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Firm Size, Wages and Unobserved Skills: Evidence from Dual Job Holdings in the UK

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Abstract

The paper examines the labour quality explanation of the employer size–wage gap: larger firms pay higher wages because they employ more skilled workers. Most previous studies control for unobserved skills of workers using longitudinal data and the fixed effects estimator thus relying on a questionable assumption of time-invariant unobserved individual heterogeneity. This paper releases this assumption by using a sample of workers who simultaneously hold two jobs; hence, identification is achieved by differencing across two jobs held at the same time rather than in different periods. A caveat of this approach is possible heterogeneity of the two jobs; this issue is discussed in details in the paper. Based on data from the UK Quarterly Labour Force Survey, this study finds little support for the labour quality explanation: controlling for unobserved skills in the sample of moonlighters does not reduce the estimate of the wage gap.

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Keywords: firm size, wages, dual job holdings.

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1. Introduction

The effect of employer size on wages is a well-documented fact in the empirical literature on wage determination. The empirical regularity that larger firms pay more relative to small ones was discovered by Moore (1911) and has been supported in subsequent studies (Oi and Idson, 1999). The magnitude of the effect is substantial: Brown *et al.* (1990) reported for the US that hourly wages in firms with 500 or more employees were 35 percent above wages in firms with fewer than 25 workers – which is comparable to the gender wage gap and exceeds the wage differential associated with race or unionism. Remarkably similar results hold for other countries, regardless of differences in their labour markets (e.g., Lallemand *et al.*, 2005).

The fact that wages rise with employer size has been widely interpreted in the framework of the human capital theory. Developed by Becker (1962) it states that differences in observed wages reflect skill differentials between workers so that no worker gets above-market wage, given his level of skills and experience. Thus, according to this theory, larger firms can pay higher wages if they employ more skilled workers. This constitutes the so-called labour quality explanation of the employer size–wage premium.

The theoretical literature provides several reasons as to why larger firms may employ more skilled workers (see Oi and Idson, 1999 for a survey). Hamermesh (1980) suggests that large firms hire more skilled labour due to their greater capital intensity (stemming e.g., from better availability and superior terms of credit) and the complementarity of physical and human capital in the production process. Dunne and Schmitz (1992) argue that large firms are more likely to employ more sophisticated capital equipment since they have larger amounts of output over which to amortize the fixed costs associated with adopting such capital. If there is a complementarity between the degree of sophistication of physical capital and the skills of workers, then large firms will employ more skilled workers. Oi (1983) suggests that higher entrepreneurial ability (that generates larger firms) increases the quantity of decision-making per hour, but does not affect the intensity of worker monitoring. In this case more able entrepreneurs, in order to save on monitoring cost, prefer to have the same quantity of labour services supplied by fewer workers and therefore employ more skilled labour. According to Kremer (1993), Kremer and Maskin (1996) and Troske (1999), employers find it advantageous to match workers of similar skills together; if hiring of more skilled workers is associated with large fixed costs (e.g., due to more-formal recruiting and training processes) which can be more easily absorbed by large firms, these firms would be more likely to match high skilled workers together.[1]

The labour quality explanation finds some support in empirical studies: there is abundant evidence that more productive employees, in terms of the usual proxies like education and experience, are found in larger firms. Indeed, adding observable characteristics of workers into a simple regression relating wages and firm size considerably reduces the coefficient on the firm size variable (Oi and Idson, 1999). However, a significant part of the employer size–wage gap remains unexplained and this rationalizes the idea that unobserved heterogeneity of workers may account for the reminder of the wage gap.

Empirical evidence on the role of unobservables in explaining the firm size-wage differential is mixed. Some studies find that the wage gap decreases after controlling for unobservable characteristics of workers via fixed effects or estimating selectivity-corrected models (e.g., the two-stage Heckman model in which the first stage is intended to predict the size category of the firm a worker would be employed). For example, by applying the fixed-effects estimator to longitudinal data from US, Brown and Medoff (1989) find that differences in unobserved characteristics of workers account for one-half of the firm size–wage gap. Evans and Leighton (1989) report even higher figures from their study based on panel data: 60 to 100 percent of the differential disappear after controlling for unobserved skills. Using information on job changes and search behaviour of workers Winter-Ebmer and Zweimueller (1999) find

[1] Explanations of the employer size–wage gap that are not related to the quality of labour refer to the efficiency wages, unionisation avoidance, compensating wage differentials and the market power of the firm, among others. For example, the efficiency wage theory states that larger employers will choose to pay higher wages to reduce the amount of monitoring if the cost of detecting shirking rises with the number of employees in the plant.

that worker heterogeneity can account for half of the differential in Switzerland. Abowd and Kramarz (2000) control for both individual unobserved skills and firm fixed effects with employer-worker matched data and find that firm-size–wage differentials are 70 percent a result of firm heterogeneity and 30 percent a result of individual heterogeneity. Using data from Italy, Brunello and Colussi (1998) estimate a Heckman type selectivity-corrected model and conclude that the wage premium is almost entirely explained by differences in the observed characteristics and by non-random allocation of workers to jobs.

Other studies report the opposite result: unobserved heterogeneity does not matter or, if it does, the non-random allocation of workers reduces rather than magnifies the firm size–wage gap. For example, Idson and Feaster (1990) who treat the size of the firm at which a worker is employed as endogenous and correct for selectivity bias, find evidence of negative/positive selection in large/small firms: a random assignment of workers would have resulted in a larger difference between wages paid by large and small firms. Using a similar methodology, Main and Reilly (1993) found no evidence of non-random sorting of workers and hence no support for the labour quality explanation of the firm size-wage gap. Using a continuous measure of establishment size, Albaek *et al.* (1998) report large plant-size effects even after controlling for individual and job characteristics as well as for selection effects (which they find unimportant). Evidence of non-random selection – that smaller firms hire workers with higher unobserved ability – is also found by Lluis (2003) and Silva (2004).

Many recent papers in the field are based on longitudinal data and eliminate unobserved individual effects by using the first differences estimator. This requires the assumption of time-invariant unobserved heterogeneity of workers, in other words, that their unmeasured skills – which may include innate ability, initiative, ambition and loyalty – do not change over time. Clearly, this assumption is questionable, especially over longer periods. It is possible to show that assuming away the time-variation of unobserved skills may result in inconsistent estimates in panel data studies. This study aims to extend the available evidence on the labour quality explanation of the firms-size wage effect by releasing this assumption. Identification is achieved by applying the fixed effects estimator to a sample of workers who simultaneously hold two jobs, rather than to a longitudinal sample of individuals observed during several periods, which has been the standard approach in previous studies. An obvious cost of the proposed approach is a non-random sample, by virtue of the fact that moonlighters – people who hold several jobs at the same time – do not represent a random draw from the population. Another issue is whether the two jobs held at the same time can be treated as similar in the same way as two consecutive jobs are normally treated. These two issues are related to the "external" and "internal" validity of the study and are discussed below.

The chapter is organized as follows. Section 2 describes the empirical strategies that have been previously used to test the labour quality hypothesis. Section 3 takes a closer look at the assumption of time-invariant unobserved heterogeneity that underlies the existing panel data studies of the firm size wage gap; and the assumptions required in a situation where the analysis is based on dual job holders. An alternative approach, based on dual job holding, is considered in section 4. The sample and data are described in section 5. In section 6 we present empirical results. Section 7 concludes.

2. Empirical strategies

The validity of the labour quality explanation has been examined in previous studies in three principal ways: (1) by augmenting the standard wage regression with various proxies for unobserved skills; (2) by estimating Heckman-type models with selection equations predicting the size category of the employer a worker would be employed or instrumenting for employer size if the respective variable is continuous (3) by treating unobserved skills as individual fixed-effects in panel data models based on longitudinal data.

2.1. Proxies for unobserved skills

This general approach, which may be traced back to the study of the return to schooling by Griliches and Mason (1972), involves the use of explicit measures of ability which are considered as proxies for unobserved skills valued in the labour market. Blackburn and Neumark (1991) employ this strategy in their analysis of inter-industry wage differentials by augmenting a cross-section earnings function with measures of IQ and Knowledge of the World of Work. An application of this approach to the study of the firm size–wage effect is a paper by Gibson (2004) who uses data from the IALS, International Adult Literacy Survey, a dataset containing information on literacy tests in which skills variables are measured contemporaneously with earnings. A fundamental problem in these analyses is the insufficient evidence that those additional variables, while characterizing some of the unobserved traits of individuals, are necessarily correlated with the type of unobserved skills that is rewarded in the labour market. Another problem stems from the fact that test scores are error-ridden measures of ability, and correction for measurement error requires identifying assumptions.

2.2. Heckman-type selection models or instrumenting for employer size

These models were first used in the analysis of the firm size–wage effect by Idson and Feaster (1990) who attempted to adjust for a selection bias by estimating an ordered probit model that predicted the size category of the employer. Similar strategies were used by Main and Reilly (1993), Albaek *et al.* (1998), Brunello and Colussi (1998) and Lluis (2003). The findings with respect to non-random selection of workers across different size classes are mixed: while Idson and Feaster (1990) and Lluis (2003) find evidence of non-random allocation, in particular positive selection bias in small firms and negative selection bias in large firms, Main and Reilly (1993) do not support the sorting hypothesis.

A major problem in this approach is the validity of instruments for the employer size if the respective variable is continuous (as in Albaek *et al.*, 1998) or identification of the coefficient associated with the inverse Mills ratio in a situation where the variable is categorical. In the latter case, identification is often achieved from ad hoc restrictions or through nonlinearities implied by the use of the normal distribution (see, e.g., Main and Reilly, 1993 and Lluis, 2003).

2.3. Panel data models

The use of longitudinal data and the fixed-effects or first differences estimators has been by far the most common approach in testing the labour quality hypothesis. The approach that dates back to Dunn (1980) and Brown and Medoff (1989) – the latter is a reference paper for many studies – involves estimating the following model:

$$ln\ W_{i(j)t} = X_{it}\beta + Z_{jt}\gamma + \theta_i + u_{i(j)t}\,, \qquad (1)$$

where $W_{i(j)t}$ – wage of worker i who is employed by firm j at time t, X_{it} – vector of worker's i observable characteristics at time t, Z_{jt} – vector of (observable) characteristics of worker's i employer j at time t, including size, θ_i – person-specific unobserved productivity effect, $u_{i(j)t}$ – error term.[2]

Then simple differencing eliminates the person-specific effects:

$$\Delta ln\ W = W_{i(j)t+1} - W_{i(j)t} = \Delta X\beta + \Delta Z\gamma + \Delta u. \qquad (2)$$

Model (1) relies on two important assumptions concerning the unobserved effect θ: first, it is person-specific and unrelated to the identity of the employer (in particular, employer size); second, it is time-invariant (hence, i is the only subscript on the parameter). However, the unobserved individual productivity effect θ may in theory vary across all three dimensions (person, firm and time). Since the model with parameter θ having all three subscripts is unidentified, additional assumptions are needed; a study based on longitudinal data requires time-invariance of unobserved heterogeneity.

[2] Subscript j appears in parentheses to indicate that the panel is in fact two- rather than three-dimensional as long as each worker is employed by (matched with) a single firm.

Model (1) has been extended in several ways to relax the assumption that the unobserved productivity effects are not firm- or job-specific. For example, Abowd *et al*. (1999) and Abowd and Kramarz (2000) introduce unobserved heterogeneity of firms (as additive term) in the model and estimate its parameters using employer-employee matched data. Their model can be written in the following way:

$$Ln\ W_{i(j)t}= X_{it}\beta + Z_{jt}\gamma + \theta_i + \varphi_j + u_{i(j)t}\ , \tag{3}$$

where φ is a firm specific effect. This extension allows decomposition of worker compensation into components related to observable employee characteristics, personal heterogeneity, firm heterogeneity, and residual variation. Importantly, this extension of the basic model (1) does not alter the two assumptions regarding the unobserved individual effects θ (time- and firm-invariance).

More recent studies attempt to release the assumption that human capital attributes are equally valued in large and small firms which leads to estimation by non-linear least squares (e.g., Ferrer and Lluis, 2004; Silva, 2004). Importantly, this strand of literature continues to rely on the supposition that individual heterogeneity is time-invariant.

3. Traditional longitudinal studies: are unobserved skills constant over time?

The assumption of time-invariant unobserved heterogeneity is a standard identifying assumption in the panel data models. Whether it is a realistic one is questionable: if observable components related to worker productivity change over time, why cannot the same be true for unobservable ones, especially if they are widely understood as embracing such factors as ability to learn, ambition and initiative? As noted by Angrist and Krueger (1999, p. 1296), "… perhaps the most important problem with this approach [fixed-effects estimators] is that the assumption that omitted variables can be captured by an additive, time-invariant individual effect is arbitrary in the sense that it usually does not come from economic theory or from information about the relevant institutions”.

To illustrate the potential cost of assuming the time-invariance of unobserved skills in the context of the firm size-wage gap studies, consider the following simplistic model. Suppose that worker’s wage depends on two variables: the size of the firm she is employed, *S*, and unobserved person-specific productivity term θ (for simplicity we abstract from any observed characteristics of workers). For individual *i* the relationship may be expressed in the following way:

$$\ln W_i = \beta S_i + \theta_i + u_i \, , \tag{4}$$

where W_i stands for the hourly wage of worker *i*, and u_i is a random disturbance uncorrelated with both S_i, and θ_i: $Cov(u_i, S_i) = Cov(u_i, \theta_i) = 0$. In addition, suppose that the allocation of workers into firms of different size-classes depends on the unobserved individual effect θ:

$$S_i = \alpha\theta_i + v_i \, . \tag{5}$$

Assume further that random disturbance v_i is uncorrelated with individual effect θ_i as well as random disturbance in the wage equation u_i: $Cov(v_i, \theta_i) = Cov(v_i, u_i) = 0$. Finally, denote for simplicity $Var(\theta_i) = \sigma_\theta^2$, $Var(u_i) = \sigma_u^2$ and $Var(v_i) = \sigma_v^2$. Were person-specific productivity θ observable, a simple regression of log wages on it and the firm size variable *S* would produce an unbiased and consistent estimate of β. However, unobservability of θ results in biased estimates of β provided that there is a selection of workers with different skills into firms of different sizes, $\alpha \neq 0$ (consistent with the labour quality hypothesis). In a cross-sectional OLS analysis the magnitude of the bias can be seen from the following expression:

$$\mathrm{E}(\hat{\beta}) = \frac{Cov(\ln W_i; S_i)}{Var(S_i)} = \beta + \frac{Cov(\theta_i; S_i)}{Var(S_i)} = \beta + \frac{\alpha\sigma_\theta^2}{\alpha^2\sigma_\theta^2 + \sigma_v^2} \, . \tag{6}$$

Now suppose that each person is observed in two periods (e.g., working for different firms), population parameters α and β are time-invariant but person-specific productivity term θ is not.

Nevertheless, θ is (incorrectly) assumed to be constant over time for each individual and the first-differences estimator is applied to the model. The true model can be written as follows:

$$\ln W_{it} = \varphi + \beta S_{it} + \theta_{it} + u_{it} \tag{7}$$

$$S_{it} = \phi + \alpha\theta_{it} + v_{it} \text{ ,}$$

where $Var(\theta_{it}) = \sigma_\theta^2$, $Var(u_{it}) = \sigma_u^2$, $Var(v_{it}) = \sigma_v^2$ and

$Cov(\theta_{it}, u_{is}) = Cov(u_{i1}, u_{i2}) = Cov(\theta_{it}, v_{ik}) = Cov(v_{i1}, v_{i2}) = Cov(v_{it}, u_{is}) = 0$.

Then differencing yields:

$$\Delta \ln W_i = \beta \Delta S_i + \Delta\theta_i + \Delta u_i \tag{8}$$

and estimating the model under the assumption that $\Delta\theta_i = 0$ generates a biased result:

$$\mathrm{E}(\hat{\beta}) = \frac{Cov(\Delta \ln W_i; \Delta S_i)}{Var(\Delta S_i)} = \beta + \frac{Cov(\Delta\theta_i; \Delta S_i)}{Var(\Delta S_i)}, \tag{9}$$

where the bias is equal:

$$E(\hat{\beta} - \beta) = \frac{Cov(\theta_{i2} - \theta_{i1}; \alpha\theta_{i2} + v_{i2} - \alpha\theta_{i1} - v_{i1})}{Var(\alpha\theta_{i2} + v_{i2} - \alpha\theta_{i1} - v_{i1})} = \frac{\alpha[\sigma_\theta^2 - Cov(\theta_{i1}; \theta_{i2})]}{\alpha^2[\sigma_\theta^2 - Cov(\theta_{i1}; \theta_{i2})] + \sigma_v^2}. \tag{10}$$

Thus, if unobserved heterogeneity of workers θ is time-varying $(\theta_1 \neq \theta_2)$ and there is selection of workers into firms of different sizes based on their unobserved productivity $(\alpha \neq 0)$, the first differences estimator is biased and inconsistent. For given α the magnitude of the bias depends on $\sigma_\theta^2 - Cov(\theta_{i1}; \theta_{i2})$.

Assuming the time-invariance of unobserved skills may be problematic in the context of the firm size-wage gap studies based on longitudinal data. To see the reasons, first note that the fixed-effects estimator requires within variation in the regressors, most importantly, in the firm size variable. In longitudinal data this may come from two sources: either workers move between firms of different size-classes or the size of firms changes while workers stay with the same employers. Thus, the firm size effect can be identified based on “movers” and “stayers”.

Identification based on movers. The group of job changers consists of workers who change jobs voluntarily; those who are laid off due to poor performance; and displaced workers.[3] The bulk of job transitions is voluntary – many people change jobs when it is profitable to do so; therefore job changers largely represent a self-selected group. Solon (1988) formally shows that identification of wage gaps based on job changers may result in inconsistency of conventional longitudinal estimation. A similar argument that first-differenced estimates may contain important self-selection biases was made by Gibbons and Katz (1992) in the context of inter-industry wage differentials.

Some studies address the selection problem by using data on displaced workers – especially those fired due to plant closures – claiming that a job change due to displacement is exogenous (Gibbons and Katz, 1992; Silva, 2004). This approach arguably mitigates the selection problem pointed out by Solon (1988), but may create others. In particular, the unemployment scarring literature has long identified costly spells of unemployment and decline in earnings following displacements; cross-country evidence is discussed in Gangl (2005). For the UK, Gregory and Jukes (2001) report that earnings losses for British men in the late 1980s and early 1990s amounted to 15 percent and typically persisted for several years after the original spell of unemployment. One of the explanations for the phenomenon is offered by the human capital theory: separations are typically accompanied by the loss of on-the-job investment in firm-specific human capital; more importantly, a spell of unemployment may also bring the deterioration of general human capital (Arulampalam *et al.*, 2001). The latter casts serious doubt on the assumption that unobserved skills are time-invariant among displaced workers.

Identification based on stayers. The employer size–wage effect may be identified from workers who stay with the same firm while it is growing or shrinking in size. This approach has an

[3] Displaced workers are usually defined as people who lost or left jobs because their plant or company closed or moved, there was insufficient work for them, or their position or shift was abolished.

obvious advantage that it does not require the assumption that individual productivity is constant across firms – during the period of observation the employee stays matched with the same employer (Söderbom *et al.*, 2002).

An important issue related to identification on stayers is a lack of variation in firm-size variables, especially if the data come from labour force surveys where firm size is usually reported in categorical variables. This magnifies measurement error problem in fixed-effects estimation and attenuates estimated coefficients. For example, Winter-Ebmer and Zweimueller (1999, p. 90) write "We consider the measurement error to be more severe for size-class changing workers who stayed with the same firm, than for job-changers as the former may result from mere legal changes, mergers, plant/establishment measurement problems, etc." This viewpoint is consistent with some empirical evidence; for example, Brown and Medoff (1989) found a positive effect of firm size on wages for movers and no effect for stayers.

A remedy from the measurement error problem in the panel data context was suggested by Griliches and Hausman (1984), who argued that increasing the period spanned by two years of data raises the amount of real change in the right-hand-side variables. Essentially, one increases the probability of observing a real change in the firm-level variables, notably, size; the problem is that observable and, importantly, unobservable characteristics of workers are also more likely to change over longer periods.

4. Identification based on moonlighters: are the two simultaneous jobs similar?

A way to identify the firm size wage gap without assuming the time-invariance of unobserved skills is by using a sample of people employed by different firms simultaneously rather than sequentially and differencing out across jobs, not over time as in traditional studies based on longitudinal data. In terms of model (8) this implies $\Delta\theta_i = 0$ and thus solves the problem of potentially inconsistent estimates. The ideal conditions for such analysis imply orthogonality between the decision to moonlight and the firm size wage effect, no systematic differences between the primary and secondary jobs (e.g., they require the same skills and reward them equally) as well as homogeneous employers (they differ by size and are otherwise similar). However, the requirement of no systematic differences between the two jobs may be problematic as evidenced in the moonlighting literature.

The theoretical literature on moonlighting suggests two main reasons as to why people hold second jobs. The first reason is hours constraints on the main job – a person who cannot work in his main job as much as he wants to in order to achieve the utility maximizing hours of work, might find a second job (a formal model was developed by Shishko and Rostker, 1976). The second reason is referred to as heterogeneous jobs motive – a worker who is not hours constrained on his primary job holds a second one because of their complementary nature. Examples include a university professor doing consultancy work (the first job provides credentials to take on a second one) or a musician who cannot make a living on his performances and therefore has to hold another job (here utility primarily comes from pecuniary benefits on one job and non-pecuniary ones on the other one). In addition, having two jobs may allow workers to pursue activities which may otherwise be unavailable to them (e.g., women who have small children may take two part-time positions if they allow for better child care than would be facilitated with one full-time job), to gain additional experience or skills that are needed in the forthcoming occupation or to insure employment if the main jobs have a high risk of termination. The theory also predicts that hours-constrained workers earn on the second job at most as much as they earn on the primary one (otherwise they would change employers); moreover, they can be expected to moonlight only temporarily until they find a job that better satisfies their preferences. In contrast, in the case of heterogeneous jobs there should not be any particular relationship between the two wages and the tenures in both jobs should be longer, compared with the moonlighters who are hours constrained.

Empirical studies tend to confirm the presence of various motives for moonlighting with hours constraints being particularly common: Kimmel and Conway (2001) provide evidence for the US, Heineck and Schwarze (2004) for Germany and the UK and Böheim and Taylor (2004) for the UK. Less is known about the determination of wages. Based on data from the Current Population Survey from 1991, Averett (2001) reports similar wage rates in primary and secondary jobs, but finds little connection between an individuals' human capital and their wage on the second job (the sample retains self-employed people). Conway and Kimmel (1998), using SIPP data and excluding self-employed workers, report that wages from the secondary jobs are considerably lower than wages from the primary ones; they also estimate selectivity-corrected wage equation for moonlighters and show differences in the valuation of workers' observables on the two jobs. Using BHPS, Böheim and Taylor (2004) report that hourly wages on the second job are more than twice the average of wages earned on the first job and the standard deviation is almost ten times higher (their sample excludes self-employed in the primary jobs).

Thus, both theory and empirical studies suggest that the primary and secondary jobs may be heterogeneous. In particular, the existing literature casts doubt on the assumption of equal reward of workers' skills on the primary and secondary jobs. Intuitively, any systematic differences between the two jobs are undesirable for the proposed approach; to see the cost involved, consider the following variation of the model (7) which takes into account important results from the moonlighting literature:

$$\ln W_{ik} = \beta S_{ik} + \gamma_k \theta_i + u_{ik} \qquad (11)$$

$$S_{ik} = \alpha_k \theta_i + v_{ik} \, ,$$

where index k refers to first and second employers rather than time and θ has a single subscript referring to individuals. In this model, the reward of skills is different across main and second employers and so is the selection of workers. The assumptions about the error terms are similar to those in model (7) and are not repeated. Applying the first-differences estimator to a panel of moonlighters yields:

$$\mathrm{E}(\hat{\beta}) = \frac{Cov(\Delta \ln W_i ; \Delta S_i)}{Var(\Delta S_i)} = \beta + (\gamma_2 - \gamma_1)\frac{Cov(\theta_i ; \Delta S_i)}{Var(\Delta S_i)} = \beta + \frac{(\gamma_2 - \gamma_1)(\alpha_2 - \alpha_1)\sigma_\theta^2}{(\alpha_2 - \alpha_1)^2 \sigma_\theta^2 + 2\sigma_v^2}. \quad (12)$$

In this simple model, the bias exists as long as primary and secondary employers reward workers' skills differently $(\gamma_2 \neq \gamma_1)$ and select different types of workers $(\alpha_2 \neq \alpha_1)$. The sign of the bias depends on the difference in skills rewarding and selection: for example, if secondary employers reward workers' skills less than primary employers and selection of more able workers into larger firms is less pronounced for secondary employers, the bias is positive.

A straightforward comparison of the biases in the model based on longitudinal data and the one based on data on moonlighters is complicated despite the fact that each model is quite simple. However, they show that the biases depend on the correlation of unobserved individual productivity over time on the one hand and systematic differences between the primary and secondary jobs on the other; overall, if unobserved ability considerably vary over time, it is plausible that the bias in the first model is larger than in the second. Since the problem of heterogeneity between the primary and secondary jobs is crucial for the validity of our approach, the analysis here attempts to address it by imposing additional restrictions on the sample of moonlighters, e.g., by removing those who worked on non-permanent basis in either of the two jobs or who had two simultaneous jobs in different occupational categories.

5. Data and sample

This study is based on data from the UK Quarterly Labour Force Survey (QLFS), a nationally representative survey of the UK population that provides reasonably detailed data about both primary and secondary jobs (employer size, job permanency, type of contracts, etc.) which in most other surveys are available for primary jobs only. The QLFS is a quarterly rotating panel, where each household (individual) participates in five consecutive quarters. Each wave contains information about 60000 individuals who had a job in the reference week (either as employed or self-employed). Of these, about 2400 individuals or 4 percent had a second job.[4]

The empirical analysis that follows is based on data from 16 recent waves of the QLFS covering period from March 2002 to February 2006. These waves of the QLFS contain respondents' estimates of the size of their employers (in primary and secondary jobs) represented in seven categories [1-10], [11-19], [20-24], [25-49], [50-249], [250-499] and [500, ∞).[5] The mentioned 16 waves of the QLFS contain 36898 reported moonlighting episodes of which 59.5 percent correspond to females.[6] Note, however, that the questions on earnings are only asked in the first and fifth (last) waves a person participates in the survey; hence, a maximum of two observations of wages per individual are available instead of five. Taking that into account, the number of relevant observations falls by 60 percent (max 14737). The time lag between the two interviews that provide data on earnings for the same respondent is one year.

As discussed above, a potential problem in the proposed approach to controlling for unobserved productivity of workers is the heterogeneity between primary and secondary jobs of which there is considerable evidence in the literature. The dimensions along which the two jobs may be heterogeneous are numerous: employees versus self-employed, permanent versus non-permanent jobs, full- versus part-time jobs, work from home or somewhere separate from home, fringe benefits provided by the employer, etc. For example, self-employed people by definition enjoy greater flexibility in choosing the number of hours of work; more important, in case of self-employed it is generally impossible to separate returns to labour from returns to capital (e.g. Kimmel and Conway, 1998). There is also evidence of substantial differences in the working conditions of permanent and temporary employees, including unpaid overtime work, wages and

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[4] A natural question is which of the two jobs should be considered as the main job and which as the second one. The QLFS seems to leave this problem to the workers. Apparently, different criteria can be used by people when considering their jobs as primary or secondary (the amount of earnings, hours of work, sequence of obtaining the two jobs, status positions and others).

[5] The reported figures refer to the establishment rather than firm size; in the literature the two effects are considered as having similar effects on wages.

[6] Excluding individuals who had two jobs because of job change in the reference week (236 observations in total).

working rights (OECD, 2002). For example, Engellandt and Riphahn (2003) report that temporary workers' "probability of working unpaid overtime exceeds that of permanently employed workers by 60 percent". Whether job is full- or part-time is one of the crucial dimensions: a sizable part-time wage penalty is widely documented in the labour economics literature (e.g., Hirsch, 2004). Working from home and somewhere separate from home imply very different working environments; moreover, hours of work may be imprecisely estimated by those working from home.

The primary and secondary jobs in the QLFS sample of moonlighters are rather different along the mentioned dimensions, for example, the probability of being self-employed on the second job is twice as large as on the primary one (31 percent versus 16 percent), the second jobs are more likely to be non-permanent and mostly part-time, they are also more likely to involve working from home.

In this study the problem of heterogeneity of the two jobs is dealt with by imposing restrictions on the sample of moonlighters. First, as discussed above, it is important to focus on people who work as <u>employees only</u> in both jobs (another reason for this restriction is unavailability of earning data for self-employed in the QLFS). Already this reduces the sample size to 8969 individuals. Second, the sample is restricted to employees having <u>permanent jobs only</u> (as opposed to the other categories distinguished in the survey: seasonal and occasional jobs, agency temping or work done on a fixed-term or fixed-task contract, etc.). Third, since most secondary jobs are part-time jobs, we only retain individuals having <u>part-time primary and secondary jobs</u>. The questionnaire contains a direct question on whether the main job is full- or part-time; therefore all full-time primary jobs are easily filtered away. In addition, we exclude all observations with weekly working hours equal or greater than 35. Next, observations of individuals <u>working from home</u> or using home as a base (at any of the two jobs) are deleted.[7]

[7] This restriction is in part motivated by the question on establishment size asked in the QLFS – "How many people worked for your employer at the place where you worked?" It calls for immediate exclusion of home workers from the sample as it yields the answer "one" in the overwhelming majority of cases.

After these operations, the resulting sample embraces 2449 observations. Of these workers 88.2 percent are females (the gender composition is most seriously affected by restrictions on part-time work, but also on self-employment and job permanency). We finally restrict the sample to women in the working age (16-59).[8]

The data on secondary jobs in the QLFS are much less extensive and may be of lesser quality than data on primary jobs. Firstly, the number of questions asked in the QLFS about secondary jobs is substantially smaller than the number of questions about primary ones. Some potentially important information – such as employment in the private or public sector, union status of job, etc. – is not available for second jobs at all. There seems to be a difference in the accuracy of data collection/processing: the data on gross wages in secondary jobs are missing for most observations in 2003 (though net wages are reported) and there is no information on the location of jobs (home or somewhere else) in the summer and autumn quarters of 2004. Hourly wages are another important case. As noted above, for primary jobs this variable is available in the dataset; it is derived from a mass of questions referring to earnings, usual hours, actual hours of work, paid and unpaid overtime, etc. In contrast, what is available for second jobs is just information about gross weekly earnings and actual hours, including overtime. The implication is that the earnings variable for secondary jobs contains a higher measurement error compared with the earnings variable for primary jobs. More important, however, would be potential differences in measurement error in the right-hand-side variables, in particular, in the employer size variables. There is some evidence to suggest that the size of the second employer is measured in the survey with a larger error compared with the size of the first employer. This can be seen from non-response rates as well as from the proportion of imprecise answers: the non-response rates for the first and second jobs are 0.8 percent and 2.5 percent, and the shares of imprecise answers (like "do not know but less than 50 employees") are 5.0 percent and 8.1

[8] Ideally, one would prefer to analyse a sample of workers whose choices to have two jobs are determined by factors other than firm size, wages, etc.: for example, when two rather than one job are chosen due to the need to combine work with family responsibilities. Unfortunately, the QLFS does not provide such information. However, by focusing on women we are presumably getting closer to such a scenario.

percent respectively (5.8 percent versus 10.6 percent cumulatively, all differences are statistically significant at the 1 percent level). As long as non-responses and approximate answers indicate the quality of information the workers have about their employers, one can expect a somewhat larger measurement error in the variable characterizing the size of the secondary employers.

In order to reduce the number of parameters to be estimated, we reclassify information on the employer size into the following five categories: [1-10], [11-24], [25-49], [50-249] and [250-∞); this results in a more equal distribution of observations across employer size categories. Data on hourly earnings from the primary job are available in the QLFS; the corresponding variable for the second job is defined as the ratio of weekly pay to actual hours of work in the reference week. All variables on earnings are CPI deflated and correspond to December 2000 prices. To deal with outliers, we drop one percent of observations with highest and lowest hourly earnings from each tail of the wage distribution[9]. Information on individual education is provided in categorical variables, to convert them into years of schooling we rely on the method used by Bonjour *et al.* (2002). Potential experience is calculated as age minus 5 minus imputed years of schooling. The definition of variables is provided in the Appendix.

The demographic characteristics of the sampled workers and basic characteristics of jobs and employers corresponding to primary and secondary jobs are shown in Table 1. For comparative purposes Table 2 shows the characteristics of a sample of women aged 16-59 who worked as employees, and who worked as employees on a part time basis; these data are from the autumn 2003 wave of the survey. There are differences between the sample of moonlighters and the sample of working women in several dimensions, e.g., the selected moonlighters are almost two years older, less educated and have more dependent children. Moonlighters' wages in both jobs are about 25 percent lower than the average earnings in the sample of working

[9] This is done separately for each job allowing for differences in the distributions of wages. However, almost nothing changes in the results if outliers are eliminated under the assumption that the wage distributions are the same. Also, the empirical results stay virtually the same regardless of the percentage of outliers eliminated (0.5 percent, 1 percent or 2.5 percent in each tail of the wage distribution).

women and more similar to wages in the sample of female employees working part-time. Interestingly, moonlighters' hourly earnings in both jobs are fairly similar, the means differ only by 1 percent. Figure 1 shows the distributions of wages in the two jobs.[10] There is a difference in the weekly hours of work in the primary and secondary jobs: in the reference week the average is 15.1 hours for the primary jobs and only 9.4 for the secondary jobs. Both primary and secondary jobs in the constructed sample of moonlighters are more likely to be among low-skilled occupations compared with the autumn 2003 sample; the distribution of secondary jobs is more skewed towards low-skilled occupations. Moonlighters are also less likely to work in jobs that involve managerial or supervisory responsibilities.

As regards employer size, the sampled moonlighters are more likely to work in smaller firms compared with the sample of female employees. The distribution of second jobs across firm size categories is more skewed towards smaller firms. An important feature of the sample is that there is no strong relation between the size of the primary and secondary employers, as shown in Table 3. Indeed, for any raw/column corresponding to a particular employer size category in the main/second job there is a substantial variation in the employer size in the other job. The share of non-diagonal elements that correspond to the observations on which the firm-size effect will be identified is close to two-thirds. The relationship between occupational categories of the two jobs is shown in Table 4: almost half of the sampled people hold the two jobs in the same occupations (broadly defined on the basis of the first digit in the ILO classification).

To summarize, while the sample of moonlighters shows differences from the population of working women so that the external validity of the study (i.e., ability to generalize to broader population groups) does not necessarily hold, there is less concerns about the internal validity of the study. In particular, the distributions of wages in primary and secondary jobs is a case in point.

[10] Two-sample Kolmogorov-Smirnov test for equality of distribution functions does not reject the null that the two wage distributions are the same at the 1 percent level, though not at the 5 percent level.

6. Regression analysis

We start by presenting a simple correlation of wages from the primary and secondary jobs with employer size dummies. Table 5 shows that both wages increase with employer size, the effect is similar quantitatively between the two jobs (the difference in any pair of coefficients is not statistically significant at the 10 percent level). Adding basic demographic characteristics of workers – years of schooling, potential experience, potential experience squared, race, marital status and the number of dependent children – reduces the employer size-wage gap; the result is consistent with the previous studies. Note that there are no statistically significant differences in the coefficients on schooling and experience in the regressions for primary and secondary jobs, which is consistent with the assumption that workers' observable characteristics are rewarded similarly in the two jobs.[11] Inclusion of additional controls, such as occupational, industry and regional dummies, as well as a dummy for supervisory responsibilities (job status), reduces the estimate of the employer size wage gap further (mostly due to occupational dummies, though). Again, the coefficients in the two equations are very similar. Note that the coefficients on the firm size dummies are typically smaller in the regressions for second jobs; the result is probably due to a higher measurement error in the size variables referring to the second employers, as discussed above. The coefficient of determination is smaller in the regressions for second jobs as well, which is also consistent with the previous discussion about errors in measuring hourly wages.

Table 6 shows estimation results from applying OLS and FE to the pooled dataset containing data for both jobs. Regressions 1-4 provide OLS estimates while regressions 5-6 are estimated using the fixed effects estimator. A separate dummy for primary jobs is included to account for any differences in the intercept between the two jobs.[12] Similar to the separate

[11] This finding is different from the results obtained in several other studies, such as previously cited Averett (2001), Conway and Kimmel (1998) and Böheim and Taylor (2004).
[12] It stays statistically insignificant at the10 percent level in most regressions.

regressions for primary and secondary jobs, the firm size wage gap falls with the inclusion of observable characteristics of workers (regressions 1 and 2; the difference in the coefficients corresponding to the largest size category is significant at the 1 percent level and for the second largest size category – at the 10 percent level). Additional controls such as occupational and industry dummies do not reduce the firm size wage gap significantly, as follows from regressions 3 and 4.[13]

Individual fixed effects are introduced in regressions 5 and 6; the fixed effects are statistically significant at the 1 percent level in the F-test. The equality of coefficients on firm size variables in regressions 2 and 5 is not rejected at the 10 percent level, for each coefficient separately and jointly for all of them. The equality of the same coefficients in regressions 4 and 6 is also not rejected at the 10 percent level. Thus, the regressions show that individual unobserved heterogeneity does not affect the firm size wage differential.[14]

Next, we restrict the sample to people having two jobs in the same occupations (distinguished by the first digit in the ILO classification code). The rationale is that such restriction provides additional argument for treating the two jobs as similar – jobs are to a large extent defined by occupations. This restriction is quite costly in terms of degrees of freedom as it reduces the sample size by more than half to 1108 observations only (i.e., 554 observations per each job, see Table 4). Results from running the wage regressions on this sub-sample are reported in Table 7 and are fairly similar to the results obtained with the non-restricted sample of moonlighters. Regression results are reported in the same format as before: columns 1-4 provide OLS estimates for the pooled data and regressions 5 and 6 are estimated using FE. Again, adding observable characteristics of workers and employers reduces the wage differential; however, controlling for unobserved characteristics of workers via fixed effects

[13] Only the difference in coefficients on the dummy for the largest size category in regression 2 and regression 4 are statistically significant at the 5 percent level.

[14] Removing some of the restrictions imposed on the sample in order to avoid heterogeneity between the primary and secondary jobs (for example, restrictions related to the maximum working hours or permanent versus non-permanent jobs) does not alter the reported results in any important way: there is still a wage gap due to the firm size which is barely affected by introduction of individual fixed effects in the regressions.

does not affect the wage gap significantly. In particular, the coefficients on the firm size variables in regressions 4 and 6 appear to be not statistically different across models.

Thus, the results for the sample of moonlighters provide little support for the labour quality explanation of the employer size-wage gap: the observed wage differential changes little when unobserved skills of workers are controlled for via fixed effects. This finding is in contrast to most previous studies that applied the fixed effects estimator to longitudinal data: as surveyed in the introduction to this chapter, they typically find that individual unobserved heterogeneity explains a sizeable part of the wage differential.

This standard result from the previous panel data studies can be easily replicated with the QLFS data. We form a longitudinal sample by stacking data from the 16 waves of the QLFS (2002-2006) together and selecting working-age women who worked as employees and reported their earnings twice (e.g., in the first and fifth waves of the survey; the time span between the two waves is one year). Note that the sample includes both movers and stayers.

Results for the longitudinal sample are shown in Table 8. Up to the point when the models are estimated using the fixed effects estimator, the results are qualitatively similar to those reported for the sample of moonlighters. In particular, regression 1 shows a highly significant correlation between wages and employer size variables and regressions 2-4 show that the wage gap falls after introducing observable characteristics of workers as well as basic characteristics of jobs and employers in the regressions. However, in contrast to the results obtained with the sample of moonlighters, adding individual fixed effects dramatically reduces the coefficient on the employer size variables.[15]

Finally, Table 9 shows regression results for the *longitudinal* sample of women who moonlighted in at least one of the two periods spanned by the panel. The sample embraces only

[15] Since the last results can be attributed to the low variation in the employer size variables in the sample that is dominated by stayers, we run regressions on workers who changed employers between the two waves of the survey). However, even with the sample of movers – with much higher variation of the employer size – the main result stays the same – introducing fixed effects significantly reduces the estimate of the firm size wage gap.

those women who had a permanent secondary job and worked as employees, part-time, and separate from home in the second job (the same criteria as applied earlier). The dependent variable captures hourly earnings in the primary jobs in two different periods. Again, the results show that introduction of fixed effects nearly sweeps away the correlation between hourly earnings and employer size.

7. Conclusion

The aim of this analysis was to assess the validity of the labour quality explanation for the employer size–wage effect without leaning on a potentially problematic assumption that unobserved skills of workers do not change over time. The firm size wage effect is identified by applying the standard fixed effects estimator to a sample of workers who hold two jobs simultaneously rather than sequentially. To the best of our knowledge, this is the first study of the firm size wage effect that releases the assumption of time invariant unobserved heterogeneity with the help of a sample of moonlighters.

Empirical analysis finds no evidence to suggest that differences in the quality of labour employed by small and large firms fully explain the firm size–wage differential. Although controlling for observable characteristics of workers such as education and experience lowers the wage gap, the introduction of unobservable characteristics via fixed effects fails to reduce it further significantly.

This result is different from the findings of most previous studies based on longitudinal data – they tend to report that unobservable characteristics of workers explain a significant fraction of the firm size wage gap. However, our result is in line with several papers that attempt to explicitly model the process of workers’ sorting into firms of different sizes: many such studies find insignificant selection or negative/positive selection of workers into large/small firms. Importantly, these studies are not relying on the assumption that unobserved individual heterogeneity is time-invariant.

Table 1. Descriptive statistics for the sample of female moonlights, primary and secondary jobs

	Primary jobs			
Variable	Mean	Std. Dev.	Min	Max
age	40.12	11.23	16	59
white	0.97	0.17	0	1
marrd	0.68	0.47	0	1
child	1.16	1.10	0	6
school	12.22	2.06	10	17
exp_p	22.95	11.64	0	44
wage	5.92	2.92	1.85	26.62
hours	15.07	8.15	0	34
occup1	0.02	0.15	0	1
occup2	0.04	0.19	0	1
occup3	0.08	0.27	0	1
occup4	0.15	0.36	0	1
occup5	0.02	0.13	0	1
occup6	0.18	0.39	0	1
occup7	0.14	0.35	0	1
occup8	0.01	0.09	0	1
occup9	0.36	0.48	0	1
boss	0.18	0.38	0	1
size1	0.30	0.46	0	1
size2	0.22	0.41	0	1
size3	0.18	0.38	0	1
size4	0.18	0.38	0	1
size5	0.12	0.33	0	1

No obs: 1202

	Secondary jobs			
Variable	Mean	Std. Dev.	Min	Max
age	40.12	11.23	16	59
white	0.97	0.17	0	1
marrd	0.68	0.47	0	1
child	1.16	1.10	0	6
school	12.22	2.06	10	17
exp_p	22.95	11.64	0	44
wage	6.00	3.38	1.82	27.61
hours	9.39	6.10	0	34
occup1	0.01	0.12	0	1
occup2	0.04	0.18	0	1
occup3	0.08	0.28	0	1
occup4	0.13	0.33	0	1
occup5	0.01	0.12	0	1
occup6	0.16	0.36	0	1
occup7	0.12	0.32	0	1
occup8	0.01	0.10	0	1
occup9	0.45	0.50	0	1
boss	0.10	0.30	0	1
size1	0.41	0.49	0	1
size2	0.21	0.40	0	1
size3	0.17	0.37	0	1
size4	0.14	0.35	0	1
size5	0.07	0.26	0	1

No obs: 1202

Table 2. Descriptive statistics for the sample of women aged 16-59 working as part-time employees and full-time employees

	Full-time employees			
Variable	Mean	Std. Dev.	Min	Max
age	38.29	11.66	16	59
white	0.95	0.23	0	1
marrd	0.66	0.47	0	1
child	0.80	1.01	0	7
school	13.08	2.47	10	17
exp_p	20.24	12.14	0	44
wage	8.06	4.70	1.45	39.47
hours	26.89	15.71	0	96
occup1	0.10	0.30	0	1
occup2	0.11	0.31	0	1
occup3	0.14	0.35	0	1
occup4	0.23	0.42	0	1
occup5	0.02	0.13	0	1
occup6	0.14	0.34	0	1
occup7	0.13	0.33	0	1
occup8	0.03	0.16	0	1
occup9	0.12	0.32	0	1
boss	0.33	0.47	0	1
size1	0.21	0.41	0	1
size2	0.16	0.36	0	1
size3	0.16	0.36	0	1
size4	0.22	0.42	0	1
size5	0.26	0.44	0	1

No obs: 24501

	Part-time employees			
Variable	Mean	Std. Dev.	Min	Max
age	39.30	11.80	16	59
white	0.96	0.19	0	1
marrd	0.73	0.45	0	1
child	1.18	1.07	0	6
school	12.46	2.15	10	17
exp_p	21.90	12.16	0	44
wage	6.69	3.88	1.45	36.60
hours	16.40	9.40	0	34
occup1	0.03	0.17	0	1
occup2	0.05	0.22	0	1
occup3	0.10	0.30	0	1
occup4	0.21	0.41	0	1
occup5	0.02	0.13	0	1
occup6	0.15	0.36	0	1
occup7	0.22	0.41	0	1
occup8	0.02	0.12	0	1
occup9	0.21	0.41	0	1
boss	0.19	0.39	0	1
size1	0.26	0.44	0	1
size2	0.18	0.39	0	1
size3	0.16	0.37	0	1
size4	0.19	0.40	0	1
size5	0.21	0.41	0	1

No obs: 8755

Figure 1. Distribution of hourly earnings: primary and secondary jobs

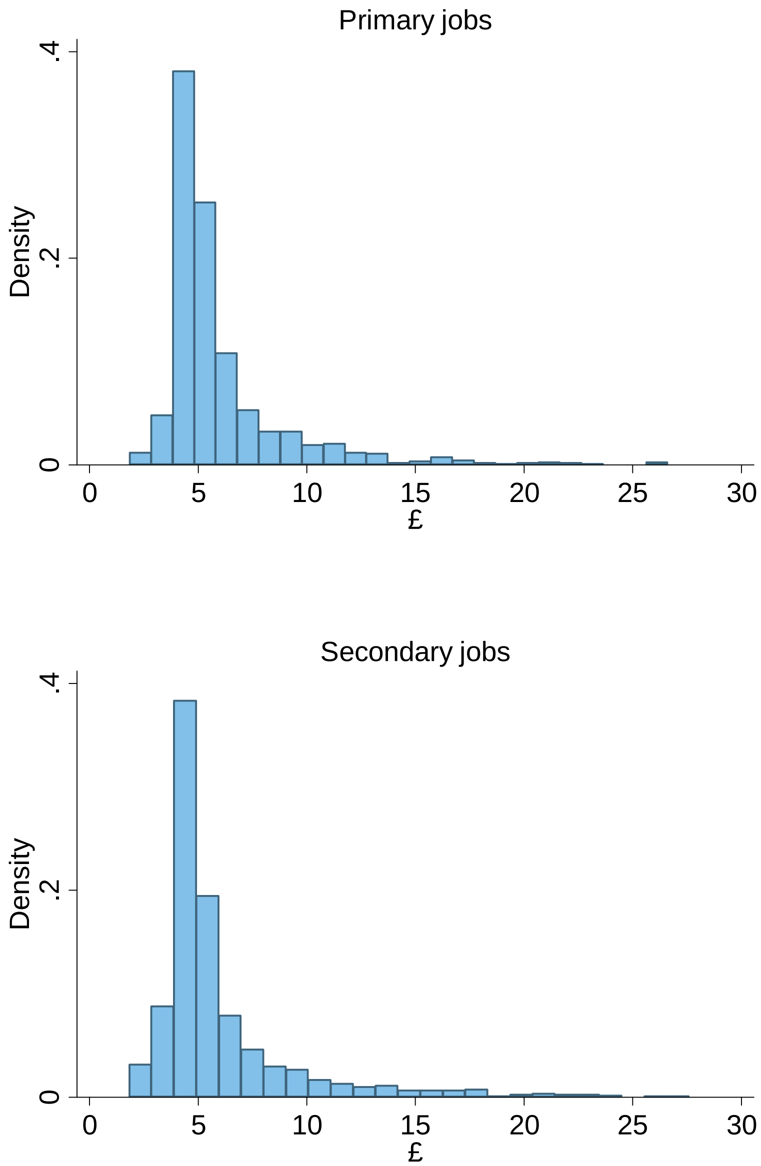

Table 3. Size distribution of employers, primary and secondary jobs

Employer size, 2nd job	Employer size, 1st job [1-10]	[11-24]	[25-49]	[50-249]	[250-∞)	Total
[1-10]	190	99	85	77	47	498
percent	15.81	8.24	7.07	6.41	3.91	41.43
[11-24]	72	88	28	38	20	246
percent	5.99	7.32	2.33	3.16	1.66	20.47
[25-49]	46	31	59	36	30	202
percent	3.83	2.58	4.91	3	2.5	16.81
[50-249]	37	30	25	51	24	167
percent	3.08	2.5	2.08	4.24	2	13.89
[250-∞)	15	14	19	13	28	89
percent	1.25	1.16	1.58	1.08	2.33	7.4
Total	360	262	216	215	149	1,202
percent	29.95	21.8	17.97	17.89	12.4	100

Table 4. Distribution of occupations in primary and secondary jobs

		Occupation on the 1st job									
	Occupation on the 2nd job	1	2	3	4	5	6	7	8	9	Total
1	managers and senior officials	5	1	1	6	1	4	2	0	7	27
	percent	0.42	0.08	0.08	0.50	0.08	0.33	0.17	0.00	0.58	2.25
2	professional occupations	0	22	8	5	1	3	2	1	3	45
	percent	0.00	1.83	0.67	0.42	0.08	0.25	0.17	0.08	0.25	3.74
3	associate professional and technical	1	3	49	9	0	15	11	1	9	98
	percent	0.08	0.25	4.08	0.75	0.00	1.25	0.92	0.08	0.75	8.15
4	administrative and secretarial	4	6	15	73	2	15	23	2	40	180
	percent	0.33	0.50	1.25	6.07	0.17	1.25	1.91	0.17	3.33	14.98
5	skilled trades occupations	0	2	0	0	2	6	2	0	10	22
	percent	0.00	0.17	0.00	0.00	0.17	0.50	0.17	0.00	0.83	1.83
6	personal service occupations	2	2	11	21	4	69	25	1	84	219
	percent	0.17	0.17	0.92	1.75	0.33	5.74	2.08	0.08	6.99	18.22
7	sales and customer	4	5	7	22	2	19	32	1	74	166
	percent	0.33	0.42	0.58	1.83	0.17	1.58	2.66	0.08	6.16	13.81
8	sales and customer service occupations	0	0	0	0	1	1	1	0	6	9
	percent	0.00	0.00	0.00	0.00	0.08	0.08	0.08	0.00	0.50	0.75
9	elementary occupations	1	1	10	17	3	56	41	5	302	436
	percent	0.08	0.08	0.83	1.41	0.25	4.66	3.41	0.42	25.12	36.27
	Total	17	42	101	153	16	188	139	11	535	1,202
	percent	1.41	3.49	8.40	12.73	1.33	15.64	11.56	0.92	44.51	100

Table 5. The sample of moonlighters: OLS results for cross-sections corresponding to primary (1,3,5) and secondary (2,4,6) jobs

Model	1	2	3	4	5	6
log wage	OLS	OLS	OLS	OLS	OLS	OLS
size2	0.031	-0.005	0.034	-0.016	0.027	0.014
	(0.028)	(0.032)	(0.025)	(0.028)	(0.022)	(0.025)
size3	0.082**	0.053	0.065*	0.048	0.066**	0.057*
	(0.030)	(0.034)	(0.026)	(0.031)	(0.022)	(0.029)
size4	0.071*	0.094*	0.057*	0.063	0.069**	0.047
	(0.032)	(0.037)	(0.027)	(0.033)	(0.022)	(0.032)
size5	0.243**	0.246**	0.170**	0.154**	0.130**	0.127**
	(0.037)	(0.050)	(0.029)	(0.041)	(0.025)	(0.035)
school			0.090**	0.087**	0.030**	0.034**
			(0.006)	(0.007)	(0.005)	(0.007)
exp_p			0.021**	0.023**	0.012**	0.016**
			(0.003)	(0.003)	(0.002)	(0.003)
exp_p2			-0.033**	-0.036**	-0.018**	-0.029**
			(0.006)	(0.007)	(0.005)	(0.007)
white			-0.084	-0.097	-0.030	-0.057
			(0.057)	(0.061)	(0.052)	(0.055)
marrd			0.041	0.022	0.025	0.018
			(0.021)	(0.025)	(0.018)	(0.022)
child			0.013	0.025*	0.007	0.013
			(0.010)	(0.012)	(0.008)	(0.011)
occup1					0.274**	0.315*
					(0.076)	(0.137)
occup2					0.740**	0.742**
					(0.065)	(0.069)
occup3					0.474**	0.432**
					(0.043)	(0.053)
occup4					0.218**	0.201**
					(0.028)	(0.036)
occup5					0.020	-0.045
					(0.042)	(0.057)
occup6					0.084**	0.065*
					(0.022)	(0.032)
occup7					0.043	-0.055
					(0.039)	(0.038)
occup8					-0.066	-0.033
					(0.048)	(0.102)
boss					0.104**	0.147**
					(0.022)	(0.040)
Quart	Yes	Yes	Yes	Yes	Yes	Yes
Ind					Yes	Yes
Regs					Yes	Yes
intcpt	1.596**	1.632**	0.281*	0.336*	0.879**	1.351**
	(0.063)	(0.085)	(0.113)	(0.134)	(0.134)	(0.184)
R-sq	0.05	0.04	0.324	0.243	0.563	0.434
No obs.	1202	1202	1202	1202	1202	1202

Models 1 and 2 are OLS regressions for the primary and secondary jobs respectively. Models 3 and 4 are the same as models 1 and 2 but with controls for observable characteristics of workers. Models 5 and 6 provide OLS estimates for the primary and secondary jobs controlling for occupations, supervisory responsibilities at the workplace, industry and regional dummies. All regressions contain time dummies (defined on the quarterly basis). Robust standard errors are reported in parentheses with $p<0.05$ = *, $p<0.01$ = **.

Table 6. The sample of moonlighters: OLS and FE results for the pooled data

Model	1	2	3	4	5	6
log wage	OLS	OLS	OLS	OLS	FE	FE
size2	0.013	0.009	0.014	0.020	-0.001	0.003
	(0.021)	(0.019)	(0.017)	(0.017)	(0.023)	(0.023)
size3	0.067**	0.057**	0.062**	0.064**	0.065**	0.069**
	(0.023)	(0.020)	(0.018)	(0.018)	(0.024)	(0.024)
size4	0.080**	0.059**	0.066**	0.061**	0.085**	0.091**
	(0.024)	(0.021)	(0.018)	(0.019)	(0.027)	(0.027)
size5	0.242**	0.161**	0.135**	0.128**	0.122**	0.120**
	(0.030)	(0.024)	(0.020)	(0.020)	(0.030)	(0.029)
mjob	-0.005	0.000	-0.018	-0.021	0.001	-0.012
	(0.016)	(0.014)	(0.012)	(0.012)	(0.011)	(0.011)
school		0.088**	0.035**	0.032**		
		(0.004)	(0.004)	(0.004)		
exp_p		0.022**	0.014**	0.014**		
		(0.002)	(0.002)	(0.002)		
exp_p2		-0.035**	-0.023**	-0.023**		
		(0.004)	(0.004)	(0.004)		
white		-0.090*	-0.10**	-0.042		
		(0.041)	(0.036)	(0.037)		
marrd		0.032*	0.028*	0.022		
		(0.016)	(0.014)	(0.014)		
child		0.019*	0.012	0.010		
		(0.008)	(0.007)	(0.007)		
occup1			0.288**	0.277**		0.102
			(0.071)	(0.072)		(0.087)
occup2			0.754**	0.736**		0.458**
			(0.049)	(0.048)		(0.081)
occup3			0.461**	0.451**		0.223**
			(0.034)	(0.034)		(0.049)
occup4			0.245**	0.208**		0.118**
			(0.022)	(0.023)		(0.040)
occup5			-0.001	-0.020		0.000
			(0.033)	(0.033)		(0.043)
occup6			0.095**	0.075**		0.035
			(0.018)	(0.019)		(0.028)
occup7			-0.029	-0.011		-0.027
			(0.016)	(0.027)		(0.034)
occup8			-0.021	-0.055		-0.070
			(0.057)	(0.064)		(0.088)
boss			0.111**	0.118**		0.054*
			(0.021)	(0.021)		(0.026)
Quart	Yes	Yes	Yes	Yes		
Ind				Yes		Yes
Regs				Yes		Yes
intcpt	1.637**	0.332**	1.011**	0.995**	1.657**	1.280**
	(0.052)	(0.096)	(0.088)	(0.111)	(0.013)	(0.170)
R-sq	0.04	0.273	0.458	0.479	0.028	0.110
No obs.	2404	2404	2404	2404	2404	2404

Models 1 to 4 are OLS regressions for the pooled data with different sets of controls. Models 5 and 6 provide fixed effects estimates (the sets of controls are the same as in regressions 2 and 4 respectively). All regressions contain time dummies (defined on the quarterly basis). Robust standard errors are reported in parentheses with p<0.05 = *, p<0.01 = **. For the FE models, the overall R-squared is shown.

Table 7. The sample of moonlighters: OLS and FE results for the pooled data, jobs in the same occupation

Model log wage	1 OLS	2 OLS	3 OLS	4 OLS	5 FE	6 FE
size2	0.037	0.020	-0.003	0.002	-0.006	-0.001
	(0.034)	(0.028)	(0.022)	(0.023)	(0.029)	(0.031)
size3	0.074*	0.076*	0.046	0.045	0.049	0.047
	(0.034)	(0.030)	(0.024)	(0.025)	(0.032)	(0.034)
size4	0.092*	0.063*	0.055*	0.052*	0.048	0.051
	(0.037)	(0.030)	(0.023)	(0.025)	(0.036)	(0.039)
size5	0.274**	0.196**	0.115**	0.117**	0.091*	0.106*
	(0.045)	(0.034)	(0.026)	(0.026)	(0.040)	(0.041)
mjob	-0.021	-0.018	-0.021	-0.019	-0.014	-0.014
	(0.024)	(0.020)	(0.016)	(0.016)	(0.014)	(0.014)
school		0.106**	0.012	0.011		
		(0.006)	(0.006)	(0.006)		
exp_p		0.020**	0.009**	0.010**		
		(0.003)	(0.003)	(0.003)		
exp_p2		-0.027**	-0.013*	-0.016**		
		(0.007)	(0.006)	(0.006)		
white		-0.058	-0.076	-0.044		
		(0.052)	(0.039)	(0.044)		
marrd		0.031	0.016	0.006		
		(0.023)	(0.018)	(0.019)		
child		0.038**	0.022*	0.020*		
		(0.011)	(0.009)	(0.009)		
occup1			0.612**	0.580**		
			(0.132)	(0.131)		
occup2			0.962**	0.945**		
			(0.060)	(0.061)		
occup3			0.722**	0.728**		
			(0.046)	(0.050)		
occup4			0.298**	0.262**		
			(0.031)	(0.031)		
occup5			0.061	-0.007		
			(0.077)	(0.080)		
occup6			0.141**	0.121**		
			(0.027)	(0.029)		
occup7			-0.030	-0.052		
			(0.034)	(0.041)		
occup8			0.000**	0.000**		
			(0.000)	(0.000)		
boss			0.068*	0.078**		0.046
			(0.028)	(0.028)		(0.037)
Quart	Yes	Yes	Yes	Yes		
Ind				Yes		Yes
Regs				Yes		Yes
intcpt	1.553**	-0.021	1.282**	1.398**	1.704**	0.912*
	(0.038)	(0.120)	(0.109)	(0.156)	(0.017)	(0.458)
R-sq	0.053	0.353	0.595	0.616	0.033	0.084
No obs.	1108	1108	1108	1108	1108	1108

Models 1 to 4 are OLS regressions for the pooled data with different sets of controls. Models 5 and 6 provide fixed effects estimates (the sets of controls are the same as in regressions 2 and 4 respectively). The dummy occup8 is dropped as there is only one observation in this occupational category. All regressions contain time dummies (defined on the quarterly basis). Robust standard errors are reported in parentheses with p<0.05 = *, p<0.01 = **. For the FE models, the overall R-squared is shown.

Table 8. Longitudinal sample from the QLFS embracing movers and stayers: OLS and FE results for the pooled data

Model	1	2	3	4	5	6
log wage	OLS	OLS	OLS	OLS	FE	FE
size2	0.090**	0.052**	0.046**	0.048**	0.005	0.005
	(0.007)	(0.006)	(0.006)	(0.005)	(0.009)	(0.009)
size3	0.159**	0.087**	0.078**	0.079**	0.008	0.007
	(0.007)	(0.006)	(0.005)	(0.005)	(0.010)	(0.010)
size4	0.217**	0.126**	0.104**	0.093**	0.019	0.019
	(0.007)	(0.006)	(0.005)	(0.005)	(0.011)	(0.011)
size5	0.306**	0.198**	0.171**	0.144**	0.034**	0.031**
	(0.006)	(0.006)	(0.005)	(0.005)	(0.012)	(0.011)
school		0.106**	0.047**	0.046**	0.007	0.007
		(0.001)	(0.001)	(0.001)	(0.007)	(0.007)
exp_p		0.032**	0.020**	0.019**	0.018*	0.017*
		(0.001)	(0.001)	(0.001)	(0.008)	(0.008)
exp_p2		-0.061**	-0.038**	-0.035**	-0.030**	-0.027**
		(-0.001)	(-0.001)	(-0.001)	(-0.007)	(-0.007)
white		-0.068**	-0.059**	0.029**	-0.03	-0.036
		(-0.010)	(-0.008)	(0.008)	(-0.077)	(-0.076)
marrd		0.027**	0.008*	0.013**	-0.007	-0.011
		(0.004)	(0.003)	(0.003)	(-0.012)	(-0.012)
child		-0.045**	-0.016**	-0.009**	-0.034**	-0.032**
		(-0.002)	(-0.002)	(-0.002)	(-0.007)	(-0.007)
occup1			0.536**	0.472**		0.073**
			(0.008)	(0.008)		(0.019)
occup2			0.662**	0.632**		0.087**
			(0.008)	(0.008)		(0.022)
occup3			0.448**	0.392**		0.066**
			(0.007)	(0.007)		(0.019)
occup4			0.270**	0.197**		0.033
			(0.005)	(0.006)		(0.019)
occup5			0.048**	0.042**		0.001
			(0.012)	(0.011)		(0.028)
occup6			0.111**	0.086**		-0.005
			(0.006)	(0.006(		(-0.019)
occup7			0.036**	0.080**		0.018
			(0.006)	(0.007)		(0.020)
occup8			0.064**	0.004		0.013
			(0.009)	(0.010)		(0.023)
boss			0.108**	0.115**		0.020**
			(0.004)	(0.004)		(0.005)
Quart	Yes	Yes	Yes	Yes	Yes	Yes
Ind				Yes		Yes
Regs				Yes		Yes
intcpt	1.912**	0.297**	0.859**	0.835**	1.794**	1.883**
	(0.014)	(0.020)	(0.019)	(0.025)	(0.282)	(0.290)
R-sq	0.056	0.374	0.544	0.587	0.066	0.215
No obs.	44792	44792	44792	44792	44792	44792

Models 1 to 4 are OLS regressions for the pooled data with different sets of controls. Models 5 and 6 provide fixed effects estimates (the sets of controls are the same as in regressions 2 and 4 respectively). All regressions contain time dummies (defined on the quarterly basis). Robust standard errors are reported in parentheses with p<0.05 = *, p<0.01 = **. For the FE models, the overall R-squared is shown.

Table 9. Longitudinal sample from the QLFS: moonlighters

Model log wage	1 OLS	2 OLS	3 OLS	4 OLS	5 FE	6 FE
size2	0.098**	0.079**	0.071**	0.084**	-0.016	0.022
	(0.028)	(0.025)	(0.023)	(0.022)	(-0.037)	(0.039)
size3	0.112**	0.120**	0.113**	0.136**	-0.020)	0.011
	(0.030)	(0.027)	(0.024)	(0.024)	(-0.044)	(0.042)
size4	0.143**	0.120**	0.109**	0.120**	0.013	0.018
	(0.031)	(0.027)	(0.024)	(0.024)	(0.047)	(0.048)
size5	0.332**	0.242**	0.180**	0.153**	0.079	0.075
	(0.032)	(0.029)	(0.026)	(0.026)	(0.050)	(0.053)
school		0.088**	0.033**	0.029**	-0.004	0.001
		(0.005)	(0.005)	(0.005)	(-0.021)	(0.022)
exp_p		0.023**	0.013**	0.013**	-0.003	0.008
		(0.003)	(0.003)	(0.003)	(-0.032)	(0.032)
exp_p2		-0.040**	-0.020**	-0.021**	-0.025	-0.036
		(-0.007)	(-0.007)	(-0.006)	(-0.045)	(-0.045)
white		-0.085	-0.041	0.040		
		(-0.061)	(-0.047)	(0.049)		
marrd		-0.008	-0.002	0.009	0.083	0.090
		(-0.020)	(-0.017)	(0.017)	(0.045)	(0.053)
child		-0.006	0.009	0.010	-0.020	-0.019
		(-0.010)	(0.009)	(0.009)	(-0.042)	(-0.040)
occup1			0.445**	0.383**		-0.004
			(0.069)	(0.061)		(-0.103)
occup2			0.688**	0.665**		0.072
			(0.047)	(0.047)		(0.087)
occup3			0.489**	0.414**		0.078
			(0.032)	(0.033)		(0.081)
occup4			0.245**	0.182**		0.055
			(0.022)	(0.024)		(0.102)
occup5			-0.005	-0.010		-0.048
			(0.056)	(-0.054)		(-0.181)
occup6			0.133**	0.084**		-0.007
			(0.022)	(0.023)		(-0.064)
occup7			0.054	0.086*		-0.036
			(0.030)	(0.036)		(-0.090)
occup8			0.089	0.043		-0.085
			(0.057)	(0.061)		(-0.122)
boss			0.126**	0.113**		0.013
			(0.021)	(0.020)		(0.030)
Quart	Yes	Yes	Yes	Yes	Yes	Yes
Ind				Yes		Yes
Regs				Yes		Yes
intcpt	1.587**	0.317**	0.869**	1.007**	1.954**	1.625*
	(0.072)	(0.116)	(0.107)	(0.156)	(0.727)	(0.750)
R-sq	0.074	0.29	0.461	0.507	0.012	0.067
No obs.	1816	1816	1816	1816	1816	1816

Models 1 to 4 are OLS regressions for the pooled data with different sets of controls. Models 5 and 6 provide fixed effects estimates (the sets of controls are the same as in regressions 2 and 4 respectively). All regressions contain time dummies (defined on the quarterly basis). Robust standard errors are reported in parentheses with p<0.05 = *, p<0.01 = **. For the FE models, the overall R-squared is shown.

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9. Appendix: Definition of variables

wage – hourly earnings of workers

hours – hours of work in the reference week

age – worker age

white – dummy for the worker's ethnic background (1 stands for white as opposed to Mixed, Asian or Asian British, Black or Black British, Chinese, Other group)

marrd – dummy for the worker's marital status (1 stands for married/cohabiting)

child – number of dependent children below 19 years old

school – years of schooling (imputed on the basis of the categorical variables describing the highest degree attained)

exp_p – potential experience, calculated as age minus years of schooling minus five

occup1 – dummy for managers and senior officials

occup2 – dummy for professional occupations

occup3 – dummy for associate professional and technical occupations

occup4 – dummy for administrative and secretarial

occup5 – dummy for skilled trades occupations

occup6 – dummy for personal service occupations

occup7 – dummy for sales and customer service occupation

occup8 – dummy for process, plant and machine operatives

occup9 – dummy for elementary occupations

boss – dummy for managerial/supervisory responsibilities at the workplace

mjob – dummy for the primary (main) job

size1 – dummy for [1-10] employees in a firm

size2 – dummy for [11-24] employees in a firm

size3 – dummy for [25-49] employees in a firm

size4 – dummy for [50-249] employees in a firm

size5 – dummy for [250-∞) employees in a firm

Inds – industry dummies (based on the one-digit classification)

Regs – regional dummies

Quart – time dummies (quarters)