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The measure of monopsony: The labour supply elasticity to the firm and its constituents

Nikhil Datta

Abstract
The estimation of labour supply elasticities is central to the measurement of monopsony power in the labour market. In this paper I provide new, firm-level estimates of the labour supply elasticity that distinguish between a recruitment elasticity (for potential new workers) and a separation elasticity (as relevant to incumbents). My study uses comprehensive HR data for a large multi-establishment firm in the UK. This setting allows me to develop job-establishment level variation in wages derived from both a government wage floor policy which only effects my firm under study and arbitrary variation in advertised wages resulting from idiosyncratic HR department decisions. My estimates show that, in contrast to common assumptions, the recruitment elasticity is almost double the size of the separation elasticity. Heterogeneity analysis is suggestive that differences in wage-saliency for job seekers versus incumbents is likely a factor in this difference. Combined the elasticities give a labour supply elasticity to the firm of 4.6 implying a wage markdown of 18% from the marginal product of labour. I find no evidence of spillovers from wage changes to the local market despite establishments being relatively large, indicating a monopsonistic wage setting framework is more suitable than an oligopsonistic one.

Key words: monopsony, recruitment elasticity, separation elasticity, markdown, market power
JEL Classification: J42; J31; J22

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Nikhil Datta, University of Warwick and Centre for Economic Performance at London School of Economics.

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Houghton Street
London WC2A 2AE

1 Introduction

Wages are the key source of household income in any market-based economy, in the UK they account for 84% of total income for the median working-age household. Understanding the wage setting behaviour of firms is therefore of first order economic importance. In a perfectly competitive market where firms are price takers, workers receive their marginal product. But in markets where firms have wage-setting power, workers receive a marked-down percentage of their marginal product, and the size of that markdown depends on the extent of monopsony power their employer exercises.[1] This paper estimates the extent of monopsony power for a firm with hundreds of establishments across the UK, utilising a novel dataset and highly credible identification strategy.

To estimate the extent of monopsony power I am interested in examining two key elasticities: the recruitment-wage elasticity and the separation-wage elasticity. The former represents the willingness-to-join of new workers at a given wage rate, and the latter the willingness-to-stay of incumbent workers. Combined, these two elasticities give the labour supply elasticity to the firm, a key measure of labour market competitiveness and monopsony power and offers an approximation for the expected markdown in wages from marginal productivity. A large labour supply elasticity to the firm suggests a more competitive market, while a low elasticity suggests large markdowns.

Studies until now have only estimated one of the two elasticities and relied on a theoretical result (Manning, 2003) that they are equal in absolute size, thus doubling their single parameter estimate to get the labour supply elasticity to the firm. While useful, this result is yet to be empirically tested and there is reason to believe it may not hold. For example, we might think that wages are a more salient feature of jobs for the average job searcher, while incumbent non-wage working conditions are more important for those deciding whether to leave their current job. Furthermore, existing estimates of the recruitment-wage elasticity typically use either completed hires, or applicants, as a measure of recruits which will likely lead to biased estimates, as in reality recruits is a latent variable which relates to those *willing to join* the firm at the going wage rate. Not all applicants join a firm despite being offered the job, while not all applicants are hired.

To estimate these parameters, I utilise a novel dataset for a large UK based local services firm, which contains rich human resources (HR), vacancy, and applicant data. In addition to the usual information such as wages, tenure, and demographic characteristics, the dataset includes detailed data on specific job roles, entire vacancy text, and details about the recruitment process. The latter includes the number of applicants for each vacancy as well as the outcome of their application. Outcomes include hires, rejections (e.g. "Unsuccessful at shortlist") and turned down offers (e.g. "Formal job offer rejected"), and this information is fundamental for constructing a true measure of recruits. While I am unable to disclose the precise industry of

[1] For canonical texts on this see Pigou (1924), Robinson (1933), Boal and Ransom (1997) and Manning (2003), and for a recent review see Manning (2021).

the firm, I provide evidence to show that it faces a similar degree of local competition as typical local services such as restaurants, pubs, hairdressers, and mechanic garages, and that the set of occupations utilised by the firm has the same rate of job and industry substitutability as the economy wide average, suggesting external validity.

Estimating the recruitment and separation-wage elasticities have historically been challenging as they require exogenous wage variation at the establishment-level as a minimum. This is because to isolate the elasticities, we require to see how recruits or separations respond when only the wage in a single establishment changes, and the rest of the market's wages remain unchanged. Any changes to wages correlated with outside options will inevitably bias the results, and eliciting such variation is non-trivial.

To overcome endogeneity concerns I use two novel instruments to establish exogenous variation in the wage at the job-establishment-time level, and the advert-job-establishment-time level respectively. The first is a location-specific Living Wage floor that only affects firms who are engaged in council procurement contracts and thus affects a tiny fraction of a percentage of firms and workers in the area. The Living Wage is however binding for The Company's establishment in that location, and only affects jobs that were previously paying less than the Living Wage, generating large wage increases in both job adverts and for incumbent workers. The instrument can thus be used to estimate both the recruitment and separation elasticity. I provide evidence using a sample of the UK's social security data to corroborate that the Living Wage has no impact on wages in the local economy and thus outside options. This lack of spillovers to other firms is instructive about the market structure, suggesting a monopsonistic wage setting framework (where firms view themselves as atomistic) is a more suitable fit for the setting than an oligopsonistic one (where strategic interactions are present).

The second instrument is related to the saliency of the advertised wage in a job advert. By law in the UK all jobs must pay 28 days annual leave and some firms decide to pay this as an hourly wage top-up, which works out to 12.07%. The Company pays this annual leave top-up to all their hourly paid staff, however only some of the advertised positions include the top-up in the posted wage. This is because of the member of HR staff who posted that particular vacancy, and thus induced by idiosyncrasies in The Company's HR department. The instrument has the effect of inducing variation in the posted wage randomly for the same job role in different vacancy adverts, within the same establishment, offering identifying variation for recruitment-elasticity.

The results are suggestive of strong levels of monopsony power in the labour market. I find a recruitment-wage elasticity of 3, and a separation-wage elasticity of -1.6. Combined, these give a labour supply elasticity to the firm of approximately 4.6 and suggest a wage markdown of 18%. The results imply recruits are more wage sensitive than incumbent workers, indicating an asymmetry in monopsony power and the presence of greater frictions for incumbent workers. The results additionally show that using completed hires instead of the true measure of recruits, biases the recruitment elasticity down by approximately 50%, while using applicants increases it by 20%.

A potential cause of the difference in monopsony power between recruits and separations could emerge from differences in wage-saliency for job seekers versus incumbents. Specifically, job seekers may be more sensitive to wages as they are more easily observable before joining a firm relative to non-wage features of the job. Once workers join a firm certain non-wage aspects of the job which were previously unobservable (e.g. autonomy, management style, relationship with co-workers) become more salient relative to wages, reducing wage sensitivity. I test this by examining the heterogeneity of separation elasticities by worker tenure and show that newly joined workers are very wage insensitive. For their first quarter of tenure their separation elasticity is not statistically different from zero. Separation elasticities are shown to increase in (absolute) size as tenure increases, and that a worker with 3-4 years tenure has the same wage sensitivity as a new recruit.

This paper makes three advances on the existing literature. First, it has an extremely credible identification strategy utilising two instruments which generate exogenous variation in wages, while leaving the remainder of the local labour market unchanged. It does this for a sample of occupations in the bottom half of the wage distribution which appear across a number of industries in the private sector, and follow a similar substitutability pattern as the national average from the workers perspective. Thus, it is not unreasonable to generalise the results. This combination of clean identification and generalisability sets it apart from existing studies which are either unable to elicit experimental variation and so rely on constructed estimates, such as AKM (Hirsch et al., 2022; Bassier et al., 2022) or spatial leave-one-out (Azar et al., 2022) instruments, or are for industries which are government run (Falch, 2017; Dal Bó et al., 2013; Staiger et al., 2010; Falch, 2010)). Such instruments, while best practice given exogenous variation constraints, are likely to suffer from a number of issues (Bonhomme et al., 2023; Betz et al., 2018), while government run industries are likely to be more akin to "natural" monopsonists, potentially lacking generalisability.

Second, until now all[2] studies have typically either attempted to estimate the recruitment elasticity, using data on applicants and completed hires[3], or the separation elasticity[4] and relied on a theoretical result from Manning (2003) that states that they are equal in absolute value in order to elicit the labour supply elasticity from a single parameter. This theoretical result has so far been untested, and relies on a number of restrictive assumptions[5], and if it were not to hold would imply many existing estimates of monopsony power were biased. This study is the

[2]To the best of the author's knowledge.

[3]For examples see Falch (2010); Dal Bó et al. (2013); Falch (2017); Belot et al. (2022); Pörtner and Hassairi (2018); Banfi and Villena-Roldan (2019); Dube et al. (2020a); Marinescu and Wolthoff (2020); Azar et al. (2022); Hirsch et al. (2022)

[4]Notable recent papers include Ransom and Sims (2010); Dube et al. (2016, 2019); Bassier et al. (2022) and for a recent survey see Sokolova and Sorensen (2021).

[5]It is derived for a market rather than an individual firm, and relies on assumptions that recruitment from unemployment is invariant to the wage, and that the recruitment and separation elasticity are both constant.

first to estimate both the recruitment and separation elasticity for the same firm and probe the reasons behind asymmetries in market power between incumbent and new workers. Third, this paper exploits detailed information on the hiring process to create an accurate picture of the latent measure of recruits- the willingness to join side of labour supply. The results suggest that using applicants will likely upward bias the recruitment elasticity as it includes workers who would not actually be willing to join the firm, while using completed hires will underestimate the labour supply elasticity as it contains labour demand effects.

Putting the second and third of the above findings together, it suggests that existing estimates within the literature may be biased. Studies using only an estimate of a recruitment elasticity, and doubling it to produce a labour supply elasticity to the firm will likely overestimate the true parameter (e.g. Dal Bó et al. (2013)), while those using only a separation elasticity will underestimate the true parameter (e.g. Bassier et al. (2022); Dube et al. (2019); Sokolova and Sorensen (2021)). For those studies using only data on applicant numbers, estimating an application elasticity, treating it as a recruitment elasticity, and doubling it will overestimate the degree of competition in the market even further (e.g. Azar et al. (2022)). It is not clear in which direction the bias will be for studies estimating a recruitment elasticity based on completed hires and doubling that (e.g. Falch (2010, 2017); Hirsch et al. (2022)) as the recruitment-wage elasticity is likely to be underestimated, but the true recruitment elasticity would be higher than the separation elasticity. This study shows incorrectly doubling the application or separation wage elasticities could misestimate the labour supply-elasticity to the firm by 40%.

The remainder of this paper is structured as follows. Section 2 introduces a simple framework mapping the elasticities to the wage markdown, and discusses how to construct a true measure of recruits. Section 3 discusses the data, identification and provides evidence on external validity. Section 4 presents the empirical framework, section 5 the results and section 6 concludes.

2 The Labour Supply Elasticity to the Firm and its Constituents

Assume a setting where there are many firms j and they consider themselves small such that we can abstract from strategic interaction. Firm j gains profits according to:

$$\Pi_j = (p_j - w_j)n(w_j) \tag{1}$$

where p_j is productivity of firm j, w_j is the wage and n is the employment.

Therefore the first order condition for the firm can be written as

$$w_j = p_j \frac{\varepsilon_{nw}}{1 + \varepsilon_{nw}} \tag{2}$$

where ε_{nw} is the elasticity of labour supply to the firm. The above is a form of the traditional monopsonistic "rate of exploitation" (Pigou, 1924) where the gap between wages and productivity grows as the firm labour supply elasticity shrinks. The limiting case where ε_{nw} tends to infinite is akin to a perfectly competitive market.

In the steady-state, labour supply is such that

$$n_j = \frac{r(w_j)}{s(w_j)} \tag{3}$$

where r is recruitment and s is separations, both a function of wages. Taking logs and differentiating by $ln(w_j)$ yields the relationship between the labour supply elasticity and the recruitment and separation elasticity.

$$\varepsilon_{nw} = \varepsilon_{rw} - \varepsilon_{sw} \tag{4}$$

Until now the literature has relied on two assumptions for estimating ε_{nw}. The first, a theoretical result from Manning (2003), that the recruitment elasticity and separation elasticity are equal in absolute value under certain restrictive assumptions. This result has enabled researchers to estimate only one of the two key elasticities, and double it to give the labour supply elasticity to the firm. While useful, this result is yet to be empirically tested. As a result this assumption will be relaxed in this paper, estimating both elasticities separately, and detailing their differences.

The second concerns estimates of ε_{rw}, where researchers have used either completed hires or applicants as a measure for recruits, which in reality is a misleadingly named, latent variable and is described below.

The term "recruits" used in the literature actually relates to those *willing to join* the firm at the going wage rate (the joining side of labour supply). As we know not all job applicants join a firm despite being offered the job, while not all applicants are hired.

Let us take the standard hiring procedure for many firms. Assume firm j posts a job advert with wage w_j, and they receive $a(w_j)$ job applicants. Firm j then invites some applicants for interviews, accordingly makes job offers and hires $h(w_j)$ workers. The firm may decide that not all workers are suitable, and similarly, workers that have applied may decide to take other jobs or stay in their current position. Therefore the relation between applicants and hires is

$$a(w_j) = h(a(w_j)) + \phi(a(w_j)) + \lambda(a(w_j)) \tag{5}$$

where ϕ is the number of applicants that the firm chooses to reject and λ is the number of applicants that decide to turn down the job.

Recruits is then synonymous with the number of job applicants that apply for the job, and would actually take the job, or similarly the number of hires, plus those that were rejected. These are the workers willing-to-join at this given wage. Thus, recruits $r(a(w_j))$ is given by

$$r(a(w_j)) = h(a(w_j)) + \phi(a(w_j)) = a(w_j) - \lambda(a(w_j)) \tag{6}$$

In a world where $\frac{\partial \phi}{\partial w} = 0$, i.e. the number of rejections is invariant to the wage, the hire-wage responsiveness is equal to the the recruitment-wage responsiveness. Similarly, when $\frac{\lambda \phi}{\partial w} = 0$, i.e. the number of turn downs is invariant to the wage, the application-wage responsiveness is synonymous with the recruitment wage responsiveness.

It's reasonable to assume neither of the above may hold. Firms often post vacancies for single (or a limited number of) positions due to the nature of their production function, or the demand for their underlying good, causing their marginal revenue product labour (MRPL) to decline rapidly after a certain point.[6] As a result a regression of hires on (exogenous) wages would capture a combination of labour supply effects (recruits) and labour demand (rejections). Similarly, job searchers often apply to numerous positions, or may only apply to a job for extra bargaining in their current job. If a higher wage attracts some workers on the margin to apply to a job, these workers may have a higher likelihood of turning down the job. Therefore a regression of applicants on (exogenous) ln wages will weakly overestimate the recruitment wage semi-elasticity, while a regression of hires on (exogenous) ln wages will weakly underestimate in. More formally, the semi-elasticities, $\frac{da}{dlnw} \geq \frac{dr}{dlnw}$ and $\frac{dh}{dlnw} \leq \frac{dr}{dlnw}$ by construction, where the inequalities become strict when $\frac{\lambda \phi}{\partial w} \neq 0$ and $\frac{\partial \phi}{\partial w} \neq 0$ respectively.[7]

To estimate the recruitment-wage elasticity in this paper I will exploit the detailed hiring data of The Company and use the definition of recruits as per equation (6) to estimate the labour supply elasticity to the firm.

It is worth noting that a similar framework to the above would apply when measuring separations. Specifically, worker exits e will be a combination of separations s (or quits) and those who get fired f, such that $e(w_j) = s(w_j) + f(w_j)$ where separations are a labour supply decision, and being fired is a labour demand decision. Given the strict labour laws concerning dismissal in the UK and Europe $e(w_j)$ and $s(w_j)$ are likely to be very similar,[8] and treating worker exits and separations is not going to introduce much in terms of bias. Studies looking at a US setting however, where at-will employment law is common would benefit much more from looking at quits rather than all exits if the data permits.

3 Data, Identification and External Validity

3.1 Data

I utilise a rich bespoke dataset for a large UK based services firm (The Company) with operations in over 350 establishments across the UK. Establishments are centrally operated by the same company using the same structure of operations and management, but there is es-

[6]For example, a firm may have a leontief production function with only a limited number of spare units of capital.

[7]While it may seem natural to apply the same restriction to the actual elasticities, it wouldn't necessarily hold due to the rescaling by the dependent variable.

[8]For the firm used in this paper, only 1.5% of exits are as a result of being dismissed or failing probation, suggesting treating exits as separations would not introduce a bias.

tablishment level autonomy over employment and workforce composition decisions. While The Company's main competitors are firms operating in the private sector, a large part of the firm's business is government procurement contracts. The dataset includes HR data for the period 2011-2019, and vacancy and applicant data for the period 2016-2019.

The HR data covers approximately 31,000 employees and includes information on demographics, job roles, pay, start and leave dates. The vacancy data includes all information that is contained in a job advert including, job role, wage, location and all text within the advert. The applicant data includes the number of applicants for each vacancy as well as the outcome of their application. Outcomes include hires, rejections (e.g. "Unsuccessful at shortlist") and turned downs (e.g. "Formal job offer rejected", "Candidate withdrawn application"). This latter information is key for constructing a true measure of recruits, relevant for measuring the labour supply elasticity to the firm. The combination of these three datasets allows me to explore all constituents of the labour supply elasticity to the firm.

Table 1 presents summary statistics for The Company in March 2019 and paints a picture of a typical firm operating in a low wage labour market, where younger and female workers are overrepresented. 60% of the firm's workforce are female, and the median worker 33 years old. Almost half of the firm's workers are classified as "Entry-Level". These jobs are typically minimum-wage jobs in the UK, and would be considered unskilled. The Company has a very large number of workers on non-salaried, hourly wage contracts[9] which is more usual for the low-pay sector. The average wage in the firm is £12.88 and approximately half of the firm's workforce are based in establishments located in London.

Table 1: Summary Statistics, March 2019

Variable	Mean	S.D.	Median
Female	0.60		
Age	35.9	14.3	33.0
Entry-Level	0.49		
Salaried	0.28		
Hourly Rate (£)	12.88	5.87	10.20
London	0.53		
N			
Workers	18,773		
Establishments	362		

Note: *The table presents worker-level summary statistics for The Company as of March 2019.*

Table 6 in the appendix contains summary statistics on the vacancy and applicant data for all and non-salaried jobs. The statistics on hourly wages, proportion of entry level, and proportion London based unsurprisingly show a very similar pattern to those from 1. The table additionally documents statistics on numbers of applicants, hires, rejects and turned downs for job adverts.

[9]For a complete description of what these types of contracts entail see Datta et al. (2019).

The data shows that the average job gets around 7 applicants, 1 of whom is hired, 1 decides they no longer want the job, and the largest proportion are rejected by The Company. These stats demonstrate how utilising data on applicants is likely to overestimate the true number of job searchers willing to work at the firm at the going wage rate, while using the number of actual hires will underestimate.

3.2 Exogenous Wage Variation

Estimating the recruitment-wage and separation-wage elasticities relies on being able to isolate exogenous variation in wages at a minimum at the establishment-level. This is necessary as these parameters identify the responsiveness of recruits and separations to a change in a single firm's wage, while the rest of the market remains unchanged. I exploit two instruments to achieve this.

Firstly, I utilise a location specific wage floor that affects a very small number of workers in an area, but is binding for The Company in that location for jobs which are paid less than the Living Wage. The Living Wage Foundation (LWF) is a charitable organisation in the UK that was established in 2011, that campaigns for employers to pay workers a living wage. Organisations can voluntarily sign up to become Living Wage employers and following appropriate audits by the LWF can achieve accredited status. As of July 2020, the LWF lists 6,562 accredited employers and included in this list are 107 local government units. When public bodies achieve accreditation, they are given a temporary amnesty on existing procurement contracts, but are required to enforce the living wage at the start, renegotiation or renewal of contracts.

The Company operates in the service sector and the majority of their business is through procurement contracts with local councils. As the firm operates hundreds of establishments across the UK, different establishments become contractually obliged to pay the Living Wage at different times. This is dependent on whether, and when, the local government unit has voluntarily signed up to the LWF's Living Wage, as well as idiosyncratic timings of contractual renewal or renegotiation. The Company's pay structure is centrally determined, and they have two regional pay scales for the UK (London and rest of the UK). When an establishment is exposed to the Living Wage, it effects only those workers within the establishment whose pay point is below the mandated Living Wage (i.e. entry-level workers), and the remainder of wages in the local labour market remain unchanged.

Between 2012 and 2019 107 local government units gained accreditation. 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[10]. As figure 1 shows, this setting gives a large amount of variation in Living Wage treatment for establishments run by The Company. In particular, over the period for which I have HR data, approximately 140 establishments went from being untreated to treated, while run by The Company. The living wage rates for London (LLW) and the rest of the UK (UKLW) are calculated each year by the LWF and the Resolution Foundation and have typically been consid-

[10] Relevant for the sample period analysed.

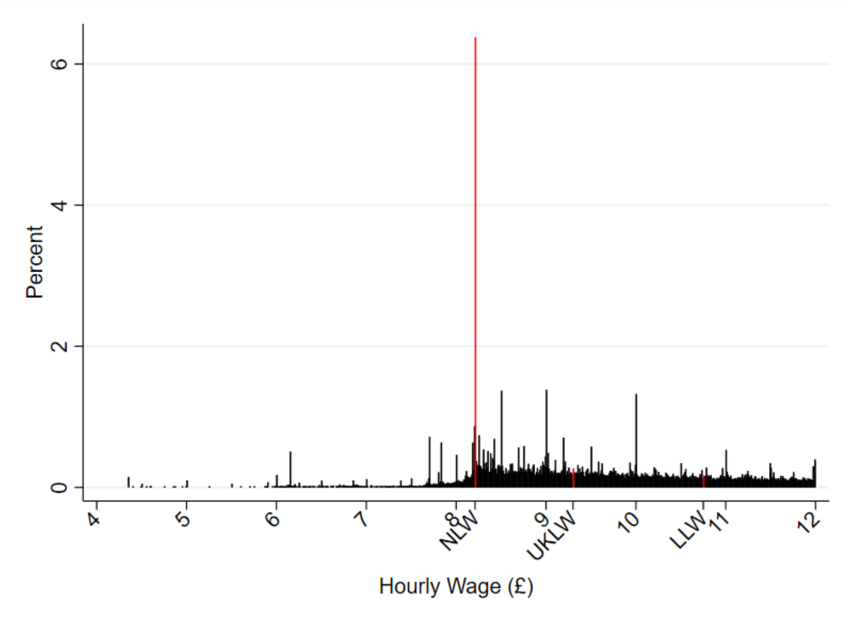
erably higher than the mandatory National Minimum Wage (NMW) and National Living Wage (NLW). The LLW rate has typically been approximately 30-35% higher than the mandatory minimums, while the UKLW has been about 15-20% higher as can be seen by figure 7 in the appendix, which in turn has staggered application across establishments.

Figure 1: Living Wage Roll-out

Note: *The figure shows the cumulative establishment-level Living Wage treatment for The Company, 2011 - 2019.*

To ensure that this instrument can causally identify the recruitment and separation elasticities, it's necessary to ensure that the Living Wage adoption within an area only affects a very few number of jobs, and has little impact on worker's options outside The Company. If for example, it affected all low pay jobs in the area the relative wage offered by the establishment would remain unchanged, and therefore it would be reasonable to assume this would have little affect on separations and recruitment. It's important to highlight therefore that when a council signs up to the LWF's living wage it only affects council employees and those who are subcontracted to do work for the council. Council employment makes up approximately 3% of employment and is usually made up of workers more skilled than would be affected by the wage floor. As an example table 7 in the appendix gives estimates of the employment counts and shares for the London Borough of Hackney, and shows council employment accounts only for 3.3% of total employment in the borough. Furthermore, examination of the pay scale documentation for the borough show that the lowest paid point is 8% above the binding LLW for 2019. This is suggestive that the council adoptions of the Living Wage affects only a fraction of a percentage of workers in the area. Further evidence of this is shown in figure 2. The figures provide histograms of the hourly wage distribution (to the penny) for the year 2019 for those whose wages are less than the national median (£12) in that year, where the red bars indicate the nationally mandated minimum wage (NLW), the UK Living Wage (UKLW) and the London Living Wage (LLW), where the last two are set by the Living Wage Foundation. The figure demonstrates that while the nationally mandated minimum is the modal rate by a long way, the UKLW and LLW do not appear to be rates with a higher propensity of being paid, in comparison to the rest of the distribution.

Figure 2: Living Wage rate propensity



Note: *The figures show the hourly wage distribution for 2019 for those paid less than the median. The red bars mark the National Living Wage (NLW), the UK Living Wage (UKLW) and the London Living Wage (LLW). The top is for the whole of the UK, while the bottom is just for London. Source: ASHE*

While a significant portion of The Company's business comes from procurement contracts with the council, their main competitors are private sector firms which would not be directly affected by the Living Wage adoption by the council, one may however have concerns regarding spillovers (e.g. Derenoncourt et al. (2021)). In addition to the above descriptive evidence, robustness checks for local spillovers are outlined in section 5.3.

Secondly, I utilise an instrument related to saliency of the wage posted. In the UK, whether a job has a salaried or hourly paid contract, the firm is required by law to give the worker 28 days paid annual leave. Due to the nature of non-salaried work[11] many firms opt to give the statutory annual leave as a top up to the wage, which calculates to a wage supplement of 12.07%. Within the vacancy data, some non-salaried jobs (approximately 20%) are advertised with the annual leave top-up included in the advertised wage, while the text of the adverts stay constant. Discussions with the HR team at The Company concluded that this occurred due to

[11] They typically have higher turnover rates so annual leave calculation becomes more difficult.

idiosyncrasies in whomever happened to be posting the job onto the HR system[12]. This lends itself for use as a seemingly random instrument. This instrument however can only be used for non-salaried jobs, as it is not well defined for salaried jobs. Furthermore, it can only be used for the recruitment elasticity estimates.

3.3 External Validity

Given the opacity of the specific industry The Company operates in (due to disclosure restrictions) combined with the fact the firm has procurement contracts with local government, one may be concerned that the firm operates in a market where they have a natural monopoly, such as refuse collection, police enforcement or schooling. Such a setting would naturally give rise to some monopsony power as workers' outside options, especially within the same occupation where their qualifications and training would be most valued, would be heavily limited. While examining the extent of monopsony power in these types of industries would be interesting, one may have concerns regarding external validity of the estimates of monopsony power to the wider economy. To alleviate these concerns I present two pieces of information. The first, offers some descriptive comparative information for the service industry The Company operates in, and the second offers information regarding substitutability of jobs the firm employs from the workers perspective. In showing that job and industry substitutability for the set of occupations utilised by the firm mirrors similar rates for the entire economy should go some way in assuaging concerns regarding external validity issues.

To demonstrate the degree of private competition within a local area I use an exemplar town located within the home counties of the United Kingdom, and that is within 25 miles of central London. The town has an area of 3 square miles (8 km^2), and a population of approximately 40,000 people. It is ranked between the 250-300th largest settlement in the UK. Within this town The Company operates an establishment which serves the public and they are contracted to do so by the local government. There are 9 other competing establishments run by other private firms offering equivalent or similar services. As a point of comparison, within the same town, there are 10 mechanic's garages, 11 pubs, 13 restaurants, and 10 hairdressers. As a result, the firm faces a similar degree of local competition in their product and labour market as the aforementioned industries, which aren't usually regarded as possessing large amounts of market power.

Workers employed by the firm work in occupations which follow similar job and occupation-industry substitutability as the national average. Evidence from the UK's social security data sample, the Annual Survey of Hours and Earnings, (Office for National Statistics, 2020) show that workers in occupations (based on 4 digit SOC codes) utilised by The Company switch jobs 26% on average every year, while the same figure for across all occupations is 25%. Similarly,

[12] This phenomenon was observed consistently over the entire time period, and there is considerable variation within establishments and jobs. Furthermore, the director of HR for The Company was unable to explain the phenomenon when it was first pointed out, and after some internal investigating concluded it was a result of whomever happened to post that particular job.

21% of workers in the social security data in occupations utilised by The Company switch occupation-industry every year, while the same figure is 18% across all occupations. There is also no statistical difference between the number of industries the occupations utilised by The Company work in and the average across all industries.[13] This is indicative that the firm does not operate in a typical natural monopoly where substitutability across industries is low (e.g. Police Officers), but rather is representative of potential substitutability in the economy wide labour force.

4 Empirical Framework

To estimate the labour supply elasticity to the firm I estimate the recruitment and separation elasticities separately and then add the negative of the separation elasticity to the recruitment elasticity.

4.1 Baseline Specifications

The baseline specification for analysing the recruitment side is of the form:

$$ln(Recruits_{ajemy}) = \beta_1 ln(Wage_{ajemy}) + \gamma_{je} + \lambda_{ey} + \nu_{ym} + \theta_{jy} + \epsilon_{ajemy} \tag{7}$$

where a are job-adverts, for job-role j in establishment e in month-year my. $Wage_{ajemy}$ refers to the advertised hourly wage (in £), γ_{je} are job-role establishment fixed effects, λ_{ey} and θ_{jy} are time-varying establishment and job-role fixed effects [14], ν_{ym} are year-month fixed effects. $Recruits_{ajemy}$ will be calculated according to

$$Recruits_{ajemy} = Hires_{ajemy} + Rejects_{ajemy} = Applicants_{ajemy} - Turn\ Downs_{ajemy} \tag{8}$$

and is the empirical counterpart to equation (6), which captures an accurate willingness to join. Adding workers who were rejected by the company to hires removes labour demand effects, while taking away those who turn down a job from applicants ensures I am not overestimating recruits. For an elasticity interpretation $Recruits$ is logged, due to the existence of a few zero observations I apply a $ln(1+Recruits)$ adjustment and for robustness use the inverse hyperbolic since (IHS). In practice however the number of zero observations is small (approximately 2%) and so it's unlikely this will bias the results.

In addition to the above, to show the differences between the recruit, application and hire elasticity I run equation (7) using ln applicants and ln hires as the dependent variable. Additionally, to probe the relationship in equation (5) and the two differentials $\frac{\partial \phi}{\partial w}$ and $\frac{\lambda \phi}{\partial w}$, I estimate equa-

[13]Specifically, occupations utilised by The Company appear in 121 industries (SD = 119) while the equivalent number across all occupations is 131 (SD = 125), in total there are 368 occupations in the full sample, and 37 utilised by the firm. Therefore $t = \frac{(121-131)}{\sqrt{\frac{14207}{37} + \frac{15675}{368}}} = -0.484$.

[14]The time varying job-role fixed effects are only used in some specifications due to saturation concerns.

tion (7) altering the dependent variable between $Applicants_{ajemy}$, $Hires_{ajemy}$, $Rejects_{ajemy}$ and $Turn\ Downs_{ajemy}$. This exercise will in particular demonstrate how using only hires or applicants will bias the estimates of the recruitment elasticity.

To estimate the separation elasticity I regress a linear probability model (LPM) of the form:

$$Separate_{ijemy} = \beta_2 ln(Wage_{ijemy}) + \gamma_{je} + \lambda_{emy} + \theta_{jmy} + \epsilon_{ijemy} \tag{9}$$

where $Separate_{ijemy}$ is an indicator variable equal to 1 if individual i leaves job-role j in establishment e in a particular year-month ym, and equal to 0 otherwise.[15]

Equations (7) and (9) utilise variation within the same job role-establishment combination while controlling for establishment-level time shocks, and job-level time shocks. They are akin to a triple-difference specification. Though these specifications are very flexible, concerns relating to endogeneity still exist. Job-location specific time shocks are still conceivable, which in turn could be correlated with wages, recruitment and separations (see Belot et al. (2022); Marinescu and Wolthoff (2020) for evidence of this). As a result I make use of the instruments outlined in section 3.2. The first LW_{jemy}, the Living Wage instrument which is defined at the establishment-job-time level, and the second, AL_{ajemy}, the annual leave wage saliency instrument which is defined at the advert level.

$$LW_{jemy} = \begin{cases} 1 & \text{if establishment is subject to LW \& LW was binding for job} \\ 0 & \text{otherwise} \end{cases} \tag{10}$$

$$AL_{ajemy} = \begin{cases} 1 & \text{if advert included annual leave in hourly rate} \\ 0 & \text{otherwise} \end{cases} \tag{11}$$

Specifically, I instrument $ln(Wage)$ in recruitment equation (7) with both instruments, and in separation equation (9) with just the Living Wage instrument, as the annual leave instrument is not well defined for separations.

Assuming parameters β_1 and β_2 are identified, then

$$\varepsilon_{nw} = \underbrace{\varepsilon_{rw}}_{\hat{\beta}_1} - \underbrace{\varepsilon_{sw}}_{\frac{\hat{\beta}_3}{E[\widehat{Separate}_{ijemy}]}} \tag{12}$$

4.2 Robustness

To strengthen the credibility of the identification strategy employed, I perform a number of robustness checks.

[15] A recent survey from Sokolova and Sorensen (2021) found that results estimated using the more straightforward LPM were not statistically different from results utilising non-linear estimation techniques.

4.2.1 Overidentification in 2SLS

The reliability of the estimate from equations (7) and (9) rely on the validity of the two instruments. When estimating the recruitment elasticity, thanks to the presence of two instruments the two stage least squares approach is over-identified and therefore I can perform a Sargan-Hansen J test to test the validity of the instruments.

4.2.2 Anticipation Effects and Parallel Trends

One may have a concern that an announcement effect may cause a bias towards zero in the separation elasticity estimates. Specifically, if workers employed by The Company in a particular establishment find out many months before the introduction of the Living Wage that they are to receive a substantial pay increase, they may decide to stay on with the firm longer. Discussions with the director of human resources suggests this is unlikely to be a concern as treated workers only found out relatively near to the treatment date. However, it is prudent to empirically test for anticipation effects. Additionally, while the estimates for the recruitment elasticity can utilise two instruments, one which induces variation at the advert level, the separation estimates rely solely on the the living wage instrument which induces variation at the job-establishment-time level. A key assumption for a credible estimate from this instrument is the presence of parallel time trends in the absence of living wage introduction.

To check both the above I estimate a triple-difference event study for separations of the form

$$Separate_{ijemy} = \sum_{l \neq -1,-11} \beta_{3,l} LW_{je,my+l} + \gamma_{je} + \lambda_{emy} + \theta_{jmy} + \epsilon_{ijemy} \tag{13}$$

where $l \in \{-12, ..., 12\}$ and the end points are binned such that $LW_{je,my+12} = 1 \; \forall \; \{l \; \geq 12 : LW_{je,my+l} = 1\}$ and $LW_{je,m-12} = 1 \; \forall \; \{l \; \leq -12 : LW_{je,my+l} = 1\}$.[16] Monthly effects are aggregated to the quarter, q such that

$$\hat{\beta}_3^q = \sum_{l \in q} \frac{1}{3} \hat{\beta_{4,l}} \tag{14}$$

and one may note that the monthly parameter effects are normalised to two periods, -1 and -11, as recommended in Borusyak et al. (2021).

4.2.3 Local spillovers from the Living Wage

As discussed, to ensure the living wage is able to identify the labour supply elasticity to the firm, the living wage must not affect other jobs in the local area. Specifically, a labour supply elasticity to the firm is identified only when there is exogenous variation to a firm's wages, while other jobs in the local market are unaffected. To add to descriptive evidence from section 3.2 I perform a difference-in-difference estimate looking at the impact on the propensity of a worker

[16]This implies that the first and last parameter estimate in 13 contain longer run pre and post effects.

to be paid the living wage rate when the local council signs up to the Living Wage foundation, using a nationally representative dataset (ASHE). I estimate

$$LW_{ilt} = \beta_4 LWLA_{lt} + \gamma_l + \lambda_t + \epsilon_{ilt} \tag{15}$$

where LW_{ilt} is an indicator if individual i working in local authority l is paid the Living Wage (specifically within 5 pence of the Living Wage) in year t. $LWLA_{lt}$ is an indicator variable indicating whether local authority l was an accredited Living Wage payer in year t, and γ_l and λ_t are local authority and year fixed effects respecitvely. The regression uses a sample of all workers within ASHE from 2012 - 2019. An estimate of $\hat{\beta_4} = 0$ would be indicative of no spillovers, and imply the wage shift from the LW does not affect the rest of the local market. Thus, a null result would indicate both that the instrument only directly effects a negligible number of firms in the market, and those that are affected do not initiate best responses from other firms and thus would be instructive of market structure and the appropriateness of using a monopsony wage setting framework.

For completeness I also estimate an event study equivalent to equation (15)

$$LW_{ilt} = \sum_{k \neq -1} \beta_5^k LWLA_{lt+k} + \gamma_l + \lambda_t + \epsilon_{ilt} \tag{16}$$

for values of k from -6 to +7 and where $LWLA_{lt+k}$ is an indicator demonstrating whether the local authority received treatment in a specific year. This is performed using both the standard TWFE estimator as well as the Sun and Abraham (2020) estimator.

4.2.4 Staggered Treatment Timing

There has been a recent interest in the workings of two-way fixed effect estimators, in particular utilising staggered treatment times (Borusyak et al., 2021; Sun and Abraham, 2020; Callaway and Sant'Anna, 2020; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021). Concerns raised include: issues identifying the linear component of the path of pre-trends in traditional event study specifications (Borusyak et al., 2021), contamination of lead and lag coefficients from other period effects (Sun and Abraham, 2020), biased estimates of treatment effects when the control group contains treated units when dynamic treatment effects are present (Goodman-Bacon, 2021) and the structure of weights assigned across treatment cohorts when estimating dynamic treatment effects (Sun and Abraham, 2020). The approach in this paper is more flexible than a two-way fixed effect estimator, and when utilising just the Living Wage instrument, akin to a triple-difference estimator.[17] Additionally, it is not obvious why some of these issues would be present in the current setting. For example, dynamic treatment effects are unlikely when studying the response of number of applicants in response to wage changes. Despite this fact, as a matter of caution to check whether any of these issues could be sullying the estimated effects when using the Living Wage instrument I implement a two-way fixed effects event study estimator at the establishment level akin to that suggested in Sun and Abraham

[17] The approach when using the annual leave instrument is unrelated to these issues.

(2020) while also implementing adjustments as recommended in Borusyak et al. (2021). This estimator is the same implemented in Datta and Machin (2023) and is described in section A.3 in the appendix. I compare these results to a traditional two-way fixed effects event-study estimator at the establishment level of the form

$$Y_{et} = \alpha_e + \lambda_t + \sum_{l \neq = -1, -12} \delta_l LW_{e,t+l} + \beta' X_{et} + \epsilon_{et} \tag{17}$$

with monthly effects aggregated to the month per

$$\hat{v}_g^{pooled} = \sum_{l \in q} \frac{1}{3} \hat{\delta}_l \tag{18}$$

to see if there is a fundamental difference between the results.

5 Results

5.1 Recruits

Table 2 presents estimates of $\hat{\beta}_1$ from specification 7, column (1) reports OLS estimates and columns (2-5) where $ln(Wage)$ is instrumented using one or both of the two instruments discussed in section 3.2. It also reports the relevant first stage coefficients for the specifications where only one of the two instruments are employed. Column (2) reports the specification utilising the entire sample and the living wage instrument, column (3) utilising only the sample of non-salaried adverts and the annual leave instrument, column (4) the non-salaried sample and the living wage instrument, and column (5) the non-salaried sample using both instruments. The specifications using the smaller sample do not include job-year fix effects to reduce saturation concerns.

All specifications report a statistically significant recruitment-wage elasticity, with estimates ranging between 2.1 to 3.9, aside from the OLS specification which underestimates the elasticity, a finding similar to other recent studies (e.g. Marinescu and Wolthoff (2020)). A possible explanation for this is that firms adjust wages for occupations to match local market conditions, and therefore wage changes within firm-jobs are correlated to outside wage options. The midpoint of the IV estimates implies a $\varepsilon_{rw} \approx 3$, and that a 10% increase in the posted wage would increase recruits by 30%. All specifications report a sizeable F-statistic, demonstrating strength in both instruments. Specification (5) additionally reports the Hansen J statistic exploiting the fact the equation is overidentified. The reported χ^2 statistic confirms that the validity of the instruments cannot be rejected.

Table 8 in the appendix presents the counterpart table using the inverse hyperbolic sine of the dependent variable. The pattern of the results is fundamentally unchanged qualitatively, and quantitatively the instrumented IHS elasticities are only marginally larger (the difference is only 0.5), but not statistically different.

Table 2: Recruitment - Wage Estimates 1

	(1) ln(Rec)	(2) ln(Rec)	(3) ln(Rec)	(4) ln(Rec)	(5) ln(Rec)
ln(Wage)	0.592***	3.856**	3.507***	2.120***	2.486***
	(0.225)	(1.913)	(1.104)	(0.634)	(0.554)
Job-Establishment FE	Yes	Yes	Yes	Yes	Yes
Establishment-Year FE	Yes	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes	Yes
Job-Year FE	Yes	Yes	No	No	No
N	5372	5372	2301	2301	2301
First Stage Coefficient	-	.029***	.064***	.086***	-
		(.005)	(.009)	(.008)	
First Stage F-Stat.	-	33.99	55.57	112.2	89.97
Hansen J Stat.	-	-	-	-	1.521
Instrumented with *AL*	No	No	No	Yes	Yes
Instrumented with *LW*	No	Yes	Yes	No	Yes
Sample	All	All	Non-Salaried	Non-Salaried	Non-Salaried

Note: *The table presents estimates of* $\hat{\beta}_1$ *from equation (7) via OLS or where* $ln(Wage)$ *is instrumented with either* LW_{jemy}, AL_{ajemy} *or both instruments. The dependent variable is the natural logarithm of* $(1 + Recruits_{ajemy})$ *where* $Recruit_{ajemy}$ *is defined as per equation (8). If only one instrument is used the table reports the accompanying first stage coefficient. If both instruments are used the table reports the over identifying test statistic. All instrumented columns report the first stage Kleibergen-Paap F statistic. Standard errors are reported in parentheses and are clustered at the establishment-job. Cols (1) and (2) includes a control for whether the job advert was for a salaried position.* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3 presents estimates of the hire and application elasticity using both instruments, and the associated recruitment elasticity (i.e. column (6) from tables 2 and 8) for comparison. As expected, the hire elasticity underestimates the recruitment elasticity, and is approximately 40% smaller, while the application elasticity overestimates the recruitment elasticity, but only by about 20%. Table 9 probes this relationship further by looking at the level changes to applications, hires, rejects and turn downs. The results suggest that a doubling of the wage would result in 32 more applications, of these 32 applicants only 5 get hired, 6 turn the job down (a labour supply effect) and 20 get rejected by the firm (a labour demand effect). The fact that the largest change is to rejects, is indicative of why the hire elasticity underestimates the true recruitment elasticity by considerably more than the application elasticity overestimates it. These rejected applicants are still willing to work for the firm at the going wage rate, but due to labour demand conditions (e.g. the firm only has one vacancy) they are not hired.

Table 3: Recruitment - Wage Estimates 2

	(1) ln(Rec)	(2) ln(Hires)	(3) ln(Apps)	(4) ihs(Rec)	(5) ihs(Hires)	(6) ihs(Apps)
ln(Wage)	2.486***	1.489***	2.912***	2.923***	1.916***	3.387***
	(0.554)	(0.560)	(0.644)	(0.678)	(0.725)	(0.787)
Job-Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Establishment-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
N	2301	2301	2301	2301	2301	2301
Hansen J Stat.	1.521	3.373	0.778	1.634	3.525	0.924
Inst. W AL	Yes	Yes	Yes	Yes	Yes	Yes
Inst. W LW	Yes	Yes	Yes	Yes	Yes	Yes

Note: *The table presents estimates of* $\hat{\beta}_1$ *from equation (7), varying the dependent variable between ln*$(1 + Recruits_{ajemy})$*, ln*$(1 + Applicants_{ajemy})$*, ln*$(1 + Hiresajemy)$*, or their inverse hyperbolic since counterparts. ln*$(Wage)$ *is instrumented with* LW_{jemy} *and* AL_{ajemy}*. The table reports the over identifying test statistic. Corresponding first stage F-statistics are identical to those in column (5) of table 2. Standard errors are reported in parentheses and are clustered at the establishment-job.* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.2 Separations

Table 4 presents estimates of $\hat{\beta}_2$ from (9) via OLS and using the living wage instrument, as well as $E[\widehat{Separate}_{ijemy}]$. Column (1) reports OLS estimates without controls, Columns (2) and (4) reports IV estimates without controls, columns (3) and (5) reports IV estimates with controls for gender, ethnic minority status, whether salaried or not (if applicable), tenure and age, while columns (2-3) use the sample of all workers and columns (4-5) use the sample of non-salaried staff to give consistency in the sample with some of the recruitment elasticity specifications in table 2. All IV specifications report the first stage and associated F statistic. The OLS estimate as before underestimates the elasticity. The IV estimate is robust across specifications, with a strong first stage, and implies $\varepsilon_{sw} \approx -1.6$, implying a 10% increase in the wage reduces the likelihood of separation in any month by 16%.

Table 4: Separation - Wage Estimates

	(1) Separate	(2) Separate	(3) Separate	(4) Separate	(5) Separate
ln(Wage)	-0.0234***	-0.0791**	-0.0854***	-0.0734***	-0.0753***
	(0.00324)	(0.0317)	(0.0323)	(0.0271)	(0.0270)
Job-Centre FE	Yes	Yes	Yes	Yes	Yes
Centre-Time FE	Yes	Yes	Yes	Yes	Yes
Job-Time FE	Yes	Yes	Yes	Yes	Yes
Controls	No	No	Yes	No	Yes
N	1055521	1055521	1055521	773907	773907
First Stage Coefficient	-	.062***	.059***	.084***	.082***
	-	(.003)	(.003)	(.004)	(.004)
First Stage F-Stat.		327.3	332.3	454.2	473.0
Instrumented with *LW*	No	Yes	Yes	Yes	Yes
Sample	All	All	All	Non-Salaried	Non-Salaried
Mean of Dep. Var		0.048		0.050	

Note: *The table presents estimates of $\hat{\beta}_2$ from equation (9) via OLS or where $ln(Wage)$ is instrumented with LW_{jemy}. Instrumented specifications report the first stage coefficient, and corresponding Kleibergen-Paap F statistic. Standard errors are reported in parentheses and are clustered at the establishment-job.* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.3 Robustness

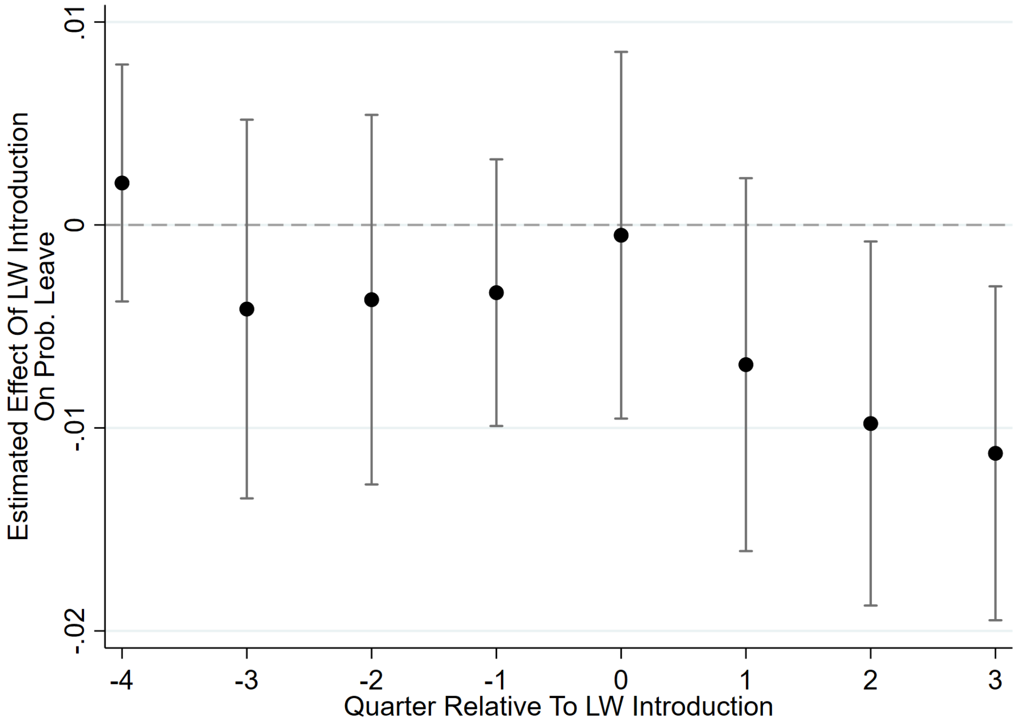
5.3.1 Anticipation Effects and Parallel Trends

Figure 3 presents estimates of the event study from equations (13) and (14). The results suggest that the restriction of parallel pre-trends can not be rejected, and there is no evidence of anticipatory effects. The figure additionally shows that after one quarter of the introduction of the Living Wage, there is a clear drop in the rate of separations which continues to fall during the second and third quarter proceeding the introduction.

5.3.2 Local Spillovers from the Living Wage

Table 5 presents the estimates of $\hat{\beta}_4$ from equation (15) using a sample of the UKs social security data. The interpretation on the coefficient is that once a local authority signs up to the living wage, the probability of a worker being paid the living wage decreases by 0.05%, and is not statistically significant. The direction of the coefficient is the opposite to what we'd expect if

Figure 3: Separations Event Study



Note: *The figure presents estimates and 95% confidence intervals for parameters* $\hat{\beta}_3^q$ from equation 14. The sample is identical to those in the counterpart regressions in columns (1)-(3) in table 4 using data from The Company. Standard errors are clustered at the establishment-job.

other workers in the area saw an increase in Living Wage rate payment.[18] The top of the 95% confidence interval would suggest a 0.05% increase, which would translate to 33.8 workers in a LA getting paid the Living Wage, which is roughly the number of workers affected by The Company in one of their establishments. As a point of comparison, approximately 4000 people in each LA get paid the nationally mandated minimum wage. The results therefore suggest a null effect of a local authority signing up to the Living Wage, on the propensity for workers within the local authority to be paid the living wage.

The results supplement the descriptive evidence given in section 3.2. Figure 4 graphically presents coefficients of $\hat{\beta}_5^k$ from the counterpart event study equation 16 using both a standard TWFE approach as well as the Sun and Abraham (2020) estimator. Both show no fundamental changes to the patterns of the probability of being paid the Living Wage after the introduction in the local authority, and the estimated parameters using the two approaches are almost identical. The fact that no spillovers are found suggests both that only an undetectable number of firms are treated, and that other firms do not respond to wage changes from our treated firm. The latter of these points suggests abstracting away from oligopsony best response behaviours is reasonable as echoed by other recent studies (Roussille and Scuderi, 2022) in high paying sectors, and the

[18] As the social security sample is only 1% of all employees it's unlikely it will include many workers from The Company.

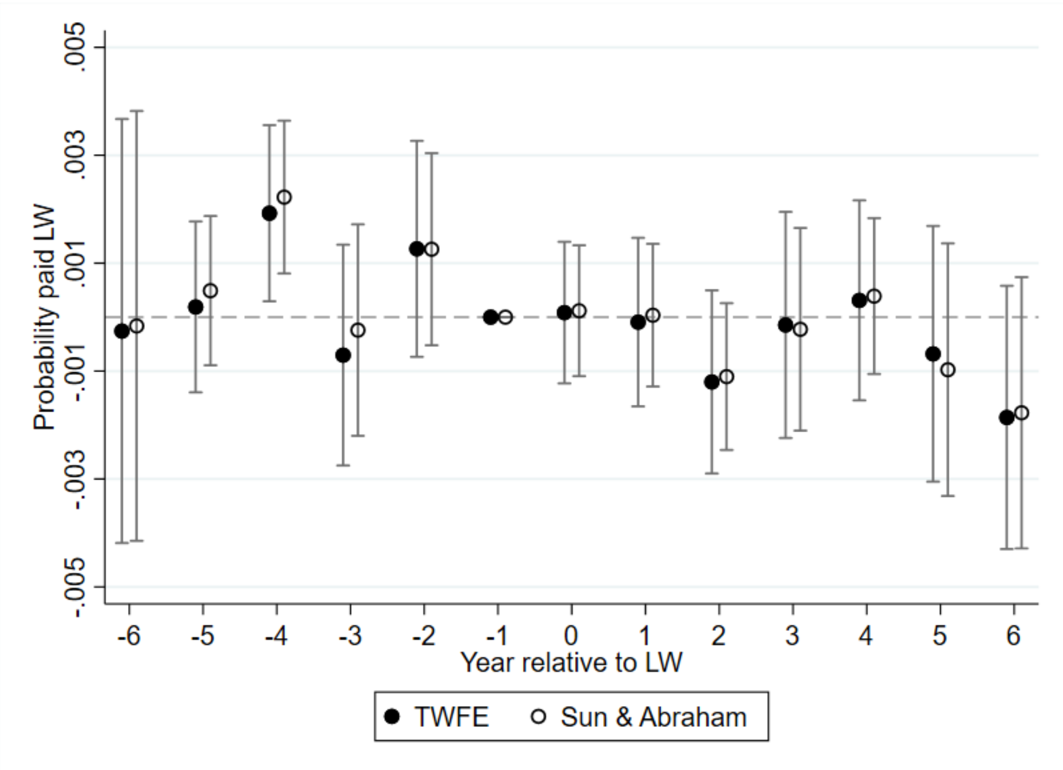
"reduced form" elasticity is synonymous with the "structural" elasticity (Berger et al., 2022). The average number of employees in an establishment for The Company is 53, which puts it into the top 10th percentile of establishment size for employers in the UK, and suggests that a monopsonistic wage setting framework for all but the largest of employers is reasonable.

Table 5: Spillover Estimates

	(1) LW
LWLA	-0.000554
	(0.000562)
Local Authority FE	Yes
Time FE	Yes
N	1442179

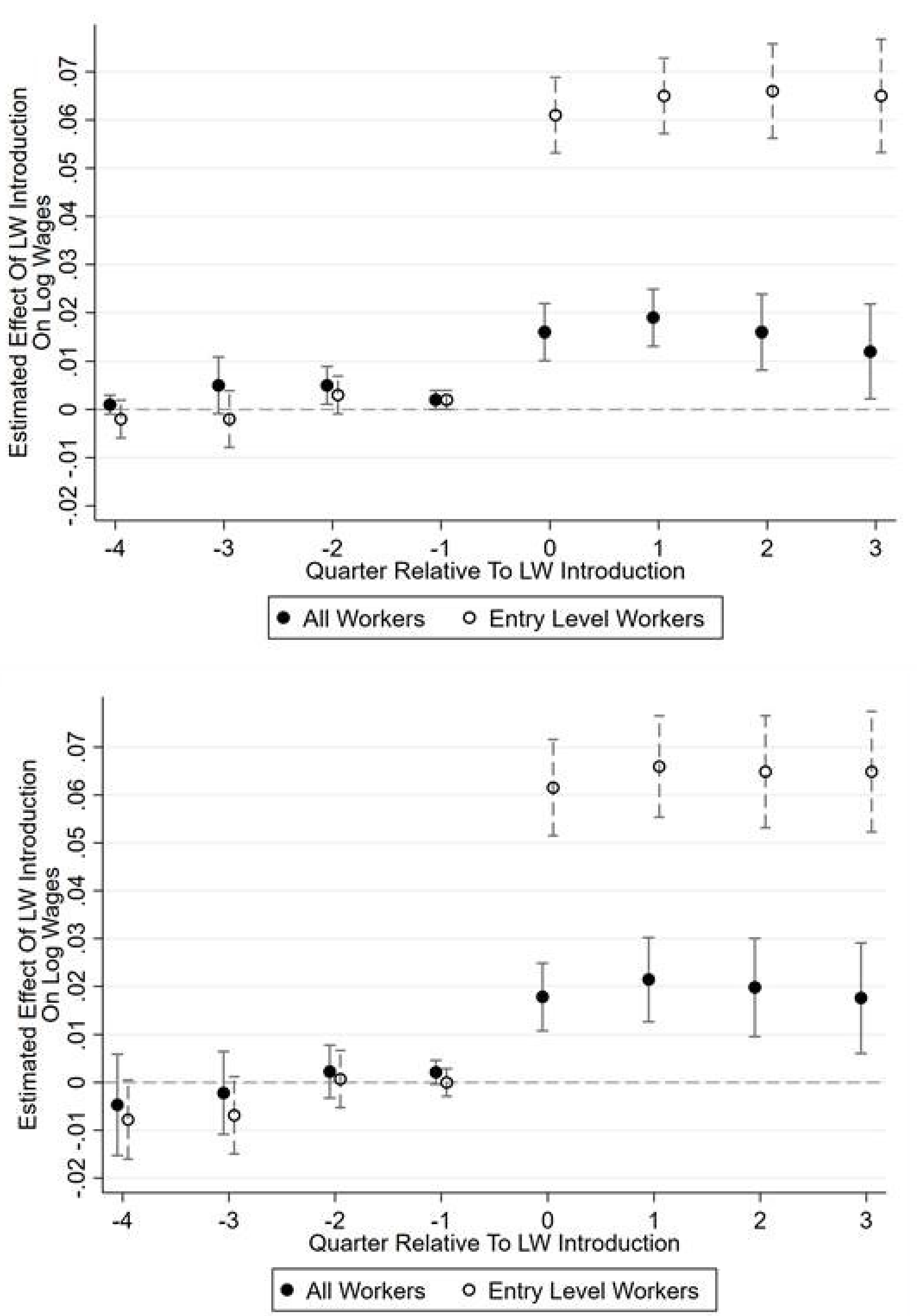
Note: *The table presents estimates of* $\hat{\beta}_4$ *from equation (15) using data from ASHE. Standard errors are reported in parentheses and are clustered at the local authority.* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 4: Spillovers Event Study



Note: *The figure presents estimates and 95% confidence intervals for parameters* $\hat{\beta}_5^q$ from equation (16) using data from ASHE. Black dots are estimated using a standard two-way fixed effect estimator, white dots are estimated according to Sun and Abraham (2020) The sample is identical to those in the counterpart regression in table 5. Standard errors are clustered at the local authority.

Figure 5: Staggered Timing Robustness



Note: *The top panel presents estimates from the Sun & Abraham style estimator, from equation 25, while the bottom panel presents results from a traditional TWFE estimate as per equation 18. Both panels use a sample of establishments run by The Company active between January 2011 and April 2019, and are based off 17,879 establishment year-month observations. The vertical bars indicate 95% confidence intervals. For the top panel these are based on 1000 bootstraps.*

5.3.3 Staggered Treatment Timing

The top panel of figure 5 presents estimates of $\hat{v}_g$ from equation 25, using the more transparent Sun and Abraham approach. The bottom panel of figure 5 presents estimates using the standard pooled two-way fixed effects estimates of $\hat{v}_g^{pooled}$ from equation 18. There is little obvious difference between the two panels for both impacts on all workers' and entry-level workers' wages at the establishment level. Both suggest parallel pre-trends, and stable dynamic treatment effects, with entry level workers experiencing 6% greater wage growth in treated establishments. This result is also consistent with the triple-difference first stage estimate from table 4. Given these results it is unlikely that the more flexible specifications utilising a triple-difference estimator will suffer from the concerns outlined in section 4.2.4. Further evidence on the effects difference between standard TWFE and Sun and Abraham estimators are present in Datta and Machin (2023), where they show using the same dataset there are no major differences in the estimated effects across a wide range of firm-level dependent variables including employment, labour-labour substitution, promotions and wage profile coarseness.

5.4 Discussion

Combining the estimates that $\varepsilon_{rw} \approx 3$ and $\varepsilon_{sw} \approx -1.6$, this implies $\varepsilon_{nw} \approx 4.6$. According to the canonical markdown equation[19] the estimates suggests a wage markdown of 18%. These results suggest considerable market power in a low wage labour market where frictions such as firm-specific capital are less likely to play a role.

The estimate of the labour supply elasticity to the firm above makes three advances on the existing literature. First, it has an extremely credible identification strategy utilising two instruments which generate exogenous variation in wages at the job-establishment-time level, and advert-job-establishment-time level respectively, while leaving the remainder of the labour market remains unchanged. It does this for a sample of low pay occupations which appear across a number of industries in the private sector, and follow a similar substitutability pattern as the national average from the workers perspective.

Second, unlike almost all existing estimates of the labour supply elasticity to the firm, it does not rely on the result from Manning (2003) that $\varepsilon_{rw} = -\varepsilon_{sw}$. It is possible there may be differences in firm's monopsony power over new recruits and separations, and the above results test this. My findings suggest that the recruitment-wage elasticity is approximately twice the size of the separation-wage elasticity, and therefore firms exercise more monopsony power over incumbent workers than in attracting new recruits. If workers' productivity increases in a job over time then incumbent workers may see higher markdowns than recruits, which in turn would be consistent with recent evidence on fair pay structures within firms (Dube et al., 2019; Giupponi and Machin, 2018) where workers with the same job-title tend to be paid the same rate. On the other hand if workers' productivity stays consistent then fair pay structures may imply that

[19] $\mu = 1 - \frac{\varepsilon_{nw}}{1+\varepsilon_{nw}}$.

firms are not exploiting all the monopsony power that they have over incumbent workers as suggested by other recent evidence (Dube et al., 2020b).

Finally, these results exploit detailed information in the hiring data to create an accurate picture of the latent measure of recruits. The results suggest that using applicants or completed hires will bias the recruitment elasticity up or downwards respectively.

Crucially, the results suggest that all existing estimates which rely solely on just a recruitment or separation estimate, and double the single elasticity are inaccurate, and those relying on applicant data are further biased. While the correct markdown is estimated to be 18%, incorrectly doubling the application or separation elasticities would results in a markdown as low as 13% or as high as 24%.

5.4.1 Heterogeneity: Probing the Differences between Recruits and Separations

The finding that $\varepsilon_{rw} > \varepsilon_{sw}$ is an interesting one, and the source of this deserves a thorough investigation which is beyond the scope of this paper. However, a possible explanation is that wages are very salient for job seekers. Many job search platforms clearly advertise salaries, or salary bands. Furthermore, certain non-wage aspects of working for a company only tend to become clear after joining, such as degree of autonomy, relationship with fellow employees and managers. As a result, job seekers may be more sensitive to wages. Conversely, once workers join a firm, the non-wage aspects of the job which were unobservable before joining become more salient relative to wages, resulting in a lower initial separation-wage elasticity. As tenure in a job increases and workers restart search, wages may in turn become more salient.[20]

To explore the plausibility of the above mechanism I estimate heterogeneity in the separation-wage elasticity by tenure. If this story of saliency of wages relative to other job characteristics were true, we would expect to see a low separation-wage elasticity for the most recent joiners, and one that grows (in absolute value) over time. I estimate a version of the original separation equation (9) that allows for heterogeneity in tenure,

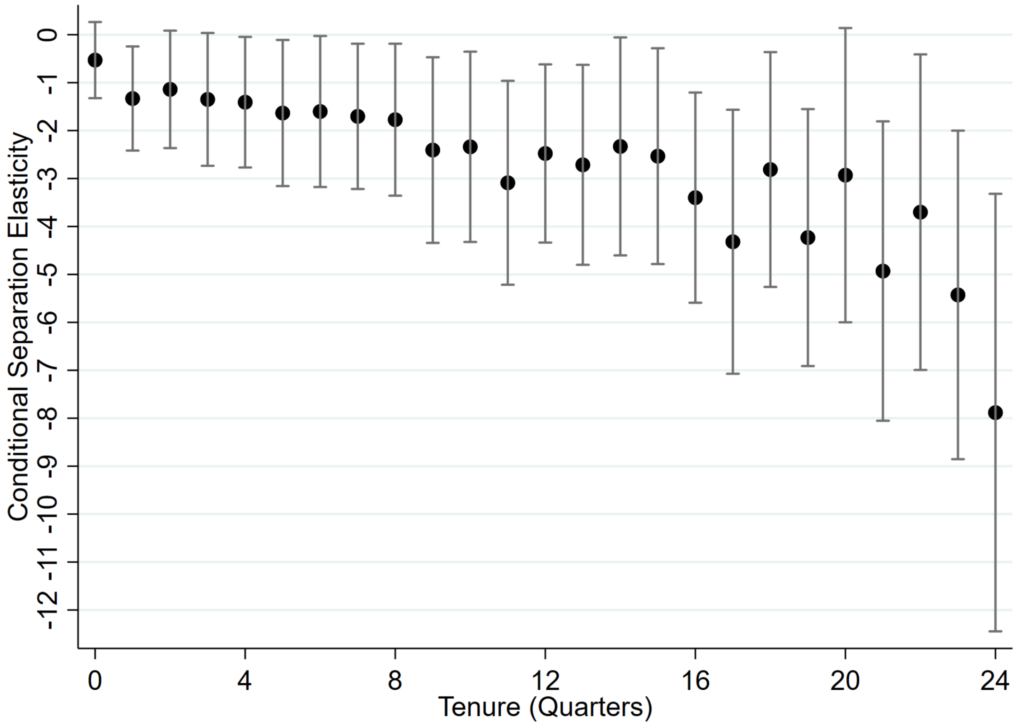
$$Separate_{ijemy} = \sum_{q \in Q} \beta_2^q ln(Wage_{ijemy}) \text{ X } \mathbb{1}[Tenure = q] + T_q\phi + \gamma_{je} + \lambda_{emy} + \theta_{jmy} + \epsilon_{ijemy} \quad (19)$$

where q are quarters of tenure, and T is a vector of indicators for tenure quarters q. As before I instrument $ln(Wage_{ijemy})$ with the Living wage instrument LW_{jemy}. To calculate the tenure-specific elasticities I use the following equation

$$\varepsilon_{sw,q} = \frac{\beta_2^q}{E[Separate_{ijemy}|Tenure = q]} \quad (20)$$

[20]The role of sunk costs in adjustment for workers is ruled out as separation rates are higher for those most recently joined.

Figure 6: Separation Elasticity by Tenure



Note: *The figure presents estimates of* β_2^q *and its 95% confidence intervals from equation (20). The sample is identical to the baseline estimate from equation (13). Standard errors are clustered at the establishment-job.*

Figure 6 graphically presents the estimates of $\hat{\varepsilon}_{sw,t}$ along with the associated 95% confidence intervals[21] against tenure. The results show a clear increase in the absolute size of the elasticity as tenure increases. Those just joining are completely insensitive to wage changes, suggesting that other non-wage factors are more important to them at that point in time. Using the midpoint of the recruitment elasticities from section 5.1 of 3, the negative of the separation elasticity becomes equal to the recruitment elasticity at approximately 3-4 years of tenure. As the average tenure within the firm is 21 months, it is unsurprising the separation elasticity is considerably lower than the recruitment elasticity.

5.4.2 Heterogeneity: Differences in Monopsony Power by Gender

In her seminal book Joan Robinson (Robinson, 1933) suggested that the gender wage gap could be in part attributed to differences in the labour supply elasticity to the firm of men versus women.[22] Specifically, if women faced a more inelastic labour supply curve to the firm, then they could face a greater markdown of wages. To test this hypothesis I estimate my baseline

[21] To calculate the standard errors the denominator in equation 20 are treated as constants, as the ratio of two normally distributed random variables is Cauchy distributed and does not have well defined moments.

[22] For recent examples of literature exploring this topic see Caldwell and Oehlsen (2022); Card et al. (2016); Webber (2016); Hirsch et al. (2010); Ransom and Sims (2010).

separation equation 9 with an interaction against female.

$$Separate_{ijemy} = \beta_6 ln(Wage_{ijemy}) + \beta_7 ln(Wage_{ijemy})\text{X } Female_i + \beta_8 Female_i \\ + \gamma_{je} + \lambda_{emy} + \theta_{jmy} + \epsilon_{ijemy} \tag{21}$$

Table 10 in the appendix reports the estimates from equation (21). The point estimate on $\hat{\beta_7}$ is tiny (almost one twentieth the size of $\hat{\beta_6}$), statistically insignificant, and negative (which would suggest greater sensitivity to wages). I thus find no evidence to suggest differences in the labour supply elasticity to the firm between men and women could be a factor in generating the gender wage gap.

6 Conclusion

This paper provides new evidence on the extent of monopsony power in the labour market. Utilising two instruments and a rich bespoke dataset that contains HR, vacancy and applicant information for a firm with hundreds of establishments across the UK, who faces similar competition and job movement to other service sector firms, I evaluate the firm's labour supply elasticity. I estimate both of its components- the recruitment-wage and separation-wage elasticities and the results suggest the existence of considerable monopsony power with wage markdowns of 18%.

The results demonstrate large differences in the recruitment and separation elasticities despite many estimates of monopsony power in the literature relying on an assumption they are equal in absolute value. The paper further documents that neither completed hires nor applicants are an accurate measure of recruits, the willingness-to-join side of labour supply, as the former is sullied with labour demand effects and the latter includes individuals who in the end would not actually accept the job. This paper utilises details on the hiring process and outcomes of all applicants to alleviate this issue and demonstrates that incorrectly doubling a separation or application elasticity can lead to differences away from the true labour supply elasticity by up to 50%.

Further empirical checks demonstrate that exogenous wage changes to an individual firm do not appear to spillover to other firms in the local area, suggesting abstracting away from strategic interaction is not unreasonable. Heterogeneity analysis suggests that differences in recruitment and separation elasticities may be driven by differences in wage saliency for job searchers versus incumbents, and there is a clear increase in the separation elasticity as tenure within a firm increases. Furthermore, there is no evidence the gender pay gap is driven by differences in their labour supply elasticity.

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A Appendix

A.1 Additional Tables

Table 6: Summary Statistics, Adverts

	All			**Non-salaried**		
Variable	Mean	S.D.	Median			
Hourly Rate (£)	11.08	3.50	10.20	12.30	4.51	10.20
Entry Level	0.47			0.51		
London	0.55		0.50			
No. Applicants	7.6	10.7	5	6.6	8.4	4
No. Hires	1.2	1.6	1	1.5	1	1.9
No. Rejects	5	9.2	2	4.1	6.6	2
No. Turned Down	1.1	2.2	0	1.0	1.9	0
N	5478			2301		

Note: *The table presents summary statistics for the job adverts for The Company.*

Table 7: London Borough of Hackney, Employment

	London Borough of Hackney (estim)	
Sector	Employment	%
All	133,000	100
Private	115,100	86
Public		
NHS	5,549	4.3
Council	4,390	3.3
Civil Service	1,790	1.4
Education (LEA)	2,148	1.6
Education (Acad.)	2,864	2.1
Other	1159	1.3

Note: *The table presents employment shares by sector for the London Borough of Hackney for the year 2019.*

Table 8: Recruitment - Wage Estimates 3

	(1)	(2)	(3)	(4)	(5)
	ihs(Rec)	ihs(Rec)	ihs(Rec)	ihs(Rec)	ihs(Rec)
ln Wage	0.664**	4.694**	4.222***	2.458***	2.923***
	(0.273)	(2.344)	(1.355)	(0.777)	(0.678)
Job-Establishment FE	Yes	Yes	Yes	Yes	Yes
Establishment-Year FE	Yes	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes	Yes
Job-Year FE	Yes	Yes	No	No	No
N	5372	5372	2301	2301	2301
Hansen J Stat.	-	-	-	-	1.634
Instrumented With AL	No	No	No	Yes	Yes
Instrumented With LW	No	Yes	Yes	No	Yes
Sample	All	All	Non-Salaried	Non-Salaried	Non-Salaried

Note: *The table presents estimates of* $\hat{\beta}_1$ *from equation (7) via OLS or where* $ln(Wage)$ *is instrumented with either* LW_{jemy}, AL_{ajemy} *or both instruments. The dependent variable is the inverse hyperbolic sine of* $Recruits_{ajemy}$ *where* $Recruit_{ajemy}$ *is defined as per equation (8). If both instruments are used the table reports the over identifying test statistic. Standard errors are reported in parentheses and are clustered at the establishment-job. Cols (1) and (2) includes a control for whether the job advert was for a salaried position. Corresponding first stage coefficients and F-statistics are identical to those in table 2.* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Recruitment - Wage Estimates 4

	(1)	(2)	(3)	(4)
	Apps	Hires	Rejects	Turn Downs
ln(Wage)	32.38***	5.244***	19.98***	6.057***
	(6.237)	(1.683)	(4.698)	(1.951)
Job-Establishment FE	Yes	Yes	Yes	Yes
Establishment-Year FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
N	2301	2301	2301	2301
Hansen J Stat.	0.00124	0.781	0.00340	1.547
Inst. W AL	Yes	Yes	Yes	Yes
Inst. W LW	Yes	Yes	Yes	Yes

Note: *The table presents estimates of* $\hat{\beta}_1$ *from equation (7) with the dependent variable as* $Applicants_{ajemy}$, $Hires_{ajemy}$, $Rejects_{ajemy}$, *or* $Turn\ Downs_{ajemy}$. $ln(Wage)$ *is instrumented with* LW_{jemy} *and* AL_{ajemy}. *The table reports the over identifying test statistic. Standard errors are reported in parentheses and are clustered at the establishment-job. Corresponding first stage F-statistics are identical to those in column (5) of table 2.* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

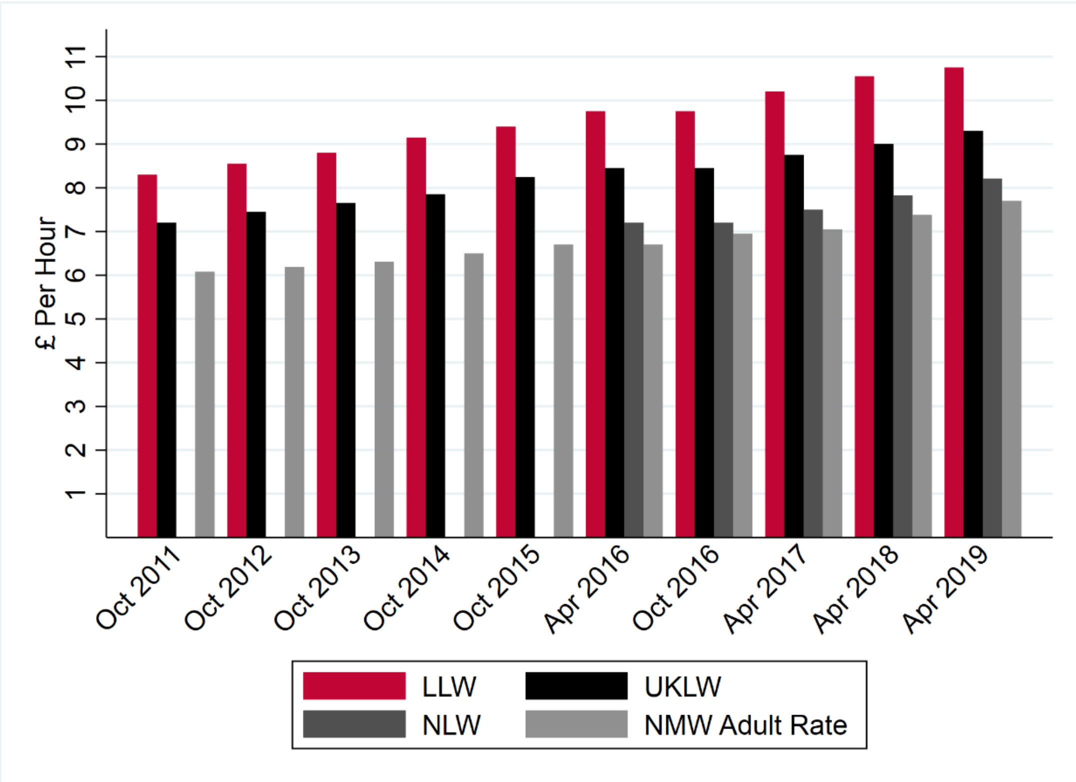
Table 10: Separation - Wage Estimates 2

	(1) Separate
ln(Wage)	-0.0826**
	(0.0328)
ln(Wage) X Female	-0.00590
	(0.00715)
N	1055521
First Stage F-Stat.	164.2
Controls	Yes
Sample	All

Note: *The table presents estimates of* $\hat{\beta}_6$ *and* $\hat{\beta}_7$ *from equation (21) where* $ln(Wage)$ *is instrumented with* LW_{jemy}. *Instrumented specifications report the corresponding Kleibergen-Paap F statistic. Standard errors are reported in parentheses and are clustered at the establishment-job.* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.2 Additional Figures

Figure 7: Living Wage and Minimum Wage Rates



Note: *The figure shows the Living Wage Foundations' London and UK wide rates, as well as the statutory National Living Wage and National Minimum Wage adult rate for 2011 - 2019.*

A.3 Staggered Estimator

The robust estimator is as follows. Borrowing notation from Sun and Abraham (2020), let Y_{et} denote some outcome for unit e at time t with treatment status $D_{et} \in \{0, 1\} : D_{et} = 1$ if e is treated in period t and $D_{et} = 0$ otherwise, where treatment is absorbing, and therefore $D_{es} \leq D_{it}$ for $s < t$. A unit's treatment path can therefore be characterised by $K_e = min\{t : D_{et} = 1\}$, and where we let $K_e = \infty$ if the unit is never treated. Units can therefore be categorized into disjoint cohorts $k \in \{t_{min}, \ldots, t_{max}, \infty\}$, where units in cohort k are first treated at the same time $\{e : K_e = k\}$. Y_{et}^k is the potential outcome in period t when unit e is first treated at time k and Y_{et}^{∞} is the potential outcome at time t if unit e never receives treatment. A cohort-specific average treatment effect on the treated l periods from treatment is thus:

$$CATT_{k,l} = E[Y_{e,k+l} - Y_{e,k+l}^{\infty} | K_e = k] \tag{22}$$

This notation allows treatment effect heterogeneity across cohorts, which in this setting may be important as the bite of the living wage may change over time. I am then interested in some weighted average of 22, 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 the average dynamic effects (which allows an analysis of the pre-trends).

For analysing the average dynamic effects I focus on the weighted average similar to that proposed in Sun and Abraham (2020).

$$v_g = \frac{1}{|g|} \sum_{l \in g} \sum_{k} CATT_{k,l} Pr\{K_e = k | K_e \in l\} \tag{23}$$

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

In practice 23 is estimated using the following methodology:

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

$$Y_{et} = \alpha_e + \lambda_t + \sum_{l \neq = -1, -12} \delta_{k,l} LW_{i,t+l} + \beta' X_{et} + \epsilon_{et} \tag{24}$$

   Where α_e is the establishment fixed effect, λ_t is a year-month fixed effect, LW_{et} is a dummy variable which represents whether an establishment pays the Living Wage and X_{et} is a set of time varying establishment level controls. For each treatment cohort e, 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 et al. (2021), I normalise the dynamic effects to two periods, -1 and -12, to deal with the underidentification issues they raise.

2. I estimate the weights $Pr\{K_e = k | K_e \in l\}$ by sample shares of each cohort in the relevant relative period l.

3. I combine steps 1 and 2, and aggregate monthly effects l, to the level of quarters g, for graphical representation by taking a simple equal weighted mean. In particular

$$\hat{v}_g = \frac{1}{3} \sum_{l \in g} \sum_{k} \hat{\delta}_{k,l} \hat{Pr}\{K_e = k | K_e \in l\} \tag{25}$$

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, 2020). 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 underidentification problems raised previously in the literature.

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Website: http://cep.lse.ac.uk Twitter: @CEP_LSE