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The impact of a wage increase on mental health: Evidence from the UK minimum wage

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Previous studies on the relationship between income and mental health focus on lottery winners and find that positive income shocks may improve mental health. We focus on low-wage earners, who have a higher propensity of experiencing mental health problems, and exploit the policy experiment provided by the introduction of the 1999 UK minimum wage to identify the impact of a wage increase on mental health. Combining matching techniques with a series of difference-in-differences models we find that the minimum wage had only limited short-run effects on the mental health of those affected by the minimum wage. Our estimates do not appear to support earlier findings that indicate that monetary shocks improve an individual's mental health. Several robustness checks controlling for measurement error and treatment and control group composition appear to confirm our main results. Our findings suggest that policies aimed at improving the mental health of low-wage earners should consider either the non-wage characteristics of employment or larger wage increases.

Keywords: Minimum Wage; Mental Health; Policy Evaluation; BHPS

JEL classifications: C21; I18, J38

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I. Introduction

Employment can improve individual mental health and well-being, is associated with lower healthcare utilisation, benefit savings and income tax gains. Mental health problems cost the UK economy around £105 billion every year, arising from treatment costs, human costs and lost productivity (Centre for Mental Health, 2010). Around 18 percent of the working-age population in England have a mental health disorder (van Stolk *et al.*, 2014).Examining the relationship between income and the mental health of low wage earners is important as the lowest income quintile of the population have twice the probability of facing mental health problems compared to average income earners (Meltzer *et al.*, 2002). It is possible that the association between low income and poor mental health exists because those with mental illness have a higher likelihood of unemployment (Tefft, 2012).

We contribute to the sparse literature on the effect of income on mental health. We use the introduction of the UK National Minimum Wage (NMW) as an exogenous shock to examine the relationship between income and mental health for low wage earners. The UK NMW was introduced in April 1999 and was an important policy change that attempted to reduce in-work poverty and acknowledge the lower bargaining power of individuals in low pay occupations. The introductory rates were supposed to raise the wages of up to nearly two million workers (Low Pay Comission, 1998). The Low Pay Comission (1998) estimated the average wage increase of covered workers to be around 30%. Prior to the minimum wage, the Trade Board Act of 1909 required four wage councils to set minimum wages for different industries and these were in place from 1909 to 1993. Between 1993 and 1999 there was no legal wage floor in the UK. The minimum wage introduced in 1999 was £3.60 at the adult rate for individuals above 21 years of age and £3.00 for those aged between 18-20 years[1].

The existing literature examining the effect of a positive monetary shock on mental health uses lottery winnings as an exogenous shock to individuals' income and employs instrumental variable techniques. The results of these studies suggest a positive but likely small effect (Apouey and Clark, 2015; Cesarini *et al.*, 2014; Lindahl, 2005). Individuals participating in lotteries might however be systematically different from the wider population. Furthermore, the NMW is a more suitable exogenous shock as it clearly affects low wage earners who have a higher prevalence of mental health problems. The wider effects of the NMW on mental health have not been explored in the literature previously. Any societal benefit of better mental health through an increased wage is likely to cancel out if the minimum wage lowers employment. The model of perfectly competitive labour markets and the basic matching model predict that a NMW would reduce employment, while the monopsony model and the matching model with endogenous labour market participation or job search effort predict an increase in employment (Cahuc, 2004; Flinn, 2006; Manning,

[1] A rate for 16-17 year olds and a rate for apprentices were added in 2004 and 2010, respectively. However, these rates are outside the period considered in this work.

2003; Neumark and Wascher, 2006). However, since employment has been shown to be a major determinant of mental health (Llena-Nozal *et al.*, 2004), a negative employment effect of the NMW could potentially exceed a positive mental health effect of a minimum wage. However, previous research has found no effect of the UK NMW on employment (Metcalf, 2008; Stewart, 2004a; Stewart, 2004b). This paper aims at filling this gap by providing evidence from a natural experiment on whether the mental health of low wage workers was affected by the introduction of the NMW.

We identify the effect of the introduction of the NMW on mental health by employing individual-level data from the British Household Panel Survey (BHPS), which measures mental health via the General Health Questionnaire (GHQ), a widely used measure of mental health. Our identification strategy exploits the policy experiment provided by the introduction of the NMW and employs difference-in-differences (DiD) methods. The control group consists of individuals not affected by the NMW (identified either through self-reporting or reported wages) and the treatment group consists of individuals experiencing a wage increase due to the NMW. Our DiD models make use of the difference in individual's GHQ scores before and after the NMW by comparing differences between individuals affected and unaffected by the NMW. Our empirical models only find a limited effect of the NMW on mental health. The majority of our estimates show a series of small and statistically insignificant treatment effects. Only a small number of specifications suggest an improvement in mental health due to the introduction of the NMW on mental health as measured by the GHQ. Overall, the results do not indicate an effect of the NMW on mental health. A host of potential explanations for our findings are explored.

II. The effect of income shocks on mental health

Several studies have examined the causal effect of income on mental health revealing that there might be a number of potential mechanisms through which income changes could affect an individual's mental health. Lindahl (2005), Gardner and Oswald (2007) and Apouey and Clark (2015) exploit lottery winnings to analyse the relationship between income and mental health. These papers find that higher income appears to improve mental health. Gardner and Oswald (2007) as well as Apouey and Clark (2015) employ the BHPS and measure mental health using the GHQ, while Lindahl (2005) uses the Swedish Level-of-Living Survey and self-reported symptoms of poor mental health. A recent paper by Cesarini et al. (2014) uses a large and extensive administrative dataset from Sweden to analyse the effect of lottery winnings on a battery of health outcomes. They find a modest effect of large lottery winnings on reductions in suicides and the uptake of anti-depressant drugs. Cesarini et al. (2014) argue that their effects are more precise than the ones from previous studies as their dataset is extremely large. Comparing their results to Gardner and Oswald (2007), they show that their estimated effect is half the size. However, all these studies identify the effect of an income shock on mental health for lottery winners. Sawkins and Dickie (2002) show the importance of gender, age, education, marital status and occupation on the decision of lottery participation. It is therefore doubtful, whether lottery participants are representative of the wider

population.

McInerney et al. (2013) and Askitas and Zimmermann (2011) exploit the 2008 financial crisis as an exogenous shock. Askitas and Zimmermann (2011) exploit changes in the pattern of internet searches for mental health self-diagnosis and treatments over the crisis period to show that the financial crisis led to a spike in these searches. McInerney et al. (2013) use the US Health and Retirement Study and find that the wealth losses caused by the financial crisis worsened the mental health of individuals. They measure mental health with the Center for Epidemiologic Studies Depression (CES-D) Scale and examine whether the individual took prescription medicine for anxiety or depression. Tseng and Petrie (2012) analyse the effect of income shocks on the mental health of individuals living in poverty exploiting a discontinuity in benefits for senior Taiwanese farmers. They find that income shocks improve mental health as measured by the CES-D. Overall, the literature suggests that there appears to be an effect of income shocks on mental health. However, the direction of the income shock matters and the size of the effect on mental health is ambiguous and likely to be small.

There are a number of potential mechanisms through which income shocks could affect mental health. Following Benzeval et al. (2014) the literature connecting income and mental health is categorized into three pathways, these are broadly defined as: *material*, *psychosocial* and *behavioural*.[2]

First, the *material pathway* can work directly through an income shock by enabling the purchase of physical goods and services which can improve an individual's mental health. It has been shown for example that mental illness is associated with poor diet, which is often caused by low income (Prince *et al.*, 2007). For example an income shock might facilitate the attainment of a healthier lifestyle or better accommodation. The material pathway can however also work through routes which enable an individual to cope with mental health problems, such as receiving mental health care. In countries with health care systems with no social insurance or not free at the point of use, higher income enables individuals to purchase better mental health care services. However, even in countries with universal and free care, such as England, an income-gradient in utilization of mental health care appears to exist (White *et al.*, 2014). The estimate of the minimum income needed in the UK to live a healthy life has been found to lie above the NMW (Morris *et al.*, 2000). If the threshold is indeed above the NMW then the introduction of the minimum wage should bring the affected individuals closer to this threshold. Given that it is likely that the relationship between income and mental health is one of decreasing returns, the effect should be non-negligible following this pathway.

Second, the *psychosocial pathway* (or *stress pathway*) concerns those psychosocial changes that may increase an individual's level of stress and lead to depression or other mental illness. For example, individuals tend to compare their income with that of their peers and this might have an impact on their mental health or

[2] Though we point out that these pathways have the sole purpose of structuring the literature. There are many complex overlaps and interactions between these theoretical pathways.

psychological well-being (Marmot, 2004; Marmot and Wilkinson, 2001; O'Donnell *et al.*, 2013). However, it is ambiguous whether higher income leads to better or worse mental health (Blanco-Perez, 2012).

Third, looking at the *behaviour pathway*, there are a number of behaviours that positively or negatively affect mental health such as unhealthy eating (Scott *et al.*, 2007), physical exercise (Penedo and Dahn, 2005), smoking (Mykletun *et al.*, 2008) or alcohol and substance misuse (Jane-Llopis and Matytsina, 2006). A theory explaining why there might be changes in health-behaviours as income increases is the Grossman model, an economic model explaining the demand for overall health and healthcare (Grossman, 1972; Grossman, 2000). In this model, a wage increase affects both costs and benefits. The benefits increase since an individual has more healthy days which can be transformed into income via labour. The costs increase as a consequence of an increase in the opportunity cost of the individual's time, which makes investment in mental health more expensive as these generally require the individual's time. However, mental health also has non-time inputs. The consumption of alcohol, for example, takes the same time as the consumption of water, keeping opportunity cost of the individual's time constant, but affecting mental health through the choice of goods. In general it is unclear what the prediction of a wage increase on mental health from the Grossman model is, but it is usually assumed that the benefits of healthy days and more healthy goods outweigh the increase in the opportunity cost[3]. Given a low opportunity cost of time, the immediate consumption benefits of unhealthy behaviours might outweigh their long-term health consequences.

An important additional aspect is stigma. A large treatment gap exists for mental illness with individuals not seeking care due to the anticipated discrimination and prejudice against individuals with mental illness (Henderson *et al.*, 2013; Thornicroft, 2008). Stigma may therefore diminish the size of the effect of an income shock on mental health if for example it reduces the probability of an individual purchasing services or seeking help which may improve their mental health.

III. Data

The data used in this paper is the British Household Panel Survey (BHPS) wave 7-9 (29th of August 1997 to 30th of April 2000) with the NMW being introduced between wave 8 and 9. The BHPS is a longitudinal, nationally representative dataset of Great Britain[4]. The survey includes information on individual and household characteristics such as health and use of health care, work and education, income and wealth as well as occasional topical variables some of which are used to identify individuals affected by the NMW. The BHPS sample dropped individuals from the sample if they were not covered by the minimum wage

[3] Yet, this makes the assumption that individuals with low wages have low opportunity costs of their time. This is a reasonable assumption when the opportunity cost is defined solely in monetary terms, however it may ignore other activities the individual might value e.g. informal care, voluntary work, etc.

[4] In wave 11 individuals from Northern Ireland were added to make it representative of the UK.

such as the armed forces, self-employed[5] and the retired. Observations were dropped if they had no GHQ score or if their wave 8 interview was during April or May 1999, as these dates were after the introduction of the NMW and wave 8 was used as the pre-treatment wave. Observations were also dropped if the individual was younger than 18 as the NMW 16-17 year old rate was only introduced in October 2004. Individuals interviewed in September 2000 were moved from wave 10 to wave 9 to increase the sample size[6]. The remainder of wave 10 was not used because it included a first increase in the NMW.

a. Mental health measure

The measure of mental health employed in this study was the self-reported General Health Questionnaire (GHQ) a validated and widely used tool n mental health research (Goldberg and Williams, 1988). The BHPS includes the reduced 12-item version of the GHQ. The 12-item version is based on the 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 depression or unhappiness. Respondents score each individual item from 0 to 3, 0 being the best score. These 12 scores are then aggregated into a Likert scale ranging from 0 to 36 *increasing in illness*, where lower scores correspond to better mental health (Goldberg and Williams, 1988; Likert, 1952). The GHQ was developed as a screening tool for psychiatric illness and is often used in health, psychological and economic research as a measure of mental health (Apouey and Clark, 2015; Gardner and Oswald, 2007). Goldberg *et al.* (1997) has shown that the 12 item version of the GHQ has a sensitivity (correctly identifying individuals with mental ill-health) of 83.7% and a specificity (correctly identifying the proportion of individuals with mental ill-health) of 79.0%. It has also been shown to be robust to re-test effects (Pevalin, 2000).

b. Identification of treatment and control groups

Two sets of treatment and control groups were employed. The first set of treatment and control groups was based on the individual's hourly wage. We follow Arulampalam *et al.* (2004) and derive the individual's hourly wage as:

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[5] We acknowledge that the self-employed might have a different relationship between wages and mental health than wage workers and refer to Rietveld *et al.* (2014) for a good study on the relationship between self-employment and mental health. We also acknowledge that retired and armed forces individuals might have systematically different mental health than the regular workforce due to several factors such as age (retired) and work-environment (armed forces).

[6] 104 observations from wave 10 are moved to wave 9.

$$\frac{\frac{12}{52}\ usual\ gross\ pay\ per\ momth}{usual\ standard\ weekly\ hours\ +\ 1.5\times\ (usual\ paid\ overtime\ weekly\ hours)} \quad (1)$$

The numerator presents the usual gross pay per month transformed into weekly pay. The denominator is the usual standard weekly hours and the usual paid overtime inflated by one and a half. Given that both numerator and denominator are weekly the ratio gives the hourly pay of the individual, the calculation follows Arulampalam *et al.* (2004).

The derived wages were deflated to 1998 values using Office for National Statistics – Consumer Price Indices. Variations to the calculation of hourly wages were tested (see Sensitivity Checks). An individual is in the first treatment group if his or her wage in wave 8 of the BHPS (before introduction of the NMW) was below the minimum wage and therefore would have needed to increase to the minimum wage between wave 8 and 9. The exact amount of the minimum wage depends on the individual's age as the NMW was introduced with three different rates: an adult rate of £3.60/hour, an 18-20 year old rate of £3.00/hour and a development rate of £3.20/hour for workers older than 21 participating in approved training programs[7]. The control group was defined as individuals whose wages fell between the NMW and 140% of the NMW before the introduction of the NMW. The threshold of 140% was used to create a group just above the minimum wage, who were not affected by the NMW. The actual number is somewhat arbitrary, Arulampalam *et al.* (2004) used 115%, but in order to increase sample size 140% was chosen. 140% in absolute terms implies an hourly wage between£3.6-£5.04 and £3-£4.2 respectively. This threshold was tested and varied in sensitivity checks. Using the derived wage data to identify treatment and control groups assumes that there are no measurement errors and that the self-reported information closely reflects the actual per hour rates. Reasons for potential measurement error in both the reported wages but also the reported hours are discussed in Stewart and Swaffield (2002). Measurement error would be a problem for the estimation strategy if the error is non-random. Several ways in which this problem is tackled are outlined in the sensitivity check section and the estimation strategy.

An alternative definition of treatment and control groups was based on a special NMW question added to wave 9 of the BHPS (Stewart and Swaffield, 2002). An individual was in the treatment group if he or she 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?*" All individuals replying "no" to this question were included in the control group. However, there are some limitations to this question. First, the question is asked to everyone who could fulfil the criteria of being eligible for the minimum wage and therefore some individuals in the control group may have large hourly wages. Hence, in order to ensure that the control group was as similar as possible to the treatment group the highest wage of the control group was

[7] Using the BHPS only individuals affected by the first two rates can be identified, but previous research has shown that it is likely that nobody in the BHPS was affected by the development rate (Arulampalam *et al.*, 2004).

restricted to £7.20 per hour. Another limiting aspect of the question routing is that the question was only asked to individuals who did not change their job between 1st April 1999 and their interview date (1st September 1999 to 30th April 2000[8]). Additionally, the question was only asked in wave 9, therefore individuals would need to be present in waves 7 and 8 as well. However, assuming that individuals do not systematically misreport, this variable should be less prone to measurement error.

As pointed out by Arulampalam et al. (2004), the cross-tabulation of the two treatment identifiers (wage-based vs. self-reported) does not present a perfect overlap. There might be two potential reasons for this. First, individuals received a pay-rise due to the NMW, but were not aware of it and therefore misreport this in the self-reported treatment identifier. The second problem concerns measurement error in the derived wage measure as extensively analysed by Stewart and Swaffield (2002). They argue that the main problems with the derived wage stem from the fact that the gross pay variable used to calculate the derived wage includes added components of wages such as bonuses, tips, etc. Individuals with large added wage components may find it difficult to report their "usual" gross pay[9].

c. Covariates

A number of control variables were added to our models to both increase the precision of the estimates and control for further observable differences between individuals in the treatment and control groups. These include: age; age-squared; gender; whether the individual works part-time; region (NUTS I level) [10]; the occupation of the individual split into primary, secondary and tertiary sector following the International Standard Classification of Occupations (ISCO 88[11]); whether the individual works in a small (1-49), medium (50-499) or large (>500) firm; the season of the year (reported by the interviewee); whether the individual has a permanent contract and the length of the individual's employment spell (i.e. the number of days in current employment).

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[8] The majority of interviews fall between September and December.

[9] It is therefore not possible to combine both treatment groups as this reduces the number of observations substantially to 86.

[10] The Nomenclature of Units for Territorial Statistics (NUTS) is a hierarchical system to identify subdivisions of EU member countries. NUTS 1 is the first division within countries. Great Britain is divided into 11 Government Office Regions making up England, while Scotland, Wales and Northern Ireland are not divided at this level.

[11] International Standard Classification of Occupations (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)

IV. Estimation Strategy

In order to address our research question examining whether an exogenous wage increase affects mental health, our empirical strategy exploits the policy experiment provided by the introduction of the NMW and involves the estimation of a series of difference-in-differences (DiD) models (Ashenfelter, 1978; Heckman and Robb, 1985). DiD is an established econometric tool for the ex-post evaluation of policy interventions (Angrist and Pischke, 2008). DiD compares the difference in outcomes (GHQ) in treated subjects pre- and post-intervention to the difference in outcomes in control subjects pre- and post-intervention (NMW)[12].

Our estimation strategy, including the definitions of control and treated groups, follows Stewart (2004b) and Arulampalam *et al.* (2004). The aim of this estimation strategy is to identify the average treatment-effect for the treated (ATET), in this case the change in GHQ brought about by the wage variation due to the NMW. First it is necessary to observe individual workers who were paid below the minimum wage before and after its introduction (treatment group) and compare them with a similar group unaffected by the NMW legislation (control group). The difference-in-differences methodology used here exploits this information by looking at the average change in GHQ over time for the treated (those affected by the NMW) minus the average change in GHQ over time for the control (those unaffected by the NMW). This approach makes the assumption that the mean change in mental health among the affected and unaffected would have developed in a similar fashion had the NMW not been introduced (i.e. the common trends assumption).

Considering descriptive statistics, the treated and untreated do already appear to be relatively similar. However, in order to ensure that individuals in treated and control groups have similar observable characteristics, we pre-process the data by employing matching techniques (Ho *et al.*, 2007) [13]. We compared nearest-neighbour (directly nearest and three-nearest neighbours), radius and kernel matching methods and choose the method with the smallest number of observation off the common support, smallest mean and median bias and smallest pseudo R^2 (see Table 1). We also compare the difference between treated and control for all variables before and after matching (see Table 2). Then weights extracted from the matching are applied to the DiD regressions.

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[12] The invention of DiD is attributed to Snow (1855) who was interested in whether cholera spread through air or water and exploited a discontinuity in the water supply system in London at the time. The study that gave rise to the use of DiD in economics in the last decades was a study by Card and Krueger (1993). They analysed the effect of minimum wage on employment in fast food restaurants using the fact that New Jersey and Pennsylvania had the same minimum wage before 1st of April 1999, when New Jersey's minimum wage increased to $5.05, while the minimum wage in Pennsylvania stayed the same. Card and Krueger (1993) found that contrary to predictions from economic theory employment rose instead of shrunk.

[13] The *psmatch2* command in Stata is used to perform the matching (Leuven and Sianesi, 2015).

[Table 1 – about here]

[Table 2 – about here]

a. Basic DiD and Fixed Effects DiD

Our basic (OLS) DiD model is:

$$Y_{it} = \beta_0 + \beta_1 T_{it} + \beta_2 A_{it} + \boldsymbol{\beta_3 T_{it} A_{it}} + \beta_4 X_{it} + \gamma_i + \mu_{it} \quad (2)$$

where

Y_{it} is the GHQ score in period t for individual i

T_{it} is a dummy equal to 1 if an individual belongs to the treatment group

A_{it} is a dummy equal to 1 in the period in which the NMW was introduced

X_{it} is a vector of individual and job characteristics determining Y_{it}

γ_i is an unobserved individual specific effect

μ_{it} is a random error term clustered at the individual level (Bertrand *et al.*, 2004)

β_3 is our parameter of interest as it provides an estimate of the treatment effect in this study (Angrist and Pischke, 2008). Given previous findings and our definitions of treated and control groups, it appears that the treated individuals are more likely to be female and to work part-time (Stewart and Swaffield, 2002). It is possible to control for gender and part-time status, but there might be other unobserved time-invariant effects in γ_i, such as genetic predisposition to mental illness. These unobservables could be correlated with T_{it} leading to selection bias. Therefore in order to control for time-invariant individual-level heterogeneity, another set of models that include time-invariant individual-level fixed effects to account for unobservables are estimated. However, unobservables that vary over time and are correlated with T_{it} might still introduce bias into the estimation. Controlling for unobservables isn't possible, but the BHPS includes job and individual characteristics (outlined in the data section), which are added as further controls.[14]

b. Wage gap estimator

Following Stewart (2004b) we uses a DiD estimation strategy and defines the treatment group as the wage gap between the actual wage and the minimum wage applicable to that individual:

[14] Bertrand *et al.* (2004) raise concerns about the consistency of the standard errors estimated in many papers using DiD methodology. Their concern is based on serial correlation. Given the small number of waves employed in our models, serial correlation is not a major concern. Even so, the standard errors are clustered at the individual level to ease potential concerns.

$$gap_{it} = \begin{cases} NMW_{it} - w_{it} \; if \; NMW_{it} > w_{it} \\ 0 \qquad\qquad else \end{cases} \tag{3}$$

where w_{it} is the individual's wage and NMW_{it} is the minimum wage applicable to the individual. In terms of (2) and (3), gap_{it} replaces T_{it}. The advantage of this adjustment to the standard DiD estimation is that individuals who received larger wage increases due to the introduction of the NMW now receive a higher weight, while standard DiD implicitly assumes that all NMW-caused wage increases have the same effect on mental health. Stewart (2004b) argues that the definition of treatment group based on the wage gap might be also affected by measurement error, especially at the bottom of the distribution. The coefficient of the interaction term between the before and after indicator and the wage gap is interpreted as the effect of a one unit increase in the gap, that is for each £1 below the threshold, on mental health.

V. Results

Figure 1 and Figure 2 Figure 1 GHQ over time split by treated and control (Wage based measure)describe the trends of the mean GHQ score for both sets of treatment and control groups separately; the vertical line in the graph represents the introduction of the NMW. For the wage based measure (Figure 1), GHQ increases slightly from 1997-98 to 1998-99 for both the treatment and control group. The mean GHQ clearly declines from 1998-99 to 1999-00. The graph shows that the trends before the introduction of the minimum wage appear quite similar. Figure 2 presents the same graph for the self-reported treatment and control group, where both treatment and control group appear to have increased their mean GHQ score from 1997-98 over 1998-99 to 1999-00. It is useful to remember that higher GHQ scores correspond to lower levels of mental health, when looking at these graphs. The graphs display approximately parallel GHQ trends before and after the introduction of the policy.

[Figure 1 & Figure 2– about here]

Table 3 presents the OLS and fixed effects estimates for the standard DiD. These are treatment effects from DiD models estimated using the wage gap to define treatment and control groups and results from the models adjusted with matching weights. Our preferred specification is the combination of matching adjusted and fixed effect estimation with further control variables, since this specification accounts for the greatest variety of potential biases. The results from these models (numbered 17&20) are 0.02 for the wage based case and -0.14 for the self-reported case, which appears to be quantitatively small and not significant different from 0. Estimating the same models only for females (Table 4) leads to slightly larger estimates of -0.53 (Model 38) and -0.16 (Model 40) though still not significantly different from 0. However, the sign of the wage-based estimate has changed from positive to negative.

For the non-wage gap models these coefficients should be interpreted as the effect of the introduction of the NMW, meaning the introduction of the NMW improves mental health by 0.14 in the self-reported

case or worsens the GHQ by 0.02 in the wage-based case respectively (higher GHQ scores corresponds with poorer mental health status. Whereas the interpretation varies slightly for the wage gap estimator, the wage gap estimates of -0.32 (Model 5) and -0.38 (Model 6) imply that a one pound Sterling (£) increase decreases (improves) the GHQ by -0.32 and -0.41 respectively.

It is difficult though to give these GHQ estimates a direct meaning. Comparison with pre-treatment averages and previous literature results are useful. For the wage-based case, the pre-treatment GHQ average is 10.88 and for the self-reported case, the pre-treatment GHQ average is 10.58. Therefore the presented estimates would imply a 0.18% (Model 18), -1.3% (Model 20), -4.8% (Model 38) and -1.47% (Model 40) change in GHQ.

One of the largest effects on GHQ identified in the literature comes from Gardner and Oswald (2007) who find that winning £1,000 or more in the lottery leads to an improvement in GHQ of between 1 and 1.90. Though, Cesarini *et al.* (2014) using large amounts of high quality administrative data find substantially smaller effects than the previous lottery winnings literature. Jones and Wildman (2008) find that relative deprivation worsens GHQ by 0.18 and Hauck and Rice (2004) find that lagged GHQ has an effect of 0.20 on GHQ in the present. Compared to these estimates our overall estimates from the main models (Model 18&20) appear small, though comparing these estimates with the ones for females (Model 38&40) they are somewhere between the effect of relative deprivation (Jones and Wildman, 2008) and the effect of lottery winnings (Gardner and Oswald, 2007).

[Table 3 – about here]

[Table 4 – about here]

a. Sensitivity checks

In order to tackle the measurement error problem, the models were re-estimated only for individuals for whom the interviewer saw the respondent's pay check. This is aimed at accounting for measurement error arising from the wage part (self-reported information) of the derived hourly wage. However it does not deal explicitly with the residual measurement error linked to the hour's part of the derived wage. Since the threshold for the control group of the wage based treatment group is somewhat arbitrary, the analysis was repeated with the threshold set to 110% and 130% respectively, and was conducted without inflating overtime pay. The results are presented in Table 5 for the entire sample and Table 6 for females and are overall small and insignificant.

The treated are predominantly female (Stewart and Swaffield, 2002), females usually having worse mental health (Rosenfield and Mouzon, 2013; Van de Velde *et al.*, 2010) and female part-time workers are the largest group affected by the NMW (Manning and Petrongolo, 2008). To account for this, we estimated models that controlled for gender and part-time work and alternatively separate models for men and

women and type of employment (part-time vs full-time employment)[15]. In both Table 5 and Table 6 the results for part-time workers for the wage-based case stand out for being identical and being large. They are identical because the part-time sample is largely female, so the estimate samples for these models are virtually identical. The coefficients are 1.89 and 3.12**. These coefficients are large and the coefficient from the model including control variables is even significantly different from 0. Though, it is necessary to be careful with these estimates as they are based on a sample of only 157 observations and despite being significant, are not very precise, with a standard error of 1.28 and 1.31 respectively.

A further robustness check was to restrict the sample to individuals who did not change their job during the study period, for the wage-based treatment identifier to make it comparable to the self-reported identifier where this was included in the design. Finally, the control group for the wage-based treatment identifier was restricted to individuals who received the same level of pay throughout the study period.

Further robustness checks such as reducing the wage based control group to 110% or restricting the control group to individuals who had a constant wage during the study period reduces the sample substantially making the coefficients unreliable. Therefore these models are not reported.

[Table 5 – about here]

[Table 6– about here]

VI. Discussion and conclusions

We exploit the natural experiment provided by the introduction of the UK national minimum wage and explore whether increases in wages for low-wage workers affect their mental health. To this aim, we employ data from the BHPS and combine matching techniques with a series of difference-in-difference specifications. The question whether the introduction of the UK NMW had an effect on mental health is particularly current both in the UK, given discussions about the so-called living wage, as well as internationally, given the introduction of a minimum wage in China in 2004 (Chinese Ministry of Labour and Social Security, 2004)[16], and more recently in Hong Kong in 2011 (Legislative Council of Hong Kong, 2010) and Germany in 2015 (CDU *et al.*, 2013). It has been established that mental health problems are concentrated among the low-wage earners (Meltzer *et al.*, 2002), but whether their low-wage/low-income causes the mental health problems or whether they earn little because of their mental health problems is still disputed. Previous research in western countries (Apouey and Clark, 2015; Cesarini *et al.*, 2014; Gardner and Oswald, 2007) mainly exploited income shocks from lottery winnings and found a positive

[15] A combination of gender and part-time employment was conducted, but this reduced the sample substantially.

[16] The Chinese minimum wage technically existed since the 1994 Labour Law, but was only defined and set up in the 2004 Minimum Wage Regulations. The Chinese minimum wage varies by province, whereas the other minimum wages were applicable across the entire country.

effect of income on mental health. Exploiting the exogenous change in wages caused by the introduction of the 1999 NMW, this study finds no clear effect of the wage increase on mental health, though a few significant results point towards a positive mental health effect. However, in the main models and the vast majority of robustness checks the effect of the NMW on mental health was not significantly different from zero. There are several potential explanations for this result.

First, it is necessary to be cautious about significance levels given the small sample size. Second, there is also a range of theoretical arguments, which can plausibly explain this result. One explanation is that individuals participating in lotteries might be different from individuals affected by the NMW. Sawkins and Dickie (2002) find that individuals participating in lotteries are more likely to be older males, while the treatment group in this study largely consists of younger females. There might also be unobservable differences between lottery participants and minimum wage recipients such as risk and time preferences or cognitive ability. Third, we are only estimating short-run effects of the introduction of the NMW as our sample runs until 30th of April 2000.

It is also possible that the wage increase caused by the NMW was not large enough to have any effect on individual mental health. However, if income and mental health are linked through the material pathway, even a small increase in wage, for individuals earning wages below £3.60 (£3 for 18-21year olds), enables the individual to invest more in their mental health. It is also worth noting that wage councils were abolished in 1993 in the UK, so employers were free to offer any wage. A counter argument would be that the general society wide wage increase was similar in relative size to the wage increase due to the NMW. In this case the relative income hypothesis, as explained by the psychosocial pathway that individual income comparisons affect mental health, would explain this result.

Assuming that income shocks do have a positive effect on mental health and that the income shock of the NMW was sufficiently large, the question arises whether the minimum wage could also have had a negative effect on mental health thus cancelling out the positive effect? Poverty-stigma is tentatively offered as such a negative effect, arguing that people being told that their wage is insufficient and requires a government determined raise might feel they are in poverty. Before the NMW these individuals might not have perceived themselves as poor. Studies show that low-income individuals perceive themselves to be stigmatized by society leading to social exclusion (Reutter *et al.*, 2009) and worse mental health (Kuruvilla and Jacob, 2007). Still assuming that the income shock element of the policy had a positive effect, it is possible that employers increased the workload for workers affected by the NMW or reduced investment in their work-environment to finance the wage increase, which could have an impact on workers' mental health through the psychosocial pathway of increased stress.

Finally, the treated individuals might have anticipated the wage increase and their mental health might have adjusted prior to the introduction of the new wage level. The minimum wage was a major policy for the Labour Party during the 1997 election. In July 1997, two months after being elected, the new Labour government created the Low Pay Commission (LPC) and in November 1997 introduced the NMW bill for the first time into parliament. The law passed in March 1999 and came into effect in April 1999. We

struggle to explore anticipation effects due to sample size restrictions. Though individuals were expecting a minimum wage, they could not be sure about the exact level. Our wage based measure uses individuals just above the minimum wage as controls. These individuals also could have reasonably expected to be affected by the NMW.

A strong policy conclusion may be premature, but it is possible that a larger increase in wages at the introduction of the NMW may have led to larger positive effects 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 illness and sustaining people in employment may outweigh costs of increased wages. Future research could address the effect of wage increases of different sizes on mental health to tease out exactly how high a wage increase is required to be to lead to a statistically and clinically significant improvement in mental health.

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VII. Tables and Graphs

Figure 1 GHQ over time split by treated and control (Wage based measure)

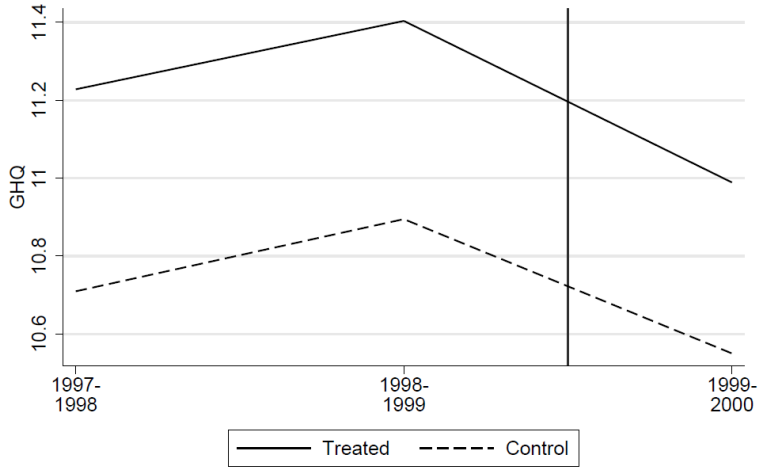

Figure 2 GHQ over time split by treated and control (Self-reported measure)

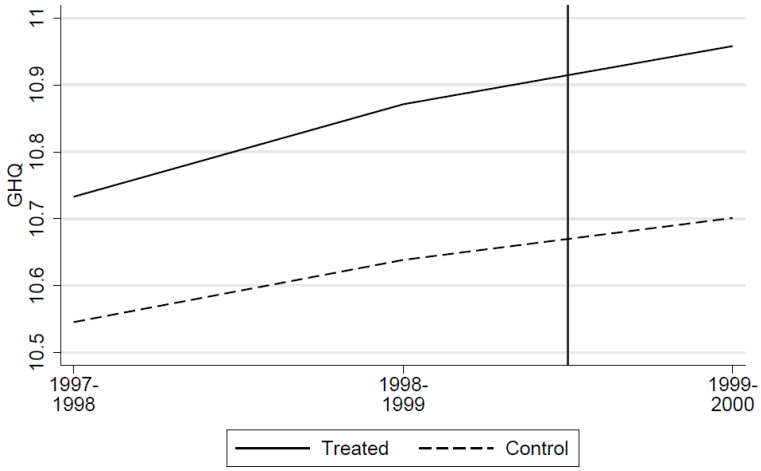

Table 1: Selecting a matching method

	Nearest Neighbour	Three nearest neighbours	Radius	Kernel
		Wage-Based		
Mean Bias	11.8	7.2	13.6	5.5
Median Bias	7.4	6.1	8.3	4.0
Pseudo R2	8.3%	3.2%	8.0%	1.9%
Obs. off support	0	0	0	5
		Self-Reported		
	Nearest Neighbour	Three nearest neighbours	Radius	Kernel
Mean Bias	6.8	3.7	no convergence	2.0
Median Bias	5.5	3.2		1.8
Pseudo R2	2.9%	1.1%		0.3%
Obs. off support	0	0		2

Table 2: Difference between treated and control before and after matching

		Wage-based			Self-reported		
Variable	Un/Matched	Treated	Control	Sig.	Treated	Control	Sig.
Age	Unmatched	38.58	36.52	***	35.35	34.75	
	Matched	38.53	38.51		34.53	34.62	
Age-squared	Unmatched	16.55	14.72	***	13.87	13.53	
	Matched	16.39	16.37		13.13	13.27	
Female	Unmatched	0.77	0.45	***	0.68	0.38	***
	Matched	0.76	0.76		0.66	0.67	
Part-time	Unmatched	0.44	0.14	***	0.34	0.10	***
	Matched	0.43	0.42		0.31	0.25	
Medium Workplace	Unmatched	0.16	0.35	***	0.28	0.41	***
	Matched	0.18	0.20		0.31	0.26	
Big Workplace	Unmatched	0.06	0.13	***	0.09	0.11	
	Matched	0.07	0.08		0.08	0.12	
Permanent	Unmatched	0.94	0.96		0.92	0.95	***
	Matched	0.93	0.92		0.95	0.94	
Employment Spell	Unmatched	1685	1910	***	1403	1499	
	Matched	1639	1669		1435	1285	
Secondary Sector	Unmatched	0.63	0.56	***	0.64	0.60	
	Matched	0.68	0.67		0.69	0.63	
Tertiary Sector	Unmatched	0.42	0.35	***	0.32	0.28	
	Matched	0.40	0.42		0.43	0.49	
Spring	Unmatched	0.08	0.08		0.03	0.04	
	Matched	0.00	0.00		0.01	0.01	
Summer	Unmatched	0.02	0.02		0.01	0.01	
	Matched	0.00	0.00		0.00	0.00	
Fall	Unmatched	0.51	0.46	**	0.49	0.49	
	Matched	0.50	0.52		0.50	0.47	
Wales	Unmatched	0.17	0.09	***	0.07	0.06	
	Matched	0.08	0.08		0.04	0.03	
N. Ireland	Unmatched	0.02	0.02		0.01	0.01	
	Matched	0.04	0.03		0.02	0.02	
London	Unmatched	0.06	0.04	**	0.03	0.04	
	Matched	0.07	0.08		0.05	0.05	
Yorkshire and the Humber	Unmatched	0.08	0.08		0.10	0.09	
	Matched	0.08	0.07		0.11	0.13	
South East	Unmatched	0.08	0.16	***	0.14	0.17	
	Matched	0.10	0.10		0.14	0.15	
South West	Unmatched	0.07	0.08		0.11	0.10	
	Matched	0.09	0.10		0.10	0.10	
East of England	Unmatched	0.04	0.04		0.04	0.04	
	Matched	0.04	0.04		0.02	0.02	
East Midlands	Unmatched	0.10	0.10		0.10	0.12	

	Matched	0.12	0.12		0.11	0.12	
West Midlands	Unmatched	0.07	0.10	**	0.14	0.14	
	Matched	0.09	0.10		0.18	0.15	
North West	Unmatched	0.09	0.10		0.07	0.10	**
	Matched	0.10	0.11		0.07	0.05	
North East	Unmatched	0.09	0.06	***	0.11	0.04	***
	Matched	0.10	0.09		0.08	0.10	

(a) This table presents the results of a test of the difference between unmatched and matched individuals

(b)* significant at 10% , ** significant at 5%, *** significant at 1%

Table 3: Main Models for the effect of the NMW on GHQ

	Wage based (OLS)		Self-reported (OLS)	
	Raw DiD	Regression-adjusted	Raw DiD	Regression-adjusted
	(1)	(2)	(3)	(4)
DiD Coeff.	-0.08	-0.18	0.05	0.16
S.E.	0.55	0.54	0.37	0.38
Observations	1,643	1,630	4,004	3,854
	Wage Gap Estimator (OLS)			
	(5)	(6)		
DiD Coeff.	-0.32	-0.38		
S.E.	0.64	0.62		
Observations	1,649	1,636		
	Using matching weights			
	(7)	(8)	(9)	(10)
DiD Coeff.	-0.13	-0.11	0.27	0.12
S.E.	1.21	1.16	0.44	0.44
Observations	992	989	2,902	2,874
	Wage based (Fixed Effect)		Self-reported (Fixed Effect)	
	(11)	(12)	(13)	(14)
DiD Coeff.	0.57	0.70	-0.16	-0.07
S.E.	0.56	0.56	0.37	0.39
Observations	1,643	1,630	4,004	3,854
	Wage Gap Estimator (Fixed Effect)			
	(15)	(16)		
DiD Coeff.	0.34	0.41		
S.E.	0.73	0.72		
Observations	1,649	1,636		
	Using matching weights (Fixed Effect)			
	(17)	(18)	(19)	(20)
DiD Coeff.	-0.39	0.02	0.05	-0.14
S.E.	1.25	1.27	0.45	0.46
Observations	992	989	2,902	2,874

(a)* significant at 10% , ** significant at 5%, *** significant at 1%

Table 4: Main Models for the effect of the NMW on GHQ for females

	Wage based (OLS)		Self-reported (OLS)	
	Raw DiD	Regression-adjusted	Raw DiD	Regression-adjusted
	(21)	(22)	(23)	(24)
DiD Coeff.	0.09	-0.03	-0.20	0.03
S.E.	0.80	0.79	0.46	0.47
Observations	803	797	2,047	1,958
	Wage Gap Estimator (OLS)			
	(25)	(26)		
DiD Coeff.	-0.13	-0.23		
S.E.	0.72	0.72		
Observations	808	802		
	Only using observations on the common support (OLS)			
	(27)	(28)	(29)	(30)
DiD Coeff.	-0.50	-0.50	0.33	0.21
S.E.	1.75	1.66	0.54	0.55
Observations	445	445	1,470	1,456
	Wage based (Fixed Effect)		Self-reported (Fixed Effect)	
	(31)	(32)	(33)	(34)
DiD Coeff.	0.62	0.72	-0.42	-0.30
S.E.	0.81	0.82	0.47	0.49
Observations	803	797	2,047	1,958
	Wage Gap Estimator (Fixed Effect)			
	(35)	(36)		
DiD Coeff.	0.15	-0.04		
S.E.	0.82	0.83		
Observations	808	802		
	Only using observations on the common support (Fixed Effect)			
	(37)	(38)	(39)	(40)
DiD Coeff.	-0.83	-0.53	0.00	-0.16
S.E.	1.81	1.88	0.56	0.56
Observations	445	445	1,470	1,456

(a)* significant at 10% , ** significant at 5%, *** significant at 1%

Table 5: Robustness Checks

Fixed effect models using only observations on the common supports

	Wage based		Self-reported	
	Interviewer has seen payslip of interviewee			
	Raw DiD	Regression-adjusted	Raw DiD	Regression-adjusted
	(41)	(42)	(43)	(44)
DiD Coeff.	0.18	0.92	-0.17	0.13
S.E.	1.26	1.04	0.99	0.99
Observations	303	302	935	931
	Part-time workers only			
	(45)	(46)	(47)	(48)
DiD Coeff.	1.89	3.12**	0.21	0.05
S.E.	1.28	1.31	0.83	0.86
Observations	157	157	549	548
	Control Group equal > NWM & <= 130%			
	(49)	(50)		
DiD Coeff.	0.77	0.83		
S.E.	0.59	0.59		
Observations	1,372	1,361		
	Unadjusted overtime pay			
	(51)	(52)		
DiD Coeff.	0.77	0.98		
S.E.	0.66	0.64		
Observations	1,349	1,336		
	No job change			
	(53)	(54)		
DiD Coeff.	0.57	0.70		
S.E.	0.56	0.56		
Observations	1,643	1,630		

(a)* significant at 10% , ** significant at 5%, *** significant at 1%

Table 6: Robustness Checks for females

Fixed effect model using only observations on the common supports

	Wage based		Self-reported	
	Interviewer has seen payslip of interviewee			
	Raw DiD	Regression-adjusted	Raw DiD	Regression-adjusted
	(55)	(56)	(57)	(58)
DiD Coeff.	0.57	2.77**	-0.04	0.21
S.E.	1.80	1.32	1.18	1.10
Observations	132	132	506	505
	Part-time workers only			
			(59)	(60)
DiD Coeff.	1.89	3.12**	0.25	0.12
S.E.	1.28	1.32	0.85	0.88
Observations	153	153	526	525
	Control Group equal > NWM & <= 130%			
	(60)	(61)		
DiD Coeff.	1.08	1.17		
S.E.	0.84	0.84		
Observations	727	722		
	Unadjusted overtime pay			
	(61)	(62)		
DiD Coeff.	0.55	0.62		
S.E.	0.85	0.82		
Observations	732	726		
	No job change			
	(63)	(64)		
DiD Coeff.	0.62	0.72		
S.E.	0.81	0.82		
Observations	803	797		

(a)* significant at 10% , ** significant at 5%, *** significant at 1%

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