CARDIFF BUSINESS SCHOOL
WORKING PAPER SERIES

Cardiff Economics
Working Papers

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The Impact of the Minimum Wage on the Incidence of Second Job Holding in Britain

E2006/14

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ISSN 1749-6101
February 2006

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Acknowledgements

The authors would like to thank the Low Pay Commission for financial support. The data used in this paper is Crown Copyright; has been made available by the Office for National Statistics through the ESRC Data Archive; and has been used by permission. Neither the ONS nor The Data Archive bear any responsibility for the analysis or interpretation of the data reported here. Becky Warpole provided useful comments along with participants at the EALE (2004) conference in Lisbon and the Royal Economics Society (2004) conference in Swansea.

Abstract

The advent of any earnings boost, such as provided by the introduction of a minimum wage, might be expected to reduce the supply of low paid individuals wanting to hold a second job. This paper uses difference-in-differences estimation on a panel of individuals matched across successive Labour Force Surveys around the time of the introduction of the national minimum wage in the United Kingdom in order to estimate the impact of the minimum wage and its subsequent upratings on second job working. There is little evidence to suggest that the extra pay provided by the introduction of the minimum wage was sufficient to affect the incidence of second job holding significantly. However, hours worked in the main job by second job holders may have risen relative to those not covered by the minimum wage; and hours worked in second jobs may have fallen for those whose second job was initially below the minimum.

JEL classification: J23, J31
Keywords: Second jobs, minimum wages

1. Introduction

Britain introduced a national minimum wage (NMW) in April 1999 with the promise that "low paid workers will see their earnings rise by an average of one-third"[1], and the intention to "tackle working poverty". Faced with such potent, one might have expected to see significant changes in the low paid labour market. Yet most existing work that examines the effects of the NMW introduction, concludes that the overall effect on the level of employment in Britain was broadly neutral,[2] although there may have been a small fall in the number of hours worked by low wage workers, Stewart and Swaffield (2005).

The net impact of the NMW on employment however, is, of course, affected by combination of both labour demand and supply effects. In the absence of constraints, the labour supply effect of an increase in the NMW would be to raise employment and hours. Whilst demand factors feature strongly in any examination of the employment effects of a minimum wage, one area where the labour supply effects of the NMW might be expected to dominate is in the holding of second jobs. Many people with second jobs are low paid and so the advent of the NMW aimed at tackling "poverty pay" might be thought to influence second job holding.

Labour demand theories regarding the effects of the NMW have little to say specifically about second job holding, other than through the effects on firms of the provision of low hours jobs on which, as shown below, many second job holders rely. On the supply side, simple classical labour supply theory, in which individuals are free to vary their hours of work in a job, cannot explain the existence of second job holding.

[1] Low Pay Comission press release 18 June 1998, http://www.lowpay.gov.uk/lowpay/press/news18_06_98.shtml

Individuals simply choose the hours-wage package in the sole job that optimises their utility. If however, labour in the two jobs is not a perfect substitute, so that the wage does not reflect fully the utility to be gained from holding a second job, Conway and Kimmell (1998) show that individuals could choose an optimal two jobs/hours package which effectively equates the marginal utility from both jobs. Individuals may also hold a second job as a hedge against unemployment, though the evidence supporting this hypothesis is weak, Bell, Hart and Wright (1997).

Alternatively, in the presence of hours constraints, individuals could take second jobs if the wage in the primary job is below the optimising wage, and constraints on hours prevent individuals from working more hours on the main job to make up any consequent income shortfall. If some workers take second jobs because the income generated by the hours/wage combination in the first job is low, then the imposition of a minimum wage might reduce the supply of individuals willing to take second jobs. In the presence of hours constraints, the effect of a minimum wage would be to raise the offered wage closer to the desired wage, with the hours constraint in the first job restricting the substitution effect so that the income effect dominates. In this case, the optimal hours-wage combination moves closer to the hours-wage package provided in the main job. Other things equal, this could act to reduce the incidence of second jobs among low paid workers, specifically among those initially below the minimum wage relative to others not covered initially by the NMW.

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[2] For example, Stewart (2004a), (2004b).

There has been relatively little research on second job holding,[3] in part because of lack of suitable data. Yet second job holding is an important issue in the debate as to whether individuals really are able to adjust their hours of work on the job in response to a change in wage rates. The existence of second jobholding is hard to reconcile with the simple competitive labour supply model of unconstrained, flexible hours in the job. If, however, firms have preferences for fixed working hours, then individuals may only be able to adjust hours following a wage change by moving to a different job. If moving is difficult because of frictions imposed by costs of mobility and information gathering, taking a second job could be viewed as one way of increasing hours of labour supplied without incurring all the costs of job change.

The literature on income targeting suggests that employees may stop working more hours once their income reaches a given target threshold.[4] Following an increase in the going wage, the target income hypothesis suggests that the income effect will dominate any substitution effect and that hours worked will fall. It could be argued that a second job enables workers to reach an income target in the presence of hours/income constraints in the main job. If so, then the introduction of the NMW, (in the main job), could reduce the need to take a second job in order to meet an income target, or reduce the number of hours needed to meet the target in either job.

The introduction and subsequent uprating of the NMW in Britain effectively generates a set of quasi-natural experiments with which to analyse the impact of the wage changes on second jobholding, by comparing the subsequent circumstances of those

[3] See Paxson and Sichermand (1996), Conway and Kimmel (1998) and Bluestone and Rose (1998) for the most recent U.S. studies; Fredriksen, Gravesen and Smith, (2001), for Denmark; and Boheim and Taylor (2004) for Britain.

whose wage was increased by the NMW and those not affected. In what follows, we also document the characteristics of individuals in, and firms most likely to provide, second jobs and how these characteristics have changed over time, around the advent of the NMW. Section 2 outlines the theoretical issues that underlie our study and Section 3 introduces our chosen data set. Sections 4 and 5 present and discuss our results. Section 6 concludes.

2. The Decision to Hold a Second Job

For those constrained by hours, the theoretical labour supply effect of an increase in the wage in the main job is straightforward, (see Shishko and Rostker (1976)). A higher wage in the main job, W_1, makes it less likely that an individual will need to take or work longer hours in a second job, H_2. The substitution effect which would tend to raise hours in the main job is restricted by hours constraints and so the income effect on reducing total hours worked dominates. If hours are constrained and the income effect is large enough, then the individual may obtain higher overall utility in the main job and be induced to give up their second job, so that $dH_2/dW_1<0$. The hypothesised effects are less clear if the wage in the second job also rises as a result of the NMW. It may now be preferable to keep the second job if the income effect generated by the wage increase in the second job dominates any income effects from the first, i.e. $dH_2/dW_2>0$.

Individuals may also hold second jobs for reasons other than income constraints. Labour in the two jobs may not be perfect substitutes, for example, so that the wage does not reflect fully the utility to be gained from holding a second job (Conway and Kimmell (1998)) and these other factors may also vary over time. For those unconstrained by

[4] See Farber (2003) for a recent U.S. study of this issue which comes out against income targetting among taxi drivers.

hours the effects will be similar though with smaller, less discrete, hours changes, (Conway and Kimmell (1998))[5]. In the unconstrained case the link between wages in the main job and hours worked in the second job is weaker, because of the existence of non-pecuniary factors.

If there are labour demand effects of the NMW, then the supply of jobs may change. Our reading of the existing literature on employment effects of the NMW in Britain, suggests that the overall supply of jobs has been little changed, but that there may have been small falls in hours offered by firms, Stewart and Swaffield (2005). There is no evidence as to where in the hours distribution the largest cuts occurred. Assuming that a rise in variable costs, such as the NMW, leads to a fall in hours offered in the main job, then this will tend to offset the income effect of the boost to hourly wages from the NMW, $dH_2/dH_1<0$. Again this would have the opposite effect on the supply of workers to second jobs to a rise in W_1. Ultimately, whether the income or substitution effects prevails and whether there is any discernible effect of the NMW on second job working become empirical matters and this is what we investigate in the following sections.

Difference-in-differences and the NMW

The introduction of the NMW created a treatment effect of differing intensity across the working population. Those workers whose pay was initially below the NMW received larger absolute and relative rises in their gross pay than most people whose pay was initially above the NMW, (Figure A1 in the appendix confirms this). This pattern also holds, although to a lesser extent, for subsequent upratings of the NMW. The data suggest that the 1998 median weekly rise in nominal gross pay for those initially below

[5] We note, in passing, that the existing theoretical literature has ignored the possibility that there may also be hours constraints in second jobs. If so, this simply increases the likelihood of observing discrete

the NMW and who stayed in the same job was around £15, compared to a median nominal mean increase of £7 for those job stayers initially above the minimum. This amounts to a 20% annual average nominal increase, not far from the Low Pay Commission's initial projection, for those below the NMW and a 5% average rise for those above.

We are interested in the probability that individual *i* holds a second job at time *t* conditional on the wage group that the individual belongs to in the main job,

$$\Pr(Two_{it}) = a_i + g_t + d^* \; Treat_{it} \qquad (1)$$

where $Two_i = 1$ is observed for individual *i* if in a second job, = 0 otherwise, a_i is an individual specific time invariant fixed effect, *g* is a time effect common to all individuals at time *t* and $Treat_i$ is a dummy variable denoting whether the individual belonged to the treatment group – those initially below the NMW. The unadjusted difference-in-differences estimator (DID) can be obtained simply by looking at the difference in the change in the sample probabilities of holding a second job of the treatment and a suitable control group over the period in which the NMW was introduced, as

$$\theta = [Two^{treat}_{99} - Two^{treat}_{98}] - [Two^{control}_{99} - Two^{control}_{98}]$$

This estimate can be obtained by pooling data over successive time periods and running logit or probit estimation on the following:

$$\Pr(Two_{it} = 1) = F[\alpha + \beta \; Treat_i + \eta \; Year_{99} + \theta \; Year_{99}{*}Treat_i \;] \qquad (2)$$

where $Year_{99}$ is a 1999 year dummy for the second year of observation – after the introduction of the NMW. The term *b* reflects base period differences between treatment and controls. The main coefficient of interest is *q*. This coefficient on the year and

changes in hours or in the number of jobs held given a shock of sufficient magnitude.

treatment interaction term gives the change in the second job holding differential between treatment and control group over the period in which the NMW was introduced, given by the marginal effect from probit/logit estimation of (2). If the parameter q is negative, it shows that second job holding fell relative to the control group between 1998 and 1999 (or any subsequent uprating period), other things equal. The size of the coefficient tells us by how much the differential between treatment and control group changed.

Differencing in this way removes any unobservable individual/group specific fixed effects. As with all DID estimation this assumes that in the absence of the NMW, the difference in the probability of holding a second job between treatment and control groups is the same in each period. It may of course be that the incidence of second job holding would develop differently over time for individuals in different parts of the wage distribution. We can test this by examining whether there was a wage effect, before the intervention took place.[6]

The DID approach also assumes that the wage taken in the main job is not influenced by the probability of having a second job and that there are no differential employment effects on the number of main jobs across groups following the NMW. It may be argued that employers substituted minimum wage workers for close, but slightly better paid, substitutes after the NMW came in, hoping to benefit from productivity gains from the more expensive workers. It may also be that there were spillover effects in the wage distribution which would work to offset the incentives to hire the higher wage workers somewhat. In order to allow for observable differences between treatment and

[6] Figure A4 in the report version of this paper http://www.lowpay.gov.uk/lowpay/research/pdf/lpc8.pdf suggests that there is little evidence that there were differential trends in second job holding between the treatment and control groups before the NMW. In the absence of the NMW before 1999 we use the bottom and next 10% of the hourly wage distribution to track changes over time.

control group not captured by the time and treatment dummies, equation (2) is augmented with a set of additional control variables that includes both individual and job characteristics. The sensitivity of the DID estimates to variations in controls is explored below. Moreover, since we have a panel, the DID approach will net out any unobservable individual fixed effects.

The size of the treatment effect varies across individuals for several reasons. First, those furthest below the minimum received a larger increase than those closer to the minimum. It seems important therefore to try and test for this possible differential effect in what follows by using distance from the minimum, rather than a simple dummy variable as the central variable of concern. We test for this possible effect by replacing the treatment dummy with a) a variable that captures the distance of the hourly wage, *hw*, away from the hourly minimum in the base year

Hourgap = 3.60-hw if hw<£3.60

= 0 otherwise

and b) a variable that captures the potential weekly income gain resulting from the NMW

Incgap = (3.60-hw)*actual hours if hw<£3.60

= 0 otherwise

To the extent that hours worked in the main job vary then the effect of these two variables need not be the same.

Those individuals with two jobs could, in theory, have received two treatments, since the NMW applies to both jobs.[7] We therefore allow the estimates to be sensitive to

[7] The data show that 30% of those with a main job below the NMW also had a second job initially below the NMW, though this is less than 2% of the sample of low paid workers in what follows.

this concern and create treatment dummy variable and income gap variables for the second job in the same way as with the main job.

Incgap2 = (3.60-hw2)*actual hours2 if hw2<£3.60

= 0 otherwise

If Incgap>Incgap2 then we might expect work preferences to move toward the main job.

Finally, Working Families Tax Credit (WFTC) replaced Family Credit (FC) in Britain in October 1999: just after the introduction of the NMW. The tax credit schemes supplement incomes to households in low paid work with children, bringing their income up to a guaranteed minimum threshold. WFTC is more generous in both the level of minimum guaranteed income and the rate of supplement withdrawal, but any household in receipt of FC (WFTC) would have received less benefit from the introduction, or uprating, of the NMW, since state benefits would have been reduced at the rate of 70 (55) pence for each £1 increase in household income. Given the presence of in-work benefits, eligible households could be less likely to take a second job, since much of the additional income would be offset by reduced welfare payments. This suggests the need to try to distinguish between households in receipt of WFTC in what follows.

3. Data

The LFS is a quarterly survey of around 60,000 households that extracts detailed information on individual characteristics and labour market status. Each LFS asks those in employment whether during the survey reference week they held an additional paid job other than that classified as the main job and, if so, whether this was because the individual had changed jobs during the reference week. An affirmative answer to the first question and a negative answer to the second identifies a second job holder in the analysis that follows.[8] There is no information on job tenure in the second job to allow us to distinguish between long and short-term second job holders.

In order to facilitate the difference-in-differences estimation we use the longitudinal element of the LFS to match individuals over a period of one year. We present aggregate estimates and separate estimates for women, the sample of low paid men being rather small. The analysis is confined to those subject to the adult NMW, namely those aged 22 and below retirement age.[9] The sample is also restricted to those in the treatment group and those in the control group whose wages lie just above the NMW in the main job, though we test the sensitivity of our estimates to the definition of these groups below.

Individuals in the LFS are followed for 5 successive quarters within a rolling panel framework. Since the spring of 1997, wage information has been ascertained on the 1st and 5th waves of the interview process. The wage response from the 1st wave allows us to construct treatment and control groups. To assess the initial impact of the NMW, we take all those in 1998 earning below the initial national minimum wage of £3.60 as the treatment group (some 9% of the sample of employees in 1998), and take those earning just above the minimum, between £3.60 and £4.20 – around 10% of all employees – for the control. The labour market experience of this group is assumed to be

[8] The definition of main job is not specified in the LFS questionnaire, but it is clear from the survey responses that it is the job where the individual spends most time. Just 5% of second job holders worked longer hours than in the main job during the reference week. Of these one third had higher weekly wages in the main job.

[9] With a different (lower) youth NMW we might expect differential behaviour between youths and adults, though the small youth sample in our data precludes further investigation of this issue here. Note the focus on adults aged 22+ excludes most students from the analysis.

closest to that of the treatment group. The LFS does not collect income information for the self-employed.

One disadvantage that surrounds the LFS is that the hourly wage has to be derived for all employees before March 1999 and for all salaried employees after this date. This generates a degree of measurement error because the variable is used to define membership of the treatment and control groups for this study and any measurement error in dummy variables will generate attenuation bias (Aigner (1973)). Aigner proposes a method of assessing the extent of any attenuation bias, showing that, in a simple regression, $p\lim(\hat{b}_{OLS}) = b(1-n-h)$ where n is the sample proportion classified as below the minimum who in truth are above it and *?* is the sample proportion classified as above the NMW when in truth they are below it. While this expression does not hold in multiple regressions or when the dependent variable is binary, we use it to provide a benchmark upper bound estimate of the effect of measurement error in the results which follow. This is feasible for those individuals in our sample for whom we have information on both the actual hourly wage and the derived hourly wage.[10]

Stewart (2004a) also argues that for the low paid, an hourly wage based on actual rather than usual hours is likely to be more accurate. For anyone paid weekly, and the low paid are relatively more likely to be, the weekly wage data and actual hours correspond to the same reference period. Moreover, the only hours information for the second job is total actual hours including overtime (paid or unpaid). We therefore

[10] Stewart (2004a) applies Aigner's solution to the problem on data similar to ours in his analysis of employment transitions and finds the correction makes a small difference of 0.3 percentage points to the magnitude of his (insignificant) employment effect of the NMW estimates.

construct the hourly wage variable in the main job by dividing actual weekly pay derived from the last pay packet received by actual paid hours worked in the reference week.[11]

Since we do have information on usual as well as actual hours worked for the main job, we test the sensitivity of the results to variations in the definition of the hourly wage in the main job in what follows. Figure A1 in the appendix does suggest however that the pattern of change in the hourly wage variables is consistent with what one would expect given the introduction of the NMW. The estimates from kernel regressions indicate that the percentage rise in wages, for job stayers, was indeed much larger for those initially below the NMW than for those above. Those furthest below the NMW also received the largest increase.[12]

Around 9% of those with second jobs report working no hours in the second job during the reference week, but do report a weekly wage for the second job. This is because, as with the main job, the LFS obtains information on earnings relating to the last time an individual was paid and not specific to the reference week of the survey. We cannot therefore compute an hourly wage for this group, but retain the group in the data set to facilitate robustness tests. We also remove outliers from the second job wage data - those earning below £1 an hour and those earning above £1000 an hour from the data, less than 1% of the sample. All results should be interpreted accordingly. The LFS identifies individuals in receipt of any "family related benefits" in addition to child benefit. We use this variable to separate individuals into those likely to be receiving in-

[11] Dickens and Manning (2003) argue that both these derived hourly pay measures suffer from measurement error and advocate the use of a propensity score weighting method based on an actual hourly pay variable. Given the absence of such a variable prior to 1999, we cannot use this technique here.

[12] The kernel regressions use Stata's Nadaraya-Watson nonparametric regression command. This draws the graph of the estimated conditional mean over the grid points used for calculation. In practice, we exclude all those earning less then £2 an hour from the treatment group because of concerns over measurement

work benefits and those not in order to allow further variation in the size of the treatment. In total, we are left with around 1700 individuals initially below the NMW, of which 75% are female with similar numbers in the control group.

In order to try and identify differential responses to the NMW from those constrained by hours in the main job and those not, we make use of LFS questions available in every wave to construct identifying tags for both groups. The LFS asks employees if they ever work over time, whether their pay varies and if so why. We define an hours constrained worker as someone who does not work overtime or whose pay does not vary because hours worked vary. Everyone else is classified as an unconstrained hours worker. Using this definition, around two thirds of the low paid sample used in this study are classified as hours constrained.[13]

Our main focus is on the period 1998/99, since the introduction of the NMW led to larger absolute and relative increases for the lowest paid workers than in any subsequent period when the NMW was uprated and hence we might expect to observe the greatest response. However, because NMW upratings occur sporadically and are not uniform, there are two other periods when NMW workers received relatively large increases in pay: 2001 when the NMW rose by 11% and 2003 when the NMW was increased by 7%, (Low Pay Commission (2005)). We therefore construct similar panels using identical methods to those outlined above for the years 2000/01 and 2002/03 to look for an NMW effect on second job holding in these periods.

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error. Some 5% of the sample are excluded in this way. The results do not change appreciably if those below £2 are included, though the standard errors of our estimates are higher.

[13] Averett (2001) estimates a similar fraction, based on different questions, in her US sample of all second job holders and shows that the hours constrained incidence of moonlighting falls with income.

4. Second Jobs and Low Pay

The number of workers holding more than one job rose steadily from the late seventies until the mid-1990s and fell back thereafter, four years before the NMW was introduced.[14] Around 4% of employees currently admit to having a second job, more than double the share observed in the late seventies, (Table 1). There are around twice as many women with a second job than men.[15] Notably, there are around twice as many low paid workers with a second job than higher up the pay distribution and of all those low paid, twice as many low paid women have two jobs than low paid men.

There are studies around documentating the characteristics of those who have two jobs, (Kimmel and Smith-Conway (2001), Boheim and Taylor (2004), but few that focus on the characteristics of those who take second jobs among the low paid. The majority of low paid second job holders work in the education, health, retail and finance sectors, (Table 2). Education and health are particularly over-represented in supplying more second job workers. Just four occupations, cleaning, childcare, other care workers and sales assistants account for around half of all low paid workers with second jobs. Other occupations account for second job holding further up the pay distribution. Table 2 therefore confirms the importance of including industry and occupation controls in the regressions that follow, in order to account for any job specific characteristics of these sectors other than the wage that might affect second job holding.

This difference in industry and occupations across multiple and second job holders is mirrored in the distribution of hours and wages in second jobs, (Table 3).

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[14] We are unable to examine the causes of the turnaround in 1995 since the LFS only contained wage information in the last interview wave (rather than, as present, the first and last waves) before 1997.
[15] This pattern differs from the United States and Canada, where the shares by gender are broadly the same, (Kimmel and Powell 1999). In Germany, men hold more second jobs, Heineck and Schwarze (2004).

Average (mean) usual hours worked by low paid second job holders in the main job are some 20% lower than the average usually worked by single job holders. The mean second job lasts around 9 hours a week. Fifty percent of all second jobs are worked for 8 hours or less and 75% of all second jobs last less than 12 hours. Hence, average total hours usually worked by second job holders is around 2 hours more than that worked by single job holders. The distribution of total hours worked for low paid second job holders, (not shown), is noticeably flatter than that for those with one job only, again consistent with the idea that hours flexibility can be achieved mainly across rather than within jobs.

Hourly wages of second job holders are similar in both primary and second job to average hourly wages of single job holders.[16] Average hourly wages in second jobs are higher than hourly wages in the main job of second job holders. The higher mean conceals the fact that, in 1998, 15% of all main jobs of two job holders paid below the minimum wage of £3.60 compared to 26% of second jobs. Despite higher hourly wages in second jobs, the low number of hours worked implies that the average second job provides a gross weekly income of just one-quarter to one-third of the average gross weekly income from the first job.[17]

5. Impact of the NMW on Second Jobs & Hours

If the NMW were to have an effect on the aggregate share of second job holders, we would expect it to have the greatest influence on low paid men and women who were paid below the NMW before its introduction. Table 4 reports the results of more formal tests for a minimum wage effect, giving the difference-in-differences marginal effect

[16] One-quarter of the second jobs of employees are classified as self-employed. While there may be a differential response from this sub-group of second job holders, the sample sizes, when disaggregated as in Table 4 are not large enough to pursue this issue further.

[17] Presumably most second jobs do not offer enough hours to allow workers to switch.

estimates from a probit on the likelihood of having a second job. For 1998/99, the treatment dummy is insignificantly different from zero, suggesting little difference in second job holding between treatment and control before the NMW was introduced. The year dummy is negative but insignificant, and the difference in difference estimate is positive but also statistically insignificant. If anything, taken together these point estimates suggest that the incidence of second job working fell among the control group, and remained static among those initially below the minimum wage. The addition of individual and job controls does not change this basic conclusion.[18]

When the sample is split into those hours constrained in the main job and those not, the differential change between treatment and control group is much more apparent for the hours constrained sample than for the unconstrained hours sample. Again the results suggest that, if anything, second job working rose relatively but remained static in absolute terms among the treatment group constrained by hours. The results for women follow similar patterns. When we repeat the exercise for the other years in which there was a substantial uprating of the NMW, there is again very little strong evidence that second job holding among those affected by the uprating changed relative to others.

Robustness Checks

Table 5 repeats the exercise for different sub-samples and for variations to the definition of control and treatment groups. The specifications are reported only with a full set of control variables. We use: income gaps rather than simple treatment dummy variables (rows 1 and 2); change the window of observation to try to avoid any effects from those whose wages may have been adjusted up to NMW in months before its introduction

[18] Using Aigner's measurement error correction, the upper bound of the true coefficient based on the simple estimate is 0.9/(1-.30-.17)=1.7.

(rows 4 & 5); vary the size of the control group to make greater distinction from the treatment group which could be blurred because of measurement error or spill-over effects (rows 6 to 8) and those not in receipt of in-work welfare payments (rows 9).[19] In most cases the difference-in-difference estimate is close to the aggregate estimates reported in the appropriate columns in Table 4, that is, small and positive but generally statistically insignificant.[20]

Effect on Hours Worked

Given the lack of evidence of any change in the incidence of second job working, we next examine whether there is any evidence that hours worked in either the second or the main job changed over our time period and whether there is any evidence that hours worked changed more for those affected by the NMW. We take the sub-set of all those with a second job before and after the NMW and run difference-in-difference estimation using number of hours worked. The first treatment year interaction variable highlighted in Table 6 is consistent with Stewart and Swaffield's earlier work. Those whose wage in the main job was initially below the NMW have experienced a fall of around 1 hour worked in the main job relative to others. For those with two jobs, row 5 confirms the suggestion of Table 3 that those with two jobs work less hours in the main job, just under 5 hours less other things equal according to Table 6. The second interaction term highlighted, row 7, suggests no differential hours changes for the subset of the treatment group who also held a second job. Further, there was no significant effect for those whose hourly wage in the second job fell below the NMW, row 9. The direction of these effects are broadly

[19] Many of these robustness checks are also used by Stewart (2004a) in his analysis of the NMW and employment transitions.

similar for the three sub-groups outlined in columns 2 to 4, but the differential hours response for second job holders is most marked for those deemed to be unconstrained by hours in the main job.[21] So it seems that those with two jobs and being paid below the NMW appear to have increased their hours, while others below the NMW appear to have experienced a fall in hours.

The results of the same exercise when hours in the second job are used as the dependent variable are given in Table 7. The sample sizes here are rather small since the sample is restricted to second job holders who remained with the same employer over the year, but there is little evidence of any NMW effect from the main job on hours worked in the second job, (row 4). However the estimates on the difference-in-difference term for the second job, (row 6), suggest that those whose second job was below the NMW may have experienced a fall in hours, particularly among those hours constrained in the main job.

6. Conclusion

Many second job holders received a relatively large increase in their hourly and weekly wages following the introduction of the NMW. However, using difference-in-differences estimation on a panel of individuals matched across successive Labour Force Surveys, we do not find much evidence of a reduction in the proportion of second job holders following the introduction of a mandatory minimum. While hours of work may have fallen in second jobs for those whose second job was initially below the NMW, hours of work in the main job have, if anything risen for those affected by the NMW.

[20] Table A1 repeats this exercise for the other sample years. There are also propsensity score matching estimates available from the authors on request, which are in line with the estimates in Tables 4 and 5. The "common support" encompasses all propensity scores for the control group without the need to trim data.
[21] We do not use the income gap variables as alternative right hand side covariates in Table 7 because of concerns over endogeneity. For those hours constrained, hours in the main job will be exogenous, but for the unconstrained it may be that there is simultaneity of the hours choice in the main and second jobs. In the absence of a good instrument, this issue is left to further work.

Why do we observe such patterns in the data? Table A2 in the appendix reports the results of probit estimates of the probability that the second job will end, conditional on the weekly wage in both the main and second job alongside the income gap variable used in Table 5 above. Just under 50% of all those with a second job in 1998 no longer had a second job one year later.[22] It is apparent from Table A2 that the lower the weekly pay that an individual receives, the more likely they are to keep a second job. Moreover the larger the individual's weekly income gap, the less likely they were to stop working in a second job. It may be then that low paid individuals require a sufficient weekly income threshold in order to induce them to stop working a second job and that the increase in weekly income generated by the NMW was insufficient to put weekly income above such a threshold for most individuals.

[22] Just 0.9% of those with only one job were not working one year later and 0.5% of those with a second job.

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Table 1. Share of Employees with Second Jobs, 1979 – 2004

	1979	*1990*	*1995*	*1998*	*1999*	*2000*	*2001*	*2002*	*2003*
All									
Total	1.8	4.1	4.8	4.3	4.3	3.9	3.8	3.8	3.6
Below NMW				7.8	7.8	6.5	5.8	7.1	5.3
Near NMW				7.8	7.1	6.7	6.1	5.3	4.7
Men									
Total	1.7	3.3	3.4	3.2	3.2	3.0	2.7	2.7	2.5
Below NMW				4.3	4.5	3.7	4.1	3.2	1.9
Near NMW				6.2	5.2	5.3	4.6	2.4	1.9
Women									
Total	1.8	5.0	6.3	5.4	5.5	5.0	5.0	4.9	4.3
Below NMW				8.8	8.8	7.3	6.3	8.1	6.1
Near NMW				8.5	7.8	7.1	6.6	6.3	5.7

Source: LFS. Standard errors of total proportions around 0.1. Below and Near proportions based on matched sample. Standard errors of these proportions are around 0.8.

Table 2. Occupation, Industry, Second Jobs and the NMW (1998)

	% in each industry/occupation					
	Treatment		Control		Others	
	1st Job	1 job only	1st Job	1 job only	1st Job	1 job only
Industry						
Retail	19.3	25.3	26.3	28.2	9.3	11.4
Health	24.7	14.8	11.7	13.0	20.7	12.1
Hotels/Restaurants	10.8	15.3	6.1	9.3	1.3	1.5
Finance	10.2	9.6	7.8	7.7	11.0	15.2
Manufacturing	8.4	12.1	8.4	15.1	10.6	22.3
Other	26.5	22.9	39.7	26.7	47.1	37.5
Occupation						
Cleaners	19.9	10.5	15.1	8.1	2.9	1.0
Sales assistants	13.9	17.6	15.7	17.1	1.5	2.5
Care assistants	10.2	6.1	6.2	4.6	3.4	1.5
Childcare	7.2	2.6	5.6	2.6	0.9	0.3
Nurses	1.2	0.3	1.1	0.3	4.6	2.9

Note: authors calculations based on LFS matched panel. Standard errors around 3.0 for 2nd job holders in both treatment and control groups and around 1.0 for 2nd job holders in others group. For those with 1 job only standard errors are around 0.4 in treatment and control groups and 0.04 in others group.

Table 3. Hours, Wages, Second Jobs and the NMW

	Treated			Control		
	1st Job	2nd Job	1 job only	1st Job	2nd Job	1 job only
1998						
Hours	23.0	8.7	28.7	23.0	9.1	30.1
	(14.3)	(6.5)	(15.9)	(12.9)	(8.9)	(14.7)
Hourly Wage	3.20	4.8	3.17	3.95	5.0	4.00
	(0.30)	(4.2)	(0.30)	(0.18)	(3.5)	(0.19)
Weekly Wage	79.7	41.0	91.7	92.1	38.1	121.4
	(84.4)	(30.5)	(51.4)	(53.1)	(21.4)	(60.4)
Household	359.4		299.5	370.7		333.9
wage	(301.8)		(254.2)	(237.7)		(241.6)
2000						
Hours	20.5	11.1	27.2	23.9	9.5	29.3
	(11.0)	(13.0)	(14.0)	(12.6)	(8.9)	(14.2)
Hourly Wage	3.82	7.0	3.83	4.44	6.40	4.40
	(0.15)	(11.4)	(0.15)	(0.17)	(8.07)	(0.18)
Weekly Wage	77.9	51.5	109.6	111.2	50.9	136.4
	(44.2)	(35.0)	(68.3)	(59.9)	(36.6)	(72.6)
Household	411.6		321.4	430.9		369.9
wage	(330.7)		(283.2)	(258.4)		(266.5)
2002						
Hours	19.8	8.1	26.0	19.5	10.0	28.4
	(11.0)	(13.0)	(13.7)	(11.5)	(14.3)	(14.0)
Hourly Wage	4.2	9.8	4.24	4.8	7.83	4.8
	(0.1)	(12.5)	(0.11)	(0.18)	(7.85)	(0.2)
Weekly Wage	85.1	46.4	113.7	103.8	62.4	141.6
	(47.6)	(40.4)	(62.7)	(76.2)	(66.7)	(76.6)
Household	329.8		349.0	512.5		378.1
wage	(311.9)		(298.5)	(480.7)		(288.4)

Note. 1. Source. Labour Force Survey matched panel.

Table 4. Difference-in-Difference Estimates: NMW Effect on 2nd Job Holding

	Total		*Constrained*		*Unconstrained*	
	No contrls	*Controls*	*No contrls*	*Controls*	*No contrls*	*Controls*
1998-1999						
All						
Below NMW	-0.002	-0.004	-0.005	-0.002	0.004	-0.007
	(0.009)	(0.008)	(0.011)	(0.010)	(0.015)	(0.014)
Year1999	-0.008	-0.009	-0.009	-0.010	-0.008	-0.008
	(0.006)	(0.006)	(0.011)	(0.008)	(0.016)	(0.010)
Below*Year1999	**0.009**	**0.008**	**0.012**	**0.011**	**0.001**	**0.001**
	(0.009)	**(0.009)**	**(0.017)**	**(0.011)**	**(0.022)**	**(0.014)**
Women						
Below NMW	-0.002	-0.006	-0.004	-0.007	-0.003	-0.006
	(0.009)	(0.009)	(0.012)	(0.011)	(0.016)	(0.013)
Year1999	-0.006	-0.005	-0.005	-0.005	-0.007	-0.008
	(0.006)	(0.006)	(0.012)	(0.007)	(0.014)	(0.009)
Below*Year1999	**-0.001**	**-0.001**	**-0.002**	**-0.001**	**0.001**	**0.003**
	(0.010)	**(0.009)**	**(0.017)**	**(0.011)**	**(0.023)**	**(0.015)**
2000/01						
All						
Below NMW	-0.008	-0.011	-0.008	-0.009	-0.014	-0.013
	(0.009)	(0.008)	(0.012)	(0.011)	(0.014)	(0.011)
Year2001	-0.010	-0.009	-0.008	-0.007	-0.012	-0.012
	(0.006)	(0.005)	(0.011)	(0.007)	(0.012)	(0.008)
Below*Year2001	**0.004**	**0.003**	**0.002**	**0.002**	**0.008**	**0.010**
	(0.010)	**(0.009)**	**(0.017)**	**(0.011)**	**(0.024)**	**(0.016)**
Women						
Below NMW	0.001	-0.000	-0.001	-0.000	0.004	-0.003
	(0.011)	(0.010)	(0.014)	(0.013)	(0.020)	(0.017)
Year2001	-0.006	-0.006	-0.008	-0.008	-0.001	-0.003
	(0.008)	(0.007)	(0.010)	(0.009)	(0.013)	(0.011)
Below*Year2001	**-0.004**	**-0.004**	**0.001**	**0.001**	**-0.017**	**-0.011**
	(0.011)	**(0.010)**	**(0.014)**	**(0.013)**	**(0.018)**	**(0.015)**
2002/03						
All						
Below NMW	0.017	0.011	0.021	0.011	0.009	0.004
	(0.010)	(0.008)	(0.013)	(0.010)	(0.016)	(0.009)
Year2003	-0.006	-0.007	-0.003	-0.004	-0.015	-0.010
	(0.006)	(0.006)	(0.011)	(0.007)	(0.015)	(0.006)
Below*Year2003	**-0.010**	**-0.009**	**-0.018**	**-0.015**	**0.012**	**0.005**
	(0.009)	**(0.007)**	**(0.015)**	**(0.009)**	**(0.027)**	**(0.012)**
Women						
Below NMW	0.018	0.016	0.024	0.017	0.002	0.004
	(0.013)	(0.012)	(0.016)	(0.013)	(0.020)	(0.012)
Year2003	-0.006	-0.008	-0.001	-0.003	-0.023	-0.015
	(0.008)	(0.008)	(0.010)	(0.009)	(0.017)	(0.011)

Below*Year2003	**-0.013**	**-0.012**	**-0.025**	**-0.023**	**0.034**	**0.022**
	(0.012)	**(0.011)**	**(0.013)**	**(0.011)***	**(0.030)**	**(0.020)**

Note. 1. Standard errors in brackets adjusted for clustering across individuals. 2 * significant at 5% level. 3. Industry, marital status, education, ethnicity, temporary job status, size of firm, job tenure, 1 digit industry and occupation dummies, age of youngest child and number of dependent children also included in controls columns but results not reported. 4. Dependent Variable: probability of having a second job in the reference week. 6. Sample: adults 22+ in work both periods. 7. Sample sizes in 1998: 3448, 2393, 1055 (All); 2627, 1903, 713 (Women); 2000: 2563, 1756, 766 (all); 1970, 1397, 563 (women). 2002: 2214, 1583, 573 (all); 1629, 1234, 395 (women).

Table 5. Robustness Checks of NMW Effect 1998/99

	All			*Women*		
Condition	*Total*	*Constrained*	*Unconstrained*	*Total*	*Constrained*	*Unconstrained*
Hourly wage distance from NMW	0.001 (0.001)	0.023 (0.013)	-0.004 (0.019)	0.006 (0.012)	0.011 (0.015)	-0.006 (0.022)
Weekly wage distance from NMW	0.006 (0.012)	0.001 (0.001)	-0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	-0.001 (0.001)
<=6 months before	0.006 (0.011)	0.009 (0.014)	0.001 (0.020)	0.007 (0.014)	0.001 (0.015)	0.022 (0.029)
<=9 months before	0.005 (0.011)	0.008 (0.012)	0.001 (0.020)	0.002 (0.013)	-0.002 (0.014)	0.013 (0.026)
Control Group II	0.010 (0.009)	0.015 (0.011)	-0.001 (0.014)	0.012 (0.010)	0.012 (0.013)	0.010 (0.019)
Treatment Group II	0.010 (0.009)	0.013 (0.011)	0.004 (0.016)	0.012 (0.011)	0.011 (0.014)	0.013 (0.019)
Control Group II & Treatment Group II	0.012 (0.009)	0.017 (0.012)	0.001 (0.014)	0.015 (0.011)	0.011 (0.014)	0.015 (0.020)
Using usual hours to derive wage	0.011 (0.008)	0.010 (0.010)	0.015 (0.014)	0.012 (0.010)	0.007 (0.011)	0.024 (0.018)
Not receiving in-work benefits	0.004 (0.009)	0.004 (0.011)	0.006 (0.015)	0.004 (0.010)	-0.001 (0.012)	0.014 (0.019)

Notes. See Table 4. Reported coefficients are the estimates for the interaction of treatment and second sample year dummy variables for each sample.

Table 6. Difference-in-Difference Estimates: Hours Worked in Main Job (Low Paid)

	All	*Women*	*Constrained*	*Unconstrained*
Constant	40.537	34.064	35.396	40.953
	(3.536)*	(2.640)*	(2.706)*	(4.294)*
Below NMW	-0.209	-0.323	0.065	-0.773
	(0.489)	(0.510)	(0.571)	(0.879)
Year1999	-0.551	0.048	0.130	-2.029
	(0.216)*	(0.222)	(0.223)	(0.479)*
Below*1999	**-1.298**	**-1.125**	**-1.133**	**-1.716**
	(0.315)*	**(0.322)***	**(0.345)***	**(0.651)***
Second Job Holder	-4.744	-3.855	-4.227	-3.897
	(1.039)*	(1.066)*	(1.212)*	(1.732)*
Below*2 Jobs	-0.481	-1.498	-0.786	-2.322
	(1.482)	(1.480)	(1.645)	(2.772)
Below*2 Jobs*1999	**2.311**	**1.683**	**1.102**	**5.246**
	(0.823)*	**(0.818)***	**(0.804)**	**(1.946)***
2^{nd} Job Below NMW	-0.837	-1.951	-2.032	2.058
	(1.648)	(1.558)	(1.705)	(3.397)
2^{nd} Job Below*1999	**0.153**	**0.859**	**0.771**	**-2.112**
	(0.930)	**(0.929)**	**(0.992)**	**(2.101)**

Notes: See Table 4. All results net of controls used in Table 4. Sample restricted to treatment and control groups with same employer in both periods and positive hours of work. Sample sizes: 2897 (All), 2234 (women), 2020 (constrained), 877 (unconstrained).

Table 7. Difference-in-Difference Estimates: Hours Worked in 2^{nd} Job 1998:1999

	All	*Women*	*Constrained*	*Unconstrained*
Constant	10.392	17.058	6.505	9.497
	(9.498)	(8.992)	(8.982)	(31.427)
Below NMW	0.574	0.934	0.799	0.670
	(1.163)	(1.055)	(1.214)	(2.752)
Year1999	0.639	0.900	0.798	0.416
	(0.859)	(0.842)	(0.776)	(3.056)
Below*1999	**0.560**	**0.318**	**0.367**	**-0.453**
	(1.198)	**(1.270)**	**(1.358)**	**(2.956)**
2^{nd} Job Below NMW	6.877	6.931	6.582	3.890
	(1.478)*	(1.627)*	(1.749)*	(3.442)
2^{nd} Job Below*1999	**-2.985**	**-2.910**	**-3.656**	**0.352**
	(1.564)#	**(1.711)#**	**(1.734)***	**(4.649)**

Notes. See Table 4. All results net of controls used in Table 4. Sample: adults 22+ with second jobs in both periods and with same employer in main job. Sample sizes 119, 106, 90 and 29. # significant at 10% level.

Appendix Table A1. Robustness Checks of NMW Effect 2000/01 & 2002/03

	All			*Women*		
	Total	*Constrained*	*Unconstrained*	*Total*	*Constrained*	*Unconstrained*
2000/01						
Hourly wage	-0.024	-0.024	-0.019	-0.024	-0.014	-0.047
distance from NMW	(0.020)	(0.023)	(0.036)	(0.025)	(0.028)	(0.046)
Weekly wage	-0.001	-0.001	-0.001	-0.001	0.001	-0.002
distance from NMW	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
<=6 months before	-0.002	-0.004	0.010	-0.010	-0.002	-0.021
	(0.012)	(0.014)	(0.023)	(0.013)	(0.016)	(0.020)
<=9 months before	-0.003	-0.011	0.029	-0.003	-0.004	0.012
	(0.011)	(0.013)	(0.026)	(0.014)	(0.016)	(0.026)
Control Group II	-0.013	-0.011	-0.013	-0.015	-0.006	-0.025
	(0.008)	(0.012)	(0.012)	(0.010)	(0.013)	(0.012)
Treatment Group II	0.003	-0.003	-0.007	-0.007	-0.007	-0.015
	(0.009)	(0.011)	(0.012)	(0.010)	(0.014)	(0.015)
Control Group II &	-0.014	-0.012	-0.014	-0.017	-0.008	-0.027
Treatment Group II	(0.009)	(0.010)	(0.012)	(0.010)	(0.014)	(0.012)*
Using usual hours	-0.010	-0.010	-0.009	-0.011	-0.004	-0.022
	(0.008)	(0.011)	(0.013)	(0.010)	(0.014)	(0.014)
Not receiving in-	-0.002	-0.006	0.009	-0.005	-0.003	0.001
work benefits	(0.009)	(0.011)	(0.017)	(0.011)	(0.014)	(0.019)
2002/03						
Hourly wage	-0.009	-0.040	0.040	-0.014	-0.059	0.062
distance from NMW	(0.021)	(0.027)	(0.024)	(0.029)	(0.037)	(0.035)*
Weekly wage	0.001	-0.001	0.001	0.001	-0.001	0.002
distance from NMW	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)*
<=6 months before	0.011	0.007	0.013	0.018	0.007	0.022
	(0.012)	(0.014)	(0.014)	(0.017)	(0.018)	(0.021)
<=9 months before	0.011	0.001	0.027	0.012	-0.003	0.030
	(0.012)	(0.012)	(0.020)	(0.015)	(0.016)	(0.024)
Control Group II	-0.010	-0.016	0.008	-0.008	-0.020	0.039
	(0.008)	(0.008)	(0.014)	(0.010)	(0.011)	(0.026)
Treatment Group II	-0.007	-0.015	0.024	-0.008	-0.018	0.036
	(0.008)	(0.009)	(0.021)	(0.012)	(0.013)	(0.025)
Control Group II &	-0.008	-0.017	0.024	-0.006	-0.018	0.060
Treatment Group II	(0.008)	(0.009)	(0.019)	(0.011)	(0.013)	(0.034)
Using usual hours	-0.006	-0.012	0.007	-0.007	-0.017	0.022
	(0.008)	(0.009)	(0.011)	(0.011)	(0.012)	(0.019)
Not receiving in-	-0.009	-0.002	0.011	0.004	-0.008	0.028
work benefits	(0.009)	(0.011)	(0.014)	(0.014)	(0.014)	(0.024)

Table A2. Probit Estimates of Likelihood of Stopping 2nd (Marginal Effects)

	All	Women	Constrained	Unconstrained
Variable				
1998/99				
Weekly Pay Main Job	0.0040	0.0033	0.0041	0.0037
at time t-1	(0.0010)*	(0.0011)*	(0.0013)*	(0.0026)
Weekly Pay 2nd Job at	-0.0010	-0.0008	-0.0006	-0.0051
time t-1	(0.0013)	(0.0015)	(0.0020)	(0.0022)*
Weekly wage distance	-0.0069	-0.0081	-0.0100	0.0054
from NMW at time t-1	(0.0034)*	(0.0037)*	(0.0044)*	(0.0072)

Note: sample is those in treatment or control group with a second job in base year and who were employed in both periods. Sample sizes are 202, 171, 150, 52. Sample means of dependent variables are 0.48, 0.46, 0.48, respectively. Independent variables are gross weekly wage in £. Regressions also include controls for personal and job characteristics as in Table 4.

Figure A1. Changes in Hourly & Weekly Wages After Minimum Wage: Job Stayers (Kernel regression estimates)