3,548	3,548	3,547	3,547	3,549	3,549
	7	8	9	10	11	12
Wage-based						
Coeff.	-0.08	-0.03	-0.06	-0.05	-0.04	0.08
95% C.I.	(-0.21 - 0.05)	(-0.15 - 0.08)	(-0.24 - 0.11)	(-0.20 - 0.11)	(-0.17 - 0.10)	(-0.06 - 0.21)
N	1,466	1,467	1,467	1,468	1,468	1,468
Self-reported						
Coeff.	0.03	-0.01	-0.02	0.05	0.00	0.00
95% C.I.	(-0.06 - 0.12)	(-0.09 - 0.08)	(-0.15 - 0.11)	(-0.06 - 0.16)	(-0.09 - 0.09)	(-0.09 - 0.09)
N	3,549	3,550	3,550	3,546	3,546	3,547

Notes: 95% confidence intervals in parentheses, *significant at 10%, **significant at 5%, ***significant at 1%. The items are: Concentration (1), Loss of sleep (2), Playing a useful role (3), Capable of making decisions (4), Constantly under strain (5), Problem overcoming difficulties (6), Enjoy day-to-day activities (7), Ability to face problems (8), Unhappy or depressed (9), Losing confidence (10), Believe in self-worth (11), General happiness (12). This table reports average treatment effects on the treated (ATT) obtained from panel data difference-in-difference (DiD) models. The outcome variable is the GHQ12 on the 0-36 scale. The table is split horizontally by whether wage information or self-reported information was used to identify treated and control groups. The 12 GHQ subscales split the table vertically.

(Meltzer et al., 2002), the direction of this effect is still disputed. This study does not identify statistically significant changes in mental health driven by wage increases, shedding some further light on the potential causal link between wages and mental health.

Moreover, our findings appear to differ from the ones of a recent paper from Reeves et al. (2016). Whereas they do not find statistically significant effects on a number of physical health outcomes and behaviours, they appear to identify a significant effect of the UK NMW on mental health. We believe this result might be mainly due to the different way we build treatment and control groups. More specifically, Reeves et al. (2016) appear to compare two alternative sets of treatment and control groups: one set compares eligible recipients versus ineligible non-recipients (individuals with wages just above the minimum wage threshold), while a second set contrasts eligible recipients with *eligible* non-recipients. Although the former set of treatment and control groups is similar to the wage-based comparison also employed here, the latter is akin to an intention to treat (ITT) analysis, i.e. it appears to be based on the initial treatment assignment (eligibility to the NMW) rather than the actual treatment received. Furthermore, both of their definitions rely on self-reported wages and, differently from our work, they do not appear to make use of the special minimum wage question included in the BHPS. Importantly, since Reeves et al. (2016) appears to find larger and more significant effects on mental health in their second comparison (the ITT analysis), our results may substantially differ in their interpretation. In any case, our definitions of treatment and controls result in a larger sample size, i.e. 172 in Reeves et al. (2016) versus 1,500 to 3,500 observations depending on the definition in our study. Further differences between our works might include the number and types of controls.[11]

[11] For further details about the differences between our analysis and the one of Reeves et al. (2016), see our replication of their results in Appendix C.

Our analysis has some potential limitations. First, since our models focus only on individuals who were already employed, they would not be able to identify changes in mental health potentially caused by increased unemployment due to the NMW. However, our robustness checks and the previous literature on the NMW find only limited evidence of significant employment effects (Card & Krueger, 1993; Metcalf, 2008; Stewart, 2004a; Stewart, 2004b). Secondly, our models and data focus on short-run effects. This implies that the long-run effects of the NMW on mental health may vary and could potentially increase over time. Thirdly, it is possible that individuals will have somehow anticipated the introduction of the NMW. While the NMW was part of the 1997 election campaign debate and so employees might have been aware of a potential wage increase, its actual level was only announced publicly on the 6th of March, i.e. 25 days before its implementation. Also because of this, anticipation might not have played a major role in our data (McCartney, 1999). Finally, stigma attached to the NMW and any potential negative effect of this on their mental health may countervail or reduce any positive wage effect, resulting in a zero net effect on mental health.

Given our findings, a strong policy conclusion may be premature. However, it is possible that larger increases in wages will lead to a more pronounced impact on mental health. Since many of the costs of mental illness, such as lost productivity or absenteeism, are borne by employers, the benefits of improved mental health and sustaining people in employment may outweigh costs of increased wages.

Appendix A. Supplementary material

Supplementary material associated with this article can be found in the online version at http://dx.doi.org/10.1016/j.ssmph.2017.08.007.

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