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Living wages and age discontinuities for low-wage workers

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Abstract

This paper considers an emerging, highly policy relevant feature of minimum wages, studying what happens when a wage floor significantly higher than a nationally legislated minimum is imposed. The consequences of age-wage discontinuities and wage floors higher than mandated minimum wages are explored in the context of a Living Wage being introduced to a large UK organisation through time. Between 2011 and 2019, the Company was exposed to a Living Wage Rate higher than the statutory National Minimum Wage, which was sequentially introduced into some of its establishments and had the effect of boosting wages and strongly increasing the age-wage discontinuity from age-related pay grades. The analysis finds positive labour supply responses at the age discontinuity before Living Wage treatment, but a fall in hours at the discontinuity following treatment. The Living Wage raised wage costs but did not affect aggregate hours, showing a within-establishment reallocation of hours by age arising from differential eligibility to be paid the Living Wage.

Key words: living wages, minimum wages, wages, hours

JEL: J31; J38; J42

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1. Introduction

Study of minimum wages, and their economic effects, has once again become a common preoccupation of researchers. There are a number of reasons why. Many countries are experiencing low real wage growth and a minimum wage policy is one that can directly boost the wages of low wage workers. Viewed through this lens, minimum wages have become the policy tool of choice as some places – like US and UK cities – have decided to raise local minimum wages above the presiding national or state legislated minimum wage. Some companies (for example, Amazon, IKEA, Wal-Mart) have raised their own lowest wage above the mandated minimum. And in the recent past some countries, most notably Germany in 2015, that did not previously have a national minimum wage have introduced one at a relatively high level.

These features of minimum wages have resulted in a sizable upsurge of recent research which has broadened the remit in this area. A clear focus has been placed on studying newer questions relative to the very sizeable past research literature that placed a principal focus on studying the employment effects of minimum wages.¹ Because of the widespread finding from a range of studies that the employment effects tend to be modest, if they exist at all, more recent research looks at other forms of adjustment to the wage cost shock that minimum wages induce (see, for example, Draca, Machin and Van Reenen, 2011, Hirsch, Kaufman and Zelenska, 2015, Bell and Machin, 2018, and Harasztori and Linder, 2019), on changes in the composition of worker wages and employment (Giuliano, 2013, Dustmann, Lindner, Schönberg, Umkehrer

¹ The various phases of minimum wage research are summarised well in reviews by Brown, Gilroy and Kohen (1982) on the first generation of time series studies, by Card and Krueger (2004) on the second generation of so-called “revisionist” quasi-experimental research and by Brown (1999) on both. See also a review making closer links between minimum wage research and monopsony in the labour market by Manning (2021).

and Von Berge, 2020, and Giupponi and Machin, 2021) and on local minimum wages (Dube and Lindner, 2021).

This paper studies an emerging, highly policy relevant feature of minimum wages. It studies what happens when a significantly higher wage floor than a nationally legislated minimum is imposed. It is able to adopt a research design where some establishments of a firm progressively raised their minimum wage floor to a level higher than the prevailing national minimum wage. The setting is the UK, where the company studied was exposed to a Living Wage Rate higher than the statutory National Minimum Wage sequentially as it was introduced into some of its establishments in a staggered sequence between 2012 and 2019. The Company is an employer of a large number of low wage workers with over 300 establishments across the UK and operates in the service sector. While the Company's main competitors are firms operating in the private sector, a large part of the firm's business is government procurement contracts.

A new focus has emerged in recent years on living wages, wage rates which are required to meet minimum standards given the costs of living. In the UK this is exemplified by the Living Wage Foundation which calculates rates for London and the rest of the UK and has accredited over 7,000 employers. In the US MIT operate a living wage calculator for different states, cities and metro areas (Glasmeier, 2020) and hundreds of cities have passed living-wage ordinances (Dube and Lindner, 2021; Sosnaud, 2016).² This interest has emerged at least in part as a result of claims that national minimum wage levels (or federal and state minima in the US) have not been adequate to meet the cost of living (Iacurci, 2021). This paper presents a

² Many of these US city living wage ordinances are lower than the rate as recommended by the MIT Living Wage Calculator for the area and are more precisely *de facto* higher local minimum wages, not living wages.

detailed account of how firms respond to a “true” living wage, calculated according to a consumption basket of goods and services deemed necessary for an acceptable standard of living.³

The paper studies the impact of introducing the Living Wage Rate on wages and hours in The Company, leveraging two sources of credibly exogenous variation: a staggered mandated Living Wage treatment and a discontinuity in the age-wage profile. Due to age eligibility criteria for the Living Wage the interaction of these two effects results in differential discontinuities in the age-wage profile for treated and untreated establishments. Thus, the setting lends itself to an event study research design, a regression discontinuity design, and the interaction of these two which is referred to as a “difference-in-discontinuity” design. The analysis finds that the Living Wage raised wages but did not damage aggregate jobs and hours. It did, however, result in a reallocation of workers by age because of differential eligibility to be paid the Living Wage. In particular we find that younger workers around the age eligibility cut-off experience a loss of hours and, in some cases, earnings as the firm is able to substitute them with older workers, as a direct result of the Living Wage introduction.

2. The Experimental Setting

The Living Wage Foundation (LWF) is a charitable organisation in the UK, established in 2011, that campaigns for employers to pay workers a living wage. Each year the LWF calculates and publishes a Living Wage rate for London (LLW) and the rest of the UK (UKLW). The LLW rate has typically been approximately 30-35% higher than the official

³ For details of the methodology underpinning the calculation, and the history of the UK Living Wage, see D’Arcy and Finch (2019).

statutory National Minimum Wage (NMW) applicable to over 21s, or National Living Wage (NLW) applicable to over 25s, while the UKLW has been about 15-20% higher as can be seen in Figure A1 of the Appendix. However, unlike the statutory NMW, the LLW and UKLW comprise only of a single rate which is applicable to all workers over the age of 18, with 16 and 17 year olds in work not being covered by the Living Wage. The NMW on the other hand is currently comprised of 4 different age-specific rates: age 16-17, age 18–20, age 21-24 and age 25+.

Organisations voluntarily sign up to become Living Wage employers and following appropriate audits by the LWF can achieve accredited status. As of July 2020, the LWF lists 6,562 accredited employers and included in this list are 107 local government units.⁴ When public bodies achieve accreditation, they are given an amnesty on existing procurement contracts, but are expected to enforce the living wage at the start, renegotiation or renewal of contracts.

The Company operates in the service sector and the majority of their business is through procurement contracts with local councils.⁵ As the firm operates hundreds of establishments across the UK, different establishments become contractually obliged to pay the LLW and UKLW at different times. This is dependent on whether, and when, the local government unit has voluntarily signed up to the LWF's Living Wage, as well as idiosyncratic timings of contractual renewal or renegotiation.

⁴ These include London Boroughs, Unitary Authorities, Metropolitan Districts, County Councils, District Councils, Local Government Districts and Parish Councils.

⁵ Councils here refer to Principal Councils which are local government authorities carrying out statutory duties in England and Wales. They are responsible for a wide range of public services including transport, education, planning, and cultural services, and operate local tax collection. There are 355 principal councils in England and Wales and this includes 33 London boroughs.

Between 2012 and 2019, 107 local government units gained accreditation. For example, of the 32 London Boroughs, 17 have received accreditation, the earliest (Islington in North London) receiving accreditation in May 2012, and the most recent (Redbridge in the East of the city) receiving accreditation in November 2018.⁶ As Exhibit 1 shows, this setting gives substantial variation in Living Wage treatment for establishments run by The Company. In particular, over the period for which we have HR data approximately 140 establishments went from being untreated to treated, while run by The Company. The remainder never pay Living Wages and the relevant minimum wage floor to them is the UK National Minimum Wage.

This variation in Living Wage treatment is combined with the fact that The Company has an already existent pay structure which operates an age-related pay scale for their entry-level (unskilled) jobs.⁷ In particular, similar to the NMW, their pay-scale has a sizeable discontinuity at age 18.⁸ The Living Wage treatment, which only treats those over 18, therefore has the effect of increasing the size of this age-wage discontinuity. It is thus possible to implement a “Differences-in-Discontinuity” design, exploiting both an exogenous treatment to wages as well as a seemingly arbitrary discontinuity in wages as a function of age, where the treatment effects the size of the discontinuity.

The setting combined with the dataset allows a novel analysis of how a wage floor could affect highly exposed young workers employed on casual contracts (specifically on zero-hours contracts⁹), both in terms of wages and employment along the intensive margin. The presence

⁶ Correct as of July 2019.

⁷ This pay-scale is centrally determined, however establishments have independent control over both intensive and extensive margin employment, as well as employment composition.

⁸ Many companies have a sizeable discontinuity at this particular age, when individuals become adults in UK law. The NMW for example as of April 2020 implies a statutory rate of £4.55 for under 18s and a statutory rate of £6.45 for age 18-20.

⁹ For a complete description of the of what these types of contracts entail see Datta, Giupponi and Machin (2019).

of the discontinuity at age 18 would likely result in both supply and demand effects, where young workers would be willing to supply more labour just after the age 18 cut-off, and budget conscious managers would be less willing to give such workers as many hours. Furthermore, when an establishment is exposed to a higher wage floor it is likely that labour supply to the establishment across the whole age range would increase, potentially suppressing demand for younger workers.¹⁰ By comparing the size of the discontinuity in wages and hours before and after treatment, the analysis is able to disentangle which of these effects dominates, and thus assess the impact on young workers.

As all establishments are operated by the same company using the same structure of operations and management, but with establishment level autonomy over employment and workforce composition, a true counterfactual, when comparing treated and untreated establishments, can be estimated. Additionally, unlike in many minimum wage papers, the approach can isolate the impact of just the individual establishment being exposed to a higher wage floor, rather than the entire market. This is because when a local government unit voluntarily signs up to the LWF's Living Wage, private companies and non-council public employees in the area remain untreated.¹¹ Furthermore, the establishment located within the treated council would not be contractually obliged to the Living Wage until renewal or renegotiation, introducing further idiosyncrasies in when the establishment must raise wages. Thus, estimates are not likely to be contaminated by general equilibrium effects.

¹⁰ See Datta (2021) for evidence of this.

¹¹ The proportion of workers treated within a local authority is a small fraction of a percentage. Council employment only makes up approximately 3% of employment and typical council jobs are paid above the wage floor. For more information see Datta (2021).

3. Data and Empirical Method

Data

This study analyses a novel dataset which contains the complete HR data of The Company for the period of 2011–2019. As stated in the introduction, The Company operates in the service sector and a large portion of its turnover is from government procurement contracts, for local council services. It is worth noting that the council services they provide are not typical natural monopolies, and other private firms compete in the same local markets.

The HR data contains very detailed information on all workers in each of The Company's establishments. In addition to the usual information such as job tenure, wages, age and demographic characteristics, there is very detailed data on the specific job role that each worker carries out, the precise dates of wage changes, and exact hours worked from timesheet data.

Table A1 in the Appendix presents summary statistics for workers employed by The Company as of 2019. The Company is big, employing approximately 19,000 workers and has a large proportion of female workers (60%). Half of the workers are based in London and 5% of all workers are less than 18 years old, this proportion is considerably higher for casual entry-level workers (12%). The workforce is younger on average (36 years) than the national average (43 years), and this is more pronounced at 31 years old for entry-level workers. Three quarters of entry-level workers are on casual contracts which puts The Company in a very flexible position to adjust employment along the intensive margin.

The average hourly wage is £12.88 per hour, which is approximately 25% lower than the average hourly wage for the UK in 2019.¹² The average hourly wage for entry-level workers

¹² This stood at £17.27, as calculated from the Annual Survey of Hours and Earnings.

is £9.38, almost half of the UK mean and about 15% higher than the UK National Living Wage shown in Appendix Figure A1. The average worker works approximately only one quarter of full-time hours, casual entry-level workers only work on average about 5 hours per week. Given the large proportion of casual staff the summary statistics suggest that permanent employees work much closer to full time hours (the exact figure is 132 hours per month for the mean permanent employee). This is in line with the national average.¹³

Empirical Method 1 – Differences-in-Discontinuity

The first part of the empirical analysis leverages a “Differences-in-Discontinuity” research design, to estimate the effect of the key variables – the Living Wage, the age discontinuity at age 18 and the interaction of the two - on the wages and hours of entry-level casual workers. Age is normalised around 18 such that $age = age^* - 18$, where age^* is the true measure of age, the discontinuity (D) is defined $D = \mathbb{1}[age \geq 0]$, and treatment from Living Wage introduction ($T = 1$ when introduced), the wage equation for entry-level casual worker i employed at establishment e in year y and month m takes the form:

$$\begin{aligned} \ln(wage)_{ieym} = & \beta_1 D_{ieym} \times T_{ieym} + \beta_2 D_{ieym} + \alpha_1 T_{ieym} + \alpha_2 age_{ieym} \\ & + \alpha_3 age_{ieym} \times D_{ieym} + \alpha_4 age_{ieym} \times T_{ieym} \\ & + \alpha_5 age_{ieym} \times D_{ieym} \times T_{ieym} + \gamma_e + \lambda_{ym} + \xi_{ieym} \end{aligned} \quad (1)$$

This equation has the same format as a typical regression discontinuity design with a linear predictor for the running variable (age in months above/below 18) either side of the discontinuity. In addition, however, the specification allows for the pre 18 and post 18 slopes to differ based on whether the Living Wage treatment has been applied. Additionally, equation (1) includes both establishment (γ) and year-month fixed effects (λ), as is conventional in a difference-in-difference specification.

¹³ 140 hours per week, as calculated from the Annual Survey of Hours and Earnings.

The main parameters of interest are β_1 and β_2 . In the specification, β_2 identifies the impact of the age discontinuity in the pay-scale schedule on wages, while β_1 identifies the impact of the Living Wage treatment on the size of this wage discontinuity. If the Living Wage raises wage levels and generates an additional premium for reaching age 18, β_1 is expected to be positive and this is the first key hypothesis to be tested in the analysis.

After the first stage wage equation, the analysis then considers whether there is an intensive margin employment response to Living Wage introduction. This first stage of wages and second stage of labour adjustment, of course, is predicated on the same logical basis as the way the big literature on minimum wages and employment proceeds, except here we are looking at a wage floor higher than the mandated minimum. To investigate this second stage, the same structure specification (1) is adopted, except now with hours worked as the dependent variable:

$$\begin{aligned} \ln(\text{hours})_{ieym} = & \beta_3 D_{ieym} x T_{eym} + \beta_4 D_{ieym} + \alpha_6 T_{eym} + \alpha_7 age_{ieym} \\ & + \alpha_8 age_{ieym} x D_{ieym} + \alpha_9 Age_{ieym} x T_{eym} \\ & + \alpha_{10} Age_{ieym} x D_{ieym} x T_{ieym} + \gamma_e + \lambda_{ym} + \xi_{ieym} \end{aligned} \quad (2)$$

Now the two main parameters of interest are β_3 and β_4 . In (2), β_4 identifies the impact of the age discontinuity in the pay-scale schedule on hours, while β_3 identifies the impact of the Living Wage treatment on the size of the hours discontinuity. Equation (1) and equation (2) are estimated on both a wide window of 24 months either side of the age cut-off (i.e. on the sample of 16 to 20 year olds), and a smaller window of 12 months either side of the age cut-off (i.e. for 17 to 19 year olds).

As discussed in section 2, the impact of the increase on wages on hours worked is *ex ante* ambiguous. This is because an increase in wages should in theory have oppositely signed labour

supply and demand responses. In the case of $\beta_4 > 0$, this would suggest the labour supply response of the age-wage discontinuity dominating the labour demand response, and vice versa. Similarly, $\beta_3 < 0$ would suggest that the Living Wage is resulting in some intensive margin unemployment effects for those workers around the age 18 discontinuity. Furthermore, depending on the sign of β_3 , it is possible to place lower bounds on the labour supply elasticity, by dividing the respective parameter by its counterpart parameter from specification (1). For example, if $\beta_4 > 0$, then $\frac{\beta_4}{\beta_2}$ gives a lower bound on the labour supply elasticity for casual youth workers. This is because the labour demand response can be reasonably assumed to be weakly negative and β_4 would contain both supply and demand effects.

Empirical Method 2 – Event Study

The approach in Empirical Method 1 is informative for generating evidence about local impacts around the age 18 cut-off, thereby precisely showing how the Company is able to adjust to the LW introductions. To study how this local adjustment translates into an impact for the establishment as a whole, and therefore to ascertain whether the Living Wage has an impact on aggregate wages, employment and hours on the intensive margin, an event study estimator is implemented at the establishment level which treats the multiple treatment timing of the setting with due caution.

Borrowing notation from Sun and Abraham (2020), let Y_{eym} denote an outcome of interest for establishment e at year-month ym with treatment status $T_{eym} \in \{0,1\}$: $T_{eym} = 1$ if e is treated in period ym and $T_{eym} = 0$ otherwise, where treatment is absorbing, and therefore $T_{es} \leq T_{eym}$ for $s < ym$. An establishment's treatment path can therefore be characterised by $K_e = \min\{ym: T_{eym} = 1\}$, and $K_e = \infty$ if the establishment is never treated. Establishments

can therefore be categorized into disjoint cohorts $k \in \{ym_{min}, \dots, ym_{max}, \infty\}$, where establishments in cohort k are first treated at the same time $\{e: K_e = k\}$. Y_{eym}^k is the potential outcome in period ym when establishment e is first treated at time k and Y_{eym}^∞ is the potential outcome at time ym if establishment e never receives treatment. A cohort-specific average treatment effect on the treated l periods from treatment is thus:

$$CATT_{k,l} = E[Y_{e,k+l} - Y_{e,k+l}^\infty | K_e = k] \quad (3)$$

This notation allows for treatment effect heterogeneity across cohorts, which in this setting may be important as the bite of the living wage may change over time. The key estimate of interest is then some weighted average of (3), for $l \in g$, to construct a relative period coefficient. As is often the case when firms face a shock to the wage floor, the interest is in the average dynamic effects (which allows an analysis of the pre-trends).

For analysing these average dynamic effects, a weighted average similar to that proposed in Sun and Abraham (2020) is

$$v_g = \frac{1}{|g|} \sum_{l \in g} \sum_k CATT_{k,l} Pr\{K_e = k | K_e \in l\} \quad (4)$$

which effectively uses weights according to the size of the treated cohort that experiences l periods relative to treatment. In practice, (4) is estimated using the following methodology:

1. For each treatment cohort estimate an adjusted form of the typical, two-way fixed effect event study specification, limiting l to 12 months before and after the treatment period.

$$Y_{eym} = \gamma_e + \lambda_{ym} + \sum_{l \neq -1, -12} \delta_{k,l} T_{e,ym+l} + \beta' X_{eym} + \varepsilon_{eym} \quad (5)$$

where variables are the same as above, and X_{eym} is a set of time varying establishment level controls. For each treatment cohort k , the control group is restricted such that

they have not received treatment within the past two years or will not receive treatment within two years of the relevant treatment cohort treatment date. This is to ensure no overlap of dynamic effects between the treated and control groups. As per the suggestion of Borusyak and Jaravel (2017), the dynamic effects are normalised to two periods, -1 and -12, to deal with the underidentification issues they raise.

2. Estimate the weights $Pr\{K_e = k | K_e \in l\}$ by sample shares of each cohort in the relevant relative period l .
3. Combine steps 1 and 2, and aggregate monthly affects l , to the level of quarters, g , for graphical representation by taking a simple equal weighted mean. In particular

$$\hat{v}_g = \frac{1}{3} \sum_{l \in g} \sum_k \hat{\delta}_{k,l} Pr\{K_e = k | K_e \in l\} \quad (6)$$

There has been a recent surge in interest in the workings of difference-in-difference and event study estimators, especially when there is variation in treatment timing and heterogenous treatment effects (for example, see Callaway and Sant’Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2020; and Borusyak and Jaravel, 2017). Concerns raised include: issues identifying the linear component of the path of pre-trends in traditional event study specifications (Borusyak and Jaravel, 2017), contamination of lead and lag coefficients from other period effects (Sun and Abraham, 2020), biased estimates of treatment effects when the control group contains treated units when dynamic treatment effects are present (Goodman-Bacon, 2021) and the structure of weights assigned across treatment cohorts when estimating dynamic treatment effects (Sun and Abraham, 2020). The estimator used here is akin to that

suggested in Sun and Abraham (2020) and also implements adjustment as recommended in Borusyak and Jaravel (2017) in an attempt to overcome the aforementioned issues.¹⁴

Empirical Method - Robustness

The approach used in the main estimating equations (1) and (2) is more flexible than the standard two-way fixed effect estimator used in a typical difference-in-difference setting as it exploits variation in both Living Wage treatment and the discontinuity at age 18. Despite this, as a matter of caution we additionally implement a robustness check where we compare the Living Wage impacts on wages from the event study estimates using the Sun and Abraham (2020) style estimator with a traditional event study regression of the form:

$$\ln(Wage)_{eym} = \gamma_e + \lambda_{ym} + \sum_{l \neq -1, -12} \delta_l LW_{e,t+l} + \beta' X_{et} + \varepsilon_{et} \quad (7)$$

aggregated to quarterly effects according to

$$\hat{v}_g = \frac{1}{3} \sum_{l \in g} \hat{\delta}_l. \quad (8)$$

Assuming the estimates of \widehat{v}_g from equation (8) are similar to those from equation (6), which is robust to the aforementioned issues, this is suggestive that the main estimating equations (1) and (2) are unlikely to suffer from the above issues.

4. Results

This section presents the findings, beginning with the first and second stage wage and hours differences-in-discontinuity-based estimates, moving to consider aggregate establishment level impacts, and then offering an interpretation of the key results.

¹⁴ For a more complete discussion of some of the discussed issues and how the implemented event study estimator deals with these, see Datta and Machin (2021).

Wages

Exhibit 2(a) reports scatter plots of the mean log wage by months relative to age 18 for entry-level casual workers in establishments treated and untreated with the Living Wage, orthogonalized against time and year fixed effects. There is a clear discontinuity at age 18 in both cases, and either side of the discontinuity the wage to age correlation appears mostly flat. The size of the discontinuity for workers in treated centres (approximately 18 log points) is considerably larger than the discontinuity in untreated centres (approximately 12 log points).

Columns (1) and (2) of Exhibit 3 present results from the counterpart regressions to Exhibit 2(a), from estimating specification (1). The parameter estimates suggest in untreated establishments as an entry-level casual worker moves from age 17 to 18 they experience a 12% increase in wages. For those in treated establishments the increase in wages is considerably higher, at about 20%. Workers within the sample also experience on average 4-5% higher wages from Living Wage treatment. This figure is likely to be higher when considering all entry-level workers rather than those just around the age 18 cut-off, given the proportion receiving direct treatment would be larger. The estimates of the main parameters of interest for specification (1) do not fundamentally vary based on the width of the window considered around the age 18 cut-off and all are highly statistically significant.

Hours

Exhibit 2(b) is of the exact same form as Exhibit 2(a) for wages, but instead shows mean log hours by months relative to age 18. Unsurprisingly, the number of log hours worked features more noise than its wage counterpart. But the figure shows a positive discontinuity in log hours at age 18 for workers untreated with the Living Wage, and a structural difference in this discontinuity for those treated with the Living Wage.

Columns 3 and 4 of Exhibit 4 reports parameter estimates for specification (2) to confirm this. In particular, the estimates show that as casual entry-level workers move from age 17 to 18, they work 10-17% more hours. The positive sign and size of this parameter suggests a strong labour supply response from casual youth workers. In the presence of no demand effects, this would imply a labour supply elasticity of approximately 1.3 according to the specification using the smaller window.¹⁵

For those in treated establishments, the discontinuity in casual hours drops between 26-33 percentage points, indicative that the change in hours around the discontinuity becomes negative. According to the specification using the ± 12 month window, hours fall for workers as they move to the positive side of the discontinuity by approximately 10% in treated establishments. Both columns 3 and 4 therefore reveal a strong negative impact on intensive margin employment for casual youth workers just over the age 18 cut-off as a result of a higher wage floor. Parameter estimates for β_1 and β_3 imply that casual youth workers around the age 18 cut-off experience a 7% increase in wages due to the Living Wage treatment, but a fall in hours of 26-33%.¹⁶

Aggregate Effects

Exhibit 4 graphically reports the coefficients from (6) and examines the aggregate effects at the establishment level of the Living Wage on intensive margin employment. Column (1) in Table A2 in the appendix presents the counterpart point estimates and standard errors. As can

¹⁵ The elasticity when using the wider window is 0.8 which, even if a little smaller in magnitude, still very clearly supports the interpretation of a strong supply side response to the Living Wage.

¹⁶ Spatial heterogeneity analysis suggests that workers around the age 18 cut-off in London experienced a 10.3% increase in wages due to the Living Wage treatment (standard error 0.027), and a fall in hours of 36.5% (standard error 0.131). The parameter estimates of β_2 (0.098, standard error 0.007) and β_4 (0.126, standard error 0.068) for the 12-month window for those in London suggest a lower bound labour supply elasticity of 1.3, identical to the main specification.

be seen there is an absence of differing pre-trends suggesting that the common time trend assumption necessary in such settings is not violated. Following the Living Wage introduction there is no change in aggregate intensive margin employment.¹⁷ Figures A2 and A3 in the Appendix, present event study figures for log wages using parameter estimates from equations (6) (the robust estimator) and (8) (the traditional estimator) respectively. Columns (2)-(5) in Table A2 in the appendix present the counterpart point estimates and standard errors. The figures show a sharp, statistically significant rise in wages for all workers (approximately 1.5%) and entry-level workers (approximately 6.5%) which is roughly consistent throughout the year following treatment. The results between the two estimators are strikingly similar which suggests our main estimating equations are unlikely to be biased by the issues mentioned in section 3.

These results suggest that the Living Wage introduction acted to affect the way that hours are distributed across workers within the establishment, but not the total number of hours worked. In particular, hours are shifted away from casual workers who have just crossed the age 18 boundary where the Living Wage becomes binding. Those in treated centres crossing the boundary experience a 19% increase in wages, and a drop in hours of 10-22% depending on specification, suggesting the impact on earnings could be negative. Against the counterfactual of those in untreated centres, who experience a 12% increase in wages and an increase in hours of 10-16% the fall in earnings is even more pronounced.

Discussion

The above results can be rationalised and explained in a simple model of a firm with monopsony power in the labour market. This is illustrated in Exhibit 5. The coefficients on β_4

¹⁷ Impacts on aggregate employment are reported in Datta and Machin (2021) and similarly show no effects.

and β_2 imply a lower bound on the labour supply elasticity to the firm for 18 year olds of approximately 1.3. This is represented on the diagram by the movement from $E_{<18}$ to E_{18} under demand condition LD_{18} . When the establishment is required to pay the Living Wage, the wage for 18 years olds increases as well as the wages for all older workers in entry-level positions. Since the firm has monopsony power in the labour market this induces a positive labour supply response from older workers to the firm and the firm is able to substitute younger 18 year olds for older workers, thus reducing demand for 18 year olds. Therefore, employment for 18 year olds falls back to the original level due to the movement along and shift in of the labour demand curve. Additionally, as employment is unaffected at the establishment level, the fall from E_{18} to $E_{18 \times LW}$ must in turn have a 1:1 increase across older workers. Assuming a positive relationship between age and productivity, it becomes clear why a firm would substitute 18-year olds for older workers.

5. Conclusion

Living wages have increasingly been receiving attention in the news and in policy circles in both the US and UK. This has come about from concerns regarding the existing mandated minimum wages arguing that they are simply too low for families to live on (Glasmeier, 2015). Interest has been magnified by concerns about stagnating real wages. Recently we have seen companies in the UK such as IKEA pledging to pay a living wage. The landscape appears to be changing in this regard. In the UK The Living Wage is a policy tool being considered at a governmental level. The Welsh government, for example, recently set up the Social Care Fair Work Forum, where one of their positions is to ensure all care workers are paid the Living Wage.

This paper offers novel evidence on how firms respond to a living wage, specifically the Living Wage Foundation's Living Wage, which is considerably higher than the UK mandated minimum wage. Using a bespoke dataset for a large service sector firm with hundreds of establishments across the UK that are as good as randomly exposed to a Living Wage, this paper shows that the Living Wage had a strong impact on wages and no aggregate impact on total hours worked. However, utilising discontinuities in the age-wage profile for The Company which change in size as a result of exposure to the Living Wage, the analysis demonstrates that exposed establishments reallocate hours away from workers just over the age 18 cut-off. Because of this hours reallocation, the results suggest that workers just over the age 18 cut-off actually experience a loss of earnings as a result of the living wage introduction, and establishments are able to do this due to increased labour supply from older, possibly more productive, workers.

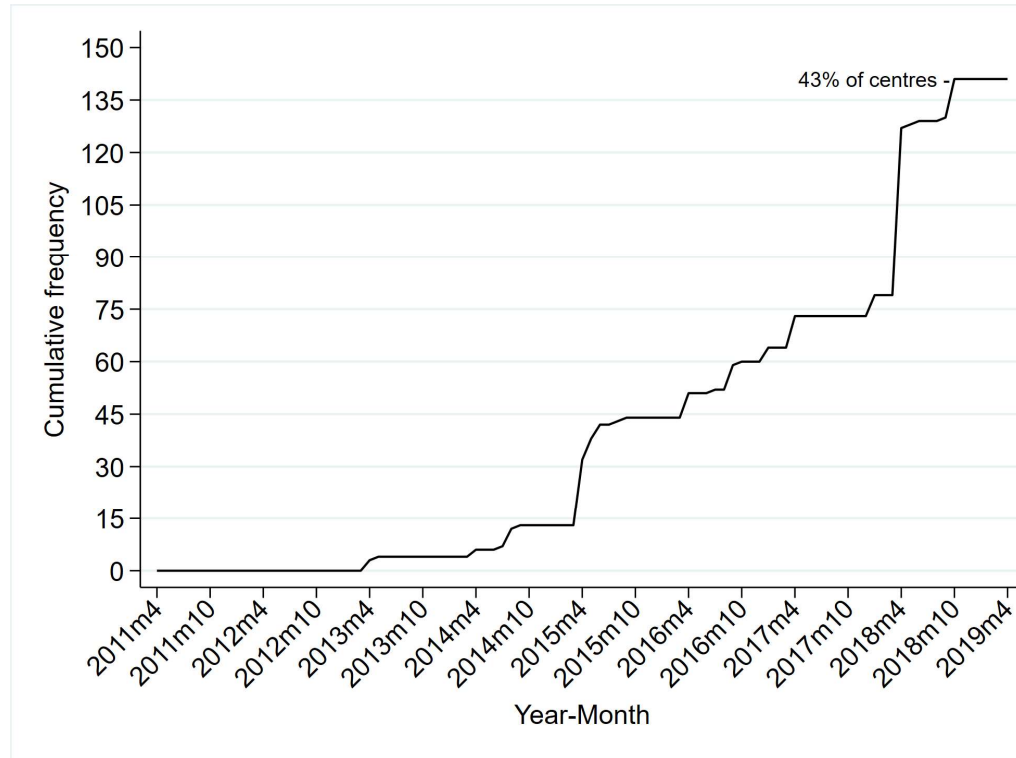
The results in this paper should be interpreted carefully, at least partly because they apply to a single firm. In a setting where all (or more) firms are exposed to a higher minimum wage it is not clear that firms would be able to reallocate hours in the same way, as labour supply responses would likely be muted. Likewise, for an industry specific Living Wage, for example the case of social care referred to above, the extent of hours reallocation will depend on the labour supply elasticity to the market rather than the firm, which is also likely to be considerably smaller. The main finding of the paper does, however, suggest that there are settings where firms can absorb higher wage costs in adjustment to a living wage level higher than the prevailing mandated minimum wage, and in the one studied here this seems largely due to the presence of monopsony power.

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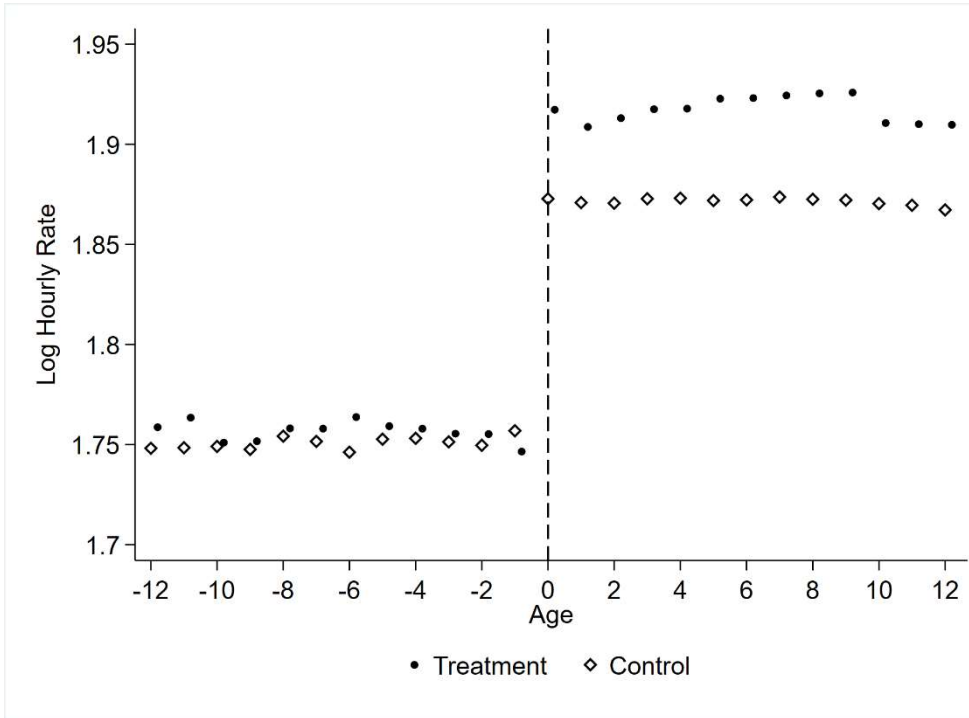
Exhibit 1 - Living Wage Treatments Over Time



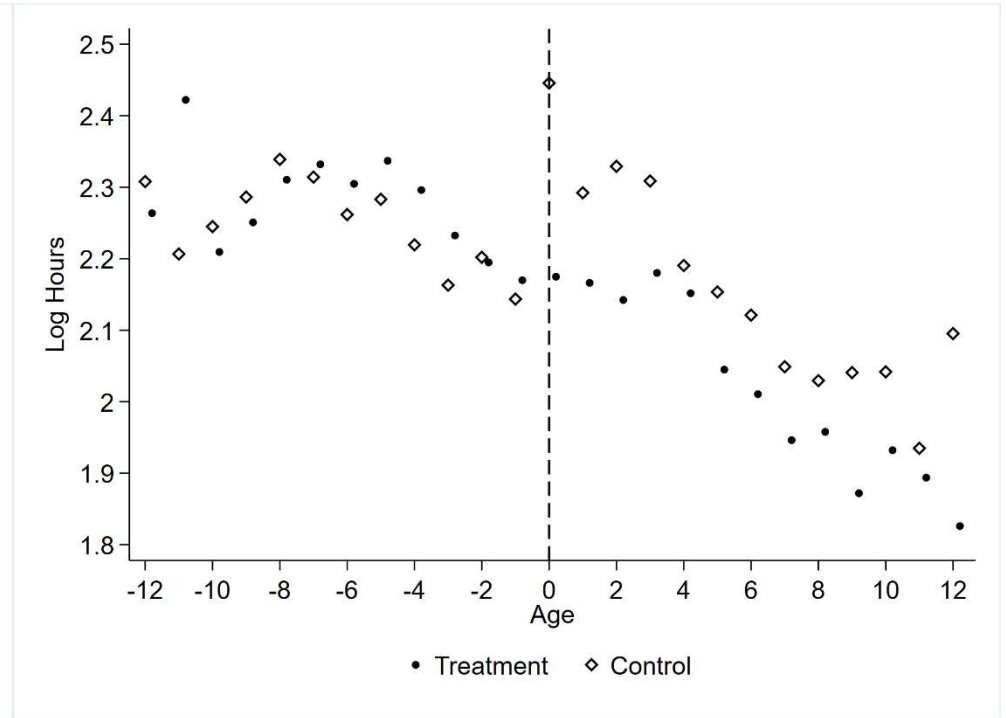
Note: The figure reports the number of treated establishments over time. The figure only includes which were treated while run by The Company. Some establishments were already subjected to the Living Wage when taken over by The Company.

Exhibit 2 - Wage and Hours Discontinuities

(a) Wages



(b) Hours



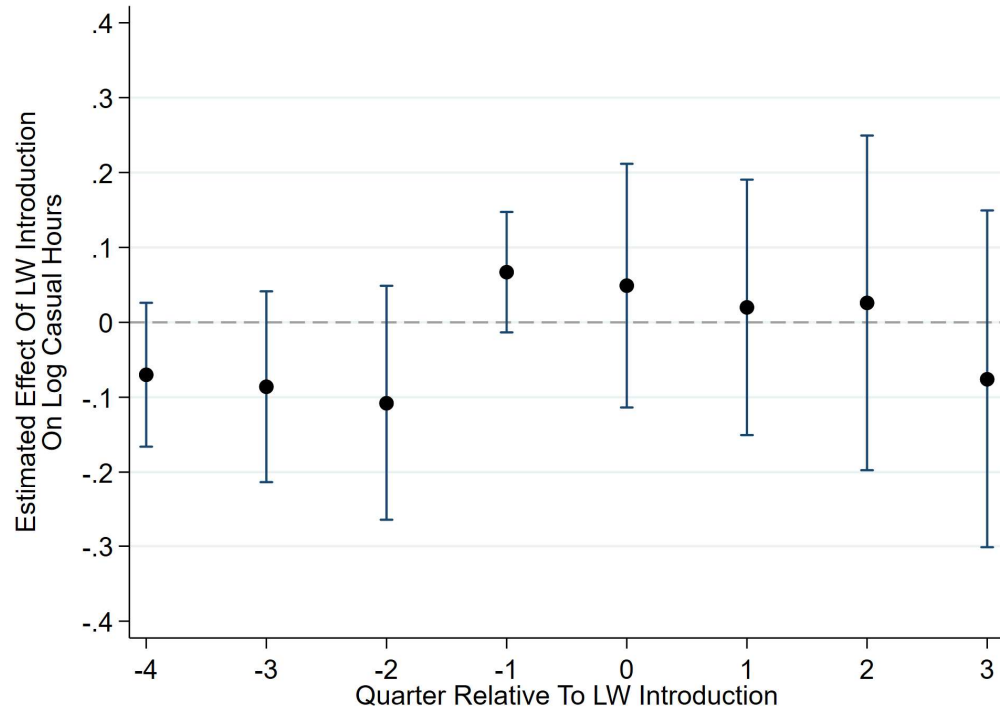
Note: The left and right figures respectively report log wages and log hours controlling for establishment and year-month fixed effects, by months relative to age 18 for 2 years before and after the cut-off, for those treated and untreated with the Living Wage. The dashed line marks the age 18 cutoff. The sample is a panel of entry-level casual workers employed by The Company active between January 2011 and April 2019.

Exhibit 3 – Wage and Hours Equations

	(1)	(2)	(3)	(4)
	Log Wage	Log Wage	Log Casual Hours	Log Casual Hours
Eighteen X Treated	0.072 (0.015)	0.072 (0.015)	-0.326 (0.102)	-0.266 (0.093)
Eighteen	0.121 (0.007)	0.128 (0.007)	0.097 (0.045)	0.166 (0.046)
Treated	0.056 (0.015)	0.052 (0.014)	0.099 (0.126)	0.084 (0.125)
Age/100	0.120 (0.037)	-0.024 (0.048)	-0.655 (0.314)	-0.801 (0.504)
Age/100 X Eighteen	-0.143 (0.042)	-0.007 (0.005)	-1.679 (0.427)	-2.502 (0.631)
Age/100 X Treated	-0.057 (0.068)	-0.090 (0.096)	1.472 (0.700)	0.381 (1.046)
Age/100 X Eighteen X Treated	0.109 (0.082)	0.122 (0.120)	-0.721 (0.940)	0.377 (1.325)
Centre FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Sample Size	94606	55176	94606	55176
R^2	0.740	0.754	0.179	0.180
Window	±24 Months	±12 Months	±24 Months	±12 Months

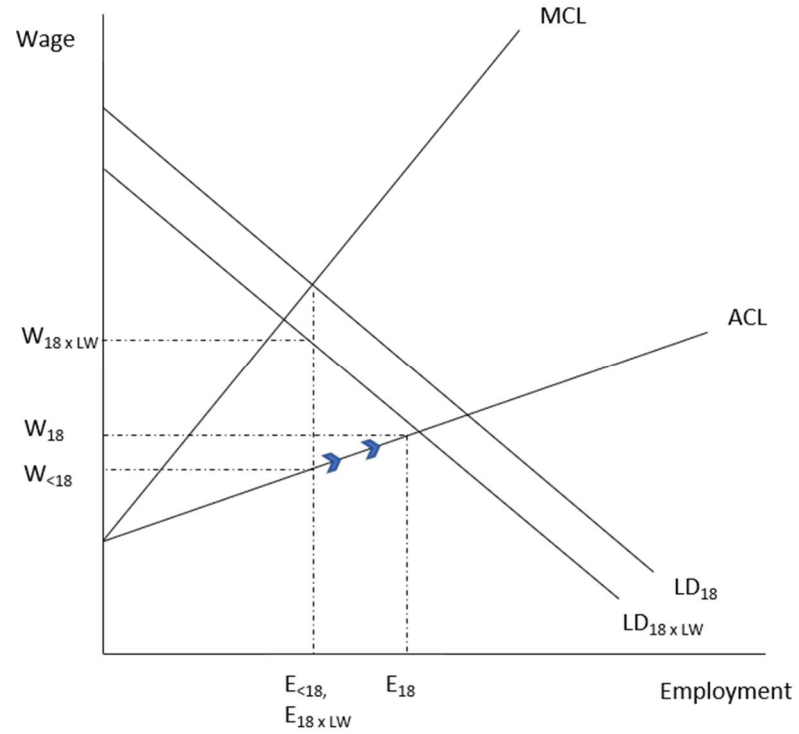
Notes: The table reports parameter estimates from model (1) and (2) under different sample windows around the age 18 cut-off. Standard errors are clustered at the establishment and are reported in parentheses.

Exhibit 4 - Living Wage Effect on Total Casual Hours



Notes: The graph reports the estimates coefficient from model (6). The sample is a panel of establishments run by The Company active between January 2011 and April 2019. The vertical bars indicate 95% confidence intervals based on bootstrapped standard errors. Parameters are normalised to month -1 and -12.

Exhibit 5: Monopsony Market for 18 Year Olds

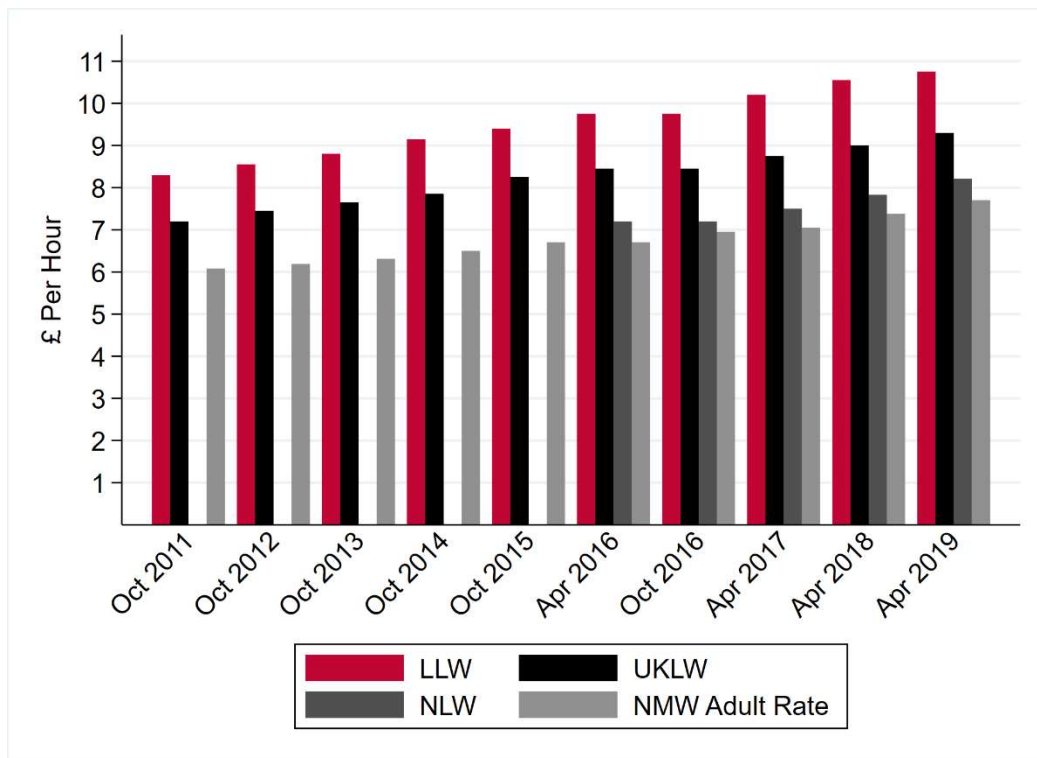


Notes: The figure presents a basic model of monopsony, with changes in the wage due to changing age from just less than 18 ($W_{<18}$) to just over 18 (W_{18}), and an increase due to the Living Wage which only affects those over 18 ($W_{18 \times LW}$), and how this impacts employment (E). The figure has lines representing the Marginal Cost of Labour (MCL), Average Cost of Labour (ACL), Labour Demand for those around age 18 (LD_{18}) and Labour Demand for those around age 18 when the establishment is subject to the living wage ($LD_{18 \times LW}$).

Online Appendix

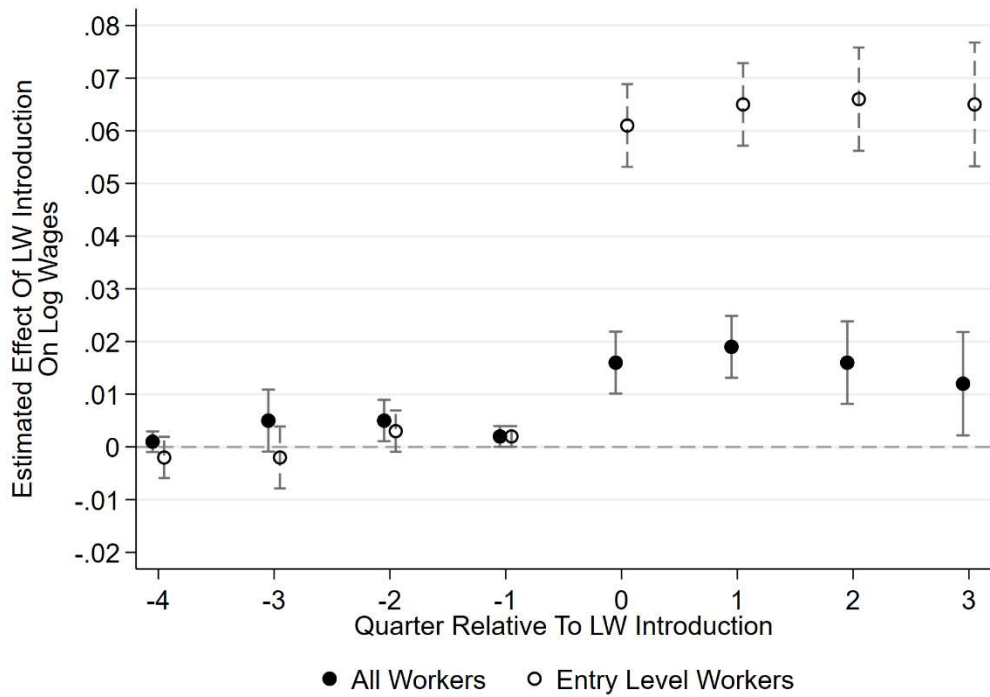
Additional Figures and Tables

Figure A1: Living Wage Rates



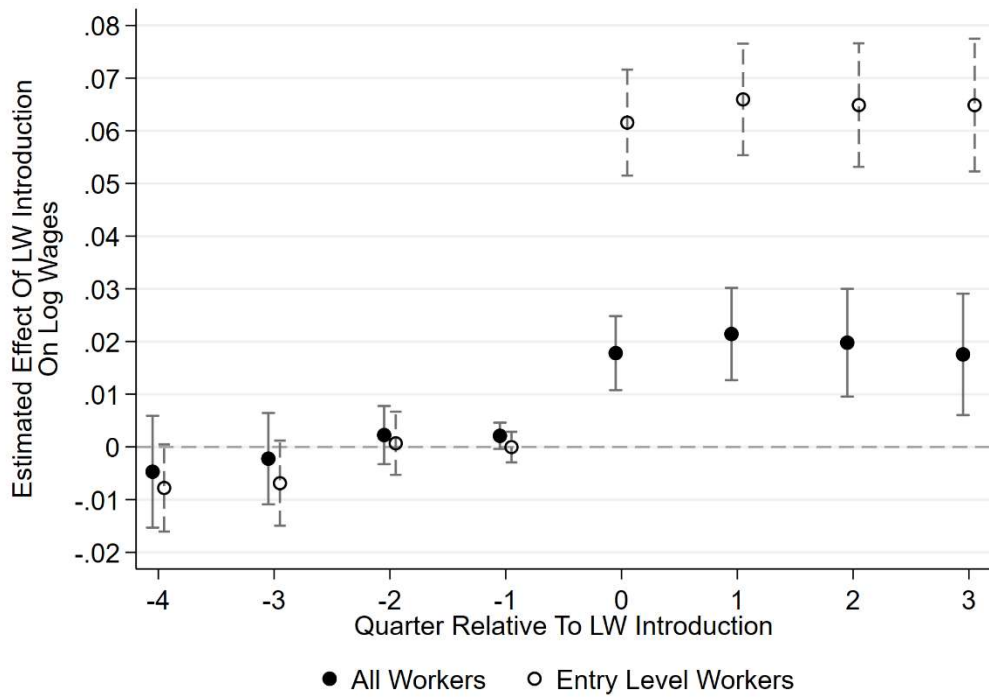
Notes: The figure presents the hourly wage rate of the nationally mandated 21+ minimum wage (NMW Adult rate), the 25+ minimum wage (NLW) and the Living Wage Foundation's non-London UK rate (UKLW) and the London rate (LLW).

Figure A2: Living Wage Effect on Wages I



Notes: The graph reports the estimates coefficient from model (6). The sample is a panel of establishments run by The Company active between January 2011 and April 2019. The vertical bars indicate 95% confidence intervals based on bootstrapped standard errors. Parameters are normalised to month -1 and -12.

Figure A3: Living Wage Effect on Wages II



Notes: The graph reports the estimates coefficient from model (8). The sample is a panel of establishments run by The Company active between January 2011 and April 2019. The vertical bars indicate 95% confidence intervals based on bootstrapped standard errors. Parameters are normalised to month -1 and -12.

Table A1: Summary Statistics

	2019			
	Mean	All S.D.	Entry-level Mean	S.D.
Female	0.60	0.49	0.55	0.50
BAME	0.22	0.42	0.25	0.43
Age	35.94	14.31	31.43	13.86
<18	0.05	0.22	0.09	0.29
<18 - Casual	0.07	0.25	0.12	0.32
London	0.53	0.50	0.50	0.50
Casual	0.72	0.45	0.75	0.43
Tenure (weeks)	194.10	214.15	139.04	157.08
Hourly Wage (£)	12.88	5.87	9.38	3.24
Hourly Wage (£) - Casual	13.17	6.12	9.34	3.63
Hours (monthly)	35.76	64.48	28.95	56.92
Hours – Casual (monthly)	14.54	26.83	19.13	29.84
Number of Workers	18,773		9,183	

Notes: The table reports worker level summary statistics for employees of The Company as of March 2019.

Table A2: Event Study Estimates

	(1)	(2)	(3)	(4)	(5)
	log(casual hours)	log(wage)	log(wage)	log(wage)	log(wage)
Treated x Quarter					
-4	-0.070 (.049)	0.001 (.001)	-0.002 (.002)	-0.005 (.005)	-0.008 (.004)
-3	-0.086 (.065)	0.005 (.003)	-0.002 (.003)	-0.002 (.004)	-0.007 (.004)
-2	-0.108 (.080)	0.005 (.002)	0.003 (.002)	0.002 (.003)	0.001 (.003)
-1	0.067 (.041)	0.002 (.001)	0.002 (.001)	0.002 (.001)	0.000 (.001)
0	0.049 (.083)	0.016 (.003)	0.061 (.004)	0.018 (.004)	0.062 (.005)
1	0.020 (.087)	0.019 (.003)	0.065 (.004)	0.021 (.004)	0.066 (.005)
2	0.026 (.114)	0.016 (.004)	0.066 (.005)	0.020 (.005)	0.065 (.006)
3	-0.076 (.115)	0.012 (.005)	0.065 (.006)	0.018 (.006)	0.065 (.006)
Centre FE	Yes	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes	Yes
Sample Size	17,879	17,879	17,879	17,879	17,879
Estimating Equation	(6)	(6)	(6)	(8)	(8)
Sample of Workers	All	All	Entry-Level	All	Entry-Level

Notes: The table reports parameter estimates from model (6) for log(casual hours) and log(wages) and from model (8) for log(wages) for different samples of workers, and are the counterpart estimates for Exhibit 4, Figure A2 and Figure A3. Columns (1), (2) and (3) report bootstrapped standard errors in parentheses and columns (4) and (5) report standard errors clustered at the establishment in parentheses. Columns (2) and (4) include controls for the proportion of entry-level workers.

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