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Government contracting and living wages > minimum wages

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Abstract
Government procurement accounts for a significant share of GDP, and environmental, social and corporate governance (ESG) clauses in government contracts have become common across developed economies. This paper studies one of these clauses: living wages that are set considerably higher than mandated minimum wages. When a local government in the UK signs up to become a living wage employer, as a significant number did in the time period we study, firms that have procurement contracts with them have to pay workers the living wage. This variation is studied with rich matched data on workers in establishments for a service sector company with many establishments located across the country. Just under half of the firm's establishments were made to comply with the living wage as a consequence of the local government becoming a living wage employer in the period between 2011 and 2019. In a staggered difference-in-differences research design, low wage workers are shown to receive a significant wage boost from the living wage introduction. Consistent with a model of monopsony power and where bottom-of-the-rung workers and supervisors are gross complements, the living wage induced labour-labour substitution in favour of the former. Further adjustment to the wage bill increase from the introduction of the living wage took place through within-establishment internal changes to the establishment pay policy structures. The overall result was that the Company was able to absorb the wage cost shock embodied in living wage adoption in a way that significantly narrowed establishment wage inequality.

Keywords: government contracting, living wages
JEL Codes: J31; J38; J42

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1. Introduction

Living wages (LW) are higher than mandated minimum wage floors. They have become increasingly commonplace, especially in settings where minimum wage floors are at modest levels or have not risen (like in some US cities, see Dube and Lindner, 2021). The living wage is typically calculated based on a consumption bundle defined to reach a minimum standard of living. Sometimes they are paid to workers in progressive private sector firms who have signed up to voluntarily pay their workers more than the minimum wages. Sometimes local or national governments, either being cautious or unable to raise the minimum wage, have introduced them by including wage floors higher than the mandated minimum as a clause in their procurement contracts.

Government living wage clauses are becoming widespread. They exist at a local government level in England and Scotland, are being introduced for the whole of Wales. In the US in April 2021 President Biden issued an executive order increasing the minimum hourly wage for federal contractors to almost double the federal minimum wage. The late 1990s and early 2000s saw approximately 140 living wage ordinances introduced in the United States and this in turn spawned a number of papers studying the topic. Similar to the early minimum wage literature, the data used was often survey data (largely, but not exclusively, from the Current Population Survey) and a prime focus was placed on wage and employment effects (Neumark et al., 2012, reviews this literature) though there was some attention given to local growth (Lester, 2011), poverty (Neumark and Adams, 2003) and consumer prices (Dube et al., 2005).[1]

Results on the impacts of these US living wage ordinances are generally mixed, ranging from no employment effects (e.g. Brenner et al., 2002 and Lester, 2011) to some negative

[1] For more details on the early literature, see Robert Pollin's pioneering book *A Measure of Fairness* (Pollin, 2008) which contains a lot of detail on both the early implementation of living wages, and on the relevant literature from the period, including empirical studies and research on the political economy of living wages.

employment impacts, but also generating mild reductions in net poverty (e.g. Neumark et al., 2012). More recently some research that has studied the economic consequences of situations where wages are regulated to be above national minimum wages. For example, there is US research where higher state/city minima are set above the federal minimum (Dube and Lindner, 2021) and evidence from Portugal where sectoral minima lie above the mandated national minimum wage (Cardoso and Card, 2022). There is however scant recent, well identified, evidence on the impact of living wage policies, and none to our knowledge concerning the case of living wages contained in public procurement contracts. Little is known about labour related ESG (environmental, social and corporate governance) clauses when applied by one of the biggest employers and purchasers of contractors – the government.[2]

This paper studies this question, investigating living wage clauses contained in local government public procurement contracts and using them to evaluate the impact of living wages set at levels significantly higher than the mandated minimum for low-wage workers in the UK. It analyses a rich firm level matched worker-establishment dataset which contains hundreds of UK based establishments. These are establishments with local procurement contracts which, importantly, are exogenously (and involuntarily) subjected to a living wage based on local government decision making.

A large set of adjustment margins, including traditional ones that feature in the big minimum wage literature and others that are not usually looked at, are studied for workers in establishments that get treated by living wages higher than the national minimum wage as compared to control establishments that are bound only by the national minimum wage. The richness of the dataset allows us to study not only traditional wage and employment margins,

[2] ESG standards in contracts have a number of commonalities to "Responsible Sourcing" standards in supply chain management which has recently received attention in the international trade literature. See Alfaro-Urena et al. (2022) for more information.

but also a range of novel and less frequently studied effects including labour-labour substitution, wage profile coarseness, promotions, use of zero-hour contracts (ZHCs)[3], consumer prices, firm exits and quality of output product.

It proves important to be able to study this wider than usual range of outcomes to understand how the Company's establishments were able to adjust to adopting living wages. The results show the living wage had sizeable positive wage impacts on the workers at the bottom of the hierarchy within an establishment, and that it increases the total wage bill. But on the employment side, and in line with many existing minimum wage studies, but obviously in our case for higher wage floors, there is no evidence of negative aggregate employment effects. Nor does establishment performance - measured by output prices, establishment exit and service quality - seem to have taken a hit from the living wage introduction.

However, the composition of employment did alter. Living wage introduction generated clear and marked labour-labour substitution effects, affecting the hierarchical composition of the workforce. In particular, the living wage increased the number of entry workers (those on the bottom rung, typically paid the minimum wage, henceforth "entry")[4] relative to managers and supervisors (henceforth "supervisors"). Thus, the LW generated winners and losers along the establishment's hierarchy. The Company's establishments that were forced to pay living wages also altered their wage contracts by reducing the number of unique pay points, therefore resulting in a coarser wage structure within exposed establishments. We find no negative impacts on a range of other employment contract outcomes including promotion rates and atypical contract use. The bottom line was that the

[3] ZHCs are a form of atypical work which in theory offer two-sided flexibility where workers are not guaranteed any hours and nor obliged to accept hours. For more information see Datta et al. (2019).

[4] We are unable to disclose the precise industry and therefore the occupations within the firm, but more widely "entry" workers may include burger flippers in the fast-food industry, shelf packers and checkout workers in the supermarket industry, waiting staff in the restaurant industry and sales assistant in the retail industry.

Company was able to absorb the wage cost shock embodied in living wage adoption in ways that significantly narrowed establishment wage inequality.

In offering these findings, the paper makes three key contributions to advance research knowledge. First, it is the first paper with a credible identification strategy to study establishment level responses to a "true", procurement clause induced living wage adoption that made employers face a higher wage floor than the going minimum wage rate. Establishments in the sample get seemingly randomly, and involuntarily, subjected to the Living Wage Foundation's (LWF) living wage through local council[5] decision making about environmental, social and corporate governance (ESG). When the local council opts to become an accredited living wage payer, as part of its ESG strategy, only the council and their contractors must pay the living wage. As the establishments in our sample are council contractors, some establishments get treated with the LW when the local council opts to become a living wage payer while the rest of the market is left unaffected. The setting lends itself to a multiple timing differences-in-differences estimate, and results are robust to state-of-the-art staggered treatment estimators (Borusyak et al., 2024; Sun and Abraham, 2021).

Secondly, facilitated by the quality of the data, the paper studies a larger than usual number of possible adjustments. This proves important because of the way the Company's adjustment to forced living wage adoption occurred. Had we not had access to these less usually studied possible adjustments, we would not have been able to show how the company was able to adjust, nor through what means. The Company's adjustment worked through relative impacts on different groups of workers via labour-labour substitution[6] and by altering their company pay policy, thereby significantly narrowing establishment wage dispersion, and not

[5] Councils are small local government units in the UK, of which there are approximately 350.

[6] The paper thus contributes to the literature on labour-labour substitution in response to wage floors. See Cengiz et al. (2019), Aaronson and Phelan (2019) and Giuliano (2013) for recent studies of labour-labour or labour-capital substitution in response to minimum wages, and Hamermesh (1996) for wider discussion of labour substitutability.

through total labour demand adjustments, price pass through, nor from harming establishment performance.

The adjustment through changing employment composition is of particular interest in its own right in how it relates to the theory of low wage labour markets. In a model of perfect competition with two labour inputs (in the context studied here, entry workers, and supervisors), where one is subjected to a wage floor, we would expect the relative employment levels to adjust according to the elasticity of substitution between the two groups and the change to the relative wage. However, in a setting where a firm has monopsony power in the labour market there can be two opposing effects to a wage floor. The first, a movement up the labour supply curve of the more exposed group, which in turn increases the proportion of entry workers relative to supervisors, and a second demand (substitution) effect which has the opposite effect. Our data set contains information on both the supply and substitution effects, and we show that using estimates of the labour supply elasticity to the firm for The Company (from Datta, 2023) and our reduced form estimates on the demand side we can pin down the elasticity of substitution, σ, between entry workers and their supervisors.

We estimate the substitution elasticity to be $\sigma = 0.58$, suggesting the two labour inputs to be gross complements. Our estimate pertains specifically to the within-establishment elasticity of substitution between entry workers and their supervisors and managers. As far as we are aware, we are the first to estimate such a within-establishment labour-labour substitutability between supervisors and who they supervise, and the most similar estimates are from Freeman and Medoff (1982) who estimate the within-manufacturing establishment elasticity of substitution between production and non-production workers, with a particular focus on union/non-union differences. This is a key parameter of importance when studying wage floors as it is quantifies the degree to which firms would be able to substitute workers exposed to the wage floor, for those higher up the hierarchy. Thus, it is instructive of the efficacy of wage floors in having relative impacts on different groups of workers, be they substitution effects or changes in composition, in general.

Finally, the paper confirms, for a living wage that is a sizeable mark up on the national minimum wage, findings from papers in various settings that high wage floors continue to show little sign of negative employment effects. This has repeatedly been shown in several developed economies including Germany (Dustmann et al. 2022), the UK (Datta et al., 2019; Giupponi and Machin, 2018; Stewart, 2004; Machin et al., 2003), the US (Wiltshire et al., 2023; Cengiz et al., 2019; Dube et al., 2010, 2016; Baskaya and Rubinstein, 2015) and Hungary (Harasztosi and Lindner, 2019).[7] The richness of the data, and the research design based on living wages > minimum wages, enable us to shed light on the mechanisms which may be underpinning this result. Specifically, disemployment effects do not arise because firms operating in low wage markets like the one studied here possess monopsony power, who are able to adjust their company wage policies and because their employment structure features workers exposed to the wage floor who are not easily substituted for workers higher up the skill distribution.

2. Government Procurement ESG Clauses and Living Wages

2.1 Government Procurement and ESG Clauses

Environmental, social and corporate governance (ESG) has become increasingly important for firm decision making over the past two decades. Projections suggest that ESG investments will account for 15 percent of all investments by 2025 (Dow Jones, 2022). As conscious consumerism has grown, so too has conscious procurement contracting for many governments, which has been estimated to be as high as 14 percent of GDP in high income

[7] This is not to say all studies find no negative employment effect of minimum wages. For a recent example in the US see Clemens and Wither (2019), and for a recent review see Neumark and Shirley (2022).

countries (Bosio and Djankov, 2020). Along with this has come a closer focus on employment and wage regulations associated with ESG.

In the UK, the government has set forward a new framework for taking account of social value when awarding central government contracts. The framework states that a minimum of 10 percent of the total score for each bid needs to be on ESG objectives. (Cabinet Office, 2020). The European Commission has designed a voluntary toolkit named "Green Public Procurement" for EU member states and the Federal US government has issued several executive orders in the past two years which are designed to steer Federal Acquisition Regulation (FAR) towards implementing ESG requirements.[8]

A key aspect of these ESG frameworks is a focus on employee relations and fair pay. This is not specifically a novel feature of government contracting and has been around for quite some time. As early as 1891 the UK passed the Fair Wage resolution which aimed at securing fair wages for employees "sub-let" through government contracts, and in 1949 the International Labour Organisation (ILO) adopted the "Labour Clauses in Public Contracts" convention, which as of 2023 has been ratified by over 60 countries.

Contemporary labour related clauses in government procurement contracts take a variety of forms and can vary between national/federal levels, and local government/state level. The UK government's Social Value Model for example, which determines ESG scoring for contracts, has five foundational principles of quality work including fair pay, participation and progression and voice and autonomy; while the Welsh Government's Code of Practice[9] covers both use of insecure working contracts (ZHCs) as well as payment of the Living Wage Foundation's living wage. In the US, a number of Federal Acts and executive orders regulate

[8] See Executive Orders 14008 and 14030 for examples.

[9] For this, the Welsh Government's Code of Practice would be the binding constraint rather than the UK government's, like how stricter State guidance acts as the binding constraint with more relaxed Federal regulation.

labour hours, pay and affirmative action for government contracts[10] and in recent years Project Labor Agreements[11] in the US has seen legislation at both the federal and state level.

One style of clause that has been gaining popularity on both sides of the Atlantic is to do with wage floors, in particular where government contractors require a higher minimum wage, or living wage, higher than the mandated minimum. Clauses exist in the UK for both Scottish and Welsh national governments, along with a growing number of local UK government units. In the United States in April 2021 President Biden issued an executive order increasing the minimum hourly wage for federal contractors to $15, almost double the federal minimum wage, while the state of Maryland requires contractors and subcontractors working on State funded service contracts to pay a living wage set by the Commissioner of Labor. Little recent evidence exists on the effects of such clauses and this paper fills this gap.

2.2 The Experimental Setting

The impact of a living wage clause contained in local government procurement contracts in the UK is studied, using institutional information on the Living Wage Foundation's (LWF) living wage. The LWF is a charitable organisation in the UK that was established in 2011 that campaigns for employers to pay workers a true living wage. Each year the LW is calculated utilising price data from a representative basket of goods and services and is published for London (LLW) and the rest of the UK (UKLW).

Figure 1 shows the living wage rates are substantially higher than government mandated minimum wages. The LLW rate has typically been approximately 30-35 percent higher than the official statutory National Minimum Wage (NMW) or National Living Wage

[10] These include the Davis-Bacon and Related Acts, the McNamara-O'Hara Service Contract Act, the Contract Work Hours and Safety Standards Act, the Copeland "Anti-Kickback" Act, the Walsh-Healey Public Contracts Act, the Rehabilitation Act of 1973 and executive order 11246.

[11] Pre-agreed collective bargaining agreements with labour unions for construction projects.

(NLW)[12] which does not vary between London and the Rest of the UK, while the UKLW has been about 15-20 percent higher.

The living wage is voluntary and not nationally mandated, unlike the NMW and NLW. Therefore, in many cases becoming a living wage employer is an endogenous decision. Once organisations sign up to become living wage employers they can achieve accredited status following audits. As of July 2020, just after the sample we study ends, the LWF lists 6,562 accredited employers. Included in this were 107 local government units.[13] When public bodies achieve accreditation, their contractors and subcontractors are additionally required to pay the LLW or UKLW, so living wage clauses become a necessity in their procurement contracts. Figure A1.1 in the Appendix gives an example set of living wage clauses for Southwark Council, a London Borough (of which there are 32) in charge of local services for approximately 300,000 people which signed up to be a living wage employer in 2012.

We utilise a novel dataset for a company (hereafter referred to as The Company) which has over three hundred establishments operating in the UK service sector, where an establishment is a single site address with on average 16 FTE employees. We are required to not disclose the identity of the company, nor its activities[14], but we can say that in general terms a large portion of The Company's turnover is from government contracts for local government services. And that, unlike many local government contractors, the service they provide is not a typical natural monopoly (such as refuse collection), as they compete with other private firms in the same local markets. An example of such a setting (which is not ours) would be a firm that operates cafés and bids for council contracts when councils wish to outsource food and coffee provision on public property (such as at libraries or train stations).

[12] The NLW was a new statutory minimum wage band introduced in April 2016 for workers aged 25 and over.
[13] These include London Boroughs, Unitary Authorities, Metropolitan Districts, County Councils, District Councils, Local Government Districts and Parish Councils.
[14] The data sharing contract strictly prohibits disclosure of the precise industry.

Different establishments have become contractually obliged to pay the LLW and UKLW at different times. This depends on whether the establishment has a government contract, and on whether and when the local government unit has voluntarily signed up to the LWF's living wage. Discussions with the directorship of The Company suggest that when establishments do become subject to the living wage as a result of local council decision making, there are no fundamental changes to their contract aside from the relevant LW clause. We are unable to present an exemplar extension contract for The Company due to anonymity issues, however having seen a version we can confirm it states "The agreement will be extended with similar terms while meeting the Living Wage commitment for the associated years."[15]

Between 2012 and 2019 107 local government units gained accreditation and this induces considerable variation in treatment over time. For example, of the 32 London Boroughs, 17 have received accreditation, the earliest (Islington) receiving accreditation in May 2012, and the most recent (Redbridge) receiving accreditation in November 2018.[16] As Figure 2 shows, this setting gives a large amount of variation in living wage treatment for establishments run by The Company. Over the period for which we have HR data, approximately 140 establishments went from being untreated to treated, while run by The Company, and by the end of our sample period this made up 43 percent of all establishments. Such variation naturally lends itself to a research design with multiple period difference-in-differences analysis, on which more detail is given later in the paper.

This setting combined with matched worker-establishment data from the Company's payroll allows a novel analysis of how firms react to an increase in their wage floor to a true living wage level that results from government ESG clauses in their procurement contract. As all the establishments in our analysis are operated by the same company using the same

[15] This sentence contains mild paraphrasing to ensure it is not traceable.
[16] Correct as of July 2019.

structure of operations and management, but with establishment level autonomy over finances, employment, and workforce composition, we can see a true counterfactual, when comparing treated and untreated establishments.

Furthermore, unlike many traditional minimum wage papers, we are able to isolate the impact of just the individual contractor 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 LW, private companies and non-council public employees in the area remain untreated. Thus, only a fraction of a percentage of workers within a specific council area would be affected. Existing research estimating the Company's labour supply elasticity to the firm with the same data and experimental setting has documented both descriptive and causal evidence showing that there are no detectable wage spillovers to the local economy (Datta, 2023).

As already noted, we are not able to disclose the precise industry that The Company operates in (due to disclosure restrictions) beyond that it is in the local service sector. This combined with the fact the firm has procurement contracts with local government, may bring about a concern some that the firm operates in an atypical market in terms of competition (e.g. a natural monopoly such as refuse collection), and therefore the results will lack external validity. To alleviate such concerns, Datta (2023) provides two pieces of information for The Company. First, he shows using an exemplar town that the firm faces a similar degree of local competition as pubs, restaurants, hairdressers and mechanics' garages. Second, he shows both job and industry substitutability for the set of occupations utilised by the firm mirrors similar rates for the entire economy. These two observations go some way in making the case that The Company is representative of a typical local services firm. Moreover, the kind of services supplied in The Company's establishments are common across the world. For example, the industry is worth at least tens of billions of dollars in the United States.

3. Data, Research Designs and Descriptive Analysis

3.1 Data

The data comprise the complete set of HR records on workers in all establishments of The Company from 2011 to 2019, including wages, hours, tenure and demographic characteristics of workers. It contains information on the specific job role that each worker carries out. Each job role is associated with a specific pay scale point, position in the establishment hierarchy and job title, and so it is possible to track within-firm worker dynamics, including whether they receive a promotion and if they move across pay points or up the hierarchy. Additionally, the data includes the number of discrete pay points each establishment operates, employment composition across job roles and contract type (permanent or ZHC). In addition to the HR data, information on establishment performance was obtained on output prices, establishment exit, and quality ratings scraped from over 100,000 google reviews.

The richness of the data set permits us to see a full picture of the wage, employment and output policies of the firm. Many existing minimum wage studies limit their analysis by placing a direct focus on wages and employment (along the extensive and intensive margin). A handful of papers do however look at a wider range of outcomes, including Harasztosi and Lindner (2019), who look at wage, employment, capital-labour and price adjustments, and Hirsch et al (2015), who look at wage, employment, prices, profits, wage compression turnover and performance standards.[17] We look at the impact of living wages > minimum wages on traditionally studied outcomes, but also extend the set of outcomes to look at a number of novel effects relating to wage and contract policies, within-establishment labour-labour substitution along the employment hierarchy, and establishment performance.

[17] See Clemens (2021) for a fuller discussion of papers studying different margins of adjustment to changing wage floors.

3.2 Research Design

3.2.1 Empirical Implementation

The research design is structured to look at outcomes associated with having a living wage > minimum wage in difference-in-differences and event-study settings which take into account the multiple treatment/staggered timing of LW introduction. The baseline difference-in-differences estimator can first be set up as:

$$Y_{it} = \alpha_i + \lambda_t + \delta\mathbb{1}[\mathrm{L}W > MW]_{it} + \beta' X_{it} + \varepsilon_{it} \quad (1)$$

where Y_{it} is the outcome of interest for establishment i at time t, α_i and λ_t are establishment and time fixed effects respectively, $\mathbb{1}[\mathrm{L}W > MW]_{it}$ is a binary indicator for whether the establishment is subject to an involuntary living wage as a result of a government labour clause for contractors, and X_{it} are time varying controls.

Generalising (1) to an event study specification with pre- and post-introduction variations in δ as:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{l} \delta_l \, \mathbb{1}[\mathrm{L}W > MW]_{it+l} + \beta' X_{it} + \varepsilon_{it} \quad (2)$$

where l defines the timings of pre- ($l < 0$) and post-time ($l \geq= 0$) period variations in the event study. The estimates shown later correspond to four quarterly pre-living wage introduction coefficients in the year before, and four post-living wage introduction coefficients in the year after.

There has been a recent upsurge of interest in the workings of difference-in-differences and event study estimators, especially in contexts where canonical components of difference-in-differences are relaxed (Roth et al., 2023; de Chaisemartin and d'Haultfoeuille, 2023). One strand of this literature concerns variation in treatment timing coupled with the heterogenous treatment effects. This is relevant for our setting. Specifically, as we have variation in treatment timing, a standard TWFE specification will only elicit a valid estimate under the assumption of treatment effect homogeneity. Concerns raised include: issues identifying the linear component of the path of pre-trends in traditional event study specification (Borusyak et al,, 2024); contamination of lead and lag coefficients from other period effects (Sun and Abraham, 2021), biased estimates of treatment effects when the control group contains treated units when dynamic treatment effects are present (Goodman-Bacon, 2021); the structure of weights assigned across treatment cohorts when estimating dynamic treatment effects (Sun and Abraham, 2021); and the structure of weights across dynamic treament effects when estimating a single treatment effect (Goodman-Bacon, 2021; de Chaisemartin and d'Haultfoeuille, 2020; Borusyak et al. 2024).

These issues are circumvented by implementing difference-in-differences and event study estimators of (1) and (2) as in Sun and Abraham (2021), while also making adjustments as recommended in Borusyak et al. (2024)[18] (henceforth BJS). For comparison, we also perform traditional two-way fixed effect (TWFE) style difference-in-differences and event study estimations for robustness. Results of these can be seen in the additional Tables contained in Appendix section A2. More formal details on the estimation strategy are presented in section A3 in the Appendix, which specifcally contains details on the choice of treatment and control groups, and the weighting methods across treatment cohorts used in the heterogenous estimates of the parameters of interest in equations (1) and (2).

3.2.2 Outcomes

Four groups of outcomes are studied using staggered and heterogenous treatment effect robust estimator versions of (1) and (2). The analysis begins first with the conventionally studied wage and employment impacts. Second, it considers measures of establishment performance. Third, by studying heterogenous impacts across the two main core groups in the

[18] Specifically they raise concerns about weights across dynamic treatment points, as a result we apply an adjustment where all post period year-month effects are given equal weighting.

employment hierarchy, it looks at relative within-establishment employment structure that could occur through labour-labour substitution. Fourth, it then moves to a much less frequently studied area of adjustment to wage floors, looking at the impact on wage and contract policies.

Considering each of these in turn:

i) Wages and employment

These pertain to the traditional outcomes of interest in the minimum wage literature – wages and employment. Other than the focus of this study being the impact of a higher than mandated wage floor, living wages > minimum wages, the approach taken is common with much of that literature. The initial interest focusses on the impact of wages (typically as a "first stage") and then, conditional upon existence of a first stage wage effect, the impact on employment (in terms of numbers and hours). In the canonical, competitive labour market framework, these elicit the labour demand response to the higher wage floor and with that estimated elasticities of employment with respect to the minimum wage.

ii) Establishment performance

Whilst the principal focus of minimum wage studies over the years has been firmly on wage and employment responses to wage floors, as referred to above, more recent work has looked at other modes of adjustment to the wage cost shock embodied in higher wage floors. One is establishment performance which we study, more specifically, in terms of whether a shock to establishment wages could induce output price adjustments, establishment exit, or changes to the quality of service.

On the first, prices, models of optimal pricing suggest a positive pass through to output prices as production costs to firms increase due to the living wage adoption. The extent to which this arises depends on market structure, the first and second order price elasticity of the demand for output, and the product's market supply elasticity (see Weyl and Fabinger, 2013, for more information). There is some evidence on this mechanism operating in the context of minimum wages (e.g. Aaronson, 2001; Harasztosi and Lindner, 2019; Leung, 2021). We use all available data on output prices at the establishment level over time. Due to data limitations, the panel for prices is less complete and does not go all the way back to 2011, however it does go beyond 2019. Given firms sell a variety of service types, we construct a consistent price index for each establishment where each product they sell is given equal weight.

The second performance outcome we study is the likelihood of establishment closure due to a wage cost shock. Here, the cost shock could induce establishment exit, especially when markets are more competitive. Thus, the probability of establishment exit is studied, with *Y* being a binary outcome in this case, where (1) and (2) are estimated as linear probability models.

Thirdly, the impact on service quality is studied. A direction of impact for this outcome is *a priori* unclear. In a world with efficiency wages, one might expect improvements in productivity, and in the service sector this could translate to better service and more satisfied customers. Alternatively, if a firm tries to save by adjusting costs in other areas, this could in turn adversely affect quality. To study this outcome, we scraped a large amount of quality data based on google reviews for each establishment, which give service quality ratings between 1-5. Like the price data, for some establishments there are not reviews going back as far as 2011, however we do have data up to 2023, and possess establishment-level treatment dates up until then.[19] In total, there are 101,641 google reviews matched to establishments. We use the level rating as the dependent variable for ease of interpretation and run specifications using reviews as the level of observation, together with a version weighting by establishment-year month to mimic all other specifications.

[19] Given we only have the HR data up to 2019, we do not run specifications with any HR level controls for prices and quality.

iii) Labour-labour substitution

In a labour market where an employer has monopsony power a wage floor affecting only a subset of workers in the firm induces two impacts. The first, a positive labour supply to the firm effect for entry workers due to the firm's upward sloping labour supply curve would increase the ratio of entry workers to supervisors. The second, a demand effect where the relative price of entry workers to supervisors increases, should decrease the ratio of entry to supervisors.[20] To assess which dominates, we can generalise the reduced form estimates in (1) and (2) with the dependent variable restructured as a relative measure across worker groups, the log ratio of wages and the log ratio of employment between entry workers and supervisors. Combining this with an estimate of the labour supply elasticity to the firm enables identification of the elasticity of substitution between entry level and supervisory workers.

Formally, consider a two labour types relative supply-demand framework, where an establishment utilises the labour inputs, entry and supervisor, and has a CES production function of the form:

$$F\left(L_{e,t}L_{s,t}\right) = \left[\alpha {L_{e,t}}^{\frac{\sigma-1}{\sigma}} + (1-\alpha){L_{s,t}}^{\frac{\sigma-1}{\sigma}}\right]^{\frac{\sigma}{\sigma-1}} \tag{3}$$

In (3), $L_{e,t}$ is entry labour at time $t, L_{s,t}$ is supervisor labour at time t, and σ is the elasticity of substitution between them, and labour is supplied to the establishment according to:

$$\begin{gathered} L_{e,1} = \gamma_e {W_{e,1}}^{\varepsilon_e} \\ L_{s,1} = \gamma_s {W_{s,1}}^{\varepsilon_s} \end{gathered} \tag{4}$$

where $W_{e,t}$ is the entry wage at time t, $W_{s,t}$ is the supervisor wage, so that ε_e is the labour supply-elasticity to the firm for entry workers, ε_s is the equivalent for supervisor workers and

[20] Some monopsony papers emphasise scope for wage floors to induce supply and demand employment responses (depending on how high the floor is relative to the monopsony wage and the competitive wage), for example Dickens, Machin and Manning (1999). The focus here is on substitution with two worker types, rather than the aggregate effects on total employment.

γ_e and γ_s are scale factors. Then it can be shown that the introduction of a living wage that is binding only for entry workers elicits the following impact on employment composition:

$$\Delta \log\left(\frac{L_e}{L_s}\right) = -\sigma\left(\log(\mu_e) + \Delta \log\left(\frac{W_e}{W_s}\right)\right) \tag{5}$$

where $\mu_e = \frac{\varepsilon_e}{1+\varepsilon_e}$ is the wage markdown in the entry labour market[21] such that $\mu_e \in [0,1]$.[22]

Equation (5) formally states the intuition from the start of this section. In a perfectly competitive labour market $\varepsilon_e = \infty$, the firm does not markdown wages, and therefore $\log(\mu_e) = 0$ so that equation (5) collapses to the typical relationship between relative input prices and relative demand which is entirely dictated by the elasticity of substitution (e.g. as in the aggregate case of Katz and Murphy, 1992, studying relative demand and supply for college and high school graduates). However, when firms possess adequate monopsony power in the labour market (i.e. $-\log(\mu_e) > \Delta \log\left(\frac{W_e}{W_s}\right)$) the introduction of a wage floor which is binding for the entry workers can actually increase the relative proportion of lower skilled workers within the firm employment hierarchy.

In section 5 we shall test to see which of these two mechanisms dominate, when considering entry and supervisor workers. Furthermore, utilising an estimate of the labour supply elasticity to the firm for entry workers from Datta (2023) for The Company we shall pin down the elasticity of substitution between the two groups of workers.

iv) Wage and contract policies

A much less studied area in the minimum wage literature is company wage and contract policies, and in particular whether wage hikes could cause employers to alter them as a mode of adjustment. The rich data we have on The Company includes establishment-specific

[21] Specifically, in the classical model of monopsony, a monopsonist would set wages at $\mu_u * p$ where p is the marginal productivity of the worker. See Manning (2003) for more details.
[22] For fuller details on the derivation, see section A4 of the Appendix.

measures of both wage and contract policies. On the wage side, the relevant ones feature variations in the number of different pay grades that establishments use to set wages. On employment, there is information on promotions and on use of zero-hours contracts.

Access to the detailed data on pay scales is useful in light of the work on company wage policies, where rather than paying each worker their precise marginal product, workers get paid through firms setting specific wage points (e.g. see Manning, 1994; Machin and Manning, 2004; Derenoncourt et al, 2021). Firms optimise by choosing how many pay points they operate, and the associated rates subject to frictions they face, such as administrative burden, and worker preferences for “fair” pay (Dube et al., 2019). Similarly, firms typically have discrete, well defined job roles, assigned to pay points (or a set of pay points) on their (discretised) wage profile. Job roles typically have an obvious hierarchy where supervisors and managers oversee, and direct workers lower down the hierarchy (Williamson, 1967). Firms optimise their employment composition across these different job roles, and this in turn can affect aggregate employment, the types of employment contract used (e.g. part time, flexible), the ratios of different worker roles, and the scope for worker progression within the firm.

The dataset at hand thus allows a novel analysis of both wage and employment contract policies of the firm, and how they respond to a true living wage floor, which is exogenously set above the prevailing minimum wage as a result of local government ESG labour clauses in their procurement contract. The specific outcomes considered are: the coarseness of the wage structure, specifically measured by the number of unique pay points; the rate at which promotions occur within an establishment; and the proportion of the workforce on ZHCs.

3.3 Descriptive Analysis

Table 1 presents summary statistics for the period 2011 to 2019, for all of the The Company’s establishments and broken down by whether they are bound by the LW (column denoted Living Wage) or by the nationally mandated minimum wage (column denoted Minimum Wage). The first thing to notice is that The Company is principally an employer of low wage labour. Mean hourly wages in The Company across all establishments are significantly lower than the UK national average. Panel A of the Table reports a mean hourly wage across the core positions of employment for the sample period of £10.47 per hour, which is approximately 2/3 of the average hourly wage for the UK over the same period.[23] This is higher at £10.72 per hour in the Living Wage group as compared to £10.00 per hour in the Minimum Wage Group.

Average employment in the establishments of The Company is 32 workers, although as the average weekly hours number of 21 shows the full-time equivalents are lower because of part-time work, as is often the case in low pay industries. The overall employment and hours structures are similar in the Living Wage and Minimum Wage establishments. The final row in Panel A also makes it very clear that living or minimum wage floors are a central part of the wage structure. Across all establishments there is a very sizable spike of 43 percent being paid the relevant wage floor among all core employment in The Company, and spikes of 46 percent and 37 percent respectively in the Living Wage and Minimum Wage establishments.

Panel B reports means of the three establishment performance metrics. The price index is very similar (near 5.7) across the two sets of establishments. Establishment closure is rare, with only 1 percent of establishments exiting over the sample period. The quality index, which has a range of ratings between 1 and 5, has a mean of 3.96, and is similar (at 3.89 and 4.13 respectively) across Living and Minimum Wage establishments.

The third Panel C looks at the two employment categories – entry and supervisor – reporting wages and employment for each. Around four fifths of core workers are entry

[23] This stood at £15.66 as calculated by the Annual Survey of Hours and Earnings.

workers, while the rest are supervisory and managerial employees further up the hierarchy. Across all establishments the entry level wage is low at £9.47 per hour, and the spike at the living/minimum wage is 55 percent. Supervisors are paid £13.19 on average, and are not paid at the wage floor. Within the two groups of establishments the concentrations of entry workers paid exactly at the wage floor are big - at 61 percent in the Living Wage establishments, and 45 percent in the Minimum Wage establishments.

Panel D gives detail on the pay and contract policies of The Company. On average has there are 10 unique pay points for all, comprising just below 6 for entry workers and just over 4 for supervisors. Interestingly, there are fewer pay points in the Living Wage establishments, and especially so for the entry group. In terms of contract policies, Panel D also shows promotion rates and incidence of zero-hour contracts. On the former, there is on average 0.2 promotions in an establishment per month, with this being distributed across entry and supervisors roughly in line with their relative employment proportions. The Company is a big user of zero-hour contracts, with this being the contract type for over half of all core workers, and 70 percent of the entry group being on a ZHC. Such very high rates are a prominent feature of the way that The Company organises its job contracts in both living and minimum wage establishments.

4. Results

4.1 Wage Distributions

The starting point of the empirical analysis is to look at wage impacts, and we begin with graphical representations of the distribution of worker hourly wages in The Company's establishments. Drawing out the before/after living wage effects is complicated by the staggered nature of treatment, so we show two sets. The first looks at all treatments in a single year, 2017, so a clear before/after effect can be shown. The second centres wages at the living wage for each associated year and shows the post-treatment wage distribution for all of the staggered treatments pooled together. In both, we show the distributions for establishments in London and in the Rest of the UK, owing to the fact that the living wage is different.

Figure 3 presents the wage histograms for all workers for establishments that received treatment in 2017, presenting both the pre and post living wage-treatment wage distributions for London and the Rest of the UK. Both pairs of figures demonstrate the striking impact of the living wage on wages – for the 2017 living wage adopters, prior to treatment there was no obvious spike at the living wage rate, but after treatment around 60-70 percent of workers were paid the living wage.

The living wage centred pooled counterpart for all years for establishments that had been treated are shown in Figure 4, again separately for London and non-London treated establishments. The wage distributions mirror that of the 2017 post treatment panels in Figure 3, with the living wage rate being both the median and modal rate, and show a large fraction of workers being paid exactly the living wage.

4.2 Wages and Employment

Figure 5 graphically reports the coefficients from the Sun and Abraham event study estimates of equation (2) (weights in Appendix equation A3.4), with log mean wages for all workers and showing the heterogenous affects across London and the rest of the UK. In all instances, there is a complete 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 a sharp, statistically significant rise in wages. The impacts are shown to be marginally higher for the rest of the UK than London, and very similar in magnitude in each of the four quarters post-living wage introduction.[24]

The counterpart estimates looking at the average long run impacts, as per specification (1) (weights in Appendix equation A3.6) are reported in Panel A of Table 2. Because of their constancy across the post-introduction quarters, they paint the same picture as the event study. In Panel A, the introduction of the Living Wage increases the wages within an establishment on average by 4.1-4.3 percent, in London by 3.2-3.5 percent and for outside of London by 4.9-5.1 percent. All estimates are unaffected by the inclusion of controls, and robustness checks using the BJS adjustment (Appendix equation A3.8) and the standard two way fixed effects (TWFE) estimator in specification (1) above are reported in Tables A2.1 and A2.2 in section A2 of the Appendix, which are highly similar to those reported in Table 2.[25]

Panel B of Table 2 reports estimates from specification (1) (with Appendix equation A3.6 weights) for log employment and Figure 6 reports the corresponding event study. The coefficient estimates for employment are always positive, at approximately 3 percent for all establishments, slightly lower for London (between 1.4 to 2.8 percent) which mirrors the slightly lower wage impact, and higher for the rest of the UK (between 5.1 and 5.3 percent), again mirroring the slightly higher wage impact. In statistical terms, the estimates for all and London are not significant, while estimates for the rest of the UK are on the bounds of significance. The confidence intervals for the event study shown in Figure 6 corroborate this.

[24] Hypothesis tests of parameter constancy in the four post-living wage introduction quarters produced p-values that are unable to reject the null that they are equal, thus showing that the wage boost is a one off increase occurring on introduction that stays at that increased level in the year after LW introduction.

[25] Using a fraction affected by the living wage measure, namely the share of workers paid beneath the living wage before its introduction, produced extremely similar results. The estimated coefficient (standard error) for the Panel A column (2) specification was 0.120 (0.016). The mean of the fraction affected variable is 0.391, producing a mean impact for treated establishments on wages of 0.120*0.391 = 0.047.

Estimates looking at the impact on hours do not fundamentally change the above picture. The impact on total hours is 0.033 (with associated standard error 0.020) without controls and 0.026 (0.017) with controls. These estimates, combined with the estimates on headcount employment, suggest no change of average worker hours. Thus, combining all the above results, this suggests the overall impact on the wage bill is 7.5 percent without controls (coefficient = 0.075, standard error = 0.019) and 6.4 percent with controls (coefficient = 0.064, standard error = 0.016).

Thus, there is evidence of a significant and sizeable wage boost for being made to comply with the living wage due to local governments becoming accredited living wage employers. But no evidence at all of disemployment effects in the aggregate. The signs on employment and hours run counter to that, and are not in line with there being any total labour demand adjustment to the imposition of a living wage above the mandated minimum wage.

4.2 Establishment Performance

The next set of results consider whether establishment performance took a hit from the wage cost shock induced by living wage introduction. They unambiguously suggest this is not the mode of adjustment that The Company used to try and absorb the cost increase.

Table 3 shows estimates on consumer price-pass through, the probability of establishment exit, and quality as measured by google reviews. All estimates are very small in magnitude, none are statistically different from zero and relatively precisely estimated. Column (1) shows the introduction of the living wage to be associated with a 0.7 percent drop in consumer prices. Columns (2) and (3) suggest that the living wage increased the probability of establishment market exit by 0.1 percent. Column (4) presents the unweighted quality results, while column (5) presents them weighted by establishment (so as to mirror the typical establishment level estimates, and to ensure establishments with more reviews do not get overrepresented). They suggest that quality dropped by 0.028-0.038, when marked on a scale of 1-5. As shown in Table 1, the average establishment rating is 3.96, which suggests the above impact implies a tiny reduction of less than 1 percent.

4.3 Labour-Labour Substitution

We next consider estimates for the two groups of core workers. Figure 7 begins by plotting the wage distributions for all post treatment establishments, for the full sample period, pooled and centred around the year specific living wage rate, now being split for entry workers and supervisors. They show that the living wage has an incredibly strong bite for entry workers, with 70 percent of them paid exactly the living wage post treatment. On the other hand, there is no bite at all for supervisors whose wage rates are typically above the living wage.

This is further drawn out in the event study plot in Figure 8, which breaks down wage impacts for entry workers and supervisors. The impact of the living wage on entry workers is immediate and sizeable, with no pre-trends for all groups, and again relatively constant in magnitude in the post-introduction quarters. Table 4 Panel A columns (1-4), presenting the difference-in-differences counterparts, shows entry workers experienced a strongly significant 6.5 percent wage increase, while supervisors experienced a 0.8 percent increase which was not statistically significant. Figure 9 and Table 4 Panel B columns (1-4) present the equivalent employment impacts. Both demonstrate evidence of labour-labour substitution, with entry worker employment seeing gains of 4-5 percent while supervisors see employment reductions of approximately 4 percent.

Figure 10 and columns (5-6) in Table 4 Panels A and B combine these results to get the relative impacts with $log\left(\frac{W_e}{W_s}\right)$ and $log\left(\frac{L_e}{L_s}\right)$ as the dependent variables. Both see a sizeable positive consistent increase following introduction of the living wage. As before the hypothesis of parallel pre-trends between treated and untreated centres cannot be rejected. The results suggest that the Living Wage introduction increased entry level wages relative to supervisors and managers wages by 5.9 percent while the employment composition of entry level to supervisory and managerial workers increased by 8.2 percent. Combining the impacts from columns (1-4) one can see the entry wage bill increased by 12.1 percent (coefficient = 0.121, standard error = 0.027) when estimated without controls while supervisors wage bill reduced by 3.1 percent (coefficient = 0.031, standard error = 0.019), with the latter driven by the reduction in supervisor employment.

These results very clearly demonstrate the presence of monopsony power in the labour market, in line with the model presented in section 3.2.2. In a perfectly competitive setting, we would expect an increase in the relative wage to result in a weak decrease (with magnitude depending on the elasticity of substitution) in the relative employment composition. However, it is evident that labour supply response dominates the demand response, resulting in an increase in the composition of entry to supervisory workers.

The reduced form results coupled with equation (5) suggest that $-\log(\mu_e) > 0.059$. Results from Datta (2023) utilising the same living wage treatment and information on recruitment and separations estimates the degree of monopsony power for entry workers, finding that $\mu_e = 0.82$. Using this estimate implies that $\sigma = 0.58$, suggesting that workers at different points of the establishment hierarchy are gross complements and there is not strong substitutability between the two. Estimates are robust to inclusion of controls and estimation approach, as seen in Tables A2.4 and A2.5 in the Appendix.[26]

[26] One might wonder, given these results, whether there is a quality adjustment to entry workers supplying their labour after the introduction of the living wage. For example, efficiency wage theory would suggest that entry workers may become more productive with higher wages, and potentially require less supervision as they are less prone to "shirk work". It is not straightforward to assess this margin as we have no direct measure of productivity. However, in Datta (2023) he examines the impact of the living wage on absenteeism using data from The Company and finds no evidence of a reduction in absenteeism. That said there may be other efficiency wage margins that raise quality of new hires (see, for example, Cappelli and Chauvin, 1991).

4.4 Wage and Contract Policies

The rich HR data from The Company enables us to consider whether, in addition to the labour-labour substitution just described, a margin of adjustment could be through altering wage and/or contract policies. On company wage policy, Figure 11 graphically reports the event study estimates from specification (2) (Appendix equation A3.4 weights) using the number of unique pay points within an establishment for all, entry and supervisor workers as the dependent variable respectively. As in the case of wages there is an absence of any obvious differing pre-trends, and immediately following the introduction of the Living Wage there is a sharp and consistent reduction in the number of pay points.

Estimates in Table 5 present the counterpart averaged estimates from specification (1) (A3.6 weights). The estimates suggest that establishments exposed to the Living Wage reduced the number of unique pay rates by approximately 1.6 for all workers, with 1.3 of that reduction coming from entry worker pay points, and the remainder coming from supervisors. Given the average number of pay points in an establishment was 11.4, the reduction in the specification for all workers represents an almost 15 percent reduction in pay points. All estimates are strongly statistically significant. It is interesting to note here that not only entry workers' wages were directly affected by the living wage, but affected establishments changed the structure of the pay structure for supervisors. As before estimates are robust to the inclusion of controls, and estimation approach as seen in tables A2.7 and A2.8 in the Appendix.

Table 6 considers promotions as a contract policy outcome of interest. It shows estimates of specification (1) for the number of promotions (measured in levels[27]) for all, entry and supervisor workers. All coefficient estimates are small and not statistically different from zero. Estimates on all workers is 0.014 to 0.018 extra promotions a month, relative to a baseline

[27] As many establishments do not promote workers every month using a dependent variable specified in logs was not possible, and using alternatives have well documented issues (Thakral and Tô, 2023).

of 0.33. Table 7 reports estimates for our last contract policy, namely utilisation of zero-hour contracts (specifically the proportion of workers on zero-hour contracts), an atypical work contract which allows greater hours flexibility in principle for both workers and firms. Results suggest relatively precise zero impacts across all workers. While the impact on all workers is around 1 percentage point, this is most likely driven by the change in employment composition towards entry workers who have a much higher propensity for being on zero-hour contracts.

4.5 Consequences for Establishment Wage Inequality

The evidence strongly suggests that establishments of The Company were able to fully absorb the government contract induced imposition of living wages > minimum wages by adjusting wage and employment relativities between their two core groups of workers and by making their pay policy more restrictive in terms of numbers of pay points available for their workers. This was facilitated by the monopsony power they were able to exert over the pay and employment contracts of their workforce.

The bottom line consequence was that within establishment wage inequality was sharply reduced. Figure 12 and Table 8 contain results showing this for measures of wage inequality - the standard deviation of log wages within the establishment, and three percentile wage ratios, the 90-10, 90-50 and 50-10. Figure 12 presents event study estimates, and the results are striking. There are no discernible pre-trends in the four quarters before living wage introduction, and sharp and sizeable reductions directly after. Table 8 presents the difference-in-differences counterparts. All inequality reductions are sizeable relative to the mean, but with the largest relative drop in the bottom half of the wage distribution as the 50-10 ratio drops by 28.5 percent relative to its pre-treatment mean. Of course, this large drop is driven by the fact that the median worker in the firm is an entry worker, and post treatment the abundance of these workers are paid exactly the same living wage rate as seen in the wage distributions in Figure 7.

The living wage introduction thus caused The Company to pay more entry workers the same wage, significantly compressing the within-establishment wage structure, as they altered their pay policies to absorb the wage cost shock from living wage introduction. Indeed, as the bars in Figure 13 show, after living wage introduction 35 percent of establishments had a 50-10 ratio of unity as the median wage was exactly the same wage as the 10^{th} percentile. This went up 26 percentage points from 9 percent pre-LW introduction, corresponding to a huge 288 percent increase. The wage structure was hugely compressed by the living wage. All of this took place for entry workers, as shown in the middle bar chart in Figure 13, where post-living wage introduction 46 percent of establishments paid their median entry worker exactly the same hourly wage rate as the 10^{th} percentile (up from 20 percent). No such increase occurred for supervisors. The regression estimates that underpin Figure 13, with and without controls, are shown in Appendix Table A2.15, along with comparable specifications for other parts of the within-establishment wage distribution, namely the 25-10, 75-10 and 90-10 wage ratios. They very clearly corroborate, with some variations across the distribution arising from differing initial pre-living wage introduction wage structures, that the strong narrowing of wage inequality was accompanied by a significant increase in paying entry workers the same hourly wage rate.

4.6 Robustness

Results Tables in sections A2 of the Appendix report estimates of $\hat{\hat{v}}$ from specification (A3.8), the BJS adjustment to the Sun and Abraham style difference-in-differences specifications, as well as estimates from the traditional TWFE difference-in-differences specification (1). All results turn out to be quantitatively and qualitatively similar, and the findings of the empirical analysis throughout the paper are fundamentally unchanged.

5. Conclusions

This paper studies the impact of living wages > minimum wages on workers and firms. It exploits exogenous raises of establishment-level wage floors to living wage levels that are considerably higher than mandated minimum wages when local governments become accredited living wage employers as part of their ESG activities. When a local government in the UK signs up to become a living wage employer, as a significant number did in the period we study, firms that have procurement contracts with them have to pay workers the living wage. This variation is studied with rich matched data on workers in establishments for a service sector company with many establishments located across the country. Just under half of the firm's establishments were made to comply with the living wage as a consequence of the local government becoming a living wage employer in the period between 2011 and 2019.

In a staggered difference-in-differences research design, low wage workers are shown to receive a significant wage boost from the living wage introduction as a very sizeable number of their workforce are paid at exactly the living wage after its introduction. Consistent with a model of monopsony power and where bottom-of-the-rung workers and supervisors are gross complements, the living wage induced labour-labour substitution in favour of the former. Further adjustment to the wage bill increases from the introduction of the living wage took place through within-establishment internal changes to the establishment pay policy structures. The overall result was that The Company, owing to the monopsony power they were able to exert over the pay and employment contracts of their workforce, was able to fully absorb the wage cost shock embodied in living wage adoption through within establishment employment reorganisation and reform of their company pay policy. This came without negative consequences for total labour demand nor establishment performance, but in a way that significantly compressed establishment wage inequality.

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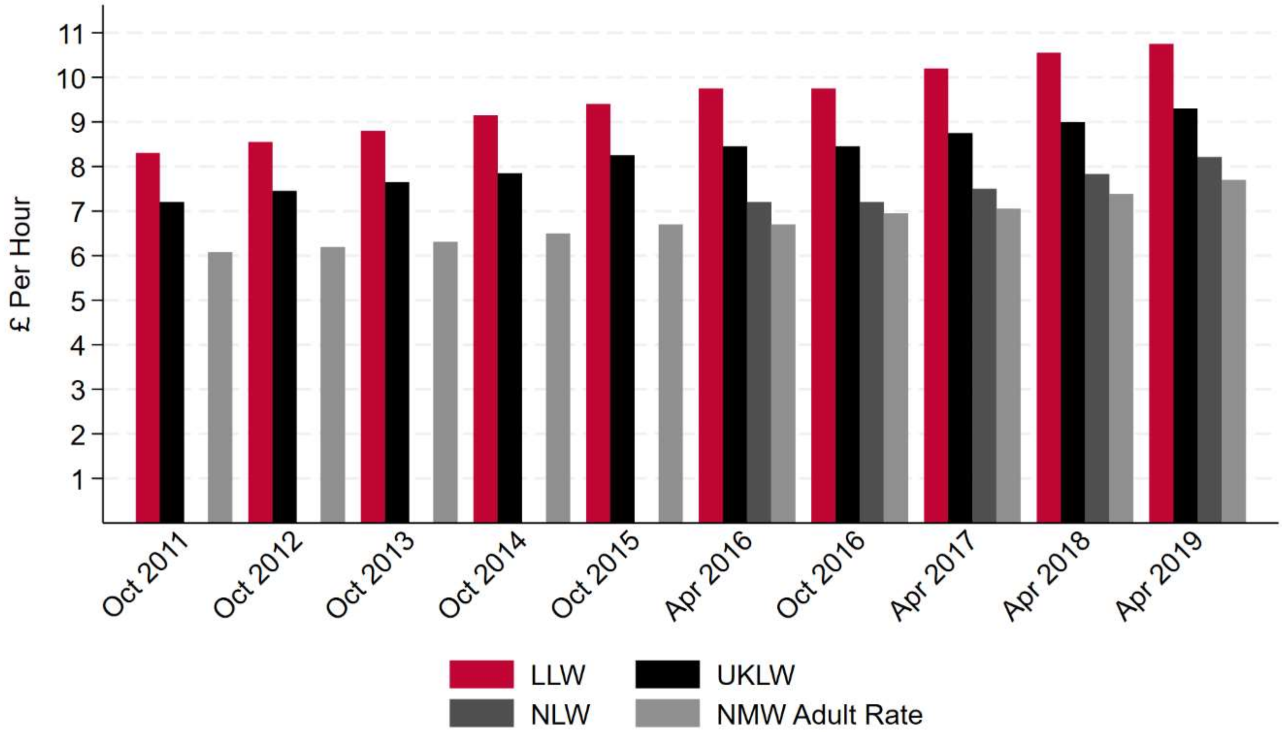
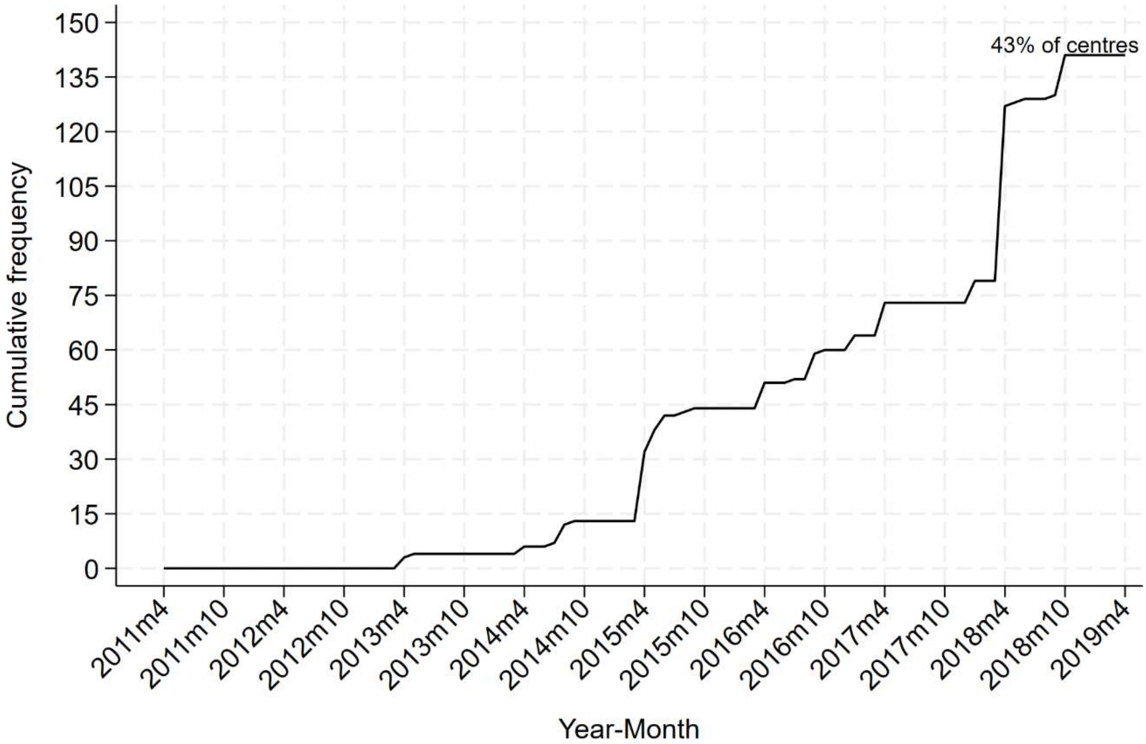
Figure 1: UK Living and Minimum Wage Rates



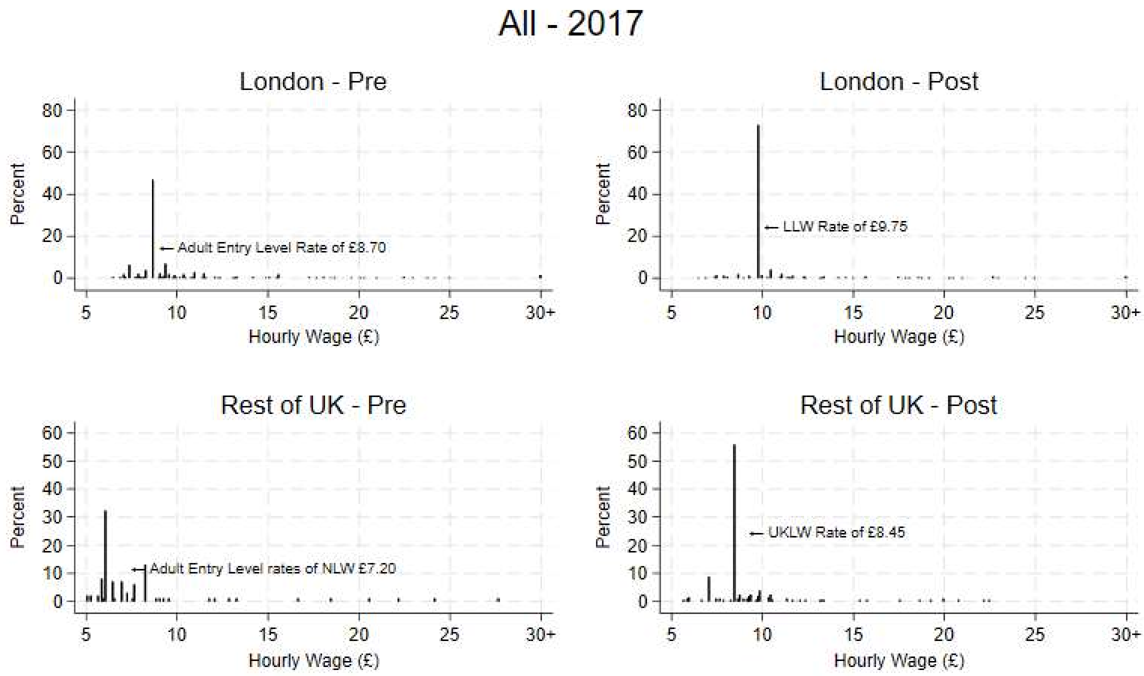
Notes: The figure presents hourly pay rates for the London Living Wage (LLW), UK Living Wage (UKLW), National Living Wage (NLW) and the National Minimum Wage (NMW Adult Rate) for 2011 to 2019. The National Living Wage was introduced in April 2016 for workers aged 25 and over, so from then onwards the NMW adult rate is for 21-24 year olds.

Figure 2: Living Wage Treatments in The Company, 2011 to 2019



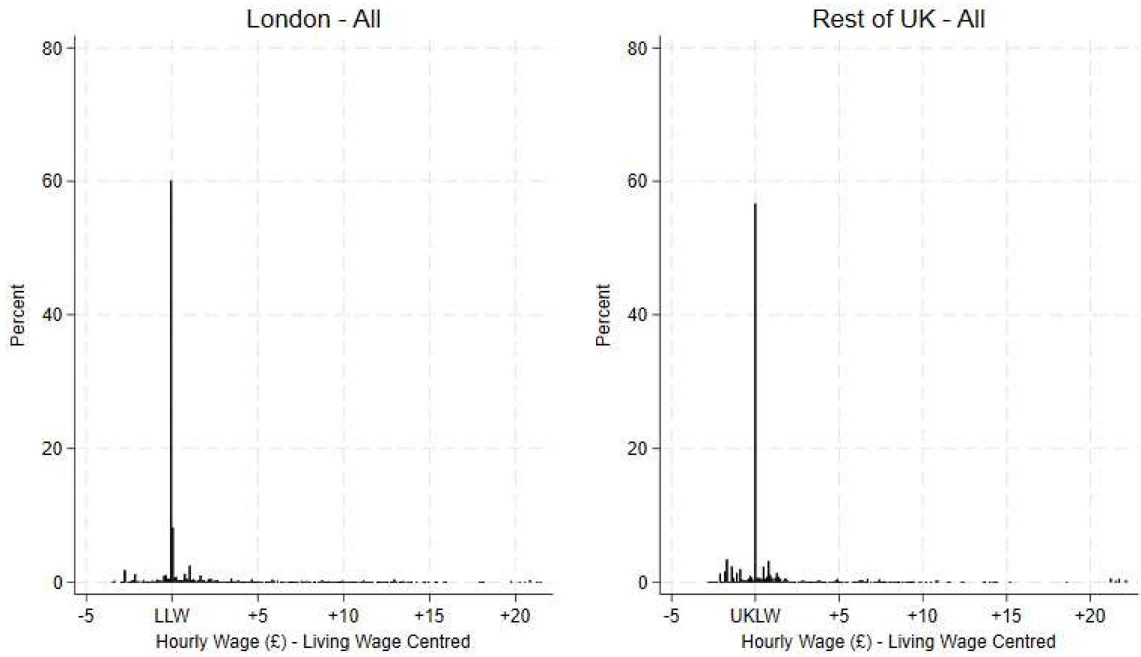
Notes: The figure reports the cumulative number of newly treated establishments over time. The figure only includes establishments which were treated while run by The Company. Some establishments were already subjected to the Living Wage when taken over by The Company in the 2011 to 2019 time window.

Figure 3: Wage Distributions Pre/Post Treatment, All – 2017



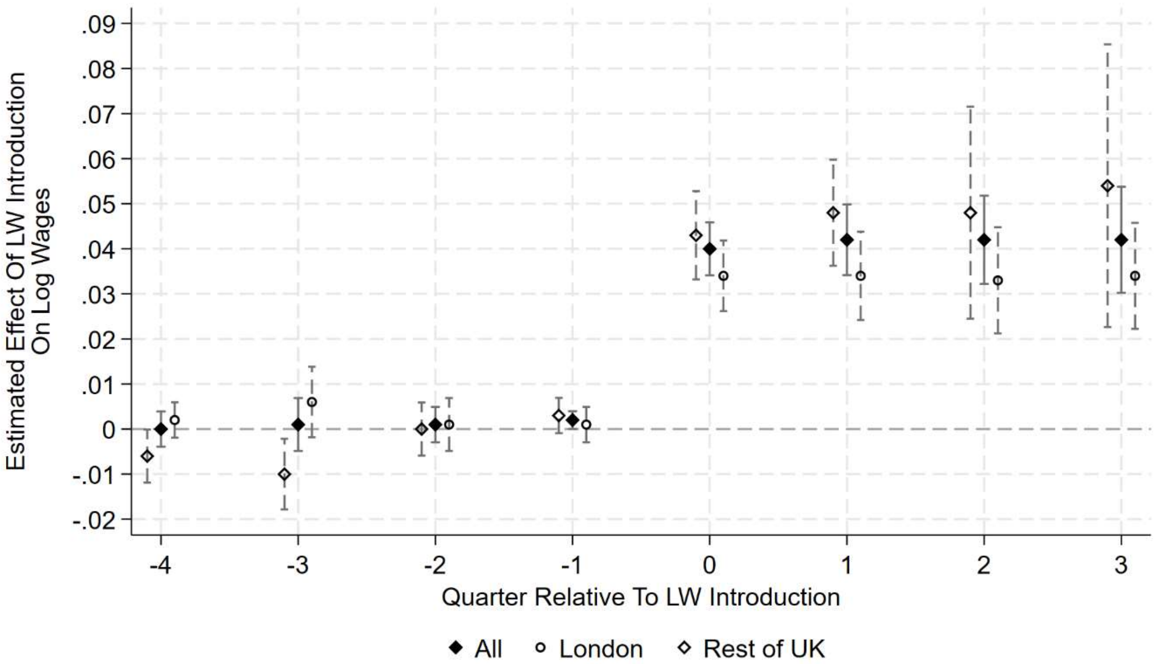
Notes: The graph shows pre and post treatment hourly wage histograms for all core workers in 21 treatments occurring in 2017, 16 in London and 5 in the Rest of the UK.

Figure 4: Wage Distributions Post Treatment, All – 2011 to 2019, Living Wage Centred



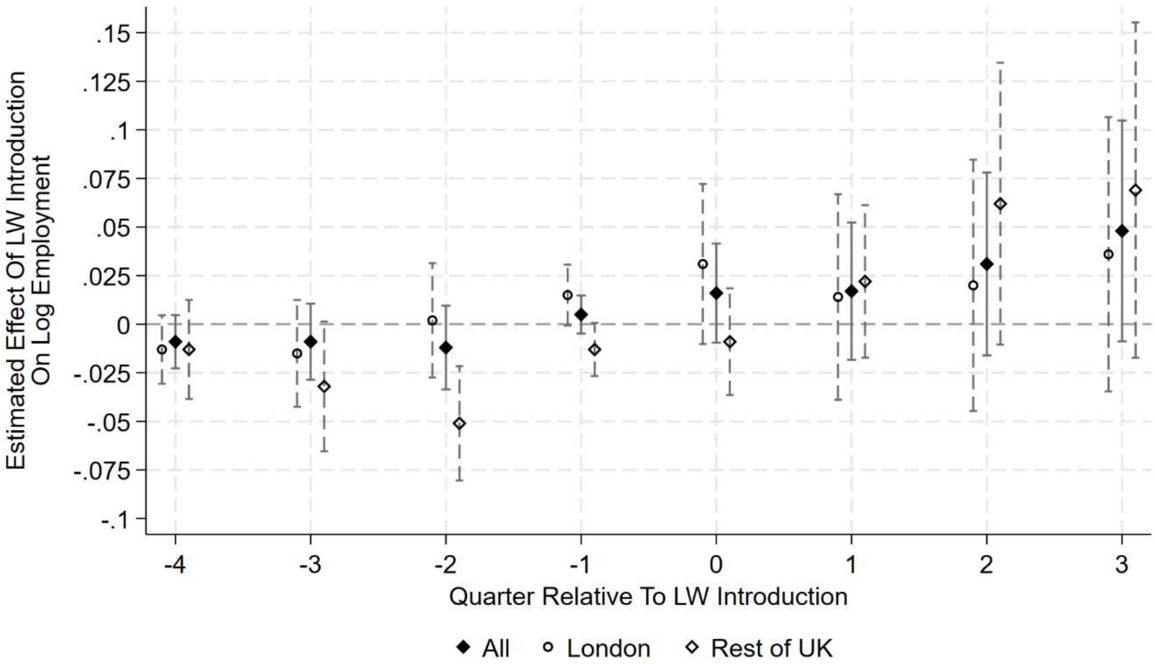
Notes: The graph shows post treatment hourly wage histograms for all core workers, centred at the relevant Living Wage, in 141 treatments between 2011 and 2019, 102 in London and 39 in the Rest of the UK.

Figure 5: Wages Event Study, All, By London and Rest of UK



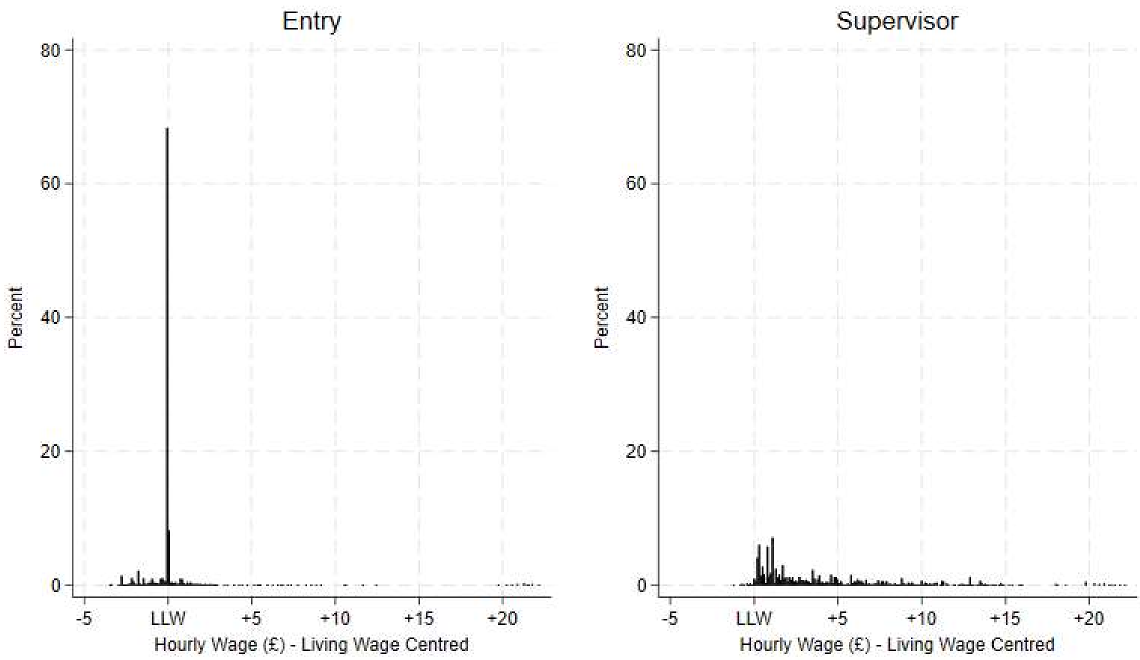
Notes: The graph reports the estimated coefficient $\widehat{v_g}$ from the Sun and Abraham event study model (A3.4) without controls. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. The vertical bars indicate 95 percent confidence intervals based on bootstrapped standard errors clustered at the establishment.

Figure 6: Employment Event Study, All, By London and Rest of UK



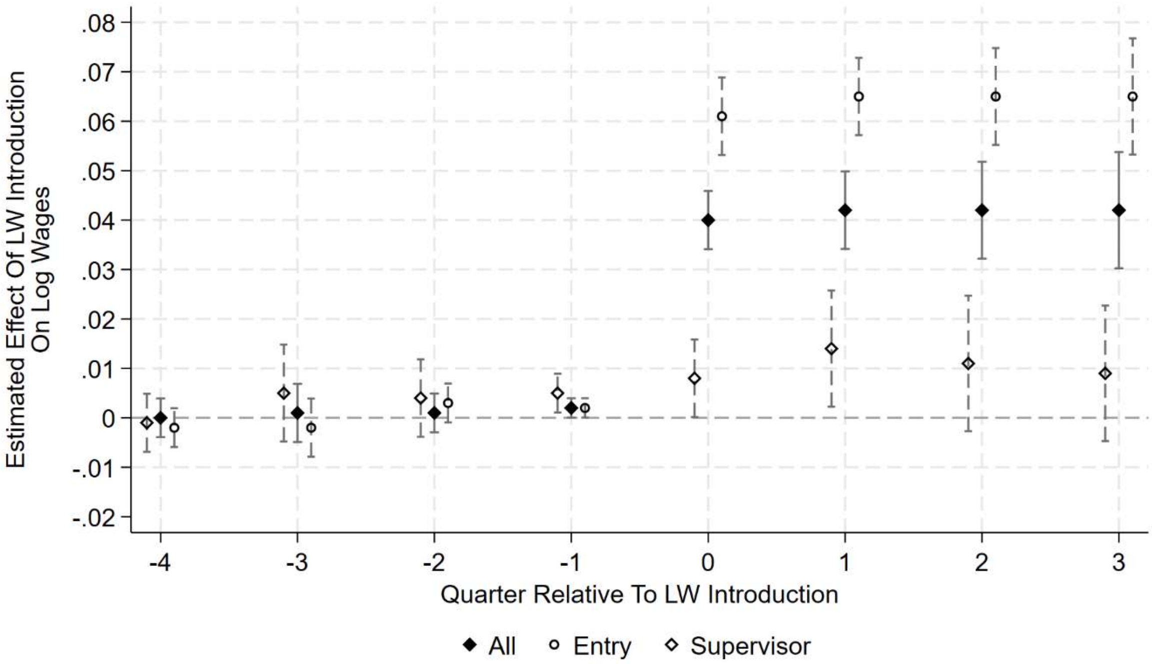
Notes: The graph reports the estimated coefficient $\widehat{v_g}$ from the Sun and Abraham event study model (A3.4) without controls. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. The vertical bars indicate 95 percent confidence intervals based on bootstrapped standard errors clustered at the establishment.

Figure 7: Wage Distributions Post Treatment, Entry and Supervisor – 2011 to 2019, Living Wage Centred



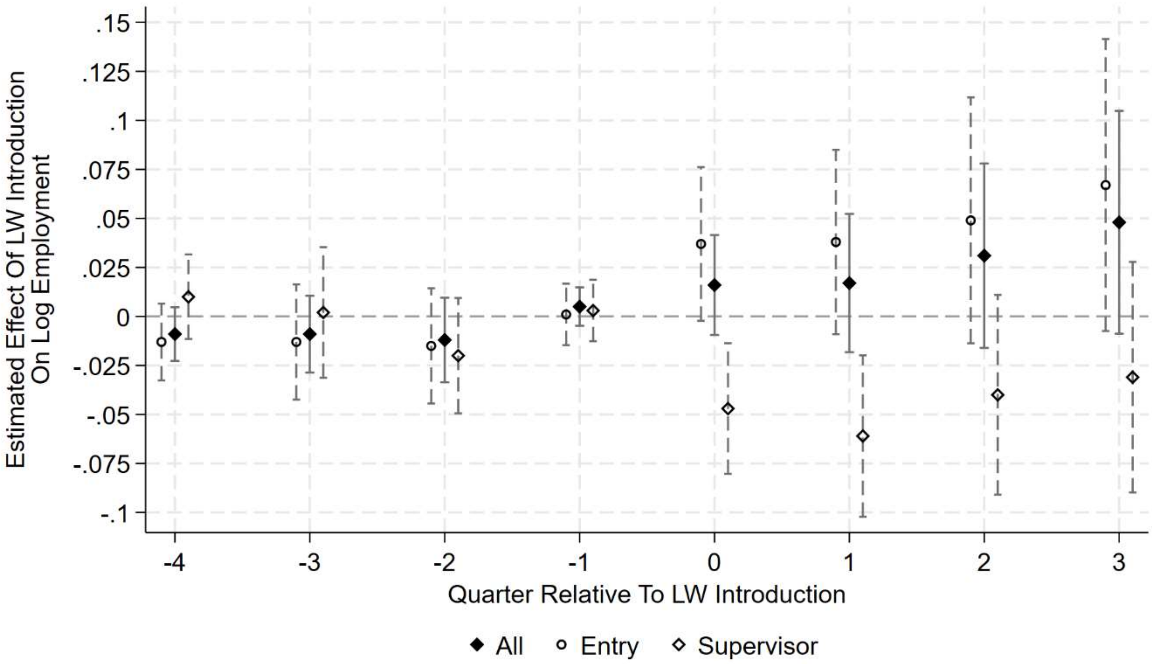
Notes: The graph shows post treatment hourly wage histograms for entry workers and supervisors, centred at the relevant Living Wage, in 141 treatments occurring between 2011 and 2019, 102 in London and 39 in the Rest of the UK.

Figure 8: Wages Event Study, By Worker Type



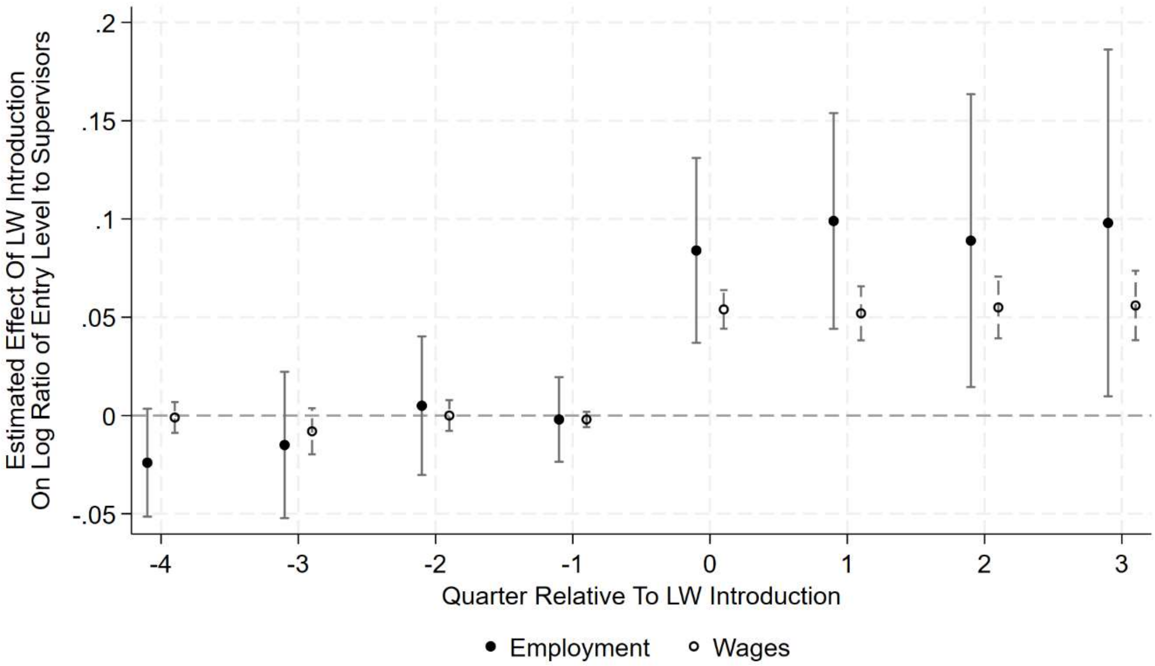
Notes: The graph reports the estimated coefficient $\widehat{v_g}$ from the Sun and Abraham event study model (A3.4) without controls. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. The vertical bars indicate 95 percent confidence intervals based on bootstrapped standard errors clustered at the establishment.

Figure 9: Employment Event Study, By Worker Type



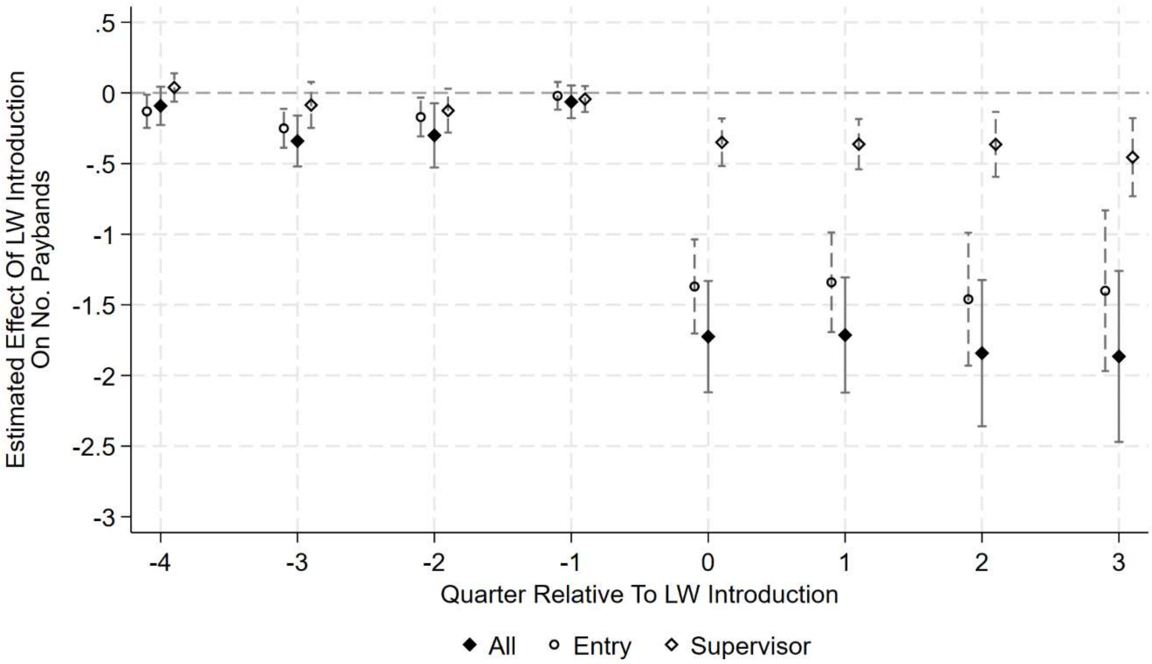
Notes: The graph reports the estimated coefficient $\widehat{v_g}$ from the Sun and Abraham event study model (A3.4) without controls. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. The vertical bars indicate 95 percent confidence intervals based on bootstrapped standard errors clustered at the establishment.

Figure 10: Labour-Labour Substitution Event Study



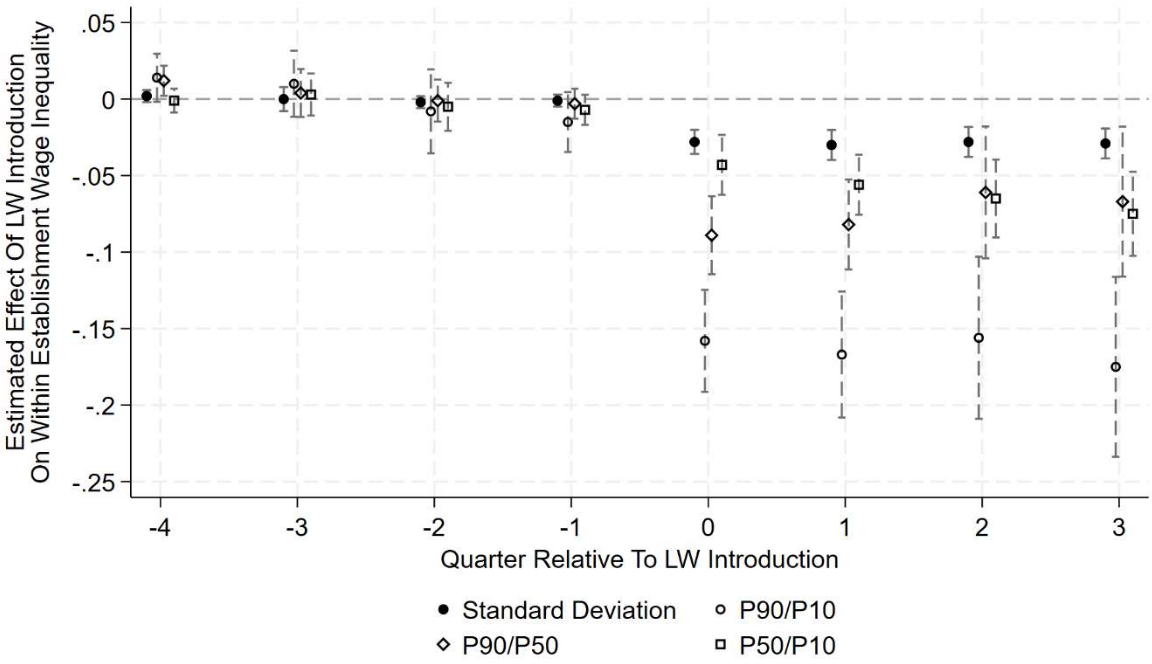
Notes: The graph reports the estimated coefficient $\widehat{\nu_g}$ from the Sun and Abraham event study model (A3.4) without controls. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. The vertical bars indicate 95 percent confidence intervals based on bootstrapped standard errors clustered at the establishment.

Figure 11: Pay Policy Event Study



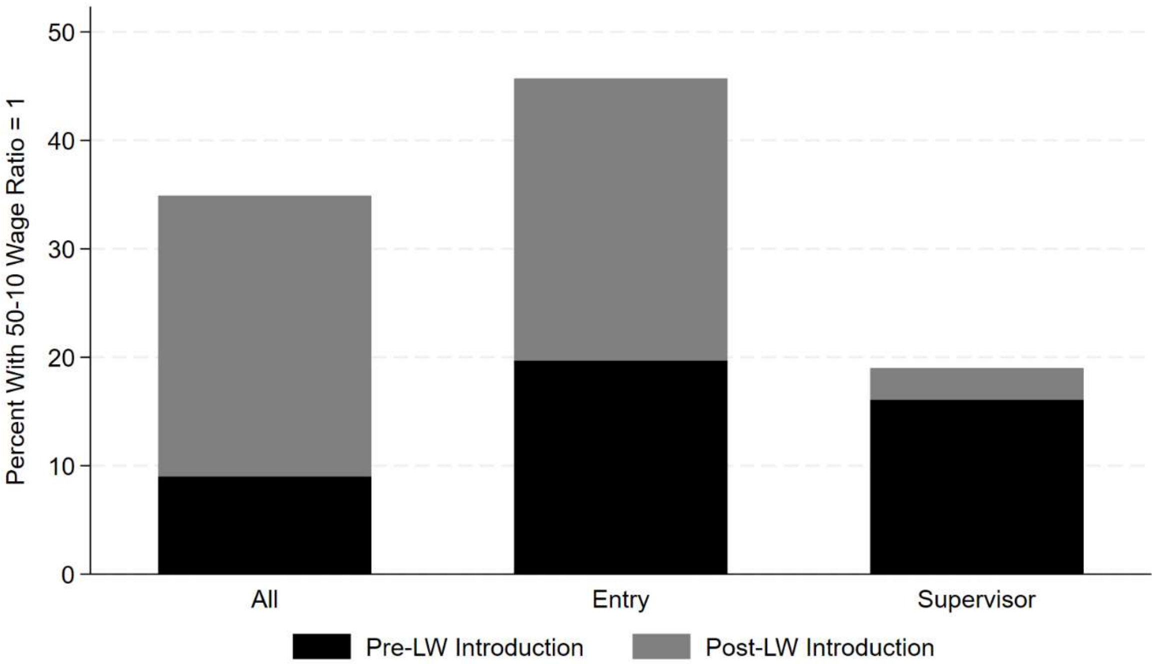
Notes: The graph reports the estimated coefficient $\widehat{v_g}$ from the Sun and Abraham event study model (A3.4) without controls. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. The vertical bars indicate 95 percent confidence intervals based on bootstrapped standard errors clustered at the establishment.

Figure 12: Wage Inequality Event Study



Notes: The graph reports the estimated coefficient $\widehat{v_g}$ from the Sun and Abraham event study model (A3.4) without controls. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. The vertical bars indicate 95 percent confidence intervals based on bootstrapped standard errors clustered at the establishment.

Figure 13: 50-10 Wage Ratios = 1, Before and After LW-Introduction



Notes: Estimates taken from Appendix Table A2.15 (specifications with no controls).

Table 1: Summary Statistics

	All	Living Wage	Minimum Wage
A. Wages and employment			
Hourly wage	10.47	10.72	10.00
Employees	31.90	33.69	28.53
Weekly hours	21.02	21.25	20.59
Percent paid living or minimum wage	0.43	0.46	0.37
B. Establishment performance			
Price index	5.73	5.75	5.68
Exit	0.01	0.01	0.02
Rating	3.96	3.89	4.13
C. Relative employment			
Entry hourly wage	9.47	9.81	8.84
Entry employees	25.99	27.56	23.04
Entry percent paid living or minimum wage	0.55	0.61	0.45
Supervisor hourly wage	13.19	13.52	12.57
Supervisor employees	5.91	6.14	5.48
Supervisor percent paid living or minimum wage	0.01	0.01	0.02
D. Wage and contract policies			
Number of pay points	9.95	9.65	10.52
Entry number of pay points	5.82	5.40	6.61
Supervisor number of pay points	4.13	4.25	3.91
Promotions	0.20	0.20	0.21
Entry promotions	0.15	0.15	0.14
Supervisor promotions	0.05	0.05	0.06
Zero-hour contracts	0.55	0.59	0.48
Entry zero-hour contracts	0.70	0.76	0.59
Supervisor zero-hour contracts	0.11	0.09	0.15
Number of establishments	334	218	116

Notes: The Table reports the mean values for a set of establishment level variables for 334 establishments active in our sample as of September 2018. The first column reports values for the full sample, the second for establishments where the living wage is the relevant binding wage floor and the third for the establishments where the minimum wage is the relevant binding wage floor.

Table 2: Wages and Employment

	Wages and Employment					
	All		London		Rest of UK	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Log (W_{it})						
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	0.041	0.043	0.032	0.035	0.051	0.049
	(0.004)	(0.004)	(0.006)	(0.006)	(0.013)	(0.013)
B. Log (L_{it})						
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	0.034	0.021	0.028	0.014	0.053	0.051
	(0.020)	(0.017)	(0.031)	(0.025)	(0.028)	(0.028)
Controls	No	Yes	No	Yes	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	17879	17879	11951	11951	5928	5928

Notes: The Table reports the estimated coefficient $\hat{\upsilon}$ from the Sun and Abraham difference-in-differences model (A3.6) without controls in columns (1), (3) and (5) and with controls in columns (2), (4) and (6). The dependent variables are log wages and log employment for all establishments and subgroups of London and non-London establishments. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

Table 3: Establishment Performance

	Establishment Performance				
	Log(Prices)	Pr(Exit)		Quality (1-5)	
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}[LW > MW]_{it}$	-0.007	0.001	0.002	-0.028	-0.038
	(0.006)	(0.002)	(0.002)	(0.043)	(0.045)
Controls	No	No	Yes	No	No
Weighting	No	No	No	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Sample size	9313	17879	17879	101461	101461

Notes: The Table reports the estimated coefficient $\hat{v}$ from the Sun and Abraham difference-in-differences model (A3.6). The dependent variables log output prices, a binary indicator for exit and quality as measured by google reviews. Columns (1)-(2) and (4) are without controls, column (3) includes controls, and column (5) is weighted at the establishment-year month level. The sample is a panel of establishments run by The Company. Column (1) uses a sample from 2016 to 2023, columns (2) and (3) the same sample active between 2011-2019 as in Table 2, columns (4) and (5) from 2011-2023. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

Table 4: Wages and Employment, By Worker Type

	Wages and Employment					
	Entry		Supervisor		Entry/Supervisor	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Log (W_{it})						
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	0.065	0.066	0.008	0.008	0.058	0.059
	(0.004)	(0.004)	(0.005)	(0.006)	(0.007)	(0.007)
B. Log (L_{it})						
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	0.056	0.039	-0.039	-0.043	0.095	0.082
	(0.028)	(0.023)	(0.019)	(0.020)	(0.029)	(0.031)
Controls	No	Yes	No	Yes	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	17879	17879	17879	17879	17879	17879

Notes: The Table reports the estimated coefficient $\hat{\upsilon}$ from the Sun and Abraham difference-in-differences model (A3.6) without controls in columns (1), (3) and (5) and with controls in columns (2), (4) and (6). The dependent variables are log wages and log employment (as measured by positions) for all workers and subgroups of entry workers and supervisors. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

Table 5: Wage Policy - Pay Points

	Wage Policy – Pay Points					
	All		Entry		Supervisor	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	-1.544	-1.591	-1.252	-1.280	-0.292	-0.311
	(0.218)	(0.219)	(0.188)	(0.182)	(0.110)	(0.121)
Controls	No	Yes	No	Yes	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	17879	17879	17879	17879	17879	17879
Mean of dependent variable	11.4		6.7		4.7	

Notes: The Table reports the estimated coefficient $\hat{v}$ from the Sun and Abraham difference-in-differences model (A3.6) with and without controls. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. The dependent variables are number of pay points for all workers and subgroups of entry workers and supervisors. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

Table 6: Contract Policy - Promotion

	Contract Policy – Promotion					
	All		Entry		Supervisor	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	0.018	0.014	0.044	0.040	-0.026	-0.026
	(0.050)	(0.047)	(0.034)	(0.035)	(0.024)	(0.027)
Controls	No	Yes	No	Yes	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	17879	17879	17879	17879	17879	17879
Mean of dependent variable	0.33		0.24		0.09	

Notes: The Table reports the estimated coefficient $\hat{\upsilon}$ from the Sun and Abraham difference-in-differences model (A3.6) with and without controls. The dependent variables are number of promotions for all workers and subgroups of entry workers and supervisors. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

Table 7: Contract Policy – Zero Hour Contracts

	Contract Policy – Zero Hour Contracts					
	All		Entry		Supervisor	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}[LW > MW]_{it}$	0.011 (0.008)	0.007 (0.007)	0.000 (0.009)	-0.003 (0.008)	0.004 (0.008)	0.004 (0.008)
Controls	No	Yes	No	Yes	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	17879	17879	17879	17879	17879	17879
Mean of dependent variable	0.56		0.72		0.07	

Notes: The Table reports the estimated coefficient $\hat{v}$ from the Sun and Abraham difference-in-differences model (A3.6) with and without controls. The dependent variables are proportion of zero-hour contracts for all workers and subgroups of entry workers and supervisors. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

Table 8: Wage Inequality

	Establishment Wage Inequality							
	Standard Deviation		90-10		90-50		50-10	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	-0.030 (0.005)	-0.032 (0.005)	-0.164 (0.018)	-0.163 (0.018)	-0.077 (0.010)	-0.075 (0.013)	-0.056 (0.009)	-0.057 (0.010)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	17879	17879	17879	17879	17879	17879	17879	17879
Mean of dependent variable	0.22		1.62		1.39		1.16	
Percent inequality reduction	12.5	13.2	22.8	22.6	17.7	17.2	28.1	28.7

Notes: The Table reports the estimated coefficient $\hat{\upsilon}$ from the Sun and Abraham difference-in-differences model (A3.6) with and without controls. The dependent variables are the within establishment standard deviation for wages, the 90-10, 90-50 and 50-10 ratio of wage percentiles. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age. The percent inequality reduction is calculated as the estimated inequality reduction divided by the pre-introduction means (X 100) where the relevant means are: standard deviation, 0.24; 90-10, 90-50 and 50-10 differences of 0.72, 0.44 and 0.20.

Appendix

A1 Additional Figures

Figure A1.1: Example of Contract Clause

SAMPLE CONTRACT CLAUSES

1. London Living Wage

1.1. Definitions

For the purposes of this Clause:

"Relevant Staff"	shall mean all employees and other staff (including without limitation temporary and casual workers and agency staff as defined by Regulation 3 of the Agency Workers Regulations 2010 as amended by the Agency Workers (Amendment) Regulations 2011, and whether such staff are engaged or employed on a full or part time basis, but not including unpaid volunteers, interns or apprentices), who are employed or engaged on the [Works or Services] for 2 or more hours of work in any given day in a week, for 8 or more consecutive weeks in a year.
"Equivalent Hourly Wage"	shall mean the hourly wage paid to an employee and calculated using the same method as prescribed by the National Minimum Wage Act 1998 and related applicable law to assess whether an employee is at any time receiving the national minimum wage (as identified in that Act),
"the London Living Wage"	shall mean the most recently identified London Living Wage hourly figure (or equivalent set figure(s)) published from time to time by the Greater London Authority or any successor body with responsibility for setting this figure,

1.2. Contractors obligations

The Contractor will:

- ensure that all Relevant Staff employed or engaged by the Contractor are paid an Equivalent Hourly Wage which is equal to or exceeds the London Living Wage;
- ensure that all Relevant Staff employed or engaged by its subcontractors (if any) pay an Equivalent Hourly Wage which is equal to or exceeds the London Living Wage;
- provide to the Employer such information concerning the London Living Wage and the performance of its obligations under this Clause [ ] as the Employer may reasonably require and within the deadlines it reasonably imposes;
- co-operate and provide all reasonable assistance to the Employer in monitoring the effects of the London Living Wage including without limitation assisting us in conducting surveys and assembling data in respect of the affect of payment of London Living Wage to Relevant Staff.

1.3. Default

1.3.1. For the avoidance of doubt, any breach by the Contractor of this Clause [ ] may be a material breach in relation to which the Employer is entitled to rely on its termination rights under the Contract.

A2 Additional Tables

Table A2.1: Wages and Employment, SA-BJS

	Wages and Employment					
	All		London		Rest of UK	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Log (W_{it})						
$\mathbb{1}[LW > MW]_{it}$	0.038	0.040	0.031	0.034	0.042	0.040
	(0.003)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)
B. Log (L_{it})						
$\mathbb{1}[LW > MW]_{it}$	0.035	0.233	0.027	0.014	0.057	0.056
	(0.018)	(0.016)	(0.029)	(0.023)	(0.019)	(0.019)
Controls	No	Yes	No	Yes	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	17879	17879	11951	11951	5928	5928

Notes: The Table reports the estimated coefficient $\hat{\hat{v}}$ from the Sun and Abraham-BJS difference-in-differences model (A3.8) without controls in columns (1), (3) and (5) and with controls in columns (2), (4) and (6). The dependent variables are log wages and log employment for all establishments and subgroups of London and non-London establishments. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

Table A2.2: Wages and Employment, TWFE

	Wages and Employment					
	All		London		Rest of UK	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Log (W_{it})						
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	0.051	0.054	0.047	0.051	0.061	0.061
	(0.007)	(0.007)	(0.009)	(0.009)	(0.014)	(0.013)
B. Log (L_{it})						
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	0.046	0.038	0.039	0.031	0.056	0.052
	(0.027)	(0.238)	(0.033)	(0.030)	(0.037)	(0.035)
Controls	No	Yes	No	Yes	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	17879	17879	11951	11951	5928	5928

Notes: The Table reports the estimated coefficient δ from the TWFE difference-in-differences model (1) without controls in columns (1), (3) and (5) and with controls in columns (2), (4) and (6). The dependent variables are log wages and log employment for all establishments and subgroups of London and non-London establishments. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

Table A2.3: Establishment Performance, SA-BJS

	Establishment Performance				
	Log(Prices)	Pr(Exit)		Quality (1-5)	
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}[LW > MW]_{it}$	-0.007	0.001	-.001	-0.015	-0.035
	(0.006)	(.002)	(.004)	(0.042)	(0.044)
Controls	No	No	Yes	No	No
Weighting	No	No	No	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Sample size	9313	17879	17879	101461	101461

Notes: The Table reports the estimated coefficient $\hat{\tilde{v}}$ from the Sun and Abraham-BJS difference-in-differences model (A3.8). The dependent variables log output prices, a binary indicator for exit and quality as measured by google reviews. Columns (1)-(2) and (4) are without controls, column (3) includes controls, and column (5) is weighted at the establishment-year month level. The sample is a panel of establishments run by The Company. Column (1) uses a sample from 2016 to 2023, columns (2) and (3) the same sample active between 2011-2019 as in Table 2, columns (4) and (5) from 2011-2023. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

Table A2.4: Establishment Performance, TWFE

	Establishment Performance				
	Log(Prices)	Pr(Exit)		Quality (1-5)	
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}[LW > MW]_{it}$	-0.026	-0.001	-0.001	-0.020	-0.022
	(0.012)	(0.002)	(0.002)	(0.029)	(0.035)
Controls	No	No	Yes	No	No
Weighting	No	No	No	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Sample size	9313	17879	17879	101461	101461

Notes: The Table reports the estimated coefficient δ from the TWFE difference-in-differences model (1). The dependent variables log output prices, a binary indicator for exit and quality as measured by google reviews. Columns (1)-(2) and (4) are without controls, column (3) includes controls, and column (5) is weighted at the establishment-year month level. The sample is a panel of establishments run by The Company. Column (1) uses a sample from 2016 to 2023, columns (2) and (3) the same sample active between 2011-2019 as in Table 2, columns (4) and (5) from 2011-2023. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

Table A2.5: Wages and Employment, By Worker Type, SA-BJS

	Wages and Employment					
	Entry		Supervisor		Entry/Supervisor	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Log (W_{it})						
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	0.061	0.062	0.006	0.006	0.055	0.056
	(0.004)	(0.004)	(0.005)	(0.004)	(0.006)	(0.007)
B. Log (L_{it})						
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	0.057	0.041	-0.038	-0.042	0.094	0.082
	(0.026)	(0.021)	(0.019)	(0.019)	(0.029)	(0.026)
Controls	No	Yes	No	Yes	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	17879	17879	17879	17879	17879	17879

Notes: The Table reports the estimated coefficient $\hat{\hat{\upsilon}}$ from the Sun and Abraham-BJS difference-in-differences model (A3.8) without controls in columns (1), (3) and (5) and with controls in columns (2), (4) and (6). The dependent variables are log wages and log employment (as measured by positions) for all workers and subgroups of entry workers and supervisors. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

Table A2.6: Wages and Employment, By Worker Type, TWFE

	Wages and Employment					
	Entry		Supervisor		Entry/Supervisor	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Log (W_{it})						
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	0.077	0.078	0.004	0.006	0.073	0.072
	(0.008)	(0.008)	(0.008)	(0.008)	(0.011)	(0.011)
B. Log (L_{it})						
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	0.068	0.054	-0.022	-0.022	0.091	0.077
	(0.036)	(0.032)	(0.033)	(0.033)	(0.045)	(0.043)
Controls	No	Yes	No	Yes	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	17879	17879	17879	17879	17879	17879

Notes: The Table reports the estimated coefficient δ from the TWFE difference-in-differences model (1) without controls in columns (1), (3) and (5) and with controls in columns (2), (4) and (6). The dependent variables are log wages and log employment (as measured by positions) for all workers and subgroups of entry workers and supervisors. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

Table A2.7: Wage Policy - Pay Points, SA-BJS

	Wage Policy – Pay Points					
	All		Entry		Supervisor	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}[\text{L}W > MW]_{it}$	-1.481 (0.204)	-1.525 (0.208)	-1.194 (0.169)	-1.221 (0.169)	-0.287 (0.106)	-0.305 (0.107)
Controls	No	Yes	No	Yes	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	17879	17879	17879	17879	17879	17879
Mean of dependent variable	11.4		6.7		4.7	

Notes: The Table reports the estimated coefficient $\hat{\hat{\upsilon}}$ from the Sun and Abraham-BJS difference-in-differences model (A3.8) with and without controls. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. The dependent variables are number of pay points for all workers and subgroups of entry workers and supervisors. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

Table A2.8: Wage Policy - Pay Points, TWFE

	Wage Policy – Pay Points					
	All		Entry		Supervisor	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	-2.079	-2.101	-1.705	-1.726	-0.374	-0.375
	(0.299)	(0.300)	(0.236)	(0.246)	(0.152)	(0.153)
Controls	No	Yes	No	Yes	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	17879	17879	17879	17879	17879	17879
Mean of dependent variable	11.4		6.7		4.7	

Notes: The Table reports the estimated coefficient δ from the TWFE difference-in-differences model (1) with and without controls. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. The dependent variables are number of pay points for all workers and subgroups of entry workers and supervisors. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

Table A2.9: Contract Policy – Promotion, SA-BJS

	Contract Policy – Promotion					
	All		Entry		Supervisor	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	0.011 (0.045)	0.006 (0.046)	0.036 (0.031)	0.031 (0.031)	-0.025 (0.023)	-0.025 (0.230)
Controls	No	Yes	No	Yes	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	17879	17879	17879	17879	17879	17879
Mean of dependent variable	0.33		0.24		0.09	

Notes: The Table reports the estimated coefficient $\hat{\hat{v}}$ from the Sun and Abraham-BJS difference-in-differences model (A3.8) with and without controls. The dependent variables are number of promotions for all workers and subgroups of entry workers and supervisors. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

Table A2.10: Contract Policy – Promotion, TWFE

	Contract Policy – Promotion					
	All		Entry		Supervisor	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	0.058	0.056	0.081	0.080	-0.022	-0.024
	(0.032)	(0.058)	(0.028)	(0.028)	(0.013)	(0.013)
Controls	No	Yes	No	Yes	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	17879	17879	17879	17879	17879	17879
Mean of dependent variable	0.33		0.24		0.09	

Notes: The Table reports the estimated coefficient δ from the TWFE difference-in-differences model (1) with and without controls. The dependent variables are number of promotions for all workers and subgroups of entry workers and supervisors. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

Table A2.11: Contract Policy – Zero Hour Contracts, SA-BJS

	Contract Policy – Zero Hour Contracts					
	All		Entry		Supervisor	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	0.012 (0.008)	0.008 (0.006)	0.001 (0.008)	-0.002 (0.008)	0.005 (0.007)	0.005 (0.007)
Controls	No	Yes	No	Yes	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	17879	17879	17879	17879	17879	17879
Mean of dependent variable	0.56		0.72		0.07	

Notes: The Table reports the estimated coefficient $\hat{\hat{v}}$ from the Sun and Abraham-BJS difference-in-differences model (A3.8) with and without controls. The dependent variables are proportion of zero-hour contracts for all workers and subgroups of entry workers and supervisors. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

Table A2.12: Contract Policy – Zero Hour Contracts, TWFE

	Contract Policy – Zero Hour Contracts					
	All		Entry		Supervisor	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}[LW > MW]_{it}$	0.020 (0.011)	0.016 (0.010)	0.011 (0.011)	0.008 (0.010)	0.008 (0.011)	0.008 (0.011)
Controls	No	Yes	No	Yes	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	17879	17879	17879	17879	17879	17879
Mean of dependent variable	0.56		0.72		0.07	

Notes: The Table reports the estimated coefficient δ from the TWFE difference-in-differences model (1) with and without controls. The dependent variables are proportion of zero-hour contracts for all workers and subgroups of entry workers and supervisors. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

Table A2.13: Wage Inequality, SA-BJS

	Establishment Wage Inequality							
	Standard Deviation		90-10		90-50		50-10	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	-0.027	-0.029	-0.156	-0.156	-0.074	-0.073	-0.053	-0.053
	(0.004)	(0.004)	(0.018)	(0.018)	(0.014)	(0.014)	(0.009)	(0.010)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	17879	17879	17879	17879	17879	17879	17879	17879
Mean of dependent variable	0.22		1.62		1.39		1.16	

Notes: The Table reports the estimated coefficient $\hat{\hat{\vartheta}}$ from the Sun and Abraham-BJS difference-in-differences model (A3.8) with and without controls. The dependent variables are the within establishment standard deviation for log wages, the 90-10, 90-50 and 50-10 ratio of wage percentiles. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

Table A2.14: Wage Inequality, TWFE

	Establishment Wage Inequality							
	Standard Deviation		90-10		90-50		50-10	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	-0.041	-0.041	-0.216	-0.214	-0.108	-0.109	-0.067	-0.065
	(0.006)	(0.006)	(0.029)	(0.029)	(0.023)	(0.023)	(0.014)	(0.014)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	17879	17879	17879	17879	17879	17879	17879	17879
Mean of dependent variable	0.22		1.62		1.39		1.16	

Notes: The Table reports the estimated coefficient δ from the TWFE difference-in-differences model (1) with and without controls. The dependent variables are the within establishment standard deviation for log wages, the 90-10, 90-50 and 50-10 ratio of wage percentiles. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

Table A2.15: Wage Inequality 2, SA

	Establishment Wage Inequality					
	All		Entry		Supervisor	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Pr(25-10) = 1						
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	0.192	0.197	0.105	0.115	-0.017	-0.014
	(0.040)	(0.041)	(0.038)	(0.037)	(0.031)	(0.032)
Pre-LW Mean	0.344		0.445		0.455	
B. Pr(50-10) = 1						
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	0.259	0.262	0.260	0.266	0.029	0.037
	(0.037)	(0.036)	(0.039)	(0.038)	(0.018)	(0.018)
Pre-LW Mean	0.090		0.197		0.161	
C. Pr(75-10) = 1						
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	0.129	0.129	0.235	0.234	0.029	0.035
	(0.023)	(0.022)	(0.035)	(0.032)	(0.016)	(0.016)
Pre-LW Mean	0.009		0.131		0.121	
D. Pr(90-10) = 1						
$\mathbb{1}[\mathrm{L}W > MW]_{it}$	0.001	0.001	0.146	0.149	0.036	0.044
	(0.001)	(0.001)	(0.026)	(0.027)	(0.016)	(0.017)
Pre-LW Mean	0.000		0.109		0.117	
Controls	No	Yes	No	Yes	No	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	17879	17879	11951	11951	5928	5928

Notes: The Table reports the estimated coefficient $\hat{v}$ from the Sun and Abraham difference-in-differences model (A3.6) without controls in columns (1), (3) and (5) and with controls in columns (2), (4) and (6). The dependent variables are an indicator for whether a particular inequality ratio is equal to 1, for all workers, entry workers and supervisors and managers. The sample is a panel of establishments run by The Company active between January 2011 and April 2019. Bootstrapped standard errors clustered at the establishment are reported in parentheses. Control variables are the proportion female, proportion BAME and mean age.

A3 Estimator

This section gives detail on the estimation methods adopted to estimate the diffference-in-differences and event study specifications detailed in equations (1) and (2) in the paper.

Borrowing notation from Sun and Abraham (2021), let Y_{it} denote an outcome of interest for unit i at time t with treatment status $D_{it} \in \{0,1\} : D_{it} = 1$ if i is treated in period t and $D_{it} = 0$ otherwise, where treatment is absorbing, and therefore $D_{is} \leq D_{it}$ for $s < t$. A unit's treatment path can therefore be characterised by $R_i = \min\{t : D_{it} = 1\}$, and where we let $R_i = \infty$ if the unit is never treated. Units can therefore be categorized into disjoint cohorts $r \in \{t_{min}, \dots, t_{max}, \infty\}$, where units in cohort r are first treated at the same time $\{i : R_i = r\}$. Y_{it}^{r} is the potential outcome in period t when unit i is first treated at time r and Y_{it}^{∞} is the potential outcome at time t if unit i never receives treatment. A cohort-specific average treatment effect on the treated l periods from treatment is thus:

$$CATT_{rl} = E[Y_{ir+l} - Y_{ir+l}^{\infty} \mid R_i = r] \tag{A3.1}$$

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. We are then interested in some weighted average of (A3.1), for some $l \in g$, to construct a relative period coefficient. As is often the case when firms face a shock to the wage floor, we are interested in both the average dynamic effects (which allows an analysis of the pre-trends) and the average "long-term" impacts.

For analysing the average dynamic effects we focus on the weighted average similar to that proposed in Sun and Abraham (2021) as:

$$v_g = \frac{1}{|g|} \sum_{l \in g} \sum_{r} CATT_{rl} \Pr\{R_i = r \mid R_i \in l\} \tag{A3.2}$$

which effectively uses weights according to the size of the treated cohort that experiences l periods relative to treatment.

In practice (A3.2) is estimated using the following methodology:

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

$$Y_{it} = \alpha_i + \lambda_t + \sum_{l} \delta_{r,l} \, \mathbb{1}[LW > MW]_{it+l} + \beta' X_{it} + \varepsilon_{it} \qquad \text{(A3.3)}$$

where α_i is the establishment fixed effect, λ_t is a year-month fixed effect, $\mathbb{1}[LW > MW]_{it}$ is a dummy variable which represents whether an establishment is subject to a LW clause which is higher than the mandated minimum, and X_{it} is a set of time varying establishment level controls. For each treatment cohort r, 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 (2018), we normalise the dynamic effects to two periods, -1 and -12, to deal with the under identification issues they raise.

2. We estimate the weights $\Pr\{R_i = r \mid R_i \in l]\}$ by sample shares of each cohort in the relevant relative period l.

3. We 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:

$$\widehat{v_g} = \frac{1}{3} \sum_{l \in g} \sum_{r} \hat{\delta}_{rl} \, \widehat{\Pr}\{R_i = r \mid R_i \in l]\} \qquad \text{(A3.4)}$$

Standard errors are estimated via 1000 cluster-bootstraps, with clustering at the establishment level.

For analysing the average long-term impacts, the methodology is very similar except that specification (A3.3) in step 1 is replaced with the typical difference-in-differences estimator

$$Y_{it} = \alpha_i + \lambda_t + \bar{\delta}_r \mathbb{1}[LW > MW]_{it} + \beta' X_{it} + \varepsilon_{it} \tag{A3.5}$$

where the weights in step 2 are simply replaced with the cohort share weights $\Pr\{R_i = r\}$ and the aggregation in step 3 is such that

$$\hat{v} = \sum_r \hat{\bar{\delta}}_r \widehat{\Pr}\{R_i = r\} \tag{A3.6}$$

For robustness we also implement an estimation strategy to elicit the average long run impacts as suggested by Borusyak et al. (2024) within our existing strategy. In particular we use a specification in step 1 of the form

$$Y_{it} = \alpha_i + \lambda_t + \sum_{l \geq 0} \tilde{\delta}_{rl} \mathbb{1}[LW > MW]_{it+l} + \beta' X_{it} + \varepsilon_{it} \tag{A3.7}$$

and average according to

$$\hat{\tilde{v}} = \frac{1}{12} \sum_{l \geq 0} \sum_{e} \hat{\tilde{\delta}}_{rl} \widehat{\Pr}\{R_i = r \mid R_i \in l\} \tag{A3.8}$$

The above methodology comes with a number of benefits. Firstly, it is completely transparent about what weights are being used between treatment cohorts in the estimation of the parameters of interest. These weights are guaranteed to be convex and non-negative, which in the typical event study specification with variation in timing is not necessarily the case Sun and Abraham (2021). Secondly, there is clarity in terms of which groups are being used as treatment and control groups in both the dynamic, and long run treatment effect estimation. Thirdly, it deals with under identification problems raised previously in the literature. Fourthly, the final robustness check as estimated in (A3.8) is additionally transparent about the weights used across the dynamic treatment effects to aggregate to an average long run effect. In particular, in this setting they are averaged across the month effects in the year following treatment and are normalised against the entire pre period.

The TWFE event-study estimates use the specification in (A3.3) and aggregate according to

$$\widehat{\beta_g} = \frac{1}{3} \sum_{l \in g} \hat{\delta}_l \tag{A3.9}$$

and the TWFE difference-in-differences estimates are based on estimating equation (A3.5). In both instances the full panel is used as happens with traditional TWFE regressions.

A4 Model Derivation

Consider a two period setting, $t \in \{1,2\}$, where there are many workers and firms, and the firm under study views itself as atomistic. The firm utilises two types of labour inputs, supervisor and entry, and has a CES production function form:

$$F(L_{e,t}L_{s,t}) = \left[\alpha L_{e,t}^{\frac{\sigma-1}{\sigma}} + (1-\alpha)L_{s,t}^{\frac{\sigma-1}{\sigma}}\right]^{\frac{\sigma}{\sigma-1}} \quad \text{(A4.1)}$$

where $L_{e,t}$ is entry labour at time $t, L_{s,t}$ is supervisory labour at time t, and σ is the elasticity of substitution between them. Define $W_{j,t}$ as the wage for group *j* (*j* = *e*, *s*) at time t.

In $t = 1$ there is no government intervention and labour supply to the firm for entry and supervisory labour respectively is:

$$L_{e,1} = \gamma_e W_{e,1}{}^{\varepsilon_e}$$
$$L_{s,1} = \gamma_s W_{s,1}{}^{\varepsilon_s} \quad \text{(A4.2)}$$

where ε_e is the labour supply-elasticity to the firm for entry workers, and ε_s is the equivalent for supervisory workers. γ_u and γ_s are scale factors.

Rearranging the group-specific equations in (A4.2) to form expressions for $W_{e,1}$ and $W_{s,1}$, multiplying through by $L_{e,1}$ and $L_{s,1}$ respectively to form the total cost of the two labour inputs, and differentiating by $L_{e,1}$ and $L_{s,1}$ respectively gives expressions for the group-specific marginal costs of labour (MCL) in period 1 respectively is:

$$MCL_{e,1} = \left(\frac{1}{\varepsilon_e} + 1\right) W_{e,1}$$
$$MCL_{s,1} = \left(\frac{1}{\varepsilon_s} + 1\right) W_{s,1} \quad \text{(A4.3)}$$

Differentiating equation (A4.1) with respect to $L_{e,1}$ and $L_{s,1}$ respectively to get the marginal products of entry and supervisor labour, setting them equal to their equivalent MCL and then dividing one by the other yields: is:

$$\frac{\alpha}{1-\alpha}\left(\frac{L_{e,1}}{L_{s,1}}\right)^{-\frac{1}{\sigma}} = \frac{\mu_s}{\mu_e}\frac{W_{e,1}}{W_{s,1}} \tag{A4.4}$$

where $\mu_j = \frac{\varepsilon_j}{1+\varepsilon_j}$, which is 1 – the wage markdown, or rather, the proportion of marginal productivity a worker gets paid.

Taking logs and rearranging gives a familiar relationship between the relative employment and relative wages in period *t = 1*:

$$\log\left(\frac{L_{e,1}}{L_{s,1}}\right) = \sigma\log\left(\frac{\alpha}{1-\alpha}\right) - \sigma\log\left(\frac{\mu_s}{\mu_e}\right) - \sigma\log\left(\frac{W_{e,1}}{W_{s,1}}\right) \tag{A4.5}$$

Now if in $t = 2$ the government introduces a living wage, $\bar{W}_{e,2}$ which is binding only for entry workers, this changes the total cost for entry level workers in period *t = 2* to:

$$TCL_{e,2} = \begin{cases} \bar{L}_{e,2}\bar{W}_{e,2} \quad if \quad L_{e,2} \leq \bar{L}_{e,2} = \gamma_e \bar{W}_{e,2}{}^{\varepsilon_e} \\ L_{e,2}\,\gamma_e W_{e,1}{}^{\varepsilon_e}\ otherwise \end{cases} \tag{A4.6}$$

Assuming the Living Wage is binding and thus the top half of equation (A4.6) holds then period *t = 1*:

$$MCL_{e,2} = \bar{W}_{e,2} \tag{A4.7}$$

Repeating what we did for period 1 for $t = 2$, the equivalent to equation (A4.5) becomes:

$$\log\left(\frac{L_{e,2}}{L_{s,2}}\right) = \sigma\log\left(\frac{\alpha}{1-\alpha}\right) - \sigma\log(\mu_s) - \sigma\log\left(\frac{\overline{We}_{u,2}}{W_{s,2}}\right) \tag{A4.8}$$

and subtracting equation (A4.5) away from equation (A4.8) we get a result for the impact of the living wage on employment composition as:

$$\Delta \log\left(\frac{L_e}{L_s}\right) = -\sigma\left(\log(\mu_e) + \Delta \log\left(\frac{W_e}{W_s}\right)\right) \tag{A4.9}$$

where $\mu_e = \frac{\varepsilon_e}{1 + \varepsilon_e}$ is a measure of the monopsony wage markdown in the entry labour market, so that equation (A4.9) collapses back to the conventional labour demand model when market $\varepsilon_e = \infty$, as the markdown is zero.

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