An Analysis of the Determinants of Pay and Well-being using Employer-Employee Data

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Neither the original collectors of the data nor the Archive bear any responsibility for the analyses or interpretations presented here.

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Declaration

I declare the following:

The material contained in this thesis is my own work, except for chapters 5 and 6 which are based upon joint work with my supervisor, Professor Andrew Oswald. In both these chapters my individual contribution was at least 50 percent and was principally concerned with empirical analysis.

No material in this thesis has been used before or has been published previously.

The thesis has not been submitted for a degree at another university.

Abstract

This thesis studies the determinants of pay and well-being. The first three chapters use new British employer-employee data to study the determinants of pay. Chapters three and four are also interested in the determinants of job satisfaction, whilst chapters five and six analyse factors that shape reported well-being.

Chapter two tests whether firms share product market rents with their employees. After controlling for worker and firm fixed effects, we observe evidence in support of rent-sharing upon weekly earnings, but no robust positive effect upon hourly pay.

The third chapter analyses the observed positive relationship between employer size and wages. It designs a test as to whether this relationship reflects a compensating differential. This is not found to offer a good explanation as to why wages are greater in large establishments. Instead, correlates of worker skill and person fixed effects are most successful in explaining the plant size-wage differential.

There has been very little research on racial differences in job satisfaction levels. Chapter 4 examines the relationship between race, pay and well-being. Workplaces that employ more ethnic minority employees are associated with lower levels of job satisfaction, for both white and non-white workers. Non-white employees are paid less than otherwise similar white employees, and are less satisfied with their pay even when pay is held constant.

One of the most fundamental ideas in economics is that money makes people happy. Chapter 5 constructs a test. In the spirit of a natural experiment, it shows individuals who receive windfalls have higher mental well-being in the following year. It calculates the size of the effect.

The final chapter studies the well-being of British public sector workers in the 1990s. Relative to private sector employees, stress levels and job satisfaction within the public sector are shown to have significantly worsened over the decade.

Chapter One

Introduction

This thesis studies the determinants of pay and well-being. These outcomes are likely to be the product of complicated interactions between workers and employers. Understanding these interactions, and how they shape behaviour, is crucial to our understanding of the working of the labour market. The need for a more complete picture of the labour market, and the relationship between individuals and firms, has been known for some time. Data sufficiently rich to allow us to study such situations have, however, only recently become available.

This study uses such data, with detailed information about both the worker and the firm, to analyse the determinants of pay. These data allow a fuller examination of the relationship between employer and employee characteristics, and their impact upon pay. Simply put, what determines wages? Is it education or skill, or the type of employer for which an individual works? Chapter two analyses whether more profitable firms pay higher wages, whilst chapter three examines competing hypotheses for the existence of a positive relationship between employer size and pay. The role of the employer in explaining the racial wage differential, and in particular the effect of the sorting of ethnic minority workers into different types of workplaces, is analysed in chapter four. This work provides a new examination of the effect of the employer upon pay, and adds to the recent research that has studied linked employer-employee data.[1] By implication this questions whether wage determination is, to some degree, non-competitive.

This thesis also examines the determinants of worker well-being, using self-reported measures of job satisfaction and mental health. Some economists have been wary of studying such subjective outcomes. Yet workers' perceptions of their jobs are likely to be an important determinant of behaviour. A more satisfied

[1] Examples include Abowd et al (1999), Entorf, Gollac and Kramarz (1999), Goux and Maurin (1999), Groshen (1991), Hildreth (1998) and Troske (1999).

worker, even when compared to an otherwise identical employee, is less likely to quit their job.[2] Job satisfaction is negatively correlated with absenteeism, non- and counter-productive work.[3] Subjective measures of well-being are also correlated, in the expected direction, with objective characteristics, such as unemployment, and with assessments of the person's happiness by others.[4] These subjective measures may then allow us to infer, albeit with some error, the relative well-being of different types of individuals.

This thesis uses these subjective measures of well-being to provide new tests of economic hypotheses. Chapter three uses these data to examine whether working conditions are inferior within large employers. In chapter four measures of job satisfaction are used to provide a new analysis as to whether ethnic minority employees are economically disadvantaged. Arguably the central tenet of economics is that money makes people happy. Chapter five constructs a test. It studies whether the recipients of financial windfalls have higher mental well-being in the following year. Finally, the well-being of British public sector workers over the 1990s is examined in chapter six, and outcomes contrasted with those in the private sector. This work adds to a recent literature, on the border between economics and psychology, which has attempted to understand the patterns in well-being data.[5]

Chapter two uses new British employer-employee data to examine the effect of the financial conditions of the firm upon worker pay. It tests whether firms, for some reason, share product market rents with their employees. The elasticity of wages with respect to firm profits per worker is estimated, at the mean, to be

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[2] See Freeman (1978).
[3] See Clark and Oswald (1996).
[4] See Konow and Earley (1999).
[5] For example: Blanchflower and Freeman (1997), Blanchflower and Oswald (1999), Clark (1996), Di Tella and MacCulloch (1999), Frank (1985, 1997) and Frey and Stutzer (1998).

approximately 0.02, after controlling for observed firm and worker characteristics. This positive effect is found to be robust to controlling for formal profit-sharing schemes, within occupations where labour supply difficulties should be limited, with respect to past firm profitability, and to not be limited to the union sector.

After controlling for worker and firm fixed effects, we observe statistically significant evidence in support of rent-sharing upon weekly earnings, with an estimated elasticity of 0.01, but no robust positive effect upon hourly pay, in a sample that potentially favours the rent-sharing hypothesis. A key issue with estimation is endogeneity: an increase in worker pay will reduce firm profitability, other things being equal. The estimated coefficient upon firm profitability may then be downward biased. Firm profitability is therefore instrumented by measures of overseas product market shocks, here captured by movements in US industry profitability. The resulting estimates of the parameter upon firm profitability are, however, either incorrectly signed or statistically insignificant. The evidence in favour of the rent-sharing hypothesis is then, at best, modest and limited to weekly earnings.

The third chapter tests competing explanations for the famous positive correlation between employer size and wages. Wages are observed to be monotonically increasing in workplace size, whilst *firm* size is found to exert a positive, but concave, effect upon pay, holding constant the size of the establishment. The addition of more refined controls for employer characteristics (the capital to labour ratio, the intensity of monitoring, and firm profitability) leave the establishment size-wage premium largely unperturbed.

The role of unobserved differences in labour productivity is analysed in two ways. Correlates of worker skill, such as the use of information technology and the skill of the establishment's workforce, are found to explain up to 15 percent of the

plant size-wage differential, and up to 30 percent of the firm size-wage premium. Secondly, controls for person fixed-effects are found to reduce the estimated effect of workplace size upon wages by over a half. Nevertheless, wages are still observed to be significantly greater in large establishments.

One of the contributions of the chapter is to design a novel test, using job satisfaction data, as to whether the relationship between employer size and worker pay reflects a compensating differential (perhaps for inferior working conditions). Job satisfaction is found to be superior in the smallest plants, yet differences in satisfaction between medium-sized and large establishments are not pronounced. A compensating-differential-for-size theory cannot then explain why pay is observed to be higher in the largest plants, relative to medium-sized workplaces. Establishment size is, furthermore, found not to exert a robust influence upon worker satisfaction once we hold constant the size of the firm. In contrast, wages are statistically significantly greater in large plants, holding firm size constant.

On this evidence, employee distaste for employer size does not offer a strong explanation for the existence of a plant size-wage premium, though it may help to explain the firm size-wage premium. Correlates of worker skill and person fixed effects are, here, found to offer the most convincing avenue from which to explain the establishment size-wage differential.

Chapter four investigates the role of the employer in explaining racial differentials in pay and job satisfaction. The effect of the ethnic composition of the plant's workforce is extensively examined. Non-white workers are found to earn lower wages in plants with a higher proportion of ethnic minority co-workers. White wages, on the other hand, are only weakly related to the racial composition of the workforce. The difference between the wages of white and ethnic minority workers is then greatest in workplaces that hire a large proportion of minority staff.

The racial differential in wages is found to be robust for workers within the same occupation and establishment. The source of the racial wage gap is then, chiefly, not that ethnic minority workers are 'crowded' into low-pay plants but, rather, they are paid less well in any given workplace.

Workers in plants that employ more non-white workers are found to have lower levels of job satisfaction. This is found for white males and females, and for ethnic minority women. Results are, however, more mixed for non-white men. The plant's rates of quits, separations, and absenteeism, are also positively related to the ethnic minority employment share. The evidence then suggests workplaces with a large proportion of minority workers are, here, associated with inferior working conditions. This is consistent with, both, models of discrimination and unobserved worker quality differences, where non-white workers are, for some reason, less productive (possibly due to pre-labour market discrimination).

Evidence is also observed consistent with ethnic minority employees trading off lower pay to work with more fellow minority co-workers, where they may perhaps encounter less prejudice and racism. Non-white pay is lower, and tenure higher, in plants with a greater proportion of ethnic minority staff. Nevertheless, ethnic minority employees, both male and female, are less satisfied with their pay, compared to otherwise similar white workers, even when pay is held constant. Results remain when we hold constant the establishment's effect upon satisfaction. This provides new evidence that would appear consistent with discrimination within the workplace.

One of the most fundamental ideas in economics is that money makes people happy. Chapter five constructs an empirical test. Its approach seems to have three advantages. First, we follow a group of individuals longitudinally, and hence can measure the same person's well-being and income level at different points in

time. Second, these data provide information on two sources of financial windfalls – inheritances and lottery wins. In the spirit of a natural experiment, they are probably as close as can be achieved to randomly occurring events in which some individuals receive money while others, in a control group, do not. Third, information is available on two measures of well-being: mental stress using a standard psychological health measure, and happiness using a simple four-point question.

We find that, as theory would suggest, a windfall of money is followed by lower mental stress and higher reported happiness. A windfall of 50,000 pounds is predicted to improve mental well-being by between 0.1 and 0.3 standard deviations.

In the early 1990s, the British government embarked on a process of reform, with the objective of improving the provision of public services. The public sector was subjected to greater scrutiny and the introduction of market forces, and tough budgetary limits imposed. Chapter six seeks to examine the well-being of British public sector workers, and contrasts outcomes with those in the private sector. At the start of the 1990s, levels of psychological health, as measured by a General Health Question score, for public sector workers were similar to those within the private sector, but by 1998 are noticeably worse. Consistent with this, a relative decline in public sector job satisfaction is also observed over the decade.

These effects are found whichever way the data are cut and irrespective of the estimation method used. It is not possible to be completely certain as to why stress has risen with the public sector. The evidence suggests it cannot be satisfactorily explained by the changing composition of public sector employment, by the relative decline in public sector pay, or by aggregate movements in economic conditions. What can be said, however, is that the evidence points to an unambiguous reduction in well-being amongst Britain's public sector workers.

Finally, the seventh chapter summarises the conclusions from the research undertaken in the thesis.

Chapter Two

Do Firms Share Rents? An analysis using Linked Employer-Employee Data

Abstract

This chapter uses new British linked employer-employee data to test whether firms share product market rents with their employees. We find OLS estimates, of the effect of firm profitability on worker wages, to be positive and statistically significant. Results are robust to controlling for formal profit-sharing schemes, within occupations where labour supply difficulties should be limited, with respect to past firm profitability, and are not limited to the union sector. After controlling for worker and firm fixed effects, we observe statistically significant evidence in support of rent-sharing upon weekly earnings, but no robust positive effect upon hourly pay. These results remain when firm profitability is instrumented by international product market shocks. The evidence in favour of the rent-sharing hypothesis is then, at best, modest and limited to weekly earnings.

2.1 Introduction

This chapter examines the effect of the financial conditions of the firm upon employee pay. By implication this questions whether wage determination is, in some way, non-competitive. Do firms, for some reason, share product market rents[1] with their employees? In the textbook competitive, employees receive compensation equal to the opportunity cost of their time, and pay is independent of firm profitability. Yet, in less simplified frameworks, alternative competitive models do allow a positive correlation between pay and profitability. An essentially competitive model, but one which allows for lags in adjustment, may predict such a relationship, albeit only in the short-run. Alternatively, principal-agent problems may lead firms to directly link pay to performance, producing a correlation between wages and firm prosperity, but one linked to worker productivity.

New employer-employee data are used to attempt to distinguish between the competing hypotheses for the existence of a positive relationship between profitability and pay. These data match individual responses from a nationally representative panel survey, the BHPS, to the company accounts information of the employing firm. Such data should allow a fuller examination of the relationship between employer and employee characteristics, and their impact upon pay. Additionally, these data have the advantage that they are not constrained to manufacturing, publicly quoted or unionised employers, though large firms are heavily represented.

The elasticity of wages with respect to firm profits per worker is estimated, at the mean, to be approximately 0.02, after controlling for observed firm and

[1] The term 'rents' here denotes super-normal returns, that is a return to the entrepreneur in excess of the competitive market level.

worker characteristics. Moving from one standard deviation below mean profitability to one standard deviation above is predicted to increase wages by approximately 8 percent. This positive effect is found to be robust to controlling for any explicit performance pay, and remains statistically well determined within a sample of low-skill occupations where 'thin' labour markets, and hiring difficulties, should be least likely. This suggests the relationship between wages and profitability cannot be simply explained by a competitive framework with hiring frictions, or by performance pay. Evidence is also presented that rent-sharing is not limited to the unionised sector.

After the addition of controls for worker and firm fixed effects, OLS estimates of the rent-sharing parameter are attenuated, from 0.045 to 0.013, and are no longer robust, in a sample that potentially favours the rent-sharing hypothesis. When weekly, rather than hourly, pay is examined as the dependent variable, OLS estimates remain positive and statistically significant, at 0.026, despite potential downward endogeneity bias.

A key issue is endogeneity: when wages increase, other things being equal, firm profits will fall. Such a feedback effect may cause the estimated parameter upon firm profitability, in a wage equation, to be downward biased. It is then desirable to instrument the profitability variable. Profits per worker are here instrumented by measures of international product market shocks, captured by movements in US industry profitability. The cross-section estimates of the rent-sharing parameter then increase by between 50 and 100 percent but are, however, not statistically robust. Where models include controls for unobserved heterogeneity, instrumentation results, both for weekly and hourly pay, in incorrectly signed or statistically insignificant coefficients.

More profitable firms are then found to pay higher wages, other things being equal. Yet, with respect to hourly pay this result is not robust to the inclusion of controls for worker and firm fixed effects. Such models are, generally, to be preferred. Whilst we find no robust positive effect of firm profitability upon hourly pay we do, however, find evidence in support of rent-sharing upon weekly pay. This may yet reflect unobserved differences in the hours of work. The evidence in favour in the rent-sharing hypothesis is then, for these data, moderate and confined to weekly pay.

The plan of the chapter is as follows. In section two, models that can explain a positive correlation between firm profitability and worker pay are outlined. Section three documents previous empirical evidence. Section four outlines the data, whilst section five discusses the econometric strategy and presents results. Finally, section six concludes.

2.2 Models of a wage-rents correlation

In a textbook model of a competitive labour market, firms are wage-takers, wages fully reflect workers' opportunity costs, and pay is independent of firm characteristics. On this view, an observed correlation between wages and firm profitability is then an artefact of unobserved, to the researcher, differences in worker productivity. Alternative models do though predict a positive relationship between profitability and pay. Three models are here studied. The first, a non-competitive model of wage setting, examines a process where bargaining determines wages, and implies rents are divided between the firm and its employees. The second, a modified competitive framework, retains the assumption that workers are wage takers but posits that firms may face short-run labour supply constraints. In

the face of a demand shock the wage can then be, temporarily, bid above its competitive level. Finally, firms may link pay to performance, as an incentive mechanism to induce greater worker effort. Wages determined in this way, whilst related to profits, reflect productivity. These models are discussed, in turn, below.

2.2.1 A bargaining model

Bargaining over wages, either explicit or implicit, may provide a link between the profitability of the firm and the wages of its employees. The union (or bargaining unit) is assumed to maximise a Utilitarian utility function[2] (Oswald, 1982), it then maximises the 'total' utility (U) of its members: max $U = nu(w) + (m - n)u(w^*)$; where $u(w)$ is the representative worker's utility derived from wage w, w^* is the alternative wage available outside the firm, m is the union's membership and n is employment within the firm ($n<m$).

The firm is assumed to maximise profits: max $\pi = \rho f(n) - wn$; where π is the firm's profits, ρ is a demand shock or output price variable and the firm's production function, f(.) is assumed dependent only on labour, n. As a simplification we assume no adjustment costs to labour.

The employer is assumed to bargain with workers over both wages and employment – an efficient bargain – and the Nash maximand[3] is expressed:

$$\text{Max}_{w,n}\ [U - U^*]^{\phi}[\pi - \pi^*]^{1-\phi} \qquad (1)$$

where ϕ can be viewed as the relative bargaining power of the unions or employees, and U^* and π^* are the threat points of workers and firms respectively. In the event

[2] This is equivalent to the expected utility form of McDonald and Solow (1981) when union membership is fixed.

[3] The Nash solution to the bargaining problem maximises the product of agent's utility net their threat points. Binmore et al (1986) show this is equivalent to the outcome of a strategic non-co-operative bargaining game of alternating offers, where delays in agreement are costly. The threat points are then interpreted as the fall back options in the case of no agreement.

of a breakdown in negotiation, we assume the firm earns zero profits ($\pi^* = 0$) and all the union members receive the alternative wage ($U^* = mu(w^*)$). The Nash maximand is then:

$$\text{Max}_{w,n}\ [nu(w) + (m - n)u(w^*) - mu(w^*)]^{\phi}[\rho f(n) - wn]^{1-\phi} \qquad (2)$$

Cancelling the $mu(w^*)$ terms and restricting attention to interior optimum, the first order conditions are:

$$w: \frac{\phi\, u'(w)}{[u(w) - u(w^*)]} - (1 - \phi)\frac{n}{\pi} = 0 \qquad (3)$$

$$n: \frac{\phi}{n} + (1 - \phi)\frac{\rho f'(n) - w}{\pi} = 0 \qquad (4)$$

Re-arranging the first order condition with respect to wages implies:

$$\frac{[u(w) - u(w^*)]}{u'(w)} = \frac{\phi}{1-\phi}\left(\frac{\pi}{n}\right) \qquad (5)$$

This result can be simplified by using the first order Taylor approximation:

$$u(w^*) \cong u(w) + (w^* - w)u'(w) \qquad (6)$$

substituting this expression into (5) and re-arranging implies:

$$w \cong w^* + \frac{\phi}{1-\phi}\left(\frac{\pi}{n}\right) \qquad (7)$$

The equilibrium wage is then an increasing function of the outside wage, the bargaining strength of the union (or worker group), and profits per employee.[4] As this relationship is derived from only the first of the first order conditions it holds irrespective of the exact form of the employment function. Conditional on efficiency, it allows employment to be located on the labour demand function, if

[4] This result can be obtained directly if one assumes the union's membership is risk-neutral (i.e. $u(w)=w$). The union then maximises the rent (the difference between the wage and the alternative wage) appropriated by its members. This is a 'strongly' efficient bargain which, indeed, Abowd's (1989) evidence supports.

indifference curves are locally horizontal (Oswald, 1993), or on an upward sloping contract curve (McDonald and Solow, 1981).[5]

This model lays the foundation for much of the subsequent empirical discussion. Given the union bargaining framework, from which it is derived, one may expect it to offer a poor description of behaviour in a non-union setting. Yet there are arguments to suggest such a relationship may still apply. If insiders derive bargaining power from skill shortages or costs in labour adjustment, and act as a *de facto* union, then the model may generalise to one of implicit bargaining.[6] Frictions upon job mobility, either due to search costs or informational lags regarding job opportunities, may generate rents and worker bargaining (see Pissarides, 1990). Equity concerns may lead to bargaining in a non-union setting, if the concept of 'fair' treatment depends upon the employer sharing some of its profits. It may then be optimal for the firm to bargain with its employees, who may otherwise withhold effort. However, it remains controversial as to whether convincing models of rent-sharing can be constructed within competitive labour markets.

2.2.2 Competitive frictions

A rent-wage correlation can also occur in a competitive model, where workers are wage takers, if there are temporary supply constraints on labour.[7] If we assume, in the short run, there exists impediments to the mobility of labour or skill shortages, the (short run) labour supply curve will slope upwards. A positive shock to firm revenue, that generates super-normal returns, can then cause firms to bid up wages,

[5] Nickell and Wadhwani (1990) develop an alternative right-to-manage model, where wages lie strictly upon the labour demand curve. The resulting wage is a convex combination of insider factors, relating to the firm's ability to pay and union power, and outside pay.
[6] For a full description of insider-outsider models see Lindbeck and Snower (1989)
[7] See Hildreth and Oswald (1997) for a fuller discussion.

and produce a positive correlation between wages and profits.[8] The model then suggests the correlation between profits and pay should be strongest within 'thin' labour markets, where constraints on hiring are most severe. Nevertheless, this can only be a temporary phenomenon, workers will migrate to the high wage firm or industry, bid down the wage and restore competitive conditions.

2.2.3 Performance-related-pay

An alternative explanation for a positive correlation between wages and profits lies in contract theory, and is linked to the research into principal-agent models.[9] Here employers link pay to performance to provide workers with incentives to supply effort. In a typical hidden action model the risk averse worker is assumed to provide effort (e) at convex cost, c(e). This effort is translated into output (y) through a stochastic production function, $y = e + \varepsilon$, where ε is a normally distributed random error, with mean zero and variance σ^2. Firms are risk neutral, and pay a wage conditional upon worker output, w(y). We assume this contract takes the linear form,[10] $w = s + by$, where s is the fixed salary and b the pay-performance sensitivity. The optimal wage contract, that maximises output subject to worker behaviour, is characterised by a sharing rate (b*) which is decreasing in the risk aversion of the agent, in the random variation in output, and where the disutility of effort increases at a faster rate. Intuitively, employees dislike variability in their income and prefer more stable wage contracts.

[8] This result requires the profit function to be homogeneous of degree one.
[9] For a review of the literature studying executive compensation see Murphy (1999). For non-executive employees see Baker et al (1988) and Burgess and Metcalfe (1999). The risk-sharing contract model (Baily, 1974, and Azariadis, 1975) is not here discussed.
[10] The linearity assumption has some theoretical support in that non-linear contracts may lead workers to shift effort to maximise current, rather than long run, performance, see Murphy (1999).

This pay structure predicts a positive relationship between wages and output. Yet it is not obvious why there should be a strong link between pay and profits. The increased productivity induced is compensated with greater pay. A further question is; how prevalent are performance based wage contracts? Baker, Jensen and Murphy (1988) suggest the compensation system, "appears to be one with little or no pay for performance" (p.599). Difficulties in setting and evaluating (the correct) targets, difficulties in up-rating standards, and in horizontal equity problems of fairness amongst employees, are suggested as reasons for this lack of, non-executive, performance pay.

Whilst performance pay does not appear pervasive, profit-sharing, in which an individual's compensation is tied to the performance of the firm, either through shares scheme or bonuses, has seen a marked growth in the UK in the 1990s. Booth and Frank (1999) document evidence that the number of employees in such schemes rose from 232,000 in March 1990 to 2,438,000 in March 1995. This was probably in part due to tax exemptions, which were subsequently phased out in 1997. Such schemes were felt to raise worker productivity, possibly due to feelings of greater involvement in firm success. Yet in large organisations the free-rider problem is considerable, an individual bears the full cost of increased effort but gains only a fraction of the increased profits. Peer pressure may, however, act to mitigate this externality. Profit-sharing schemes, for incentive reasons, should then be less likely in large plants or firms, or in plants or firms where there are a large number of managers (monitoring offsets the need to provide incentives).

2.3 Existing Empirical Evidence

2.3.1 Industry and Establishment effects upon Worker wages

Some of the earliest evidence as to whether more profitable firms pay higher wages is that of Slichter (1950), who documented empirical regularities in the inter-industry wage structure for the United States in the early 1940s. Whilst highly aggregated, the evidence suggested a positive relationship between industry profit margins and wages. The data also revealed a strong correlation between the pay of skilled and unskilled labour within industries, and that the inter-industry wage structure remained relatively stable over time.

More modern research by Dickens and Katz (1987) and Krueger and Summers (1987, 1988) analysed these empirical regularities using more rigorous regression techniques. The unexplained inter-industry wage differentials is found to be relatively stable across time and between countries, and for workers of different age, gender, skill type and occupation. In line with Slichter's earlier findings, the estimated US industry wage differentials are positively correlated with industry profitability, and Krueger and Summers (1987, 1988) suggest these wage differentials represent some form of rent-sharing. Subsequent research has, mainly, focussed upon whether the competitive model can explain these results.

That inter-industry wage differences reflect different working conditions would appear unlikely as, Dickens and Katz (1987) have shown, industry wage differences are present within detailed occupation groups. For example, secretaries, for whom presumably working conditions are generally very similar, are paid more in highly profitable industries. Krueger and Summers (1988), using a sample of displaced workers and employing a correction for false industry transitions, examine whether the inter-industry wage differential is robust to controls for unobserved worker fixed effects. The estimated industry effects are found to be very similar to those estimated in the cross-section. Gibbons and Katz (1992), who study a sample of workers specifically displaced by plant closure, find similar evidence. Blackburn

and Neumark (1992) instead proxy the effect of unobserved worker skill by IQ scores and find ability, as measured by test scores, has little impact upon the industry wage differential.

In contrast, Murphy and Topel (1987) find only a third of the wage change predicted, for a worker moving between industries, by cross-section estimates is actually captured by employees when they change jobs. However, pay is defined by annual earnings and so is liable to include the effect of both the pre- and post-mobility industry. Estimates may then be biased downwards. Abowd, Kramarz and Margolis (1999) and Goux and Maurin (1999), studying distinct French employer-employee panel data, also find individual effects to be the primary force behind inter-industry wage differentials.[11]

The evidence as to whether the inter-industry wage differential represents unobserved worker skill is then, at best, mixed. Furthermore, evidence at the industry level may be biased, due to omitted market characteristics (such as the state of technology) or the aggregation assumptions implicit in industry analysis. Together these suggest analysis should be, within industries, at the level of the firm. Groshen (1991), in one of the first studies to examine data with information regarding the worker and their workplace, finds significant wage variation within industries is attributable to establishment wage differentials, after controlling for occupation, gender and region. Abowd, Kramarz and Margolis (1999) and Goux and Maurin (1999) also find evidence of substantial firm heterogeneity in wages.

2.3.2 The Employer's 'ability to pay' and Employee wages

[11] Hamermesh (1999) has suggested that with limited controls for observed worker characteristics such estimates may place too much emphasis upon unobserved *individual* rather than *firm* heterogeneity. This may bias results in favour of the individual heterogeneity explanation.

Subsequent to the research of Dickens and Katz (1987) and Krueger and Summers (1987, 1988) a second generation of studies has sought to test the rent-sharing hypothesis more directly, by estimating versions of (7) using firm or establishment data.

Blanchflower, Oswald and Garett (1990) use the 1984 Workplace Industrial Relations Survey (WIRS), of establishments with 25 employees or more, to examine insider and outsider influences upon pay. Employer effects, as measured by the extent of product-market competition and (manager-rated) firm performance, are found to be significant determinants of the pay of skilled employees, but not that of unskilled workers. Stewart (1990), using the same data, examines the factors that determine the union's ability to extract rents. Where competitive market conditions are present the union mark up is estimated to be close to zero. In contrast, where a firm has market power the union wage differential is estimated to be 8 percent. Even when unions are not present, plants with market power are estimated to pay significantly higher wages than plants in competitive product markets.

Nevertheless, as cross-section evidence, results are potentially contaminated by unobserved differences in employee skill. Subsequent studies have then, largely, examined wage equations using company or establishment panels, and controlled for unobserved heterogeneity by including employer fixed effects. Typically the measure of earnings is the average annual pay per employee, and the external wage is captured by region and industry effects. Econometric analysis of the effect of employer 'ability to pay' upon wages then faces two main problems. Firstly, wages and profits may be endogenously related. An increase in wages, *ceteris paribus*, will cause profits to fall, and the parameter upon profits per worker may then be biased downwards. Secondly, as the dependent variable is the average pay per worker and the key variable of interest profits per worker, any measurement error in firm

employment will cause a spurious positive correlation between pay and profitability. In response to these problems some authors have replaced, or instrumented, the profitability term by its lag. These variables are assumed to be pre-determined, and so uncorrelated with the wage equations error term. Other studies have sought to identify exogenous shifts in firm profits.

Nickell and Wadhwani (1990) estimate firm-level earnings equations using a panel of 219 publicly quoted UK manufacturing companies from 1972 to 1982. The firm's ability to pay is measured by its sales per worker which, due to potential endogeneity and measurement error, is instrumented by its lagged values. The long run elasticity of wages with respect to sales per worker is then estimated to be 0.11. Currie and McConnell (1992) observe similar results for the US. Using a large panel of union contracts the authors estimate a positive and statistically significant effect of sales upon wages. Nickell, Vainiomaki and Wadhwani (1994) extend the earlier work of Nickell and Wadhwani (1990). They study a sample of 800 manufacturing firms, and examine the role of market power upon pay. Market share is found to enter the wage equation positively and statistically significantly, with an estimated long run elasticity of 0.08. This effect is found to be more pronounced for large, non-unionised, and non-competitive firms.

Christofides and Oswald (1992) examine the effects of lagged company profitability on wages, in a sample of some 600 Canadian union labour contracts from 1978 to 1984. The long run elasticity of wages with respect to profits per employee is estimated to be 0.006, and statistically significant. A 100 percent increase in firm profitability is predicted to increase worker wages by 0.6 percent. This effect is small but non-negligible. A movement from mean to maximum profits per worker is predicted to increase pay by some 14 percent. Denny and Machin (1991) similarly examine the impact of lagged profits upon wages (instrumented by

further lags), using a sample of 436 British manufacturing firms for the period 1976 to 1986. The estimated long run elasticity is very similar to that of Christofides and Oswald (1992) at 0.005, and again statistically well determined.

Hildreth and Oswald (1997), using company and establishment panels for the UK, examine long lags upon profits per employee. Current profitability is also included, and is again instrumented by its past values. The long run elasticity is estimated to lie between 0.02 and 0.04. Blanchflower, Oswald and Sanfey (1996) instead use CPS individual-level data from 1964 to 1985, for full-time workers in US manufacturing, merged with industry profitability data. Lagged profitability and the cost of energy are used to instrument industry profits per employee, and the estimated elasticity is 0.04. There are, however, no controls for unobserved worker heterogeneity. The authors then examine a panel of industries, by taking cell-means of individual characteristics, profitability is entered in lags (but not instrumented) and equations include controls for industry fixed effects. The elasticity of wages with respect to profits per worker is estimated to be 0.08.

These studies of rent-sharing, examining plants, firms and industries, predict a long run effect of profits per worker upon wages ranging between 0.005 and 0.08. Abowd and Lemieux (1993) criticise such estimates as characterising a 'trivial' amount of rent-sharing. Oswald (1996) counters by demonstrating that, given the volatility in the profit data, even relatively small point estimates can generate large variations in pay. A more damaging critique is applied to the choice of lagged profitability as an instrument. Past values are at best ad-hoc instruments, and may be weak predictors of the growth in profitability. Results may then underestimate the true extent of rent-sharing, and Abowd and Lemieux assert exogenous instruments are required. The authors study a panel of some 1100 Canadian union contracts from 1963 to 1985 and adopt industry import and export prices as

instruments, assumed to capture foreign competition shocks upon the domestic product market. These should be correlated with demand shifts but not, directly, with the error term in the wage equation. In the authors' preferred specification the estimated effect of quasi-rents[12] per employee increases more then ten-fold after instrumentation, from 0.018 to 0.195, implying a long run elasticity of wages with respect to quasi-rents of 0.283.

Van Reenen (1996) instead examines technology shocks, in the form of firm and industry innovations, as instruments for firm profitability using a panel of 600 large British manufacturing companies. These instruments should cause positive shocks to firm profits. They may, however, also raise the productivity of employees, and then be an independent shifter of wages. The majority of innovations are though associated with products rather than processes, and tests do not reject instrument validity. The long run elasticity of wages with respect to profits per head is then estimated to be 0.365. Teal (1996) presents similar evidence for a sample of Ghanaian manufacturing firms from the early 1990s. The amount of foreign borrowing per employee and the share of intermediate costs in total output are used to instrument profits, and the rapid decline in Ghana's exchange rate, in the period, is assumed to generate exogenous shifts in profitability. The elasticity of wages with respect to firm profits is then 0.200. Estevao and Tevlin (2000) study a panel of US manufacturing industries from 1958-1994, and instrument profitability by output movements in the sector to which an industry sells. The comparable elasticity is estimated to be 0.143.

Whilst the exogenous approach to instrumentation is preferred, estimates of the magnitude observed appear implausibly large. A four standard deviation increase

[12] Quasi-rents are defined as 'profits' per employee when the wage is set equal to the alternative, rather than the actual, wage.

in firm profitability is predicted to raise wages by between 90 and 120 percent.[13] These estimates may potentially be biased due to the pay measure commonly employed. A product market shock, that increases profits, could induce an expansion in worker hours, satisfied by overtime, increasing the wage bill but not necessarily employment. The authors may then capture increased overtime hours. Also, where workers do capture a share of firm rents employers may seek to upgrade the quality of their workforce. Whilst firm fixed-effects will capture the skill of workers that remains constant over time, skill upgrading may cause the rent-sharing effect to be overstated. These factors indicate the benefits to analysing the firm's effect on pay at the individual level, incorporating the hours of work and personal characteristics.

Black and Strahan (1999) offer an interesting, alternative, approach to estimating whether rents are shared with employees. The authors analyse the impact of the deregulation of the US banking industry upon pay levels. They hypothesise regulation impeded competition and generated product market rents. Deregulation is assumed to act as a negative shock upon bank profitability, and to be exogenous to the labour market. Post-deregulation, banking wages are estimated to fall, on average, by 6 percent. This reduction in wages was observed to be greater in states that had more restrictive legislation *ex-ante*. The evidence suggests these phenomena cannot be explained by demand shocks or changes in the observed skill of banking employees, but rather are consistent with rent-sharing. These results are of particular interest as unions are largely absent from the US banking industry.

[13] These figures are based upon Abowd and Lemieux (1993), Van Reenen (1996) and Teal (1996). The comparable calculation was not possible for Estevao and Tevlin (2000). Estimates of this size are logically possible, if high wage (skill) workers are disproportionately employed in unprofitable firms (see Oswald, 1996).

2.3.3 *Recent Research using Linked Employer-Employee data*

One of the first studies to analyse linked individual-firm panel data is that of Abowd, Kramarz, and Margolis (1999). They examine a large French panel to estimate the unobserved worker and firm fixed-effects on wages, assuming exogenous job mobility and conditional orthogonality between the unobserved worker and firm heterogeneity. Firms that hire high-wage (skill) workers are found to be more productive and capital intensive, but not more profitable. High-wage firms are both more productive and more profitable. This latter evidence is potentially consistent with rent-sharing, but cannot distinguish cause and effect. In contrast, the evidence for the US, in Abowd et al (1998), is that high-wage firms are associated with reduced profitability. Interestingly, the authors find only a weak correlation between the (unobserved) worker and firm fixed-effects. Goux and Maurin (1999) using alternative French data, and a different estimation technique, observe a similar result.

Studies that have used employer-employee data to analyse the rent-sharing model are, as yet, relatively scarce. Troske (1999) studies a cross-section of employees with linked establishment data and examines whether rent-sharing can explain the employer size-wage differential. The firm's market share is found to enter negatively, but statistically insignificantly, industry concentration with a positive and well-determined effect. The author also mentions that, in results not reported, profits per head and value added per head entered positively and statistically significantly.

Hildreth (1998) studies a cross-section of British manufacturing establishments, where detailed information concerning two employees (the most recent hire and a randomly selected employee) is recorded. Profits per head are observed to enter wages positively and statistically significantly, with an estimated

elasticity (at the mean) of 0.02. When firm profitability is instrumented by foreign price shocks this estimate increases to 0.168, when instead profitability is instrumented by product or process innovations the estimate is 0.251. These estimates are comparable to those observed by Abowd and Lemieux (1993) and Van Reenen (1996). Nevertheless, some caution should be attached to the results, as cross-section estimates unobserved person-specific effects may be captured. With respect to innovation, Doms et al (1997) find that plants that introduce new technology employ more skilled workers both pre- and post-adoption. Whilst, Entorf et al (1999) find that workers in 'new technology' jobs were already more highly paid in previous employment spells.

This chapter adds to this, currently, relatively limited literature that studies rent-sharing using employer-employee data.

2.4 Data

The data used in this study come from the British Household Panel Survey (BHPS). The BHPS is nationally representative sample of more than 5,000 British households, containing over 10,000 adult individuals, conducted late each year since 1991 (see Appendix 1 for discussion). These data include detailed information concerning earnings, education, employment characteristics, and demographics. Attention is here restricted to those individuals aged less than 65 and in private sector employment at the survey date, approximately 3,500 respondents in any one year.

In 1991 and 1994 the BHPS included a question, asking respondents:

"What is the exact name of your employer or the trading name if one is used"

Responses to this question produced a list of company names used to merge in firm data, held in the Dun and Bradstreet (DB) database of UK enterprises.[14] This is a privately run database that seeks to sample the universe of 'large' firms, in the UK. All enterprises that have 60 employees or more, a sales turnover in excess of £3.5 million, or total assets in excess of £10 million are contacted (there is the possibility of missing companies if no details are held at Company House, or with DB themselves). Enterprises that do not fulfil these criteria may still be sampled if they volunteer, or if previously sampled. Of the firms contacted, generally only one percent refuses to respond. The database then includes some 50,000 companies, and has the advantage of not being limited to publicly listed companies or the manufacturing sector. For the key firm variables studied, the disclosure rate is approximately 89 percent with respect to firm employment, and 84 percent for company profits.

However, in linking the BHPS employee data to the DB company data, attrition is likely, as a result of non-response and coding error with respect to the employer name, and due to the limited coverage of small enterprises within the DB data. This is examined in more detail below.

2.4.1 Properties of the Linked Employer-Employee Data

Taking as the starting point the employee data, there are 2092 responses to the question regarding company name. Of these 596 could not be located within the DB database, leaving 1496 matched worker-firm pairs. Cleaning the data and conditioning on non-missing observations of the variables of interest left 1113 observations. For this sample there are a maximum of two waves of data for each individual present, here 868 observations are recorded for respondents observed

[14] Data made available by Andrew Hildreth.

twice. The remainder, as single data points, are the result of moves into or out of employment or, more prosaically, that the matching of the data is successful in only one period. Finally, of the 868 observed multiple time-period observations some 774 are observed to work for the same firm at both points in time. To capture unobserved heterogeneity, of both the worker and the firm, analysis is here restricted to this balanced, worker-firm, panel (see section 5). The final sample then includes 387 workers, within 267 firms, observed for two periods.

Controlling for worker and firm fixed effects has strong merits, but here is achieved at a cost; the resulting sample may overstate the returns to firm profitability. By focussing upon individuals working in the same firm in both 1991 and 1994 we are liable to observe large and well-established firms, for whom we witness repeat observations over time, who may be more likely to share rents. Secondly, we may capture workers who benefit most from any sharing of rents. Those workers who are 'outsiders', who are low paid and do not share in firm success, are possibly more likely to quit the firm.[15] (For a further discussion see Appendix 2.)

The properties of the sample, and how it compares to the full BHPS data, are examined in Table 2.1, where summary statistics are presented. As suggested, large employers are more prevalent in the linked data and workers are observed to be more likely to work within large workplaces. Union recognition and performance-related pay are also more prevalent. In part this may reflect the preponderance of large employers. Workers with accompanying firm data are slightly more likely to be found within manufacturing industries, yet differences are relatively small. For occupation, gender and race, the composition of the linked data

[15] The general tenor of result is, nevertheless, similar for the unrestricted sample (1113 observations).

seems comparable to that observed for the BHPS as a whole. Finally, workers with matched firm data are, on average, slightly less educated, but have greater job tenure, potential experience, and higher pay.

2.5 Results

2.5.1 Summary Statistics

Whether there are systematic differences in employee characteristics, according to the firm's profitability, is now investigated for the employer-employee (BHPS-DB) data in Table 2.2. Column one presents summary statistics for workers in firms with below median profitability, column two firms with above median profitability.

More profitable firms are observed to be both larger (greater firm employment) and more capital intensive (a higher capital to labour ratio). They are also associated with slightly smaller workplaces. Mean profits per worker for firms with below median profitability are here, for the years 1991 and 1994, negative. This may reflect the difficult trading conditions within the UK in the early 1990s. More profitable companies are observed to pay higher wages, but also to hire more educated workers. There is thus some evidence of more able workers working within more profitable firms.

Whilst workers within more profitable firms are, on average, more educated their levels of potential experience (age - years of schooling - six) and job tenure are lower. With respect to gender and race, the proportions of male and ethnic minority employees are approximately equal in the high and low profitability samples. More profitable firms are also observed to be more unionised – where there are more rents to bargain over the gains to unionisation are likely to be greater. Finally, more profitable firms are more likely to run performance-related compensation schemes.

2.5.2 Estimation Strategy

To investigate issues in more detail we turn to regression analysis, and estimate an extended version of equation (7). Wages are modeled as a function of personal characteristics (such as education, experience, gender and race) and employer characteristics (e.g. firm profitability, establishment size, firm size, industry, etc). Hourly pay[16] for individual i in time period[17] t and employer j, is expressed in log-linear form as:

$$w_{itj} = x_{itj}'\beta + z_{tj}'\gamma + \varepsilon_{itj} \qquad i = 1, \ldots, n \qquad (8)$$

$$t = 1, \ldots, T$$

$$j = j(i, t) = 1, \ldots, m.$$

Where, w is log hourly pay, x the vector of worker characteristics, z the vector of employer characteristics, ε the conformable error term with mean zero and constant variance, and β and γ the vectors of parameters to be estimated.

More precisely, quadratics in potential experience and job tenure are included to capture the effects of general labour market experience and firm specific skills, respectively. Years of schooling and the highest academic qualification capture the returns to general human-capital investment, and potentially signalling. Gender, race, and occupation are entered as variables that potentially determine both the own wage within the workplace, and the wage an individual can earn outside their current establishment. Industry and region are added as variables that determine the alternative wage.[18] Together these variables are also thought to capture any

[16] Hourly pay is here defined as gross weekly pay divided by overtime adjusted hours:

$$w_{it} = \ln\{PAY/[HS+1.5*HOT]\}.$$

where PAY is usual gross pay (deflated to 1991 values), HS is standard hours, and HOT overtime hours. Alternative overtime corrections yielded similar results.

[17] Where, t=1991, 1994 for the linked BHPS-DB data.

[18] Industry controls are entered at the one-digit level. Whilst more disaggregation would be preferred, with only 387 individuals (observed twice) such an approach is not, here, feasible.

autoregression in earnings as, given the limited time span of these data, explicit dynamics in wages are not here possible.

Employer effects upon wages are captured by firm profitability (profits per employee), firm size (log firm employment), the firm's capital use (log capital to labour ratio), and establishment size. Firm profits are obtained from published company accounts in the interview year.[19] In 54 percent of cases the company accounts date pre-dates the individual survey date, with the remainder company reports published at the end of the year. All monetary variables are deflated to 1991 prices.

Models are also estimated which account for the potential unobserved heterogeneity of employees and employers, by including person-specific (f_i) and firm-specific (g_j) effects upon wages that are assumed constant over time. No assumptions are made regarding the distribution of these effects and we allow for potential correlation with regressors.

$$w_{itj} = x_{itj}'\beta + f_i + g_j + \varepsilon_{it} \qquad (9)$$

(For ease of analysis we subsume employer characteristics into x.) Implicitly this assumes a common market return to unobserved worker skill, f_i, and that the difference in wages of any two workers in the same firm is solely attributable to individual characteristics.

In the presence of both unobserved person and firm heterogeneity, estimation is slightly more complicated than is standard (see Abowd and Kramarz, 1999). Analysing within-person variation implies the (unobserved) firm effect remains for those individuals who move between firms. Examining within-firm

[19] Fisher and McGowan (1983) criticise the use of accounting profits as being a misleading measure of economic rates of return. Mayer (1988), on the other hand, suggests examination of accounts data can provide information upon economic profits. The latter view is here favoured, and we assume accounting profits are a, potentially noisy, signal of economic returns. Controlling for firm fixed effects, and examining changes over time, may also solve some of these problems.

effects consigns individual effects to the error term (as deviations from the firm mean person-effect). A natural solution is to estimate the wage equation including an indicator variable for each worker and firm. To identify firm effects (g_j) requires multiple individuals to be observed within any one firm. Table 2.3 reveals this is not here the case. Some 82 percent of firms are associated with a single employee. These single worker-firm pairs also account for 56 percent of employees.

To separately identify unobserved worker and firm heterogeneity is then not, here, possible. Instead we form person-firm groups (h_{ij}), that is a unique indicator for each individual and each employer they are observed working for.

$$w_{itj} = x_{itj}'\beta + h_{ij} + \varepsilon_{it} \tag{10}$$

The model is estimated by taking first differences (subtracting lagged values) within the worker-firm group[20]:

$$(w_{itj} - w_{it-1j}) = (x_{itj} - x_{it-1j})'\beta + (\varepsilon_{itj} - \varepsilon_{it-1j}) \tag{11}$$

$$\Delta w_{itj} = \Delta x_{itj}'\beta + \Delta\varepsilon_{itj} \tag{12}$$

This removes all individual and firm heterogeneity, both observed and unobserved, that remains constant over time and inference is driven by time-varying characteristics.

2.5.3 Further Econometric Issues

The estimation of wage equations, such as (8) or (12), where firm profitability is an explanatory variable, faces further potential pitfalls. First, from (7), the rent-sharing parameter may be expected to vary across firms. Second, there may be measurement error in using accounting, rather than economic, profits. Third, there may be

[20] The sample is then conditional upon observing the worker for at least two consecutive periods (1991 and 1994) in the same firm.

endogeneity bias. Firm profitability will be decreasing in worker wages, other things being equal. It is then desirable to instrument firm profitability.

Two approaches are here pursued. The first examines the effect of past profitability, in this case one and two-year lags, upon pay. The linked sample includes only two waves of firm data, whilst the BHPS has followed the same individuals throughout the 1990s. The estimation strategy is then to examine how profitability (in 1991 and 1994) affects worker wages in subsequent years, for those workers who do not change employer.[21] Past values of firm profitability can be thought of as pre-determined variables that are correlated with current profitability, but not directly related to the current wage. They should then be robust to potential reverse causality. Nevertheless, they cannot be thought of as an exogenous shock to the firm's revenue, and in models with no controls for unobserved heterogeneity may be correlated with person or firm fixed-effects.

The second approach is to instrument UK firm profitability by US industry profits and price levels. These variables are assumed to capture exogenous variation in profits, via international product market shocks, to demand or competition. For these to be suitable instruments, they should be independent of the error term in the wage equation. It is not clear *a priori* why US profits or prices should be, directly, correlated with wages within the UK, especially once we control for unobserved worker heterogeneity. Implicitly this assumes the behaviour of UK wages and firms does not significantly impact upon US prices and output. Movements in US prices and profits are then viewed as external shocks to UK industry, independent of worker and firm behaviour. Estimation is by Two Stage Least Squares (2SLS) and tests are presented for instrument validity.

[21] This restriction may upward bias the rent-sharing coefficient due to omitting low pay 'outsiders', who may be more likely to quit.

2.5.4 *Profits per employee, Worker skill and Worker wages*

The preceding discussion suggested the importance of controlling for a potential correlation between worker skill and firm profitability. It then seems natural to examine what impact observed worker skill has upon the estimated rent-sharing parameter. Column one of Table 2.4 presents OLS estimates where the wage is regressed upon employer variables, column two adds job and person characteristics, whilst column three adds education variables. Parameter estimates generally match standard earnings equation predictions and attention is here focussed upon the effect of employer variables.

In each case profits per head enters positively and statistically significantly, despite the potential downward bias due to the endogeneity of wages and profits. Moving from column one to column two of Table 2.4, the estimated rent-sharing coefficient increases from 0.042 to 0.055. Moving from column two to three the parameter then falls to 0.045. The rent-sharing parameter is then relatively stable as controls are introduced. Results suggest more profitable firms employ workers with lower wage earning characteristics with respect to occupation, tenure and experience. Yet, conditional upon these characteristics, more profitable firms hire more able (educated) employees. This matches the intuition displayed in the sample means. Given observed worker skill is positively associated with firm profitability, it then may also be the case for unobserved worker heterogeneity.

With respect to other firm characteristics, the coefficient upon the log capital to labour ratio falls from 0.075 to 0.049 when moving from column one to column three, though it remains statistically significantly different from zero. More capital intensive firms, here, employ more skilled labour. With respect to log firm employment, coefficient estimates, whilst generally positive, are not well

determined. In contrast, establishment size enters positively and (jointly) statistically significantly in all cases.

The estimates, in column three of Table 2.4, imply an elasticity of wages with respect to profits per employee (evaluated at the mean) of 0.02.[22] Moving from a firm with profitability one standard deviation below the mean to a firm with profitability one standard deviation above the mean is predicted to increase wages by 8.6 percent. This is broadly in line with the previous studies that examined lagged profitability, and with the figure Hildreth (1998) observed when profitability was entered directly in the wage equation. However, such a result is also potentially consistent with performance pay, frictions upon labour supply or unobserved worker quality differences. These hypotheses are analysed in turn.

2.5.5 Profits per employee and Performance Related Pay

The BHPS contains the following question regarding performance-related pay:

"Does your pay ever include performance bonuses or profit-related pay? Note: Includes any extra payments including performance bonuses and sales commissions, but not overtime."

The question then includes both profit sharing and other forms of incentive pay. A disadvantage of the question is that between 1992 and 1994 individuals were only asked to respond if they had changed employers. We may then underestimate the growth, within firms, of such schemes. Still, it would seem to broadly capture the incidence of performance pay.

Column four of Table 2.4 examines whether the observed relationship between wages and profitability, in fact, captures a pay-performance correlation, by adding the incentive pay variable to the wage equation. The performance-related pay

[22] The elasticity is calculated by multiplying the parameter by the mean value of profits per worker.

(PRP) indicator enters positively and statistically significantly, with an estimated wage premium of 6.2 percent. This is likely to capture greater effort, induced by performance pay, and worker quality effects, as more able employees are attracted to such schemes. Booth and Frank (1999) observe comparable estimates for the BHPS in this period, of 5.6 percent for females and 9.3 percent for males, though they include public sector employees. Nevertheless, the estimated coefficient upon firm profitability is largely unaffected by the introduction of the PRP variable.

Alternative evidence is presented in Table 2.5, where wage equations are estimated for the sample of employees who are paid PRP (column one) and those who are not (column two). Whilst the coefficient upon profits per employee is larger in the sample paid PRP, the effect remains positive and statistically robust for those who are not and the difference in parameter estimates is not statistically significant at conventional levels. Whilst the measure of performance pay may be imperfect, the evidence suggests the wage-rents correlation cannot be explained by incentive pay schemes.

The discussion of incentive pay models in section two suggested a weaker link between pay and performance in large organisations, where the measurement of effort is more difficult and free-rider problems, associated with group bonuses, more severe. Small firms may, however, find it easier to monitor worker effort and hence be less likely to pay PRP. These ideas are examined in columns three to six of Table 2.5. The returns to PRP are positive, similar, and well determined in small and large firms. In contrast, the coefficient upon PRP is close to zero and insignificant amongst establishments with 200 or more employees, but positive and well determined in small workplaces. The problems of measuring performance and free-rider behaviour may then limit PRP to small workplaces. However, some care must be taken with interpretation due to the relatively small number of

observations. The effect of profits per head upon pay seems more pronounced in large firms, where the problems of group incentives should be greatest. This last finding runs counter to the evidence of Rayton (1996) but matches that of Nickell, Vainiomaki and Wadhwani (1994).

Together these results suggest performance pay does not provide a satisfactory explanation of the rent-wage correlation.

2.5.6 Is rent-sharing limited to the union sector?

Given the model of unionised bargaining outlined, in section two, rent-sharing may be expected to be limited to the union sample. This is investigated in columns one and two of Table 2.6a, where wage equations are estimated for unionised and non-unionised workplaces respectively. Firm profitability enters positively in both cases, with parameter estimates of 0.038 and 0.026. This difference is relatively small and is not statistically significant at the 5 percent level. One issue with these estimates is that union plants are associated with higher mean levels of profits per employee (0.54 compared to 0.25 for non-union plants). This should not be surprising, unions can only extract a wage mark-up where rents exist. Yet if the degree of rent-sharing is positively related to the rents to be shared estimates may in part reflect the different levels of profitability.

This is examined in column three, where the wage equation is estimated for the entire sample and union recognition interacted with profits per employee. (This constrains the remaining coefficients to be the same for both union and non-union workplaces.) The interaction term is estimated to be negative, suggesting that, for the same level of profits per employee, rent-sharing is *greater* amongst non-union employers. The effect is not, however, statistically significantly different from zero.

The evidence then suggests rent-sharing is, here, not limited to the unionised sector. Hildreth and Oswald (1997) document similar evidence, for company panel data.

2.5.7 *Is the rent-wage correlation a result of short-run competitive frictions?*

Were the rent-wage correlation to be explained by a competitive model with short-run frictions upon labour supply, one may expect the correlation between profits and wages to be strongest within 'thin' labour markets. This proposition is analysed in Table 2.6b, and wage equations estimated for samples of workers by occupation. Column one examines professional occupations (managerial, professional, associated professional and technical occupations), column two all remaining occupations and column three manual workers (personal and protective, machine operatives and other manual occupations). In all cases the estimated rent-sharing parameter is positive and statistically well determined. Within professional occupations the coefficient upon profits per worker is estimated to be 0.066. This is over twice as large as that estimated for all other occupations, at 0.031. However, when we examine manual occupations, where jobs are relatively unskilled and labour shortages unlikely, we observe a rent-sharing parameter of 0.069. This would seem to run counter to the competitive friction hypothesis. Moreover, in the sample period (1991 and 1994) the labour market was relatively depressed and hiring problems, particularly for unskilled labour, should have been less important.

2.5.8 *The effect of lagged profitability*

The evidence suggests the observed correlation between profitability and pay cannot, here, be explained by performance pay or by a competitive model with slow adjustment. However, the rent-sharing parameter may still be downward biased. An increase in wages will reduce profits, other things being equal. Table 2.7 examines

the effect of past profitability upon pay. Lagged profits per employee are pre-determined variables that are correlated with current profitability but not, directly, related to current pay. Estimates should then be robust to potential reverse causality. The employer data includes just two waves of firm information, whilst the BHPS has followed the same individuals throughout the 1990s. To maintain the signal in the model we examine the impact of profitability, in 1991 and 1994, upon wages in subsequent years, for those workers who do not change employer.[23]

Column one of Table 2.7 restates the basic estimates for the years 1991 and 1994. Column two estimates the wage equation for those individuals observed to work for the same employer in the subsequent year.[24] The coefficient upon profits per employee is estimated to be similar, at 0.045 and 0.044 respectively, and statistically significantly different from zero in both cases. Column three then examines the effect of firm profits in 1991 and 1994 upon wages in 1992 and 1995. The estimated coefficient upon profits per employee, lagged once, is then slightly smaller, at 0.031, and on the border of statistical significance. For those workers who do not change employers within two years, the estimated, current, rent-sharing coefficient is 0.038. When we instead examine the two-year lag of profits per worker, and wages in 1993 and 1996, the parameter estimate is 0.030. In both cases, coefficients are statistically robust.

The effect of lagged profitability upon wages is then positive and statistically well determined. This runs counter to the suggestion that the rent-wage correlation reflects a form of labour demand curve – under certain functional forms it is relatively easy to generate a positive profit-pay correlation – as the lags on profits per employee make it difficult to believe causality runs from pay to profitability. The

[23] Defined as individuals who remain in the same employer and do not change job, or who change jobs but remain within the same organisation.
[24] This sample is unbalanced.

labour demand interpretation also does not square with the fact that total firm profits were found to enter wages positively and statistically significantly. In contrast, along a demand curve, wages and total profits are negatively related. Whilst results may be explained by unobserved worker skill, if more profitable firms employ more able workers, they do not seem to fit the labour demand interpretation well.

2.5.9 Firm profitability and US Product Market Influences

Whilst lagged profits are likely to be pre-determined they are unlikely to be exogenous. What is required is a variable that captures exogenous shocks to firm profitability, and that is independent of the error term in the wage equation. Such an instrument will allow consistent estimation of the rent-sharing parameter in the face of measurement error and endogeneity. If the instruments are independent of unobserved, worker and firm, heterogeneity in pay, they are sufficient to identify the causal impact of firm profitability.

Here we instrument UK firm profitability by US industry profits and prices.[25] These are thought of as capturing exogenous variation in UK profits, via international product market shocks. An increase in US profitability, or prices, may reflect a preference shift or technology or cost shock that is, potentially, shared across nations. In this sense the US is regarded as assuming a leadership role in the global economy. That the UK is a relatively open economy suggests foreign product market shocks may be suitable instruments for domestic market conditions.

For the US product market variables to be suitable instruments, they should be independent of the error term in the UK wage equation. Implicitly this assumes

US output and prices are not related to UK behaviour. The size of the US economy, relative to the UK, and its much larger domestic markets may provide some support for this hypothesis. Whilst it is unclear as to why US profitability should, directly, affect wages in the UK, there are two potential avenues for an indirect link. Firstly, there may be common types of technologies, within industries in both the UK and US, that produces a pay-profitability correlation in the absence of rent-sharing. Secondly, Budd and Slaughter (2000), analysing over 1000 union wage contracts from 1980 to 1992, have found that Canadian wages are a positive function of US industry profits for US owned companies. US firms then 'import' US wages, and rent-sharing, into their Canadian enterprises. This may reflect the close geographic proximity and economic integration of Canada and the US, and need not follow for the UK. Yet if such a transmission mechanism is present in the UK, wages may be correlated with US profitability. These issues are examined below. In all estimates that follow we also include a control for the industry wage to capture the common industry impact of any shock.[26]

Table 2.8 compares selected sample properties for the linked data and the sub-sample with matched US industry data. In general, sample means are closely aligned. Regression evidence is presented in column one of Table 2.9a. The estimated rent-sharing parameter, for the sample with US data, is, at 0.045, identical to that observed previously. Moreover, coefficients upon the other explanatory variables are very similar. Column two of Table 2.9a adds a control for the industry

[25] The DB data includes US defined SIC codes, from which US industry data (obtained from the Bureau of Economic Analysis: http://www.bea.doc.gov) were merged in. There are some 60 (US) industry groups of which 51 are observed in the BHPS-DB data.

[26] These data are NES industry averages merged using UK industry codes. They are, hence, coded differently to the US industry data. To check whether this biased results the UK average wage was replaced by average compensation in the US industry. Results were essentially unchanged.

wage. Results are essentially unchanged.[27] Column three analyses the direct impact of US profitability upon UK worker pay, and enters current, one-year, and two-year lags of US Industry profits per worker in the wage equation. These terms enter jointly, and individually, statistically insignificantly. Omitting UK firm profits per employee (in column four), or instead entering the US profitability terms individually, did not alter this conclusion. There is hence little statistical evidence that US profitability is correlated with the error term in the wage equation. The possibility cannot though be entirely discounted.

Table 2.9b estimates wage equations by Two Stage Least Squares (2SLS), with the US profitability measures as instruments (the upper panel reports coefficient estimates from the equation of interest, the lower panel the instrumental variable equation).[28] In all cases the instruments are jointly statistically significant predictors of firm profitability. This corresponds to the rank condition for identification. Bound, Jaeger and Baker (1995) show that 2SLS is biased towards OLS estimates when instruments are only weak predictors of the endogenous variable. The additional (adjusted) R^2, that the instruments add to explaining profits per employee, is then also reported. US industry profits explain approximately 5 percent of the variation in UK profitability. The instruments then appear to be significant predictors of firm profitability. The Sargan test of overidentifying restrictions is also presented. Under the null, instruments are exogenous and hence uncorrelated with the residuals from the wage equation, and the test statistic is calculated as the correlation of the residuals with the instrument matrix. In all three cases, where equations are overidentified, we fail to reject the null of instrument validity.

[27] Previous conclusions are robust to the inclusion of the industry wage.
[28] We later include US prices in the instrument set. Yet as price indices they have no substantive meaning in cross-section analysis.

Table 2.9b finds UK firm profitability to be increasing in the level of US industry profits. The long run effect of US profits per employee, upon UK profits per worker, is approximately 0.260 in all four cases. UK profitability is also more closely related to lagged, rather than current, US profits. This would fit with the view of the US fulfilling a leadership role in the global economy, with a lagged transmission to the UK. The instruments may, however, capture demand or supply shocks. A positive correlation between UK and US profits could reflect common shifts in consumer preferences, or changes in production techniques adopted internationally. In either case, the instruments are valid so long as they are not correlated with the error in the wage equation. On this point the statistical evidence is supportive.

The 2SLS estimates of the rent-sharing parameter are then observed to lie in the range 0.070 to 0.101, between one and a half and twice as large as the OLS estimates. This implies an elasticity of the wage with respect to profit per employee (at the mean) of between 0.03 and 0.04. However, the rent-sharing coefficients are not statistically robust (the t-statistics lie between 1.1 and 1.2). Whilst the increased magnitude of the point estimates suggests a more pronounced rent-sharing effect, the statistical insignificance of the estimates may lead one to temper this conclusion. Doubts may also remain that we do not capture unobserved worker-firm heterogeneity. The diagnostic tests, regarding instrument validity, nevertheless appear satisfactory and if US profitability is exogenous parameter estimates are consistent.

2.5.10 Alternative Measures of Pay and Firm Prosperity

As is common in many studies of wage determination we have, to this point, estimated log wage equations. The model of bargaining outlined however suggests a

link between the *level* of pay and profitability. Table 2.11a then estimates wage equations upon the level of hourly pay. Column one presents the base specification, whilst column two adds a control for industry wages. In both cases the rent-sharing parameter is positive and statistically well determined. Column three estimates the model for the sample where US data are available. The rent-sharing coefficient is of a similar magnitude but statistically robust only at the 10 percent significance level. Instrumental variable estimates are presented in columns four and five (comparable to columns three and four of Table 2.9b). Table 2.10 summarises the estimated elasticity of wages with respect to profits per employee, and the predicted wage gain from moving from one standard deviation below mean profitability to one standard deviation above. Results are very similar to those observed for the log wage equations.

Table 2.11b estimates log wage equations with an alternative measure of the employer's 'ability to pay', the log sales per employee ratio. As with profits per worker the log sales per employee ratio enters positively and statistically significantly in OLS regressions, with an estimated elasticity of 0.077. (This elasticity is not directly comparable to that with respect to firm profitability.) Nevertheless, the predicted wage gain from a two standard deviation increase in firm sales is broadly comparable to that observed for firm profitability, and results substantially similar for both measures of firm wealth.

2.5.11 Worker-Firm Fixed Effects

Whilst the results above suggest firms share rents with their employees, estimates may be contaminated by unobserved worker and firm fixed effects. More profitable firms may employ more skilled employees. If so, the estimated rent-sharing parameter will be biased upwards. That we observe more educated employees to

be, here, employed in more profitable firms offers some support for this hypothesis. Yet workers in such firms are also, on average, less experienced and have lower levels of job tenure. The direction of any bias is not then clear.

Table 2.12 estimates wage equations with controls for unobserved worker and firm heterogeneity. Column one estimates the wage equation, transformed into within person-firm first differences (three-year changes), by OLS. The estimated rent-sharing parameter is now 0.013, compared to 0.045 in the cross-section, and no longer statistically robust.[29] The elasticity, at the mean, is estimated to be 0.006, and moving from one standard deviation below mean profitability to one standard deviation above is predicted to increase worker wages by approximately 2.2 percent. These effects are quite small and seem to suggest previous estimates were, to a large degree, capturing unobserved worker-firm differences.

The coefficient upon profitability is, nevertheless, likely to be downward biased, due to the endogeneity of profits and pay and any measurement error in accounting profits. Columns two to five then instrument the change in firm profitability, between 1991 and 1994, by current and lagged changes in US profits and prices (using the same three year gap). In all four cases the instruments are (jointly) statistically significant predictors of the change in firm profitability. For columns two to four we cannot reject the test of over-identifying restrictions at conventional levels, but for column five the null of exogeneity can be rejected at the 5 percent level. The long run effect of the change in US industry profits, upon the change in UK firm profitability, is estimated to lie between 0.160 and 0.285. As was the case previously, UK profits per worker are more closely correlated with lagged, rather than current, US profits. Faster growth in US prices, one year previously,

[29] Adopting a more parsimonious approach, and dropping variables statistically insignificant in the first difference regression, left results essentially unchanged.

predicts an increase in UK profits, but the current and two-year lagged growth in US prices predict falling UK profitability. The resulting (2SLS) parameter estimates upon profits per employee are, however, incorrectly signed, but statistically insignificantly different from zero.

One concern with estimates is that the instruments rely on within-industry time variance for their power, and hence may be weak predictors of firm profitability. The evidence of Table 2.12, however, suggests the instruments capture significant variation in firm prosperity. Movements in UK profitability, in response to US industrial prosperity, may, nevertheless, reflect demand or supply shocks. For example, growth in UK profits may reflect common international demand shifts, or, changes in working practices that are diffused globally over time. In both cases US profitability is a valid instrument if it is not directly related to UK pay. Statistical tests are generally supportive of this assumption.

There are, nevertheless, two potential weaknesses of the approach followed here. First, the use of accounting profits, in conjunction with industry instruments, may introduce noise into estimates. Rents are defined as profits to the firm in excess of the market return. A common industry shock can then raise accounting profits for all firms in an industry, but leave the economic rate of return largely unaltered. We may then overestimate any increase in rents, and subsequently underestimate the rent-sharing parameter. Firm-specific shocks to profitability would be preferred but were, here, not available.[30] Secondly, rent-sharing may occur through annual bonuses[31] and non-pecuniary benefits, or pay may adjust sluggishly to changes in market conditions. In both cases we may underestimate the gains associated with rent-sharing by examining current pay.

[30] Ideally we would also analyse the effect of firm profitability at a relatively disaggregated level.
[31] These are thought of as separate to the performance bonuses discussed earlier.

Where firms face a positive product market shock one may, potentially, expect an increased intensity of work. Table 2.13 examines the effect of firm profitability per worker upon weekly, rather than hourly, pay. This approach also avoids the likely measurement error in reported hours of work adding noise to the regression. The rent-sharing parameter is estimated to be 0.057 in the cross-section and 0.026 in first differences, and is in both cases statistically significantly different from zero, despite potential endogeneity bias. These effects correspond to an estimated elasticity of pay with respect to profits per employee of 0.025 and 0.011. The increase in pay associated with a two standard deviation increase in firm profitability is 9.8 and 4.0 percent, respectively. The fixed-effects estimates then suggest a moderate, but non-negligible, degree of rent-sharing upon pay.

For hourly pay the rent-sharing parameter is estimated to be 0.045 in the cross-section and 0.013 in first differences. Only the former is statistically well determined. Together with the results upon weekly pay this suggests profitability and the hours of work may, here, be positively correlated. We may then overstate the degree of rent-sharing in the labour market where controls for the hours of work are absent. Examining the instrumental variable estimates with respect to weekly pay, the rent-sharing parameters are larger, but again are insignificantly different from zero. The larger point estimates, compared to hourly pay, suggest the instruments capture, in part, the effect of increased hours of work. If pay adjusts slowly to shocks to profitability, and changing hours, the instruments may then predict falls in hourly pay.[32] This may help to explain the negative coefficients observed in Table 2.12. Table 2.14 then repeats the analysis in Table 2.12 for profitability lagged one period, where pay adjustments are likely to have occurred.

[32] The change in US profitability was found to positively predict the change in hours, the effect was not, however, well determined.

OLS estimates of the rent-sharing parameter are here negative, 2SLS estimates positive, neither are though statistically different from zero. As with Table 2.9b instrumentation increases the point estimates but results are not statistically robust.

In summary, the estimates, which include worker and firm fixed effects, offer support for a moderate degree of rent-sharing with respect to weekly pay. Yet, there is no statistically robust evidence for rent-sharing upon hourly earnings.

2.6 Conclusions

This chapter has used new UK employer-employee data to examine the effect of firm profitability upon pay. OLS estimates of the elasticity of wages with respect to (current) firm profits per employee are approximately equal to 0.02, and moving one standard deviation below the mean of firm profitability to one standard deviation above is predicted to increase wages by 8 percent.

The effect of formal incentive pay schemes upon the pay-profitability correlation are extensively examined, and whilst performance pay enters wages positively and statistically significantly the rent-sharing parameter is largely unperturbed. Moreover, the coefficient upon firm profitability is positive and well determined for the sample of workers with no formal performance-pay. Were a competitive model with slow adjustment to explain the rents-wage correlation the effect of firm profitability upon wages should be largely observed within 'thin' labour markets. Yet, the coefficient upon profits per employee remains positive and well determined for low-skill employees where hiring difficulties should be least likely. Competitive frictions and performance pay do not then seem to offer a sufficient explanation for the rents-wage correlation. Evidence is also presented that rent-sharing is not limited to the unionised sector.

The chapter adopts the novel approach of using cross-country comparisons to instrument firm profitability. International shocks to product demand are here captured by the correlation of UK firm profitability with US industry profitability. Instrumental variable estimates of the rent-sharing parameter then increase by between 50 and 100 percent, relative to OLS. Coefficients are not, however, statistically well determined.

When controls for worker and firm fixed effects are added, OLS estimates of the rent-sharing parameter are attenuated from 0.045 to 0.013 and are no longer robust. In comparison when weekly, rather than hourly, pay is the dependent variable OLS estimates are, respectively, 0.057 and 0.026, and in both cases are statistically significantly different from zero. When UK profits are instrumented by US profitability the resulting coefficient estimates are, both for weekly and hourly pay, incorrectly signed or statistically insignificantly different from zero. Examining lagged profitability, and allowing for a longer adjustment process on pay, increases the estimated rent-sharing coefficients, but they remain statistically insignificant.

After controlling for observed worker skill and unobserved worker and firm fixed effects, we then find some moderate evidence for rent-sharing upon weekly earnings. Yet, these estimates may capture a possible positive correlation between the hours of work and firm profitability. When instead we examine hourly wages, and thus control for the intensity of work, we can find no robust positive rent-sharing effect, within a sample that is, if anything, likely to overestimate the effect of firm profitability.

TABLE 2.1
Sample Means (1991 & 1994)
A comparison of the Linked data and the BHPS sample

Variable	*BHPS*	*BHPS-DB*
Hourly Pay	5.72 (3.58)	6.24 (3.19)
Potential Experience	19.68 (11.72)	22.59 (11.32)
Job Tenure	5.02 (5.72)	6.24 (5.77)
Years of Schooling	11.23 (2.68)	10.93 (2.50)
Degree	0.09 (0.28)	0.07 (0.25)
Ethnic minority	0.03 (0.16)	0.03 (0.16)
Male	0.56 (0.50)	0.58 (0.49)
Union recognised plant	0.39 (0.49)	0.63 (0.48)
Performance related pay	0.38 (0.49)	0.47 (0.50)
Workplace Size 25 plus	0.67 (0.47)	0.85 (0.36)
Profits per employee		0.43 (0.95)
Capital-labour ratio		3.08 (7.21)
Firm employment		29.65 (48.72)
Industry		
Agriculture	0.02 (0.13)	0.00 (0.05)
Energy	0.03 (0.17)	0.08 (0.27)
Extraction	0.05 (0.23)	0.08 (0.27)
Metal Goods	0.16 (0.37)	0.16 (0.37)
Other Manufacturing	0.15 (0.35)	0.14 (0.34)
Construction	0.03 (0.18)	0.03 (0.16)
Distribution	0.25 (0.43)	0.26 (0.44)
Transport	0.06 (0.23)	0.09 (0.29)
Banking	0.19 (0.39)	0.13 (0.34)
Other Services	0.06 (0.24)	0.03 (0.17)
Occupation		
Managers	0.15 (0.35)	0.14 (0.34)
Professional	0.06 (0.24)	0.07 (0.25)
Associated Professional	0.08 (0.28)	0.07 (0.26)
Clerical	0.23 (0.42)	0.28 (0.45)
Craft & related	0.15 (0.36)	0.14 (0.34)
Personal & Protective	0.05 (0.22)	0.02 (0.15)
Sales	0.09 (0.29)	0.07 (0.26)
Plant Operative	0.14 (0.35)	0.16 (0.37)
Other	0.05 (0.21)	0.05 (0.21)
Number of Firms		267
Number of Individuals	1751	387
Number of Observations	3502	774

- Standard deviations are in parentheses. Pay is deflated to 1991 values.
- Profits per employee and the capital-labour ratio are measured in £0,000's per worker. Firm employment is measured in 000's of employees.
- The BHPS sample is the sample of all private sector employees in the BHPS in 1991 and 1994.
- The BHPS-DB sample is the sample of all private sector employees in the BHPS in 1991 and 1994 with matched Company accounts data.

TABLE 2.2
BHPS-DB Sample Means (1991 & 1994)
Above and below median Profits per employee

Variable	*≤ Median Profits per employee*	*> Median Profits per employee*
Profits per employee	-0.09 (0.65)	0.96 (0.93)
Capital-labour ratio	1.04 (1.51)	5.13 (9.67)
Firm employment	17.15 (32.18)	42.15 (58.34)
Hourly Pay	5.69 (2.75)	6.80 (3.49)
Potential Experience	22.87 (11.98)	22.31 (10.63)
Job Tenure	5.86 (5.51)	6.63 (6.00)
Years of Schooling	10.79 (2.45)	11.07 (2.54)
Degree	0.05 (0.23)	0.09 (0.28)
Ethnic minority	0.02 (0.15)	0.03 (0.17)
Male	0.58 (0.49)	0.58 (0.49)
Union recognised plant	0.54 (0.50)	0.72 (0.45)
Performance related pay	0.43 (0.50)	0.51 (0.50)
Workplace Size 25 plus	0.87 (0.34)	0.83 (0.38)
Number of Firms	194	156
Number of Individuals	257	257
Number of Observations	387	387

- Standard deviations are in parentheses. Pay is deflated to 1991 values.
- Profits per employee and the capital-labour ratio are measured in £0,000's per worker. Firm employment is measured in 000's of employees.
- The BHPS-DB sample is the sample of all private sector employees in the BHPS in 1991 and 1994 with matched data.

TABLE 2.3
Number of Worker-Firm Groups in the Linked (BHPS-DB) Data

Workers per Firm	*Number of Individuals*	*Percent*	*Number of Firms*	*Percent*
1	220	56.9	220	82.4
2	44	11.4	22	8.2
3	36	9.3	12	4.5
4	20	5.2	5	1.9
5	15	3.9	3	1.1
7	7	1.8	1	0.4
9	18	4.7	2	0.8
12	12	3.1	1	0.4
15	15	3.9	1	0.4
Total	387	100	267	100.0

- To calculate the total number of observations associated with the number of workers per firm, multiply the number of individuals by 2.

TABLE 2.4
The Effect of Firm Profitability upon Pay (BHPS-DB)
The Impact of Controlling for Worker Skill
Dependent Variable: Ln(wage)

Regressor	*ALL*	*ALL*	*ALL*	*ALL*
Profits per employee	0.042	0.055	0.045	0.045
	(0.015)	(0.012)	(0.014)	(0.014)
Ln(capital-labour ratio)	0.075	0.052	0.049	0.048
	(0.021)	(0.015)	(0.015)	(0.014)
Ln(firm employment)	-0.002	0.007	0.006	0.006
	(0.010)	(0.008)	(0.008)	(0.008)
Performance related Pay				0.062
				(0.029)
Workplace Size:10-49	0.162	0.102	0.092	0.091
	(0.077)	(0.061)	(0.061)	(0.061)
Workplace Size:50-199	0.120	0.077	0.065	0.061
	(0.075)	(0.060)	(0.061)	(0.062)
Workplace Size:200-999	0.128	0.091	0.062	0.063
	(0.081)	(0.064)	(0.064)	(0.064)
Workplace Size:1000+	0.356	0.213	0.188	0.189
	(0.088)	(0.070)	(0.069)	(0.069)
Years of schooling			0.014	0.015
			(0.008)	(0.008)
O-levels			0.045	0.042
			(0.039)	(0.039)
A-levels			0.080	0.076
			(0.048)	(0.048)
HND, HNC			0.207	0.200
			(0.073)	(0.072)
Degree			0.278	0.257
			(0.085)	(0.086)
Higher Degree			0.564	0.541
			(0.173)	(0.167)
Union recognised workplace	-0.047	-0.012	0.002	-0.004
	(0.046)	(0.038)	(0.037)	(0.037)
Individual characteristics	No	Yes	Yes	Yes
Observations				
Number of Firms	267	267	267	267
Number of Individuals	387	387	387	387
Number of Observations	774	774	774	774
Adjusted R^2	0.28	0.56	0.59	0.60

1. All equations are estimated by OLS. Standard errors are in parentheses and are robust to arbitrary heteroscedasticity and the repeat sampling of individuals within firms.
2. All equations include controls for industry (SIC code at the one-digit level), region, and time period. Individual characteristics include quadratics in potential experience and job tenure, and controls for occupation (SOC code at the one-digit level), temporary job, gender, race, and marital status. Parameter estimates are not reported.
3. The workplace size coefficients are with respect to the omitted category, 1-2 employees.
4. Capital and profits are in £10,000s (1991 values). Mean value profits per head is 0.434, standard deviation 0.954.
5. Column three above corresponds to column three in Table A2.1 in the appendix.

TABLE 2.5
The Effect of Firm Profitability upon Pay (BHPS-DB)
Is it Performance-Related Pay?
Dependent Variable: Ln(wage)

Regressor	*PRP*	*NO PRP*	*SMALL FIRM*	*LARGE FIRM*	*SMALL PLANT*	*LARGE PLANT*
Profits per employee	0.075	0.039	0.020	0.053	0.059	0.034
	(0.020)	(0.017)	(0.019)	(0.021)	(0.019)	(0.025)
Performance related Pay			0.061	0.076	0.094	-0.002
			(0.040)	(0.039)	(0.040)	(0.037)
Ln(capital-labour ratio)	0.067	0.035	0.042	0.052	0.016	0.059
	(0.027)	(0.015)	(0.022)	(0.017)	(0.021)	(0.016)
Ln(firm employment)	0.013	0.008	-0.025	-0.009	0.008	0.012
	(0.011)	(0.009)	(0.018)	(0.022)	(0.011)	(0.010)
Observations						
Number of Firms	134	174	186	86	193	133
Number of Individuals	196	218	196	196	254	192
Number of Observations	365	409	387	387	449	325
Adjusted R^2	0.63	0.59	0.52	0.70	0.57	0.68
Mean Profits per worker	0.47	0.40	0.24	0.62	0.42	0.45
(Std. Deviation)	(0.79)	(1.08)	(0.79)	(1.06)	(1.00)	(0.89)

1. All equations are estimated by OLS. Standard errors are in parentheses and are robust to arbitrary heteroscedasticity and the repeat sampling of individuals within firms.
2. All regressions also include quadratics in potential experience and job tenure, and controls for temporary job, workplace size, education, gender, race, union recognition, occupation (SOC Code at the one-digit level), industry (SIC code at the one-digit level), region, marital status, and time period. Parameter estimates are not reported. All equations are estimated by OLS.
3. Capital and profits are in £10,000s (1991 values).
4. PRP denotes performance-related pay. A small firm is defined as one with employment less than, or equal to, median employment. Large firms have employment above the median. Small establishments are defined as those with less than 200 workers, large establishments those with 200 employees or more.
5. The last row represents the wage gain from moving one standard deviation below mean profits per employee to one standard deviation above.

TABLE 2.6a
The Effect of Firm Profitability upon Pay (BHPS-DB)
Rent-sharing and Unionisation
Dependent Variable: Ln(wage)

Regressor	*UNION*	*NON-UNION*	*ALL*
Profits per employee	0.038	0.026	0.064
	(0.017)	(0.017)	(0.017)
Profits per employee * Union			-0.032
			(0.026)
Union recognised workplace			0.009
			(0.038)
Performance related Pay	0.019	0.067	0.059
	(0.035)	(0.048)	(0.029)
Ln(capital-labour ratio)	0.047	0.034	0.050
	(0.013)	(0.024)	(0.015)
Ln(firm employment)	0.015	-0.000	0.005
	(0.010)	(0.012)	(0.008)
Observations			
Number of Firms	151	129	267
Number of Individuals	246	145	387
Number of Observations	488	286	774
Adjusted R^2	0.61	0.66	0.60
Mean profits per worker	0.54	0.25	0.43
(Std. Deviation)	(0.94)	(0.96)	(0.95)

1. See notes to Table 2.5. Union denotes a union recognised workplace (worker defined).

TABLE 2.6b
The Effect of Firm Profitability upon Pay (BHPS-DB)
Results by Occupation
Dependent Variable: Ln(wage)

Regressor	*PROFESS*	*OTHER*	*MANUAL*
Profits per employee	0.066	0.031	0.069
	(0.023)	(0.015)	(0.021)
Performance related Pay	0.095	0.065	0.028
	(0.048)	(0.031)	(0.056)
Ln(capital-labour ratio)	0.060	0.042	0.050
	(0.021)	(0.016)	(0.018)
Ln(firm employment)	-0.001	0.007	0.014
	(0.014)	(0.008)	(0.014)
Observations			
Number of Firms	104	210	88
Number of Individuals	124	294	102
Number of Observations	217	557	179
Adjusted R^2	0.52	0.48	0.54
Mean profits per worker	0.39	0.45	0.32
(Std. Deviation)	(0.88)	(0.98)	(1.02)

1. See notes to Table 2.5.
2. Professional occupations (PROFESS) include managers, professionals, associate professionals and technical occupations. Other occupations (OTHER) include all groups of workers. Manual occupations (MANUAL) include personal and protective services, plant operatives and other manual work.

TABLE 2.7
The Effect of Lagged Firm Profitability upon Pay (BHPS-DB)
Dependent Variable: Ln(wage)

Regressor	*ALL 1991/94*	*LAG1 1991/94*	*LAG1 1992/95*	*LAG2 1991/94*	*LAG2 1993/96*
Profits per employee$_t$	0.045	0.044		0.038	
	(0.014)	(0.016)		(0.017)	
Profits per employee$_{t-1}$			0.031		
			(0.016)		
Profits per employee$_{t-2}$					0.030
					(0.014)
Ln(capital-labour ratio)$_t$	0.049	0.052		0.060	
	(0.015)	(0.015)		(0.014)	
Ln(capital-labour ratio)$_{t-1}$			0.051		
			(0.014)		
Ln(capital-labour ratio)$_{t-2}$					0.067
					(0.014)
Ln(firm employment)$_t$	0.006	0.005		0.004	
	(0.008)	(0.008)		(0.008)	
Ln(firm employment)$_{t-1}$			0.007		
			(0.008)		
Ln(firm employment)$_{t-2}$					-0.000
					(0.008)
Observations					
Number of Firms	267	265	265	261	261
Number of Individuals	387	384	384	376	376
Number of Observations	774	699	699	636	636
Adjusted R^2	0.59	0.58	0.54	0.57	0.57

1. All equations are estimated by OLS. Standard errors are in parentheses and are robust to arbitrary heteroscedasticity and the repeat sampling of individuals within firms.
2. All regressions also include quadratics in potential experience and job tenure, and controls for temporary job, workplace size, education, gender, race, union recognition, occupation (SOC Code at the one-digit level), region, marital status, and time period. These controls are recorded at the BHPS interview date. Employer variables, profits per employee, the capital-labour ratio, firm employment and industry (SIC code at the one-digit level), are derived from firm data (1991 and 1994). Parameter estimates are not reported.
3. Capital and profits are in £10,000s (1991 values).
4. ALL denotes the complete BHPS-DB linked sample. LAG1 denotes the sample of employees who are working within the same employer in year t+1, where one-year lagged firm information is available. The 1991/1994 sample then regresses current wage on current firm variables for this sample. The 1992/1995 sample then estimates a wage equation upon one-year lagged firm variables, for this sample. LAG2 is defined analogously for two-year lags upon the firm variables.

TABLE 2.8
BHPS-DB Sample Means (1991 & 1994)
A Comparison of the Complete Sample and the Sub-sample with non-missing US Industry Data

Variable	*ALL*	*US*
Hourly Pay	6.24 (3.19)	6.15 (3.18)
Profits per employee	0.43 (0.95)	0.43 (0.86)
Total profits	1.93 (4.67)	2.08 (4.71)
Sales per worker	11.21 (14.02)	10.37 (12.89)
Ln(sales per worker)	2.09 (0.75)	2.05 (0.71)

- Standard deviations are in parentheses. Monetary values are deflated to 1991 values.
- Profits per employee and sales per employee are measured in £0,000's per worker. Total profits are measured in £100million.
- ALL is the complete BHPS-DB sample, US the sub-sample matched to US industry data.

TABLE 2.9a
The Effect of US Industry Profitability upon Pay (BHPS-DB)
Sample: Non-missing US Industry Data
Dependent Variable: Ln(wage)

Regressor	*US OLS*	*US OLS*	*US OLS*	*US OLS*
Profits per employee$_t$	0.045	0.045	0.042	
	(0.017)	(0.016)	(0.017)	
Ln(capital-labour ratio)$_t$	0.047	0.037	0.035	0.035
	(0.017)	(0.018)	(0.017)	(0.018)
Ln(firm employment)$_t$	0.004	0.004	0.002	0.002
	(0.008)	(0.008)	(0.008)	(0.008)
Ln(Industry wage)$_t$		0.275	0.257	0.247
		(0.143)	(0.147)	(0.150)
US Industry Profits per employee$_t$			0.019	0.025
			(0.051)	(0.052)
US Industry Profits per employee$_{t-1}$			0.007	-0.019
			(0.108)	(0.110)
US Industry Profits per employee$_{t-2}$			-0.011	0.020
			(0.066)	(0.065)
Observations				
Number of Firms	252	252	252	252
Number of Individuals	362	362	362	362
Number of Observations	724	724	724	724
Adjusted R^2	0.59	0.59	0.59	0.59

1. See notes to Table 2.5.
2. US denotes the sample where non-missing US industry data are available. US industry profits per employee are measured in $10,000s (1991 values).
3. The industry wage is merged from New Earnings Survey (NES) industry averages.

TABLE 2.9b
Instrumental Variables (IV) estimates of Rent-Sharing (BHPS-DB)
US Product Market Shocks as Instruments
Dependent Variable: Ln(wage)

Regressor	*US 2SLS*	*US 2SLS*	*US 2SLS*	*US 2SLS*
Profits per employee$_t$	0.070	0.082	0.069	0.101
	(0.063)	(0.074)	(0.063)	(0.084)
Ln(capital-labour ratio)$_t$	0.036	0.035	0.036	0.034
	(0.017)	(0.017)	(0.017)	(0.017)
Ln(firm employment)$_t$	0.003	0.003	0.003	0.003
	(0.008)	(0.008)	(0.008)	(0.008)
Ln(Industry wage)$_t$	0.272	0.271	0.272	0.269
	(0.142)	(0.142)	(0.142)	(0.142)
Observations				
Number of Firms	252	252	252	252
Number of Individuals	362	362	362	362
Number of Observations	724	724	724	724
Adjusted R^2	0.59	0.59	0.59	0.58
Instruments				
US Industry Profits per employee$_t$	0.138	-0.245		
	(0.164)	(0.136)		
US Industry Profits per employee$_{t-1}$	-0.609	0.508	-0.332	0.259
	(0.384)	(0.144)	(0.168)	(0.034)
US Industry Profits per employee$_{t-2}$	0.728		0.591	
	(0.240)		(0.163)	
F-test instruments	23.41	29.84	34.79	56.79
[p-value]	[0.00]	[0.00]	[0.00]	[0.00]
Test of over-identifying restrictions	1.33	1.08	1.24	
[p-value]	[0.52]	[0.30]	[0.27]	
Additional Adjusted R^2	0.06	0.05	0.06	0.04

1. See notes to Table 2.9a.
2. The additional (adjusted) R^2 represents the change in the adjusted R^2 as a result of adding the instruments to the second stage equation.

TABLE 2.10
The effect of the Firm's 'Ability to Pay' upon Worker Wages
A Comparison of Estimates

Dependent Variable	*Firm 'Ability to Pay' Measure*	*Extent of Rent-Sharing*	*US OLS*	*US 2SLS*	*US 2SLS*
Ln(wage)	Profits per employee	Elasticity (at the mean)	0.019	0.030	0.043
		% Wage gain +/-1 std.dev	0.077	0.119	0.174
Hourly Pay	Profits per employee	Elasticity (at the mean)	0.016	0.025	0.045
		% Wage gain +/-1 std.dev	0.066	0.100	0.180
Ln(wage)	Ln(sales per employee)	Elasticity	0.077	0.152	0.151
		% Wage gain +/-1 std.dev	0.109	0.216	0.214

1. See notes to Table 2.9a. All estimates include the standard controls and the industry wage.
2. Column 1 records the OLS estimates. Column 2 the 2SLS estimates with US Industry profits per worker dated t-1 and t-2 as instruments (column 3, Table 2.9b). Column 3 the 2SLS estimates with US Industry profits per worker at time t-1 (column 4, Table 2.9b).

TABLE 2.11a
IV estimates of Rent-Sharing (BHPS-DB)
US Product Market Shocks as Instruments
Dependent Variable: Hourly Wage

Regressor	*ALL OLS*	*ALL OLS*	*US OLS*	*US 2SLS*	*US 2SLS*
Profits per employee$_t$	0.253	0.205	0.235	0.356	0.643
	(0.108)	(0.101)	(0.124)	(0.472)	(0.638)
Ln(capital-labour ratio)$_t$	0.309	0.272	0.226	0.220	0.206
	(0.095)	(0.094)	(0.114)	(0.114)	(0.116)
Ln(firm employment)$_t$	0.022	0.013	0.006	0.003	-0.005
	(0.053)	(0.052)	(0.055)	(0.053)	(0.053)
Industry wage$_t$		0.279	0.227	0.220	0.202
		(0.119)	(0.156)	(0.158)	(0.157)
Observations					
Number of Firms	267	267	252	252	252
Number of Individuals	387	387	362	362	362
Number of Observations	774	774	724	724	724
Adjusted R^2	0.58	0.58	0.58	0.58	0.57
Instrument Set	None	None	None	Full	Restricted
F-test instruments				33.14	51.94
[p-value]				[0.00]	[0.00]
Test of over-identifying restrictions				1.64	
[p-value]				[0.20]	
Additional Adjusted R^2				0.06	0.04

TABLE 2.11b
IV estimates of Rent-Sharing (BHPS-DB)
US Product Market Shocks as Instruments
Dependent Variable: Ln(wage)

Regressor	*ALL OLS*	*ALL OLS*	*US OLS*	*US 2SLS*	*US 2SLS*
Ln(sales per employee)$_t$	0.104	0.087	0.077	0.152	0.151
	(0.022)	(0.024)	(0.027)	(0.134)	(0.134)
Ln(capital-labour ratio)$_t$	0.023	0.021	0.018	0.001	0.002
	(0.015)	(0.015)	(0.019)	(0.031)	(0.031)
Ln(firm employment)$_t$	0.017	0.015	0.014	0.020	0.020
	(0.008)	(0.008)	(0.008)	(0.013)	(0.013)
Ln(Industry wage)$_t$		0.243	0.219	0.101	0.103
		(0.137)	(0.162)	(0.268)	(0.269)
Observations					
Number of Firms	252	252	237	237	237
Number of Individuals	387	387	328	328	328
Number of Observations	685	685	635	724	635
Adjusted R^2	0.62	0.62	0.60	0.60	0.60
Instrument Set	None	None	None	Full	Restricted
F-test instruments				4.74	9.37
[p-value]				[0.01]	[0.00]
Test of over-identifying restrictions				0.08	
[p-value]				[0.77]	
Additional Adjusted R^2				0.03	0.03

1. See notes to Table 2.9b.
2. The Full instrument set includes US Industry profits per worker dated t-1 to t-2 (column 3, Table 2.9b). The restricted instrument set includes US Industry profits per worker at time t-1 (column 4).

TABLE 2.12
The Effect of Profitability upon Pay (BHPS-DB)
Worker-Firm Fixed Effects and Instrumental Variables Estimation
US Product Market Shocks as Instruments
Dependent Variable: Ln(wage)

Regressor	*US OLS*	*US 2SLS*	*US 2SLS*	*US 2SLS*	*US 2SLS*
Profits per employee$_t$	0.013	-0.039	-0.063	-0.036	-0.061
	(0.010)	(0.038)	(0.059)	(0.040)	(0.062)
Ln(capital-labour ratio)$_t$	0.006	0.013	0.017	0.013	0.016
	(0.021)	(0.023)	(0.024)	(0.023)	(0.024)
Ln(firm employment)$_t$	0.023	0.017	0.014	0.017	0.014
	(0.060)	(0.059)	(0.061)	(0.059)	(0.061)
Observations					
Number of Firms	252	252	252	252	252
Number of Individuals	362	362	362	362	362
Number of Observations	362	362	362	362	362
Instruments					
US Industry Profits per employee$_t$		0.220	-0.127		
		(0.244)	(0.178)		
US Industry Profits per employee$_{t-1}$		-1.009	0.335	-0.852	0.302
		(0.455)	(0.172)	(0.434)	(0.164)
US Industry Profits per employee$_{t-2}$		1.074		1.012	
		(0.301)		(0.288)	
US Industry Prices$_t$		-1.328	-2.674		
		(1.610)	(1.705)		
US Industry Prices$_{t-1}$		3.904	3.540	3.365	2.045
		(2.009)	(1.760)	(1.564)	(0.870)
US Industry Prices$_{t-2}$		-3.087		-3.977	
		(2.017)		(1.982)	
F-test instruments		5.57	2.46	7.96	3.82
[p-value]		[0.00]	[0.05]	[0.00]	[0.02]
Test of over-identifying restrictions		5.38	4.15	4.83	3.83
[p-value]		[0.37]	[0.25]	[0.18]	[0.05]
Additional Adjusted R^2		0.09	0.05	0.09	0.04

1. All variables are measured in First Differences (1994 values – 1991 values).
2. Standard errors are in parentheses and are robust to the repeat sampling of individuals within firms.
3. All equations include controls for potential experience, job tenure, occupation, workplace size, temporary job and marital status. Time invariant characteristics drop out of the equation. Parameter estimates are not reported.
4. Capital and profits are in £10,000s (1991 values).
1. US denotes the sample where non-missing US industry data are available. US industry profits per employee are measured in $10,000s (1991 values).
2. The industry wage is included but not reported.
3. The additional (adjusted) R^2 represents the change in the adjusted R^2 as a result of adding the instruments to the second stage equation.

TABLE 2.13
The Effect of Profitability upon Pay (BHPS-DB)
Worker-Firm Fixed Effects and Instrumental Variables Estimation
US Product Market Shocks as Instruments
Dependent Variable: Ln(pay)

Regressor	*US OLS*	*US 2SLS*	*US 2SLS*
Profits per employee$_t$	0.026	-0.004	0.041
	(0.012)	(0.041)	(0.063)
Ln(capital-labour ratio)$_t$	0.009	0.013	0.006
	(0.024)	(0.024)	(0.026)
Ln(firm employment)$_t$	0.039	0.036	0.041
	(0.066)	(0.066)	(0.066)
Observations			
Number of Firms	252	252	252
Number of Individuals	362	362	362
Number of Observations	362	362	362
Instrument Set	None	Full	Restricted
F-test instruments		7.96	3.82
[p-value]		[0.00]	[0.02]
Test of over-identifying restrictions		2.34	0.38
[p-value]		[0.50]	[0.54]
Additional Adjusted R^2		0.09	0.04

1. See notes Table 2.12.
2. The Full instrument set includes US Industry variables dated t-1 to t-2 (column 4, Table 2.12). The restricted instrument set includes variables at time t-1 (column 5, Table 2.12).

TABLE 2.14
The Effect of Lagged Profitability upon Pay (BHPS-DB)
Worker-Firm Fixed Effects and Instrumental Variables Estimation
US Product Market Shocks as Instruments
Dependent Variable: Ln(wage)

Regressor	*US OLS*	*US 2SLS*	*US 2SLS*
Profits per employee$_{t-1}$	-0.019	0.038	0.023
	(0.011)	(0.032)	(0.053)
Ln(capital-labour ratio)$_{t-1}$	0.010	0.000	0.003
	(0.023)	(0.025)	(0.025)
Ln(firm employment)$_{t-1}$	0.064	0.073	0.071
	(0.048)	(0.051)	(0.049)
Observations			
Number of Firms	216	216	216
Number of Individuals	302	302	302
Number of Observations	302	302	302
Instrument Set	None	Full	Restricted
F-test instruments		6.89	3.72
[p-value]		[0.00]	[0.03]
Test of over-identifying restrictions		3.19	2.98
[p-value]		[0.36]	[0.08]
Additional Adjusted R^2		0.08	0.04

1. See notes Table 2.13.

APPENDIX 1: Non-response and Attrition Bias in the BHPS

As we link company accounts data to worker responses, it seems natural to examine whether the employee data has remained representative over time. The BHPS is a nationally representative sample of more than 5,000 British households, containing over 10,000 adults. Respondents are interviewed annually. If an individual leaves their original household all adult members of their new household are also interviewed. Children are interviewed once aged 16. Together these should ensure the sample remains representative of the British population.

Nathan (1999) has undertaken a more systematic analysis of the effects of attrition. The BHPS is compared to Census data, the General Household Survey (GHS) and the Family Expenditure Survey (FES), with respect to age, sex, marital status, socio-economic group, ethnicity, employment status and some household characteristics. The author concludes that cumulative attrition in the BHPS is limited and does not lead to serious bias in inference.

APPENDIX 2: Sample Selectivity within the BHPS-DB Linked Data

The process by which the DB firm data are merged with the BHPS employee data may produce a non-random sample. Individual response rates, to the question regarding company name, may differ, and, large organisations are more likely to be observed in the DB database.

The summary statistics in Table 2.1 show that workers within the linked data are more likely to work in large or unionised workplaces and to be paid performance-related pay. They are also more likely to work in manufacturing industries, yet differences are here small. With respect to occupation, gender and race, the composition of the linked sample is comparable to that for the BHPS as a whole. Finally, workers with accompanying firm data are slightly less educated, though they have greater job tenure and potential experience.

Table A1 estimates hourly pay equations for the sample with linked firm data (column two), and compares parameters to those obtained for the BHPS as a whole (column one). Equations include as explanatory variables: firm and establishment size, occupation, industry and education. To account for potential variation in coefficients, by observed characteristics, standard errors are robust to arbitrary heteroskedasticity and the non-independence of errors within the same firm.[33] The sample with firm data predicts lower returns to tenure and a weaker effect of workplace size. Remaining coefficient estimates are, however, comparable, and substantive conclusions hold for both samples. Results suggest that the estimated parameters upon individual and workplace variables do not suffer unduly from selection bias.

[33] Ignoring the clustering of individuals within firms can, potentially, significantly underestimate standard errors (see Moulton, 1986).

An issue with the analysis carried out in this chapter is that, by examining the panel of workers within the same firm, we omit company switchers and those respondents observed once. This seems the most natural way to control for unobserved heterogeneity, in these data. Yet by focussing upon large and well-established firms, for whom we observe repeat observations, and workers who remain in the same firm, who may be more likely to be 'insiders', we may overstate the returns to firm profitability. Wage equations, including firm variables, are then estimated, and compared, for the balanced worker-firm data (column three), for the unbalanced data (column four) and for the balanced data including company movers (column five).

Within the balanced worker-firm data the estimated coefficient upon profits per employee is 0.045, for the balanced data that includes firm mobility 0.038, and for the unbalanced data 0.029. In all three cases the rent-sharing effect is statistically significantly different from zero. These estimates, whilst qualitatively similar, suggest the balanced worker-firm sample may overstate the returns to firm profitability. Controls for worker and firm heterogeneity may though be hoped, in part, to mitigate this selectivity.

TABLE A1
A Comparison of Estimates from the BHPS sample and the Linked data
Dependent Variable: Ln(wage)

Regressor	*BHPS*	*Firm Balanced BHPS-DB*	*Firm Balanced BHPS-DB*	*Unbalanced BHPS-DB*	*Worker Balanced BHPS-DB*
Potential Experience	0.024	0.022	0.022	0.023	0.024
	(0.002)	(0.005)	(0.005)	(0.004)	(0.005)
Potential Experience^2/100	-0.042	-0.033	-0.032	-0.036	-0.036
	(0.005)	(0.010)	(0.009)	(0.008)	(0.009)
Job tenure	0.007	0.002	0.002	0.002	0.000
	(0.003)	(0.007)	(0.007)	(0.006)	(0.006)
Job tenure^2/100	-0.014	-0.002	-0.000	0.000	0.007
	(0.011)	(0.030)	(0.029)	(0.025)	(0.027)
Years of schooling	0.017	0.015	0.014	0.022	0.016
	(0.004)	(0.008)	(0.008)	(0.006)	(0.008)
Workplace Size:10-49	0.150	0.112	0.092	0.130	0.088
	(0.022)	(0.060)	(0.061)	(0.049)	(0.058)
Workplace Size:50-199	0.213	0.088	0.065	0.132	0.067
	(0.025)	(0.059)	(0.061)	(0.051)	(0.057)
Workplace Size:200-999	0.236	0.086	0.062	0.111	0.076
	(0.025)	(0.061)	(0.064)	(0.050)	(0.060)
Workplace Size:1000+	0.315	0.232	0.188	0.268	0.201
	(0.030)	(0.068)	(0.069)	(0.054)	(0.065)
O-levels	0.079	0.043	0.045	0.048	0.060
	(0.020)	(0.041)	(0.039)	(0.032)	(0.036)
A-levels	0.111	0.089	0.080	0.089	0.098
	(0.025)	(0.050)	(0.048)	(0.040)	(0.045)
HND, HNC	0.180	0.203	0.207	0.191	0.215
	(0.038)	(0.073)	(0.073)	(0.058)	(0.069)
Degree	0.296	0.320	0.278	0.248	0.290
	(0.040)	(0.086)	(0.085)	(0.070)	(0.081)
Higher Degree	0.336	0.567	0.564	0.432	0.491
	(0.114)	(0.176)	(0.173)	(0.154)	(0.144)
Female	-0.225	-0.197	-0.204	-0.188	-0.187
	(0.017)	(0.037)	(0.036)	(0.027)	(0.031)
Union recognised workplace	0.056	0.049	0.002	0.034	0.017
	(0.016)	(0.036)	(0.037)	(0.029)	(0.033)
Profits per employee			0.045	0.029	0.038
			(0.014)	(0.012)	(0.014)
Ln(capital-labour ratio)			0.049	0.042	0.048
			(0.015)	(0.011)	(0.014)
Ln(firm employment)			0.006	0.001	0.002
			(0.008)	(0.006)	(0.007)
Observations					
Number of Firms		267	267	501	333
Number of Individuals	1751	387	387	679	434
Number of Observations	3502	774	774	1113	868
Adjusted R^2	0.55	0.57	0.59	0.57	0.59

1. See notes for Table 2.4. All estimates are by OLS. All equations include controls for temporary employment, occupation (SOC code at the one-digit level), industry (SIC code at the one-digit level), region, time period, race and marital status.
2. The firm-balanced sample includes all workers observed in the same firm in 1991 and 1994. The worker-balanced sample includes workers observed in different firms in 1991 and 1994. The unbalanced sample includes all workers observed linked to the firm.

Chapter Three

Employer Size, Wages and Worker Well-being

Abstract

This chapter tests competing hypotheses for the famous positive relationship between employer size and wages. The addition of more refined controls for firm characteristics (the capital to labour ratio, the intensity of monitoring, and firm profitability) leaves the establishment size-wage premium largely unperturbed. Whether the size-wage relation reflects a compensating differential is tested using worker well-being data. Whilst job satisfaction is observed to be higher within small employers there is little difference in satisfaction scores between medium-sized and large plants. Worker distaste for employer size is then found not to offer a good explanation of the relationship between establishment size and pay. Instead correlates of worker skill and person fixed-effects, find most favour in explaining the establishment size-wage differential, capturing up to half of the observed relationship. A large unexplained wage premium, to those employees working in the largest plants, does however remain.

3.1 Introduction

The finding of an unexplained positive relationship between establishment size and worker pay, even after the inclusion of standard human capital and industry controls, is a pervasive regularity in studies of wage determination. The recent finding of an employer size differential in recorded job satisfaction levels has, however, been less well documented. It is probably not yet widely known among labour economists. A persuasive consensus upon the source of these effects has yet to come to the fore.

This chapter uses three sources of data, the British Household Panel Survey (BHPS), the Workplace Employee Relations Survey (WERS) and the National Child Development Study (NCDS), to attempt to distinguish between competing hypotheses for the existence of a size-wage relationship. The first has the advantage of being a nationally representative panel survey, allowing controls to be made for unobserved worker heterogeneity. The second benefits from the linking of establishment data to employee responses, permitting a more comprehensive examination of the role of employer characteristics. Finally, the NCDS is a representative cohort study that includes measures of childhood ability, providing more robust controls for worker skill.

The elasticity of wages with respect to workplace size, in the private sector, is here estimated to be approximately 0.04. The role of unobserved labour productivity differences is analysed in two ways. Firstly, correlates of worker skill, such as childhood test scores, the use of IT, and the skill of the workforce, are added to wage equations, and are found to capture up to 15 percent of the establishment size-wage premium and up to 30 percent of the firm size-wage premium. Secondly, fixed effect models are analysed. These remove unobserved

heterogeneity that remains constant over time. The estimated effect of workplace size upon wages is then reduced by over a half, compared to cross-section estimates. Nevertheless, wages are still observed to be significantly greater in large workplaces.

One of the contributions of the chapter is to design a novel test as to whether the employer size-wage relation reflects a compensating differential (that large plants offer inferior working conditions compensated by higher pay). Evidence is found in support of the model's underlying assumption. Small workplaces are associated with higher levels of job satisfaction. Yet observed dissatisfaction is felt predominantly when moving from small to medium-sized plants, with little difference in recorded well-being levels between large and medium-sized establishments. Employee preferences for very small workplaces may then help to explain the lower wages in these plants but, on this evidence, do not explain why wages are observed to monotonically increase with establishment size, even amongst relatively large establishments. Moreover, establishment size is found not to exert a robust independent influence upon job satisfaction once we hold constant the size of the firm. Whilst this may support a compensating differential with respect to firm size, it does not support one with respect to workplace size.

Alternative institutional explanations are also examined. Models of monitoring costs and rent-sharing are, here, found to offer poor explanations of the size-wage correlation. The unobserved productivity hypothesis, where workers in large plants are for some reason more productive, instead appears to offer the most convincing avenue from which to explain the plant size-wage differential.

The plan of the chapter is as follows. Section two outlines potential rationales for an observed employer size-wage differential. Section three documents the evidence of previous studies. The data are discussed in section four and regression results presented in section five. Finally, section six concludes.

3.2 The Employer Size-Wage Relation: Theories and Explanations[1]

The existence of a positive relationship between employer size and pay is a well-documented empirical regularity. Explanations for its existence are, however, more contentious. Wages determined within the competitive paradigm are assumed to fully reflect workers' opportunity costs of employment. An observed size-wage correlation then captures unobserved differences, in productivity (large employers hire more able individuals) or working conditions (a compensating differential to inferior conditions in large workplaces).

Institutional explanations have taken four main forms. The first suggests the size-wage premium captures a trade off between monitoring and paying efficiency wages. The second a payment to prevent unionisation. The third the sharing of firm rents with their employees. Finally, in the presence of labour market frictions, counter to the competitive model, labour supply may not be perfectly elastic to any one firm, but rather upward sloping in the short run. The size-wage profile may then map out the labour supply curve. These explanations are discussed in more detail below.[2]

3.2.1 Differences in Productivity

For unobserved skill differences to explain the relationship between employer size and wages, large employers must hire more able employees. Possible avenues for such behaviour are here discussed.

Worker ability may be an input in production complementary with physical capital (Hamermesh, 1993) or the skill of capital (Reilly, 1995). If so plants with

[1] The term employer size will be used to denote applicability to *either* firm or plant size.
[2] For a review of the literature regarding the relationship between employer size and wages see Oi and Idson (1999) and Troske (1999).

greater capital usage, or more sophisticated technology, will hire more able employees. As large employers have greater output over which to amortise fixed costs and are liable to face lower input prices (via bulk purchase discounts) they are likely to be the more intensive users of capital, and hence hire more able employees. The use of capital may itself raise employee productivity, independent of any worker selection effect.

Kremer (1993) builds a model explicitly assuming joint production and team working. Output (Y) is a multiplicative function of capital (K) and the labour inputs of the N team members (q_i).

$$Y = AK^{\alpha}\left(\prod_{j=1}^{N} q_j\right)^{\beta} = AK^{\alpha}\left(q_1 q_2 q_3 \ldots q_N\right)^{\beta} \qquad (1)$$

For a given team size, N, output is increasing in mean worker quality, and decreasing in its variance. It is then profitable for employers to match or segregate employees by skill. If teamwork is more prevalent in large plants or there are large fixed costs associated with hiring high wage employees (e.g. more formal recruitment process), large plants are more likely to select a high skill workforce.

These models offer avenues by which large plants or firms may hire more able employees. Yet where the researcher has good controls for worker ability such sorting should not contaminate results. Oi and Idson (1999) propose that equally able workers are more productive in large establishments, either due to greater effort levels, or, for the service sector, the presence of increasing returns. In the latter case it is suggested the number of consumers (C per period) is an essential input in the firm's production function, Y = f(N,K,C). Minimum manning requirements create a reserve pool of labour for which utilisation rates and efficiency increase as C rises.

3.2.2 A Compensating Differential

The compensating differential model (for a comprehensive discussion see Rosen, 1986) extends the basic intuition that labour is a bad to allow the disamenity of employment to vary between jobs. Workers choose between bundles of employment characteristics including, pay, hours, the pace of work, etc. For simplicity, we restrict attention to wages and a composite term measuring the disamenity of all other aspects of employment (D). Utility for an employed individual is defined by $U = u(C, D)$, where C is a composite consumption good and we assume $U_C>0$ and $U_D<0$. Finally, we define two types of job, small disamenity, D_S, and large disamenity, D_L, where $D_S < D_L$.

For an individual to work in the high disutility job their level of welfare must be at least as great as that from working in the alternative occupation. The individual is just compensated when utility levels are equalised:

$$u(C_S, D_S) = u(C_L, D_L) \qquad (2)$$

Hence $Z = C_L - C_S$, defines the extra income the individual requires to work in a high disamenity plant, the shadow price. Let $W_L - W_S$ be the actual wage differential. An individual will work in the L plant only if, $W_L - W_S > Z$. Then, for equally able individuals, preferences, embodied by Z, define labour supply and equilibrium is established by wage adjustments where labour demand equals supply.

For the model to explain the relationship between wages and establishment size, small plants must offer superior working conditions. The greater regimentation of production, lower levels of autonomy, and more impersonal working conditions within large employers may provide support for such a hypothesis. In equilibrium, however, workers with the smallest distaste for large plants will be matched to these workplaces. If there are a sufficiently large number of workers with little or no

distaste for large employers, even if working conditions are inferior to some, there will be no compensating wage differential.

3.2.3 *Institutional Explanations*

In many situations any one individual's contribution to production may be only imprecisely observed (e.g. within teams of workers). Shapiro and Stiglitz (1984) assume that, equally able, workers are able to exert less effort than is optimal for the employer. To deter shirking firms face a trade off between, more intensive monitoring of worker performance, or, raising wages and firing those caught shirking.

In a simplified version of the Shapiro-Stiglitz model a worker receives utility, $U = W - e$, where W is the wage and e the effort level. If they shirk (provide zero effort) they are detected with probability p. Upon detection they are immediately fired and receive the outside option V. The 'No Shirking Condition' is then the wage that makes workers indifferent between shirking and providing effort.[3]

$$W - e = pV + (1-p)W \tag{3}$$

rearranging terms:

$$W = \frac{e}{p} + V \tag{4}$$

Wages are then an increasing function of the required effort level and a decreasing function of the monitoring intensity. If large employers face higher monitoring costs, the greater wages in such establishments reflect a trade off for less intensive supervision.

Alternative mechanisms do exist to counter the shirking problem. Deferred compensation schemes, where employees receive higher pay upon proof of non-

[3] Expected utility is equalised. We assume that when indifferent, effort is provided.

shirking behaviour, also provide workers with incentives to supply effort. The enforcement of such schemes is bedevilled by moral hazard and reputation problems, yet these seem less applicable to large employers.

An alternative institutional explanation of the size-wage differential concerns union avoidance. Unions seek to bargain up wages, improve working conditions and regulate disciplinary procedures, and so raise labour costs. Employers then have an incentive to prevent union recognition. Assume W_U is the wage that would prevail if a union were recognised and c the cost per employee of running a campaign to obtain that recognition. Non-union firms can then pay a wage just sufficient to prevent unionisation, $W = W_U - c$. If unions' costs of organising an workplace exhibit decreasing returns to scale, and these costs decline at a faster rate than firms' costs of opposition, then unions will optimally target large plants, who will pay higher wages to deter recognition. Within the UK, since the 1984 Trade Unions Act,[4] there is no legal compulsion to recognise a union. Thus, whilst a union can still mount a campaign for recognition, the firm's cost of opposition would appear much reduced.

Mellow (1982) hypothesised that the size-wage relation reflected the greater product market power and rents enjoyed by large firms. For reasons of union bargaining, insider power, or gift exchange, these rents are shared with employees in the form of higher wages. A simple relationship between wages (W) and profits (Π) per employee (N) is here expressed (for a more detailed discussion see chapter 2, section 1):

$$W = W^* + \delta\frac{\Pi}{N} \qquad (5)$$

[4] See Disney et al (1996) for a full discussion of the consequences of this act upon union recognition.

Where W^* is the alternative wage and δ the rent-sharing parameter. The observed size-wage relationship is then an artefact of failing to control for firm profitability.

As of yet models have assumed frictionless job mobility and perfect information. These assumptions are now relaxed and the model of Burdett and Mortensen (1998) examined. Search frictions here take the form of lags in the arrival of information regarding job offers. The authors assume there exist a large fixed number of identical workers and firms and all jobs are identical apart from the wage paid. Job offers arrive at random intervals and employees are assumed to search randomly amongst employers. The unemployed accept any wage offer at least as large as their reservation wage, and the employed any offer in excess of their current wage. Equilibrium is achieved, subject to these search conditions, where firms are profit maximising and profits are equalised amongst all firms.

The authors then show that any steady state equilibrium must be characterised by a non-degenerate distribution of wage offers, if the arrival rate of jobs is positive and finite. Intuitively, if all firms offer the same wage, W^m, any employer offering a slightly higher wage will attract a significantly larger labour force with only slightly diminished profits per worker. Competitive logic suggests such profitable deviations will occur until the wage is bid up to the marginal product and profits are zero. Yet it is then profitable to deviate to a lower wage, as the search frictions imply an employer will retain a positive labour force with positive profits. A unique wage is, hence, not profit maximising, and the non co-operative equilibrium is characterised by a distribution of wages positively related to workplace size.

3.3 Existing Empirical Evidence[5]

3.3.1 Differences in Productivity

Possibly the most influential study as to the source of the size-wage differential is that of Brown and Medoff (1989), who use two individual level data sets, the May 1979 CPS and the 1972-77 QES panel, and three plant level surveys to examine alternative hypotheses. The effect of log workplace size upon wages is estimated to between 0.015 and 0.038 for the individual data, and 0.008 and 0.032 for the plant data. Whether these results reflect unobserved productivity differences is tested in two ways.

First, the establishment size-wage relationship is examined for narrowly defined occupations, where there is less scope for productivity differences. Parameter estimates upon workplace size are essentially unchanged. Second, the authors estimate a fixed-effects model of the change in wages upon the change in establishment size, between 1972 and 1977. This removes all unobserved individual heterogeneity that remains constant over time. The estimated parameter upon log establishment size declines from 0.038 to 0.021, but remains statistically significant. The potential inconsistency caused by measurement error and the self-selection of job changers is investigated, by including dummies for job changers and voluntary job changers, but results are not affected.

Idson and Fester (1990) examine unobserved ability differences using a Heckman type selectivity model. Using the May 1979 CPS, for males, the worker choice of establishment size is identified by whether the individual is, separated, never married, or a veteran. These are assumed to determine matching decisions, but not the wage, and are used to construct correction terms for worker selectivity,

[5] Unless otherwise stated studies examine US evidence.

added to wage regressions for each size category. Results predict selection inversely related to establishment size. That is the most able, with regards unobserved characteristics (education is held constant), exhibit a preference for small establishments.[6] Unobserved productivity differences would then imply the size-wage differential observed in the cross-section understates the true relationship. In contrast, for the UK, Main and Reilly (1993) identify the worker's choice of establishment by the number and age of dependent children, but find no evidence of non-random sorting of workers across plant size.

Troske (1999) uses the Worker Establishment Characteristics Database (WECD), which matches census data to the employing workplace, to investigate four avenues for a positive correlation between employer size and worker quality. The complementarity of worker skill with physical capital, the skill of the manager, the skill of capital, and the skill of co-workers are examined. Only the skill of capital (new investment in IT) and the skill of the workforce (mean experience, proportion skilled and proportion with a degree) exert any substantive effect upon the size-wage correlation. The skill of the labour force accounts for 20 percent of both the establishment and firm size-wage premiums. The addition of the skill of capital implies they, together, explain some 45 percent of the firm size elasticity. Nevertheless, substantial wage variation by plant and firm size remains.

Abowd, Kramarz and Margolis (1999) use a large French data set of workers and firms to examine fixed effect estimates. The authors suggest a weakness of Brown and Medoff (1989), and other similar first difference models, is the lack of controls for the employer. Inference is then consistent only if size is orthogonal to the omitted firm fixed effect (conditional on the person fixed effect). Models are estimated with controls for both person and firm unobserved heterogeneity. Abowd

[6] Idson and Fester propose this reflects a taste for independence.

et al find the firm size-average (estimated) person fixed effect is a more powerful predictor of the firm size-wage relationship than is the firm size-average (estimated) firm fixed effect. Nevertheless, the employer effect is an important and non-negligible factor in explaining the size-wage correlation.

Two criticisms remain. First, the worker and firm fixed-effects are identified under the assumption of exogenous worker mobility. The self-selection of individuals who change employers may then bias results. Second, as Hamermesh (1999) has pointed out, with limited controls for worker characteristics such an estimation procedure may place too much emphasis upon unobserved individual, rather than employer, heterogeneity.

Finally, Winter-Ebmer and Zweimuller (1999) use the Swiss Labour Force Survey (SLFS) to identify the extent of bias that endogenous mobility causes in fixed effects estimates. Following a methodology similar to Murphy and Topel (1987), they examine what proportion of the cross-section wage differential workers actually capture when they move between workplace size categories. If the cross-section coefficients capture worker ability, wage changes will be unrelated to employer size and the proportion of the predicted differential captured zero. In fact, 43 percent of the wage gains predicted, by cross-section estimates, are realised. Endogenous mobility may bias results if the wage gains from moving to larger plants far outweigh the losses from moving to a smaller establishment. The evidence, though, suggests approximately symmetric effects.

The studies documented suggest individual heterogeneity, does explain a portion of the size-wage relationship. Yet it does not seem to provide a single complete argument for its existence.

3.3.2 A Compensating Differential

Brown and Medoff (1989) examine whether variations in working conditions can explain the relationship between establishment size and wages using the 1972-77 QES data. The authors characterise 42 working conditions, of which only 21 are identified as inferior in large plants. Moreover, the addition of these variables to the wage equation has little impact upon the estimated coefficients upon workplace size. The authors then analyse quit rates and employer tenure, as indicators of well-being. Large establishments are estimated to have a lower rate of quits and greater years of tenure, holding wages constant, suggesting large plants offer *superior* working conditions. This may, in part, capture the effect of higher rates of internal mobility within large establishments. Indeed when job, as opposed to employer, tenure is analysed plant size enters positively but is not statistically well determined.

Idson (1990) offers an alternative examination of whether large plants offer inferior working conditions, though this is not related to wage determination. The author hypothesises that within large plants there exist more formalised and regimented working practice. Using the 1977 QES Idson demonstrates for a range of measure of work rigidity that establishment size does indeed exert a positive and statistically significant effect. In addition employer size is estimated to have a robust negative effect upon job satisfaction, net of wages and fringe benefits. Adding the measures of work rigidity captures some of the size-satisfaction correlation, attenuating the coefficient by over a third, but the plant size effect remains statistically robust.

For the UK, Green et al (1996) find that 67 percent of respondents in the 1989 British Social Attitudes Survey would prefer to work in a small workplace. Furthermore, work in the largest establishments is observed to be more boring, dangerous, unhealthy and unpleasant, but also less physical. However, echoing

earlier work by Main and Reilly (1993), when these job attributes are entered in a wage equation they are, generally, statistically insignificant or incorrectly signed.

The evidence as to whether working conditions are superior in small establishments is then mixed, with little evidence they explain the size-wage correlation.

3.3.3 Institutional Explanations

Pearce (1990) examines the relationship between tenure, unions, workplace size and wages using the May 1979 CPS. The estimated size-wage premium is observed to be larger in non-unionised plants and in lightly unionised occupations, whilst for non-unionised employees the industry union density exerts a well-determined positive effect upon wages only in the largest plants. Such evidence seems to support a union avoidance hypothesis. Yet, as Brown and Medoff (1989) state, that the size-wage relation exists in unionised plants suggests this cannot be the only explanation. Brown and Medoff also examine groups of workers, within industries, where the threat of unionisation is minimal, where union avoidance should play a negligible role. Estimates are very similar to those for all non-union employees. Green et al (1996) observe similar evidence for the UK, using the 1983 General Household Survey (GHS) and the 1991 wave of the BHPS.

Troske (1999) tests the hypothesis that the size-wage relation represents a trade off between wages and monitoring costs, by controlling for the monitoring intensity (the number of supervisors per employee) within the plant. Whilst the monitoring intensity enters negatively and statistically significantly the estimated firm and establishment size parameters are essentially unchanged, and monitoring intensity is found to be largely uncorrelated with employer size. Troske also examines the impact of a plant's product market power upon the estimated wage-

size differential. Whilst the measures of market power (the Herfindahl index, value added per worker, and profits per worker) enter positively they do not disturb the parameters upon firm or plant size.

Green et al (1996) suggest the model of labour market frictions of Burdett and Mortensen (1998) is akin to a model of dynamic monopsony. The authors conjecture such monopsony power will be greater, and the wage-size relation more pronounced, for women, than for men, and for non-union employees.[7] The evidence from the 1983 GHS and the 1991 BHPS is generally supportive. van den Berg and Ridder (1998) use Dutch data to estimate the Burdett-Mortensen (1998) search model. Whilst the empirical model is found to provide a good fit of the observed wage distribution, the estimated structural parameters suggest search frictions are relatively small. Individuals then move quite rapidly to more highly paid jobs, and most wages within a segment are similar to the competitive wage. This would seem to limit the extent to which this model can generate a positive relationship between employer size and wages. Nevertheless, search frictions are found to explain around 20 percent of the variation in wages.

3.4 Data

The data used in this study come from three sources, the British Household Panel Survey (BHPS), the Workplace Employee Relations Survey (WERS) and the National Child Development Study (NCDS).

The BHPS is nationally representative sample of more than 5,000 British households, containing over 10,000 adult individuals, interviewed late each year

[7] For this to be consistent with the model of search frictions these groups have to participate in segmented labour markets.

from 1991 to 1998 (for a further discussion see Appendix 1). These data include detailed information regarding earnings, education, employment characteristics, demographics, and job satisfaction. Attention is here restricted to those individuals aged less than 65 and in employment at the survey date, approximately 5,000 respondents in any one year.

The WERS is a cross-section survey that was completed between October 1997 and June 1998. It is a random sample[8] of around 2,200 British establishments with ten or more employees. Within these workplaces 25 worker questionnaires were randomly allocated amongst the employees. For establishments with less than 25 employees the population of workers was sampled, yielding approximately 28,000 individual responses matched to 1,800 workplaces (for a fuller discussion of sampling issues see Appendix 2). The employee data includes questions on earnings, education, workplace characteristics and a rich source of information on worker attitudes. These data are augmented by a management questionnaire regarding establishment characteristics.

The NCDS is a random survey of individuals born in Britain in the first week of March 1958. Subsequent interviews were completed when respondents were aged 7, 11, 16, 23 and 33. The first three waves contains detailed data upon education (obtained both from the child and the school) and parental background. The last two waves contain data concerning earnings and employment characteristics. Focus is here restricted to those individuals in employment at the last interview date.

[8] The survey population excludes establishments in the following SIC (1992) divisions: A (Agriculture, hunting and forestry), B (fishing), C (Mining and quarrying), P (Private households with employed persons), and Q (Extra-territorial organisations).

3.4.1 The Job Satisfaction Data

Within the BHPS working respondents are asked to rate their level of satisfaction with respect to seven aspects of their employment: promotion prospects, total pay, relations with supervisor, job security, ability to work on own initiative, the actual work itself, and, the hours of work. Each of these categories is assigned a rank between 1 and 7, 1 representing 'not satisfied at all', 7 indicating 'completely satisfied' and the numbers from 2 to 6 corresponding to intermediate levels of satisfaction (where 4 is 'neither satisfied or dissatisfied').[9] Finally, and subsequent to these seven questions, a question was asked:

"All things considered, how satisfied or dissatisfied are you with your present job overall using the same 1-7 scale?"

The method in which the questions were asked suggests individuals evaluated many attributes of their job package when responding. It seems probable this approach will elicit responses more closely approximating satisfaction at the workplace, than would a simple direct question. The responses to this last question form the basis of analysis of job satisfaction within the BHPS.

Unfortunately there is no comparable overall job satisfaction question in the WERS. There are, however, analogous prelimianry questions, with respect to influence over the job, total pay, sense of achievement, and, the respect received from supervisors. Each of these questions was answered on a 1 to 5 scale, where 1 corresponded to the highest level of satisfaction and 5 the lowest. For ease of comparison with the BHPS this scale was reversed, so 1 represents the lowest level of well-being (very dissatisfied), 5 the highest (very satisfied) and 2 to 4 intermediate

[9] In wave one the categories 1, 4 and 7 are given the descriptions outlined, whilst 2, 3, 5 and 6 are left unlabeled. From wave two onwards all values were given a label, with the descriptors 'mostly' and 'somewhat' added. The question itself was a constant. This discrepancy is treated as noise.

values. Analysis within the WERS then focuses upon these four satisfaction questions.[10]

3.4.2 Comparison of Satisfaction Responses: Interpersonal and over time

Job satisfaction reflects both objective circumstances, working conditions, and subjective factors, aspirations and expectations. This subjectivity has led some economists to be sceptical of the concept's worth. Scores may be random draws and interpersonal comparisons meaningless. Yet one may then not expect to observe the systematic patterns of correlation, between job satisfaction and observed events and actions, that have been documented. Satisfaction has been found to influence subsequent labour market behaviour. It is a significant predictor of quits (Freeman, 1978) and is negatively related to absenteeism, non- and counter-productive work. Furthermore, it is related, in the expected direction, with other indicators of well-being: poor mental health, length of life and coronary heart disease (see Clark and Oswald, 1996). Such a pattern of results can probably not be reconciled with a purely idiosyncratic variable.

A more rigorous argument in favour of the ability of the researcher to make use of satisfaction data is found in Kahneman et al (1997) who argue that functions that relate subjective intensity to physical variables are similar for different types of people. They suggest the well-being of any event have a basic scale, pleasant, neutral, and unpleasant. Other scales may expand the positive or negative categories to a finer degree but the neutral case is a constant. It is argued the distinctiveness of this neutral value provides a focal point that allows some confidence in matching subjective experiences across time for a given individual and to support interpersonal comparisons.

[10] For an alternative discussion and approach to the satisfaction data see Rose (2000).

Whilst it has been suggested that satisfaction data do capture well-being, have descriptive power and can be compared between individuals and over time, they are nevertheless imperfect. They are qualitative, not quantitative, often banded and there remains considerable potential for measurement error, though this would be less easily handled if satisfaction were to be used as an independent variable.

3.5 Results

The measures of employer size adopted here are the number of employees within the establishment and the number of employees within the employing firm. To examine the impact of firm size upon wages it seems natural to focus upon private sector organisations and results that follow restrict attention to that sample.

3.5.1 Summary Statistics

The properties of the data sets are investigated in Tables 3.1a to 3.1c, and summary statistics presented. We define a small employer as an establishment with less than 25 employees for the BHPS and WERS, and less than 26 employees for the NCDS.

Within the BHPS, employees within small establishments are, on average, lower paid,[11] and have slightly lower levels of potential experience (age - years of schooling - six) and job tenure. Employees in large plants are more educated and more likely to have a degree or vocational qualification. At least with respect to observed characteristics, large workplaces are observed to be more skilled. Workers within large establishments are also more likely to be male, and to be working in a union recognised plant.

[11] Where hourly pay is defined as weekly pay divided by overtime adjusted hours, H+1.5OT.

The WERS samples establishments with ten or more employees, so there is likely to be a bias towards large employers in these data. This is reflected in the very high percentage of individuals working in plants with 25 or more employees. Results are, however, substantially similar to those for the BHPS. Workers in large establishments are, on average, more highly paid, have greater tenure, are slightly more experienced (here measured by age), and are slightly more educated. Employees in large plants are again more likely to be male, and to be working in a union recognised plant.

Table 3.1c examines the NCDS cohort when aged 33. The patterns in sample means are comparable to those for the BHPS, with interpretation as before. Large establishments employ individuals who receive higher wages, who are more educated and have greater levels of tenure, and who are more likely to be unionised.

3.5.2 Estimation strategy

To investigate issues in more detail we turn to regression analysis. Wages are here modeled as a function of personal characteristics (such as education, experience, gender and race) and employer characteristics (e.g. establishment size, firm size, industry, etc). Hourly pay[12] for individual i in time period[13] t and employer j, is then expressed in log-linear form as:

$$w_{itj} = x_{itj}'\beta + z_{tj}'\gamma + \varepsilon_{itj} \qquad i = 1, \ldots, n \qquad (6)$$

$$t = 1, \ldots, T$$

$$j = j(i, t) = 1, \ldots, m.$$

[12] The WERS pay data are observed only as a grouped variable. Here mid-points are taken and pay approximated as continuous. Estimates of pay equations using these mid-point and the more robust Grouped regression method of Stewart (1983a) yield practically identical predictions. Results were found not to be sensitive to alternative corrections at the upper tail.

[13] Respectively, t = 1 for the NCDS and for the WERS, and t=1, ..., 8 for the BHPS.

Where, w is log hourly pay, x the vector of worker characteristics, z the vector of employer characteristics, ε the conformable error term with mean zero and constant variance, and β and γ the vectors of parameters to be estimated.

For the BHPS, models are also estimated which account for the, potential, unobserved heterogeneity of employees, by including a person-specific effect upon wages (f_i) that is constant over time and potentially correlated with observed characteristics:

$$w_{it} = x_{it}'\beta + f_i + \varepsilon_{it} \qquad (7)$$

(For ease of presentation we subsume employer characteristics into x and drop the j subscript.) Implicitly this assumes a common market return to unobserved skill. The model can then be estimated by taking first differences (subtracting lagged values):

$$(w_{it} - w_{it-1}) = (x_{it} - x_{it-1})'\beta + (\varepsilon_{it} - \varepsilon_{it-1}) \qquad (8)$$

$$\Delta w_{it} = \Delta x_{it}'\beta + \Delta\varepsilon_{it} \qquad (9)$$

This methodology removes all, observed and unobserved, individual heterogeneity that remains constant over time and inference is driven by time-varying characteristics.

3.5.3 Employer Size and Wages

The effect of employer size upon wages is examined in Tables 3.2, 3.3 and 3.4, for the BHPS, WERS and NCDS respectively. Estimates generally match standard earnings equation predictions and attention is here restricted to the coefficients upon employer size.

For the BHPS, workplace size is identified by a banded categorical variable. The regression evidence, in Table 3.2, suggests hourly pay rises monotonically with establishment size, and all coefficients are observed to be statistically significantly

different from zero. Moving from a workplace with one or two employees to one with 1000 or more is predicted to increase wages by some 44 percent.[14] The comparable figure estimated by Green et al (1996) for the first wave of the BHPS was 54 percent.

Estimates for the WERS instead enter workplace size as a continuous variable, in natural logarithms. The coefficient upon log workplace employment is estimated to be 0.036. When the workplace size bands in the BHPS were replaced by a continuous proxy (using mid-points) the coefficient estimate was 0.041.[15] When attention is restricted to the WERS sampling frame the estimate, for the BHPS, is 0.035, suggesting comparable estimates between the data sets.

Within the WERS data the size of the employing organisation is also known.[16] The addition of this variable to the regression equation (column two, Table 3.3) attenuates the plant size coefficient, from 0.036 to 0.031, but the parameter remains statistically robust. The relationship between wages and firm size, holding constant plant size, is here estimated to be concave, with pay highest in medium size firms and statistically insignificantly different between the largest and smallest companies. The firm size-wage premium here peaks at 4.8 percent for employees working in firms with between 1,000 and 10,000 workers.

3.5.4 Comparability with previous evidence

How do these estimates compare with other UK and US evidence? For the UK, Green et al (1996) estimated the coefficient on log workplace size to lie in the range

[14] Calculated as: percentage effect = exp (β) – 1.
[15] The use of employee responses and mid-points may introduce measurement error into the plant size variable, and hence cause attenuation bias. Albæk et al (1998) investigate this issue using Scandinavian data, and find any bias to be negligible. This is due to the, observed, negative correlation of the error with true plant size. This mean reversion acts to offset attenuation bias.
[16] Both workplace size and firm size are employer reported.

0.037 to 0.053, using the 1990 WIRS establishment-level survey.[17] For the US, Brown and Medoff (1989) observed a range of estimates lying between 0.008 and 0.038, whilst the comparable estimate for Troske (1999) is 0.047. An alternative comparison is to examine the predicted wage for a worker moving from a plant with log employment one standard deviation below the mean to a plant with log employment one standard deviation above the mean. For the WERS data[18] this wage gain is predicted to be approximately 10 percent. Brown and Medoff (1989) estimated a wage gain of between 6 and 15 percent for the US. Estimates then seem comparable to previous UK and US evidence.

With respect to results concerning the size of the firm, Brown and Medoff (1989) also observe a more muted firm size effect upon wages, compared to the establishment size-wage premium. The parameter upon log firm size is estimated to be between 0.01 and 0.013 and in around half the cases is not statistically different from zero.

3.5.5 Labour Quality Differences: Correlates of Productivity

Along observed dimensions large workplaces employ more skilled – educated – workers (see Tables 3.1a to 3.1c). Whether the addition of a richer set of skill variables can then eliminate the size-wage correlation is investigated in Table 3.4, for the NCDS. Scores from reading and maths tests when respondents where aged seven and eleven (normalised to a percentage score) are assumed to capture aspects of worker ability. Test scores at age seven may reflect innate ability. Those at age eleven, the effects of social background upon schooling. An alternative correlate of

[17] This is the predecessor survey to the WERS, but does not include employee data and sampled only plants with at least 25 employees. Worker hours information is poor and the authors estimates equations upon weekly pay.
[18] Weighted mean of log workplace size equals 4.97. Standard deviation equals 1.57.

worker ability, whether the respondent was an IT user in 1991, is also examined. These controls are in addition to education qualification dummies.

The first two columns of Table 3.4 report estimates for the sample of all private sector employees, the next three columns those respondents with non-missing test scores. Coefficients with respect to workplace size and IT use are comparable in both samples. Workers in the largest size category are predicted to earn 36 percent more than otherwise comparable individuals working in a plant with less than ten workers. When we control for computer use this falls to 33 percent, but the establishment size coefficients remain robust. IT use itself is associated with a statistically significant wage premium of 12 percent. This is likely to combine both returns to new technology and labour quality variations.[19]

Test scores, both at age 7 and at age 11, enter with the expected sign. Wages are estimated to be 12 percent higher for those who answered all maths questions correctly at age 7, relative to those with zero scores. This effect is statistically significantly different from zero. The wage gain to reading ability at age 7 is 2 percent and not well determined. The effect of mathematics and reading scores at age 11 are both statistically insignificant at conventional confidence levels, with wage gains of around 5 and 7 percent respectively. Conditional upon education, childhood test scores appear to be uncorrelated with plant size, as parameter estimates upon workplace size are essentially unaffected, and if anything increase, after their introduction.[20] This remains true when we do not condition upon IT use.

Three potential correlates of labour quality are examined for the WERS in Table 3.5, the skill of co-workers, the use of technology, and the (average) time required for a worker to become proficient at the job. The skill of the workplace is

[19] The evidence of Entorf et al (1999) favours the latter view.
[20] When education is not included in the regression, test scores are correlated with establishment size and IT use in the expected way.

measured by the proportion of the *sampled* employees, within the plant, with degrees, with A or O-levels, with five years or more tenure, and aged 40 or more. As these are sample means we restrict attention to workplaces with at least 15 worker responses. All of these measures bar the age term enter the wage equation with a positive and statistically robust effect. The impact of these variables upon the establishment size parameter is to reduce it by some 15 percent, from 0.026 to 0.022, though it remains well determined. The estimated firm size coefficients are attenuated by around 30 percent, suggesting they, partly, capture the effect of worker skill.

The measure of technology use adopted is whether the respondent has access to email at work. As with the NCDS, this enters the wage equation positively and statistically significantly, with an associated premium of 10 percent. The workplace size parameter is attenuated by approximately 13 percent, but remains robust. The estimated firm size coefficients fall by some 30 percent, but remain jointly statistically significant. The time required for job proficiency is derived from the WERS manager questionnaire, and records the length of time, on average, it takes a new worker (entering the most common occupation) to reach the proficiency levels of an incumbent employee. Whilst earnings are, here, increasing in the length of time to proficiency, the coefficients upon plant and firm size are, in essence, unaffected.

The correlates of labour quality examined, here, then provide some support for the unobserved productivity hypothesis.

3.5.6 Labour Quality Differences: Fixed Effects Estimates

An alternative method of investigating the role of unobserved worker skill is to analyse how wages change over time. Such a technique removes unobserved

heterogeneity that is time invariant. This approach is followed in Table 3.6 for the BHPS. Column one estimates the wage equation for the sample of private sector employees observed in two consecutive periods – the unbalanced panel. Results are virtually identical to previously. Column two estimates the model transformed into first differences. The establishment size parameters are now considerably attenuated, and the largest size category is associated with an hourly pay premium of 5 percent. This is, however, statistically significantly different from zero at the five percent confidence level. This suggests unobserved person heterogeneity may be the major determinant of the establishment size-wage differential.

Measurement error may, however, contaminate estimates. Coefficients are identified by the change in wages of those individuals who change size category. Observed 'false' size changes then reduce the signal to noise ratio and attenuate coefficient estimates. In columns three and four of Table 3.6, attention is restricted to workers who have changed jobs and whose response (at time t) regarding the size of establishment in the previous year (t-1) matches their actual response at that time. For these individuals, it is proposed, an observed size change is more likely to be true. Cross-section parameter estimates, for this sample, are presented in column three and are comparable to those in column one. When the model is estimated in first differences (column four) the estimated plant size-wage effect is, however, nearly three times as large as that observed for the unbalanced panel (column two). A worker in the largest size category is predicted to earn a wage premium of 14 percent, relative to the smallest establishments. Coefficients are, however, not statistically well determined.

Whilst these estimates seem to indicate the importance of accounting for measurement error, endogenous job mobility may bias results. If voluntary job mobility is predominantly characterised by individuals moving successively from

small to large workplaces for higher pay, which reflects productivity differences, estimates will overstate the return to employer size. Yet where voluntary job mobility occurs both into small and large employers, the direction of any bias is uncertain.

The impact of controlling for firm heterogeneity is examined in Table 3.7. For a sub-sample of respondents in the BHPS, in 1991 and 1994, we are able to link the employee (via the company name) to their employing organisation's company accounts data.[21] These data are potentially biased toward older and larger employers, though not necessarily establishments, for whom company accounts data are available (see chapter 2, section 4). Given a relatively small number of observations, approximately 400 individuals, the establishment size dummies are consolidated into a smaller number of categories.[22] Furthermore, as these data are limited to two periods, attention is restricted to the balanced panel.

Column one of Table 3.7 estimates wage equations for the BHPS, for the years 1991 and 1994, column two of Table 3.7 for the sub-sample with linked employer data. The linked sample is found to exhibit lower, but still substantial and statistically significant, returns to workplace size. The role of unobserved firm heterogeneity upon pay is examined in column four, and a firm fixed effect entered in the wage equation. This is assumed common to all workers within the organisation and captures all effects of the firm, upon pay, that remain constant over time, and also the mean person effect within the firm.[23] Parameter estimates suggest the higher pay in the very largest plants, in the linked sample, do partially capture an employer effect. In the cross-section, plants with 1000 or more workers

[21] Data made available by Andrew Hildreth. The company accounts data were originally obtained from the Dun and Bradstreet files and are thus not restricted to publicly listed companies.
[22] Industry is, here, coded at the one-digit level, for all other equations the two-digit level.
[23] This is particularly problematic for these data as for 56 percent of observations only 1 person is observed within the firm.

are predicted to pay wages 27 percent higher than establishments with less than ten employees. This figure falls to 17 percent when firm effects are added. Whether this reflects the mean skill of workers or a pure employer differential cannot, here, be ascertained. Nevertheless, the effect of workplace size upon wages remains sizeable and statistically well determined suggesting it is, here, robust to firm heterogeneity.

Controls for unobserved person heterogeneity are added in columns five and six of Table 3.7. Column five displays the result of estimating a first difference model for the BHPS between 1991 and 1994. Examining the change in wages over a longer time span has increased – or rather lessened the decline of – the estimated response of wages to workplace size, compared to Table 3.6. The wage premium associated with working in the largest plants, relative to the smallest, is estimated to fall by approximately a half, from 37 percent in the cross-section estimates, to 19 percent once the model is estimated in first differences. Results remain statistically significant. This may be due to observing a greater number of true changes in workplace size, and more changes in size within the same workplace, increasing the signal in the model.

Column six controls for unobserved heterogeneity of both workers and firms, in the sample with employer data. It specifies a worker fixed effect specific to each worker-firm pair, and examines how an individual's wages change over time within the same employer. This does not eliminate the possibility that endogenous mobility contaminates results, though it must now be associated with internal mobility or with exiting and rejoining the employer within the three-year period. The addition of controls for person, as well as firm, heterogeneity further weakens the predicted size-wage relation, which is no longer statistically well determined. The wage premium in the largest establishments is, however, still observed to be non-negligible at around 10 percent.

The evidence is then supportive of the hypothesis that the relationship between wages and establishment size captures, to some degree, unobserved worker quality differences. A significant unexplained effect does, however, remain.

3.5.7 Alternative Explanations of the Employer size-wage Correlation

Alternative explanations for a positive relationship between employer size and wages are investigated in Table 3.7 and Table 3.8.

Table 3.7 examines the BHPS sub-sample for which employer data are available. The addition of more refined controls for firm characteristics, the firm's capital to labour ratio and profits per employee, attenuates the estimated establishment size coefficients by around 25 percent, but they remain sizeable and statistically significant. The coefficient upon firm employment is, here, estimated to be weak and statistically insignificantly different from zero. Experimentation with non-linear terms did not alter this conclusion. The log capital-to-labour ratio is found to exert a well-determined positive effect upon wages, with an estimated parameter of 0.053. Profits per worker enters pay positively and statistically significantly with an estimated parameter of 0.050[24], despite the potential downward bias due to the simultaneity of wages and profits. This is consistent with behaviour observed, for individual data, by Hildreth (1998) and Troske (1999).

Column five of Table 3.7 adds controls for firm heterogeneity, by including a firm fixed effect upon pay. The parameter upon firm profitability is driven to zero whilst the coefficient upon the capital-to-labour ratio is just over a third of that observed previously. Both are statistically insignificant. As noted in the previous section, the workplace size parameters are here largely robust to the inclusion of firm, but possibly not worker, fixed effects. The size-wage relation then largely

remains after conditioning upon firm characteristics, and rent-sharing does not seem to offer a convincing explanation for the establishment size-wage differential.

The monitoring hypothesis is analysed in Table 3.8 using the WERS data. The establishment's monitoring intensity is here measured by the proportion of supervisors amongst non-managerial staff (as defined by the manager), and attention is restricted to employees within non-managerial and non-professional occupations. The plant's monitoring intensity is found to enter the wage equation negatively and statistically significantly. In line with results observed for the US (Troske, 1999), more intensive supervision predicts lower pay. However, the coefficients upon establishment and firm size are largely unchanged, and indeed slightly increase. The monitoring hypothesis does not then appear, here, a convincing avenue by which to explain the relationship between employer size and pay.

The union avoidance hypothesis is here largely discounted, as the establishment size-wage differential was observed as positive and statistically well determined in the union sample. Moreover, robust positive effects were observed amongst non-unionised workplaces within industries with a very low union intensity (the wholesale and retail trades, hotels and catering), where union threat effects should be negligible.[25] The search model of Burdett and Mortensen (1998) cannot here be directly analysed, as we lack good measures of search costs or monopsony power. However, existing evidence is not necessarily consistent with such a model explaining the relationship between employer size and wages. The estimates of van den Berg and Ridder (1998) suggest search frictions, which generate employers' monopsony power and the positive size-wage profile, are small. Brown and Medoff (1989) also find no evidence of a weaker size-pay relationship where the employer is

[24] The estimated elasticity of wages with respect to profitability, at the mean, is then 0.022.
[25] These results were found both for the WERS and BHPS data, and match the earlier findings of Brown and Medoff (1989) and Green et al (1996).

small relative to the hiring pool, where monopsony power would be expected to be smaller.

3.5.8 *Job satisfaction and Worker Well-being*

The potential for compensating differentials to explain the relationship between employer size and wages is now examined using job satisfaction data.

Worker well-being is modeled as a function of personal characteristics (such as education, experience, gender and race), employer characteristics (e.g. establishment size, firm size, industry, etc) and variables associated with the labour contract (income, hours of work, occupation). Job satisfaction for individual i in time t and employer j, is then expressed:

$$s^*_{itj} = y_{itj}'\varphi + x_{itj}'\beta + z_{tj}'\gamma + u_{itj} \qquad i = 1, \ldots, n \qquad (10)$$

$$t = 1, \ldots, T$$

$$j = j(i, t) = 1, \ldots, m.$$

Where, s* is the satisfaction variable, y the vector of pay and hours variables, x the vector of worker characteristics, z the vector of employer characteristics, u the conformable error term with mean zero and constant variance, and φ, β and γ the vectors of parameters to be estimated.[26]

The BHPS and WERS satisfaction data are observed as ordered categorical responses (on a scale 1, 2, ..., K). These map latent well-being (s*) into discrete scores (s) as below:

$$s_{itj} = k \qquad \text{if } \mu_{k-1} < s^* \leq \mu_k \; \forall \; k = 1, \ldots, K \qquad (11)$$

Estimation is then by the Ordered Probit technique of McKelvey and Zavoina (1975). This imposes the restrictions; $\mu_o \leq \mu_1 \leq \ldots \leq \mu_K$; $\mu_o = -\infty$; $\mu_K = \infty$; and the

[26] As with all models of job satisfaction this implicitly assumes responses are cardinal.

normalisation $\mu_1 = 0$ and $\sigma_U = 1$. The probability of observing a response within a category, k, is then:

$$\Pr(s = k) = \Phi(\mu_k - y_{itj}'\varphi - x_{itj}'\beta - z_{tj}'\gamma) - \Phi(\mu_{k-1} - y_{itj}'\varphi - x_{itj}'\beta - z_{tj}'\gamma) \qquad (12)$$

Where, $\Phi(.)$ is the standard normal distribution function and parameters are estimated by maximum likelihood. Positive coefficients indicate higher levels of satisfaction are more likely.

Table 3.9a estimates job satisfaction equations for the BHPS data. Column one reports that levels of overall of job satisfaction in large plants are statistically significantly lower than within establishments with one or two employees. The relationship exhibits a moderate u-shape, with satisfaction lowest in medium-sized plants.[27] Table 3.9b reports the marginal effects of the estimates. Compared to working in a plant with 1 to 2 employees, an employee working in a plant with between 100 and 199 workers is predicted to be 1.6 percent more likely to respond with the lowest level of satisfaction and -9.4 percent less likely the highest. For employees in the largest plants, 1000 workers or more, the comparable figures are 1.2 percent and -7.9 percent respectively.

Column two of Table 3.9a demonstrates that the higher levels of pay in large plants do mitigate some of this dissatisfaction, when pay is omitted the size-satisfaction profile flattens but remains statistically well determined.

The job characteristic's with which dissatisfaction, within large plants, is felt most strongly is examined in Table 3.10. In column one overall satisfaction is the dependent variable. In columns two to eight, the item-specific satisfaction questions. In all cases there is a negative effect of workplace size upon satisfaction, which is nearly always most pronounced in medium size establishments. Only with

[27] If we instead estimate the model for the years 1992 to 1997, omitting the 1991 survey, very similar results are produced.

respect to job security does workplace size exert a negligible and statistically insignificant effect. The effects upon satisfaction with hours and promotion prospects are, however, less pronounced. Dissatisfaction within large plants is here most conspicuous with respect to aspects of employment that can be broadly related to the degree of worker freedom – the use of initiative and the work itself.

The WERS satisfaction data are examined in Table 3.11a. Satisfaction with respect to influence over the job and the sense of achievement both exhibit the negative and statistically significant effect of workplace size observed for the BHPS. For satisfaction with pay and the respect of management, the establishment size coefficient is negative, but not well determined. This in part reflects the WERS exclusion of workplaces with less than 10 employees. For the BHPS (Table 3.10), satisfaction with respect to pay is relatively flat as we move from a plant with 10 to 24 workers to an establishments with over a 1000 employees.

Table 3.11b examines the influence of firm size upon satisfaction. All four columns show a marked, and jointly statistically significant, negative effect of firm size upon satisfaction. The estimates also suggest a considerable part of the dissatisfaction associated with large plants, in Table 3.11a, is actually attributable to the size of the parent firm. For both satisfaction with influence and satisfaction with achievement, after holding firm size constant, the establishment size parameter is attenuated by over a half and is no longer statistically significantly different from zero. For satisfaction with pay and satisfaction the respect of management, the effect of plant size is indeed now positive, though not statistically well determined.

Alternative measures of well-being are analysed, for the WERS, in Table 3.12a. Respondents were asked to state their degree of agreement with four statements regarding their job; whether it required hard work, whether they lacked time, whether they felt secure in the workplace, and whether they worried about

their job. Estimation is by the Ordered Probit technique and positive coefficients signify agreement is more likely. Conditions appear, if anything, superior in large plants. Employees in large plants are statistically significantly less likely to agree that they lack time or worry, and more likely to feel secure. Results with respect to hard work, whilst negative, are, however, not robust. The effect of firm size is again to reduce well-being, as employees are more likely to agree they worry, feel their job requires hard work, or lack time and disagree that they feel secure. Results are, however, statistically well determined only in the last two cases.

Table 3.12b examines the extent of worker influence over the job. Again equations are estimated by Ordered Probit. Positive coefficients represent greater worker influence. Complementary to the evidence for the BHPS, in Table 3.10, large employers allow less influence over the range of tasks, the pace of work, and the method by which work is done. Whilst the coefficient upon workplace size is robust only with respect to influence over the job tasks, the firm size effects enter negatively and statistically significantly in all three cases.

The evidence suggests firm size exerts a marked, and statistically significant, negative effect upon worker well-being, whilst establishment size exerts a rather weaker influence. Possible rationales for such a finding may lay in more prescriptive, rigid, and bureaucratic working practices in large organisations. These may facilitate centralised management of a large organisation, but potentially may also lead to diminished levels of involvement and status. Some evidence consistent with this hypothesis is observed in Table 3.12b, large firms allow significantly less worker influence over the job. Furthermore, the addition of the worker influence variables to the job satisfaction equations attenuated the coefficients upon firm size, but there remained a large, and statistically significant, negative firm size effect. A

complete explanation of the source of this firm size effect is, nevertheless, not here possible.

3.5.9 Can employee dissatisfaction explain the employer size-wage premium?

Within both the BHPS and WERS, large employers are associated with lower levels of worker well-being for otherwise comparable individuals. Yet it is doubtful, given the evidence here, that a compensating-differential-for-size theory can explain the establishment size-wage differential, though it may help to explain the firm size-wage premium.

For the BHPS, we observe a u-shaped relationship between workplace size and job satisfaction, with well-being lowest in medium-sized plants. Indeed, overall satisfaction scores are relatively flat amongst plants with more than 50 employees. In contrast wages increase monotonically with establishment size. The compensating differential explanation may then have a role in explaining the lower wages observed for the very smallest plants, but seems limited in its ability to explain the positive relationship between wages and plant size observed for medium-sized and large establishments.

The WERS samples workplaces with 10 or more employees, and so omits the very smallest establishments were satisfaction is highest. Nevertheless, within these medium-sized and large workplaces, worker well-being is shown to be lower in large plants. Yet once we condition upon firm size no independent, and statistically robust, effect of establishment size is observed. There is then little evidence, in these data, of employee distaste for working in large workplaces, independent of the size of the parent organisation. In contrast, wages are observed to be statistically significantly greater in large plants, holding firm size constant.

Employee distaste for employer size, as captured by job satisfaction, does not then, here, appear to explain the relationship between workplace size and pay. The finding of a robust negative effect of firm size upon job satisfaction is, however, consistent with employee distaste for working within large companies, and may then help to explain the pay premium earned by workers in large firms.

How do these findings, regarding worker satisfaction and employer size, fit with alternative explanations of a positive correlation between plant size and wages? The models of rent-sharing and search frictions imply large employers pay higher wages for equally productive employees. The size-wage premium then represents a rent to workers and one may expect a satisfaction premium in large plants. The evidence, here, contradicts this is in virtually all cases. There is, however, some weak evidence that satisfaction with pay may be greater in large plants, relative to medium-sized establishments. If wages solely reflect worker productivity then pay, and job satisfaction, should be independent of employer size. Yet if large firms or establishments select more ambitious employees then, for similar working conditions, well-being may be observed to be lower. Alternatively, if large employers recruit high achieving employees, satisfaction may be observed to be greater. Predictions concerning satisfaction, with regards unobserved worker skill differences, are then ambiguous.

3.6 Conclusion

This chapter has examined four explanations for the observed positive relationship between employer size and worker pay: unobserved productivity differences, compensating differentials, rent-sharing and differences in monitoring intensity.

The model of unobserved productivity differences finds most favour in explaining the establishment size-wage premium.

The elasticity of wages with respect to workplace size, in the private sector, is estimated to be approximately 0.04. Correlates of worker skill, such as the use of information technology and the skill of the establishment's labour force, are found to explain up to 15 percent of the plant size-wage differential, and up to 30 percent of the firm size-wage premium. Both the establishment size and firm size parameters, however, remain robustly positive. Controlling for unobserved individual heterogeneity reduces the estimated wage gain of moving from a plant with one or two employees to one with 1000 workers or more, from 47 percent to 5 percent. When account is taken of measurement error this estimate rises to 15 percent. Alternatively, when wage changes over a three-year period are examined, where a higher prevalence of true changes in workplace size are likely, the estimated effect is larger still, and statistically significant. Hence, whilst worker skill accounts for a significant share of the size-wage relation a non-trivial unexplained effect remains. Workers in large establishments do apparently earn more in the UK.

Using employer-employee data we observe a concave positive effect of firm size upon worker wages, holding constant the size of the establishment. The firm's capital to labour ratio and profits per head are found to enter the wage equation positively and statistically significantly, yet their influence upon the workplace size coefficients are limited. Whether large employers pay higher wages to economise on the costs of supervision is tested using measures of monitoring intensity. These enter wages with the expected negative sign, and are statistically well determined, but do not perturb the parameter upon workplace size. There is hence no strong evidence, here, in favour of either rent-sharing or monitoring costs explaining the size-wage correlation.

Job satisfaction data are used to test whether the relationship between employer size and wages reflects a compensating differential. For the BHPS and WERS, job satisfaction levels are statistically significantly higher in small plants. Whilst this is supportive of lower levels of well-being in large workplaces, it does not seem to offer a complete explanation of the behaviour of wages. For the BHPS, job satisfaction is markedly superior in the smallest plants. Yet differences in satisfaction between medium-sized and large establishments are not pronounced, whilst wages are substantially higher. On this evidence, employee distaste for establishment size does not seem to offer a convincing route as to why pay is greater in the largest plants, relative to medium-sized establishments.

The WERS data also demonstrate that much of the dissatisfaction associated with workplace size is attributable to the size of the parent company, holding constant firm size we observe no robust independent influence of establishment size upon job satisfaction. This is, potentially, consistent with a compensating differential with respect to firm size, but does not explain the observed relationship between workplace size and pay.

Unobserved productivity differences, here captured by correlates of worker skill and person fixed-effects, then find most favour in explaining the establishment size-wage premium, capturing up to half of the observed differential.

TABLE 3.1a
BHPS Sample Means (1991-1998)
Private Sector Employees

	SMALL	*LARGE*
Hourly Pay	4.71 (3.78)	6.15 (4.01)
Potential Experience	19.21 (12.50)	19.52 (11.80)
Tenure	4.32 (5.72)	4.67 (5.71)
Years of Schooling	11.15 (2.59)	11.35 (2.71)
Qualification: HND	0.03 (0.18)	0.07 (0.25)
Qualification: Degree	0.08 (0.26)	0.11 (0.31)
Male	0.48 (0.50)	0.60 (0.49)
Temporary Job	0.06 (0.24)	0.05 (0.22)
Union Recognised at workplace	0.17 (0.38)	0.46 (0.50)
Number of Individuals	3122	4163
Number of Observations	8086	14441

- Small denotes workplaces with less than 25 employees, large plants with 25 workers or more.
- Standard deviations are in parentheses. Pay is deflated to 1991 values.
- Total number of individuals in sample = 5866.

TABLE 3.1b
WERS Sample Means (1998)
Private Sector Employees

	SMALL	*LARGE*
Hourly Pay	5.65 (5.62)	6.85 (4.67)
Age: 40 or more	0.43 (0.50)	0.44 (0.50)
Tenure: 5 years or more	0.38 (0.49)	0.47 (0.50)
Qualification: Degree	0.16 (0.36)	0.17 (0.37)
Male	0.42 (0.49)	0.58 (0.49)
Temporary Job	0.10 (0.29)	0.05 (0.21)
Union Recognised at workplace	0.16 (0.36)	0.50 (0.50)
Number of Workplaces	159	952
Number of Observations	1337	14281

- Small denotes workplaces with less than 25 employees, large plants with 25 workers or more.
- Standard deviations are in parentheses. Statistics use WERS sample weights.

TABLE 3.1c
NCDS Sample Means (1991 – Age 33)
Private Sector Employees

	SMALL	*LARGE*
Hourly Pay	5.63 (4.92)	7.65 (5.51)
Tenure	4.67 (4.79)	6.65 (5.54)
Qualification: Degree	0.09 (0.29)	0.13 (0.34)
Male	0.45 (0.50)	0.65 (0.48)
Temporary Job	0.04 (0.17)	0.04 (0.20)
Union member	0.14 (0.34)	0.37 (0.48)
Number of Individuals	1056	1960

- Small denotes workplaces with less than 26 employees, large plants with 26 workers or more.
- Standard deviations are in parentheses.

TABLE 3.2
Workplace Size and Wages (BHPS)
Private Sector Employees
Dependent Variable: Ln(wage)

REGRESSOR	*ALL*
Workplace Size:3-9	0.113
	(0.025)
Workplace Size:10-24	0.193
	(0.026)
Workplace Size:25-49	0.218
	(0.027)
Workplace Size:50-99	0.272
	(0.027)
Workplace Size:100-199	0.273
	(0.027)
Workplace Size:200-499	0.295
	(0.027)
Workplace Size:500-999	0.329
	(0.028)
Workplace Size:1000+	0.362
	(0.029)
Observations	
Individuals	5866
Panel Total	22528
Adjusted R^2	0.55

1. All regressions also include quadratics in potential experience and job tenure, and controls for temporary job, education, gender, race, union recognition, occupation (SOC Code at the one-digit level), industry (SIC code at the two-digit level), region, marital status, and time period. Parameter estimates are not reported.
2. Standard errors are in parentheses and are robust to arbitrary heteroscedasticity and the repeat sampling of individuals over time.
3. The workplace size coefficients are with respect to the omitted category, 1-2 employees.

TABLE 3.3
Employer Size and Wages (WERS)
Private Sector Employees
Dependent Variable: Ln(wage)

REGRESSOR	*ALL*	*ALL*
Ln(workplace size)	0.036	0.031
	(0.006)	(0.006)
Firm Size:100-999		0.028
		(0.019)
Firm Size:1,000-9,999		0.048
		(0.020)
Firm Size:10,000+		0.045
		(0.023)
Firm Size:50,000+		0.005
		(0.025)
Observations		
Workplaces	1111	1111
Individuals	15618	15618
Adjusted R^2	0.62	0.62

1. All regressions also include controls for age, tenure, temporary job, education, gender, race, union recognition, occupation (SOC Code at the one-digit level), industry (SIC code at the two-digit level), region and marital status. Coefficients are not reported.
2. Standard errors are in parentheses and are robust to arbitrary heteroscedasticity and the repeat sampling of individuals within establishments.
3. The firm size coefficients are with respect to the omitted category, 1-99 employees.
4. Mean value ln(workplace size) is 4.97, standard deviation 1.57

TABLE 3.4
The Effect of Worker Skill (NCDS Age 33)
Private Sector Employees
Dependent Variable: Ln(wage)

REGRESSOR	*ALL*	*ALL*	*TEST SCORES*	*TEST SCORES*	*TEST SCORES*
Workplace Size:11-25	0.137	0.134	0.129	0.126	0.130
	(0.024)	(0.024)	(0.027)	(0.027)	(0.027)
Workplace Size:26-99	0.176	0.167	0.178	0.168	0.173
	(0.023)	(0.023)	(0.025)	(0.025)	(0.025)
Workplace Size:100-499	0.236	0.220	0.231	0.215	0.224
	(0.023)	(0.023)	(0.026)	(0.026)	(0.026)
Workplace Size:500+	0.304	0.284	0.304	0.282	0.288
	(0.027)	(0.027)	(0.030)	(0.030)	(0.030)
IT		0.117		0.121	0.113
		(0.018)		(0.021)	(0.020)
Math Score Age 7					0.114
					(0.038)
Math Score Age11					0.046
					(0.051)
Reading Score Age 7					0.021
					(0.042)
Reading Score Age 11					0.067
					(0.069)
Other Qualification	0.042	0.027	0.036	0.021	0.007
	(0.023)	(0.023)	(0.026)	(0.026)	(0.026)
O-Level	0.116	0.092	0.098	0.075	0.042
	(0.022)	(0.022)	(0.025)	(0.024)	(0.026)
A-Level	0.196	0.171	0.179	0.156	0.111
	(0.026)	(0.026)	(0.028)	(0.028)	(0.030)
Other Higher	0.310	0.272	0.293	0.255	0.206
	(0.030)	(0.030)	(0.034)	(0.034)	(0.036)
Degree	0.458	0.418	0.433	0.393	0.314
	(0.033)	(0.034)	(0.037)	(0.038)	(0.043)
Observations	3016	3016	2388	2388	2388
Adjusted R^2	0.58	0.58	0.56	0.57	0.58

1. All regressions also include a quadratic in job tenure, and controls for, temporary job, education, gender, race, union membership, occupation (SOC Code at the one-digit level), industry (SIC code at the two-digit level), region and marital status. Parameter estimates are not reported.
2. Standard errors are in parentheses and are robust to arbitrary heteroscedasticity.
3. The workplace size coefficients are with respect to the omitted category, 1-2 employees.

TABLE 3.5
Proxies for Worker Skill (WERS)
Private Sector Employees
Dependent Variable: Ln(wage)

REGRESSOR	*15 PLUS*	*15 PLUS*	*ALL*	*ALL*	*ALL*	*ALL*
Ln(workplace size)	0.026	0.022	0.030	0.026	0.030	0.031
	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Firm Size:100-999	0.043	0.021	0.028	0.018	0.028	0.029
	(0.022)	(0.021)	(0.020)	(0.019)	(0.020)	(0.019)
Firm Size:1,000-9,999	0.068	0.046	0.053	0.042	0.052	0.053
	(0.024)	(0.022)	(0.020)	(0.019)	(0.020)	(0.019)
Firm Size:10,000+	0.068	0.051	0.052	0.038	0.049	0.048
	(0.027)	(0.026)	(0.023)	(0.022)	(0.023)	(0.023)
Firm Size:50,000+	0.040	0.028	0.008	-0.002	0.009	0.008
	(0.030)	(0.028)	(0.026)	(0.025)	(0.025)	(0.025)
Proportion Degree		0.533				
		(0.044)				
Proportion O/A-Level		0.324				
		(0.048)				
Proportion Tenure 5yr+		0.122				
		(0.035)				
Proportion Age 40+		-0.019				
		(0.045)				
Email				0.096		
				(0.009)		
Proficiency: 1week-1month						0.092
						(0.030)
Proficiency: 1-6months						0.119
						(0.028)
Proficiency: 6months-1year						0.102
						(0.030)
Proficiency: 1year+						0.165
						(0.038)
Observations						
Workplaces	677	677	1109	1109	1095	1095
Individuals	12098	12098	14344	14344	15397	15397
Adjusted R^2	0.63	0.64	0.62	0.63	0.62	0.62

1. See notes to Table 3.3
2. The workplace proportions are based upon the employee sample data so attention is restricted to those workplaces where we observe 15 non-missing responses.
3. Email refers to whether the respondent has access to email.
4. Proficiency is a manager derived variable and measures the length of time for a new employee, in the largest occupational group, to become proficient at their job (the omitted category is less than one week).

TABLE 3.6
Controlling for Unobserved Worker Fixed-Effects (BHPS)
Private Sector Employees
Dependent Variable: Ln(wage)/ ΔLn(wage)

REGRESSOR	*OLS PANEL*	*FD PANEL*	*OLS CHANGE JOBS*	*FD CHANGE JOBS*
Workplace Size:3-9	0.114	0.004	0.071	0.081
	(0.029)	(0.020)	(0.058)	(0.069)
Workplace Size:10-24	0.201	0.017	0.149	0.086
	(0.031)	(0.021)	(0.059)	(0.070)
Workplace Size:25-49	0.223	0.016	0.165	0.082
	(0.031)	(0.022)	(0.059)	(0.071)
Workplace Size:50-99	0.280	0.017	0.245	0.121
	(0.032)	(0.022)	(0.060)	(0.075)
Workplace Size:100-199	0.275	0.016	0.233	0.124
	(0.032)	(0.022)	(0.062)	(0.080)
Workplace Size:200-499	0.299	0.023	0.290	0.128
	(0.031)	(0.022)	(0.060)	(0.081)
Workplace Size:500-999	0.329	0.021	0.298	0.088
	(0.033)	(0.023)	(0.064)	(0.090)
Workplace Size:1000+	0.371	0.051	0.369	0.142
	(0.033)	(0.024)	(0.063)	(0.085)
Observations				
Individuals	4041	4041	1245	1245
Panel Total	19733	15278	3177	1703
Adjusted R^2	0.56	0.02	0.58	0.05

1. See notes for Table 3.2.

TABLE 3.7
The Role of Firm Characteristics in the Linked Worker-Firm Sample (BHPS-DB)
Private Sector Employees
Dependent Variable: Ln(wage)/ ΔLn(wage)

REGRESSOR	*OLS BHPS*	*OLS FIRM*	*OLS FIRM*	*OLS FIRM*	*FD BHPS*	*FD FIRM*
Workplace size:10-49	0.150	0.134	0.108	0.142	0.094	0.112
	(0.022)	(0.057)	(0.060)	(0.057)	(0.033)	(0.074)
Workplace size:50-199	0.216	0.106	0.073	0.090	0.102	0.041
	(0.025)	(0.055)	(0.059)	(0.058)	(0.034)	(0.079)
Workplace size:200-999	0.238	0.098	0.064	0.125	0.094	0.044
	(0.025)	(0.058)	(0.062)	(0.063)	(0.036)	(0.085)
Workplace size:1000+	0.316	0.239	0.183	0.158	0.172	0.096
	(0.030)	(0.065)	(0.067)	(0.069)	(0.041)	(0.089)
Profits per employee			0.050	0.004		0.006
			(0.015)	(0.015)		(0.010)
Log(Capital Labour Ratio)			0.053	0.020		0.017
			(0.014)	(0.022)		(0.015)
Log(Firm Employment)			0.007	0.006		0.037
			(0.008)	(0.050)		(0.052)
Firm Effects	No	No	No	Yes	No	Yes
Observations						
Firms	-	267	267	267	-	267
Workers	1755	387	387	387	1755	387
Panel Total	3510	774	774	774	1755	387
R^2	0.55	0.57	0.60	0.78	0.05	0.01

1. See notes for Table 3.2.
2. The BHPS estimates refer to the sample of private sector employees in 1991 and 1994. This is the comparable sample, to the Linked Employer-Employee data.
3. The BHPS first difference model captures a person fixed effect, the matched model a worker-firm fixed effect.
4. Capital and profits are in £000s (1991 values).
5. Mean value profits per head is 0.434, standard deviation 0.954.

TABLE 3.8
Monitoring and the Employer Size-Wage Relationship (WERS)
Private Sector Employees
Dependent Variable: Ln(wage)

REGRESSOR	*NON-MANAGERIAL*	*NON-MANAGERIAL*
Ln(workplace size)	0.029	0.031
	(0.006)	(0.006)
Firm Size:100-999	0.030	0.032
	(0.020)	(0.020)
Firm Size:1,000-9,999	0.055	0.057
	(0.020)	(0.020)
Firm Size:10,000+	0.058	0.056
	(0.023)	(0.023)
Firm Size:50,000+	0.013	0.013
	(0.026)	(0.026)
Supervisors per worker:1-19%		-0.050
		(0.026)
Supervisors per worker:20-39%		-0.069
		(0.027)
Supervisors per worker:40% +		-0.057
		(0.030)
Observations		
Workplaces	1104	1104
Individuals	11909	11909
R^2	0.50	0.51

1. See notes to Table 3.3.
2. Managerial employees here includes managers and administrators (SOC major group), and other (SOC group 2). The remaining SOC groups are then present in the sample.
3. The number of supervisors per worker refers to the proportion of non-managerial employees that have job duties that involve supervising other employees. This includes line-managers and foremen. The omitted category is zero supervisors per employee.

TABLE 3.9a
Workplace Size and Worker Well-being (BHPS)
Private Sector Employees
Dependent Variable: Overall Job Satisfaction

REGRESSOR	*ALL*	*ALL*
Workplace Size:3-9	-0.118	-0.107
	(0.077)	(0.077)
Workplace Size:10-24	-0.236	-0.211
	(0.079)	(0.079)
Workplace Size:25-49	-0.237	-0.206
	(0.081)	(0.080)
Workplace Size:50-99	-0.350	-0.313
	(0.081)	(0.080)
Workplace Size:100-199	-0.357	-0.320
	(0.082)	(0.081)
Workplace Size:200-499	-0.351	-0.311
	(0.082)	(0.080)
Workplace Size:500-999	-0.343	-0.298
	(0.086)	(0.085)
Workplace Size:1000+	-0.290	-0.242
	(0.088)	(0.087)
Ln(pay)	0.113	
	(0.030)	
Observations		
Individuals	5127	5127
Panel Total	16723	16723
Log-L	-26189.7	-26202.6
Pseudo R^2	0.087	0.085

1. See notes for Table 3.2. Additionally normal hours are controlled for.
2. All columns are estimated by the Ordered Probit technique.
3. Consistent Job satisfaction data cover only the period 1991-1997.
4. The Pseudo R^2 is calculated using the method of McKelvey and Zavoina (1975).

TABLE 3.9b
Marginal Effects upon Overall Job Satisfaction

	Overall Satisfaction Score						
Variable	*1*	*2*	*3*	*4*	*5*	*6*	*7*
Workplace Size:3-9	0.004	0.004	0.009	0.011	0.016	-0.009	-0.035
Workplace Size:10-24	0.009	0.009	0.019	0.022	0.030	-0.023	-0.066
Workplace Size:25-49	0.009	0.009	0.019	0.022	0.030	-0.024	-0.066
Workplace Size:50-99	0.015	0.015	0.030	0.032	0.042	-0.042	-0.093
Workplace Size:100-199	0.016	0.015	0.031	0.033	0.042	-0.043	-0.094
Workplace Size:200-499	0.015	0.015	0.030	0.032	0.042	-0.042	-0.093
Workplace Size:500-999	0.015	0.015	0.030	0.032	0.041	-0.040	-0.091
Workplace Size:1000+	0.012	0.012	0.024	0.027	0.036	-0.032	-0.079
Ln(pay)	-0.005	-0.005	-0.010	-0.010	-0.011	0.015	0.025

1. Marginal effects are based upon column one of Table 3.9a above.
2. Marginal Effects for the workplace size dummies are calculated as the difference in the predicted probability, of satisfaction score k, of that size category relative to the omitted base.
3. Marginal effects for ln(pay) are calculated as the change in predicted probability associated with moving half a standard deviation below to half a standard deviation above the mean.

TABLE 3.10
Workplace Size and Job Satisfaction (BHPS)
Private Sector Employees
Dependent Variable: Item specific Job Satisfaction Scores

REGRESSOR	*Overall* ALL	*Promotion* ALL	*Pay* ALL	*Boss* ALL	*Security* ALL	*Initiative* ALL	*Work Itself* ALL	*Hours* ALL
Workplace Size:3-9	-0.118	-0.011	-0.224	-0.105	0.045	-0.191	-0.146	-0.078
	(0.077)	(0.075)	(0.077)	(0.077)	(0.076)	(0.076)	(0.074)	(0.072)
Workplace Size:10-24	-0.236	-0.121	-0.333	-0.279	-0.041	-0.339	-0.292	-0.162
	(0.079)	(0.077)	(0.079)	(0.080)	(0.078)	(0.079)	(0.076)	(0.074)
Workplace Size:25-49	-0.237	-0.136	-0.347	-0.259	-0.032	-0.368	-0.335	-0.131
	(0.081)	(0.078)	(0.080)	(0.081)	(0.080)	(0.079)	(0.078)	(0.075)
Workplace Size:50-99	-0.350	-0.181	-0.358	-0.337	-0.038	-0.433	-0.452	-0.203
	(0.081)	(0.077)	(0.081)	(0.081)	(0.079)	(0.080)	(0.078)	(0.075)
Workplace Size:100-199	-0.357	-0.174	-0.347	-0.383	-0.098	-0.500	-0.412	-0.228
	(0.082)	(0.079)	(0.081)	(0.082)	(0.080)	(0.081)	(0.078)	(0.075)
Workplace Size:200-499	-0.351	-0.165	-0.367	-0.379	-0.023	-0.493	-0.448	-0.244
	(0.082)	(0.079)	(0.081)	(0.081)	(0.080)	(0.080)	(0.078)	(0.075)
Workplace Size:500-999	-0.343	-0.188	-0.354	-0.428	-0.059	-0.491	-0.453	-0.219
	(0.086)	(0.083)	(0.085)	(0.086)	(0.085)	(0.086)	(0.083)	(0.080)
Workplace Size:1000+	-0.290	-0.101	-0.286	-0.334	-0.012	-0.500	-0.427	-0.172
	(0.088)	(0.085)	(0.086)	(0.087)	(0.086)	(0.087)	(0.085)	(0.081)
Ln(pay)	0.113	0.125	0.651	-0.063	-0.017	0.190	0.080	0.038
	(0.030)	(0.030)	(0.034)	(0.028)	(0.029)	(0.030)	(0.029)	(0.030)
Observations								
Individuals	5127	5127	5127	5127	5127	5127	5127	5127
Panel Total	16723	16723	16723	16723	16723	16723	16723	16723
Log-L	-26189.7	-30700.2	-29709.9	-26630.4	-28327.0	-25031.5	-26117.0	-28137.9
Pseudo R^2	0.087	0.087	0.111	0.074	0.116	0.099	0.095	0.100

1. See notes for Table 3.9a.

TABLE 3.11a
Employer Size and Worker Well-being (WERS)
Private Sector Employees
Dependent Variable: Item specific Job Satisfaction Scores

REGRESSOR	*Influence* ALL	*Pay* ALL	*Achievement* ALL	*Respect of Boss* ALL
Ln(workplace size)	-0.029	-0.007	-0.019	-0.008
	(0.010)	(0.011)	(0.010)	(0.010)
Ln(pay)	0.147	0.595	0.142	0.091
	(0.027)	(0.033)	(0.026)	(0.026)
Observations				
Workplaces	1111	1111	1111	1111
Individuals	15087	15087	15087	15087
Log-L	-19317.7	-20787.9	-19523.0	-20978.6
Pseudo R^2	0.082	0.132	0.092	0.083

1. See notes for Table 3.3. Additionally normal hours are controlled for.
2. All columns are estimated by the Ordered Probit technique.
3. The Pseudo R^2 is calculated using the method of McKelvey and Zavoina (1975).

TABLE 3.11b
Employer Size and Worker Well-being (WERS)
Private Sector Employees
Dependent Variable: Item specific Job Satisfaction Scores

REGRESSOR	*Influence* ALL	*Pay* ALL	*Achievement* ALL	*Respect of Boss* ALL
Ln(workplace size)	-0.011	0.014	-0.003	0.004
	(0.011)	(0.012)	(0.011)	(0.011)
Firm Size:100-999	-0.087	-0.158	-0.078	-0.073
	(0.038)	(0.043)	(0.039)	(0.041)
Firm Size:1,000-9,999	-0.115	-0.140	-0.082	-0.068
	(0.039)	(0.043)	(0.040)	(0.043)
Firm Size:10,000+	-0.194	-0.148	-0.198	-0.124
	(0.046)	(0.057)	(0.048)	(0.054)
Firm Size:50,000+	-0.219	-0.207	-0.175	-0.135
	(0.048)	(0.058)	(0.052)	(0.055)
Ln(pay)	0.151	0.602	0.146	0.094
	(0.026)	(0.032)	(0.026)	(0.026)
Observations				
Workplaces	1111	1111	1111	1111
Individuals	15087	15087	15087	15087
Log-L	-19302.8	-20770.5	-19509.1	-20972.2
Pseudo R^2	0.084	0.135	0.094	0.084

1. See notes for Table 3.11a.

TABLE 3.12a
Employer Size and Worker Well-being (WERS)
Private Sector Employees
Dependent Variables: Characteristics of Employment

REGRESSOR	*Hard work* ALL	*Lack time* ALL	*Security* ALL	*Worry* ALL
Ln(workplace size)	-0.009	-0.045	0.042	-0.047
	(0.012)	(0.012)	(0.016)	(0.011)
Firm Size:100-999	-0.058	0.080	-0.126	0.011
	(0.041)	(0.038)	(0.047)	(0.035)
Firm Size:1,000-9,999	0.033	0.200	-0.206	0.034
	(0.041)	(0.041)	(0.049)	(0.036)
Firm Size:10,000+	0.029	0.221	-0.251	0.018
	(0.053)	(0.051)	(0.064)	(0.044)
Firm Size:50,000+	0.094	0.357	-0.161	0.061
	(0.056)	(0.059)	(0.072)	(0.051)
Ln(pay)	0.141	0.158	-0.025	0.159
	(0.027)	(0.026)	(0.029)	(0.026)
Observations				
Workplaces	1108	1108	1108	1108
Individuals	14441	14441	14441	14441
Log-L	-16095.0	-19776.4	-19236.9	-20543.6
Pseudo R^2	0.083	0.120	0.131	0.114

1. See notes for Table 3.11a.
2. All columns are estimated by the Ordered Probit technique. Positive coefficients signify (greater) agreement with the statement.

TABLE 3.12b
Employer Size and Worker Well-being (WERS)
Private Sector Employees
Dependent Variables: Influence over Job

REGRESSOR	*Task* ALL	*Pace* ALL	*Methods* ALL
Ln(workplace size)	-0.025	-0.017	-0.008
	(0.011)	(0.010)	(0.012)
Firm Size:100-999	-0.025	-0.003	-0.058
	(0.039)	(0.036)	(0.038)
Firm Size:1,000-9,999	-0.108	-0.092	-0.167
	(0.039)	(0.037)	(0.038)
Firm Size:10,000+	-0.157	-0.127	-0.189
	(0.047)	(0.045)	(0.047)
Firm Size:50,000+	-0.150	-0.105	-0.163
	(0.053)	(0.049)	(0.053)
Ln(pay)	0.322	0.178	0.324
	(0.026)	(0.026)	(0.027)
Observations			
Workplaces	1111	1111	1111
Individuals	15195	15195	15195
Log-L	-18760.6	-19018.4	-15934.2
Pseudo R^2	0.193	0.081	0.143

1. See notes for Table 3.12a.

APPENDIX 1: Non-response and Attrition Bias in the BHPS

The BHPS is a nationally representative sample of more than 5,000 British households, containing over 10,000 adults. Respondents are interviewed annually. If an individual leaves their original household all adult members of their new household are also interviewed. Children are interviewed once aged 16. Together these should ensure the sample remains broadly representative of the British population.

Nathan (1999) undertakes a more systematic analysis of the effects of attrition. The BHPS is compared to Census data, the General Household Survey (GHS) and the Family Expenditure Survey (FES), with respect to age, sex, marital status, socio-economic group, ethnicity, employment status and some household characteristics. The author concludes that cumulative attrition in the BHPS is limited, and does not lead to serious bias in inference.

This issue was further examined by estimating models using the BHPS longitudinal survey weights. These weights restrict attention to the sample of individuals observed in every wave, and then adjust for differential probabilities of response in the panel. Sample properties should then match those of the population. The resulting coefficient estimates are compared to the unweighted estimates in Table A1 below.

Whilst the potential endogeneity of any non-response, and the use of income in the construction of weights, makes them of doubtful usefulness in the current setting, it is reassuring that the effect of workplace size upon wages is estimated to be very similar, to the unweighted estimates.

Overall, this suggests any bias due to attrition in the BHPS is limited.

APPENDIX 2: Sample Selectivity within the WERS

The WERS survey is a cluster stratified random sample, where the sampling fraction is increasing in establishment size and varies by industry code. Large establishments are then over-sampled. Secondly, the sampling strategy of interviewing a maximum of 25 workers per establishment implies that a plant with 25 employees can (potentially) contribute the same number of employee data points as a workplace with 100 or 1000 staff. Hence, within the sample of WERS plants, the individual data exhibits over-sampling of respondents within small establishments. The data then suffers from sample selectivity in two, opposite directions. Yet, since the chief selection mechanisms, establishment size and industry, are explanatory variables in all analyses it is not clear *a priori* as to why results should be biased.

As a check to whether coefficients suffer from any selection bias they are compared to estimates using the WERS sample survey weights (see Airey et al, 1999, and Cully et al, 1999). DuMouchel and Duncan (1983) provide a discussion as to the merits and use of sample survey weights. Weighting is unnecessary where the model holds independent of the stratification. That is, where parameters are the same for each stratum, or where we are able to include amongst the explanatory variables the variables upon which selection is based, then intuitively we control for selection. In this case, both weighted and unweighted estimates are consistent and the use of sample weights should be avoided, weighting the variance covariance matrix, when unnecessary, inflates standard errors and introduces random variation in coefficients.

Table A2 estimates hourly pay equations using standard and sample weighted methods. Qualitative interpretation of results is consistent in both sets of estimates. Estimates using sample survey weights predict a slightly more marked

establishment size-wage premium and a marginally weaker firm size effect. Substantive conclusions, however, hold both for weighted and unweighted estimates.

As a further check results are compared to those from the BHPS. The BHPS categorical workplace size variable is replaced by a continuous proxy (using mid-points) and regressions run for the analogous sample and time period as the WERS. Log establishment size is estimated to enter the wage equation with a coefficient 0.035.[28] The comparable estimates for the WERS data are observed in columns one and three of Table A2 where, as with the BHPS, there are no controls for firm size. The estimated parameter is 0.036 for the unweighted estimates, and 0.044 for the weighted.

These estimates suggest that the unweighted estimates do not suffer unduly from selection bias. The estimation strategy adopted is to include establishment size and industry as control variables in all regressions and to estimate by standard techniques. To account for potential variation in the coefficients, by observed characteristics, all standard errors are robust to arbitrary heteroskedasticity. The non-independence of errors within the same plant is also corrected for, as ignoring the clustering of individuals within plants can potentially significantly underestimate standard errors (see Moulton, 1986).

[28] The same coefficient estimate was obtained when the workplace size categories were instead replaced by the (weighted) mean establishment size, for that category, from the WERS data.

TABLE A1
A Comparison using Longitudinal Sample Weights (BHPS)
Private Sector Employees
Dependent Variable: Ln(wage)

REGRESSOR	*UNWEIGHTED*	*WEIGHTED*
Workplace Size:3-9	0.113	0.135
	(0.025)	(0.031)
Workplace Size:10-24	0.193	0.224
	(0.026)	(0.033)
Workplace Size:25-49	0.218	0.250
	(0.027)	(0.033)
Workplace Size:50-99	0.272	0.304
	(0.027)	(0.034)
Workplace Size:100-199	0.273	0.289
	(0.027)	(0.033)
Workplace Size:200-499	0.295	0.315
	(0.027)	(0.033)
Workplace Size:500-999	0.329	0.350
	(0.028)	(0.035)
Workplace Size:1000+	0.362	0.382
	(0.029)	(0.036)
Observations		
Individuals	5866	3013
Panel Total	22528	15317
Adjusted R^2	0.55	0.56

1. See notes Table 3.2.

TABLE A2
The Impact of Sample Weights upon the WERS
Private Sector Employees
Dependent Variable: Ln(wage)

	UNWEIGHTED		*WEIGHTED*	
REGRESSOR	*ALL*	*ALL*	*ALL*	*ALL*
Ln(workplace size)	0.036	0.031	0.044	0.041
	(0.006)	(0.006)	(0.006)	(0.006)
Firm Size:100-999		0.028		0.023
		(0.019)		(0.023)
Firm Size:1,000-9,999		0.048		0.043
		(0.020)		(0.020)
Firm Size:10,000+		0.045		0.036
		(0.023)		(0.024)
Firm Size:50,000+		0.005		-0.001
		(0.025)		(0.026)
Observations				
Workplaces	1111	1111	1111	1111
Individuals	15618	15618	15618	15618
Adjusted R^2	0.62	0.62	0.60	0.60

1. See notes to Table 3.3.

Chapter Four

Race, Wages and Worker Well-being

Abstract

This chapter investigates the role of the employer in explaining racial differentials in pay and job satisfaction. Ethnic minority workers are found to earn higher wages in plants with a smaller non-white employment share. White wages, in contrast, are only weakly related to the racial composition of the plant. The racial wage gap is then greater in establishments with a higher proportion of non-white staff. Establishments that employ more minority staff are also associated with lower levels of job satisfaction. The evidence is consistent with workplaces with a large proportion of minority workers offering inferior working conditions. Moreover, non-white workers are found to be less satisfied with their pay, compared to otherwise similar white workers, even when pay is held constant. Non-white men are -4.2 percent less likely to respond as satisfied or very satisfied with their pay, than white males, non-white women -6.0 percent less likely than white females. This provides new evidence potentially consistent with discrimination in pay.

4.1 Introduction

The finding of an unexplained wage differential between white and ethnic minority[1] employees is a pervasive regularity in studies of wage determination. The finding of a racial differential in recorded job satisfaction levels has, however, been less well documented. In both cases, for the UK at least, a persuasive consensus upon the source of these differentials has not yet come to the fore.

This paper uses two sources of data, the Quarterly Labour Force Survey and the Workplace Employee Relations Survey, to attempt to distinguish between the competing hypotheses to explain the racial wage gap's existence. The first has the advantage of being a large national survey, where the sample of ethnic minority respondents should be representative of that in the population. Data regarding worker attitudes and the employer are however limited. The second benefits from the linking of establishment information to employee responses. The role of the employer upon the relationship between race, pay and job satisfaction can then be more fully analysed.

The effect of the establishment's ethnic composition upon pay is extensively examined. Non-white employees are observed to earn lower wages in plants with a higher proportion of ethnic minority co-workers. White wages are, however, only weakly related to the racial composition of the plant. The racial wage differential is then greatest in workplaces that hire a larger proportion of minority workers. The estimated racial differential in wages was further found to be robust for workers within the same occupation and establishment. The primary avenue for the racial

[1] The terms non-white, minority and ethnic minority will be used to denote any member of a non-white racial group. The term black refers solely to individuals of Afro-Caribbean descent.

wage gap is then not that ethnic minority workers are employed in low-pay plants but, rather, they are paid less well in any given workplace.

Job satisfaction data are used to provide a new analysis of racial disadvantage in the labour market. Establishments that employ more non-white individuals are observed to be associated with lower levels of job satisfaction for white males, white females and for ethnic minority women. Results are, however, mixed for non-white men. The plant's rates of quits, separations, and absenteeism, are also positively related to the ethnic minority employment share.

The evidence suggests workplaces with a large proportion of minority staff are, here, associated with inferior working conditions. Nevertheless, this is consistent with, both, discrimination and unobserved worker quality differences, where non-white workers are, for some reason, less productive. Ethnic minority employees are, however, found to be less satisfied with their pay, compared to otherwise similar white workers, even when pay is held constant. This provides new evidence that appears consistent with discrimination.

The outline of the chapter is as follows. Section two examines rationales for the finding of a racial wage differential. Section three documents the evidence of previous UK and US studies. The data are discussed in section four and regression results presented in section five. Finally, section six concludes.

4.2 The Racial Wage Differential: Theories and Hypotheses

The finding of a racial differential in pay is a pervasive empirical regularity.[2] Explanations for its existence are, however, more contentious and are found in four

[2] For an excellent review of studies of discrimination see Altonji and Blank (1999).

principal areas. The first suggests it captures differences in productivity, possibly due to pre-labour market or historical discrimination. On this view, the observed wage differential is an artefact of the researcher's inability to fully control for worker ability. Secondly, theories originating from Becker (1957) model wages as being shaped by the prejudicial preferences of white labour market participants.

Thirdly, in a variant of the prejudicial tastes and unobserved productivity models, minority employees may be 'crowded' into low-pay occupations. Finally, where firms have limited information regarding worker skill they have an incentive to use correlates of worker productivity to differentiate between employees. Employers may then statistically discriminate between workers on the basis of race. These explanations are discussed in more detail below.

4.2.1 Differences in Productivity

The explanation for the racial wage gap that has come to the fore in the US is that it reflects unobserved productivity differences. Whilst not, in general, suggesting inherent ability differences (see Neal and Johnson, 1996), the authors propose a lower quality of schooling due to racial or economic segregation prior to entering the labour market. Whilst this may reflect discrimination, wages themselves are non-discriminatory and reflect worker skill.

4.2.2 Taste based models of discrimination

Within this class of model, a subset of white labour market participants (employers, employees, or consumers) are assumed to hold prejudicial preferences against interacting with non-white workers. In a static model of discriminating employers, firms are assumed to maximise a value function (V) equal to profits adjusted for the (money metric) disutility of hiring a non-white employee:

$$\text{Firm j max } V_j = pf(n_a + n_b) - w_a n_a - w_b n_b - d_j n_b \qquad (1)$$

Where p is the output price, f(.) the production function, n_g employment of group g within the firm, w_g the corresponding wage level, g = {a, b} the racial group indicator (a white, b non-white), and d_j the disutility of hiring a minority employee in firm j ($d_j \geq 0$).

A firm will then hire an ethnic minority worker only if non-white wages fall sufficiently below white pay to offset the discrimination parameter, $w_a - w_b > d$. This yields the prediction that workplaces will be completely segregated according to the intensity of the employer's prejudice. By itself this does not generate a racial wage differential. Unprejudiced firms hire non-white employees and competitive forces amongst these firms bid wages to the marginal product. Only if the number of prejudiced firms is sufficiently large, to negate these forces, can wage discrimination exist. Even this situation cannot persist in the long run. Unprejudiced firms will enter the market and earn rents from hiring non-white workers, drive prejudiced firms out of the market, and bid up minority wages until the competitive wage is restored.

A model of employee discrimination assumes prejudiced white employees dislike working alongside non-white co-workers. White workers must then be compensated, with higher pay, whenever they work in a racially mixed plant. Yet, colour-blind profit maximising employers would never choose to have an integrated workforce, as this necessitates paying white workers a compensating differential when no more productive than non-white employees. Instead, workers are completely segregated according to race. Furthermore, employee prejudice cannot, by itself, generate a racial wage differential between equally skilled workers. Firms hire whichever labour is cheaper, and competition for labour equalises wages between segregated plants.

A model of consumer discrimination assumes prejudiced white customers derive less utility if they purchase goods or services from a minority worker. White consumers will then only purchase goods from a non-white worker when the price falls sufficiently to offset their discrimination. A racial wage differential then requires a sufficiently large number of prejudiced consumers, and that it is not possible to costlessly substitute minority workers into jobs with no customer contact.

Black (1995) extends Becker's (1957) model of employer discrimination to incorporate search frictions. Information concerning potential employment opportunities is costly to obtain and workers search, on-the-job, amongst employers. Search costs here afford the employer a degree of monopsony power in setting wages. *Unprejudiced* firms then offer non-white workers lower wages, as they take into account the reduced set of employment opportunities for minority workers in the presence of discrimination.

Workers are assumed to be equally productive, have the same preferences over leisure, and have identical search costs (c per period). Employers are prejudiced, p, or unprejudiced, u, with labour market proportions (θ, 1-θ). This distribution is common knowledge. Prejudiced firms hire only type a workers at wage rate w_a^p. Unprejudiced firms hire type a and b employees and pay wages, w_a^u and w_b^u respectively. Employees derive utility, per period, $U = w^j + \alpha$, where w^j is the wage in firm j and α a match specific random variable capturing the non-pecuniary benefits of the job, known prior to the job acceptance decision.[3]

[3] Constant returns to scale technology and profit maximisation implies wages are equalised, across firms, for each group of workers. The match quality component, α, is then the sole source of heterogeneity in individual utility.

Workers and firms meet once each period and employees accept offers with utility above some reservation level:

$$w^j + \alpha > U_g^r \qquad g = \{a, b\} \qquad (2)$$

The value of search (net of costs) for white individuals is then:

$$U_a = \theta E_{max}\{w_a^p + \alpha, U_a\} + (1-\theta)E_{max}\{w_a^u + \alpha, U_a\} - c \qquad (3)$$

For minority workers:

$$U_b = \theta U_b + (1-\theta)E_{max}\{w_b^u + \alpha, U_b\} - c \qquad (4)$$

where θ is the probability of meeting a prejudiced employer.[4]

The presence of prejudiced firms increases the likelihood of a minority worker encountering an unproductive match, and thus raises their expected search costs. This reduces non-white reservation wages and, hence, paid wages in equilibrium. This result holds given the presence of *any* prejudiced employer (a non-zero θ), and the wage differential is increasing in the number of prejudiced firms. The racial differential in pay does fall, however, as the size of the minority population increases. A larger non-white labour force raises the profits of unprejudiced employers, reducing the number of prejudiced firms who survive in equilibrium, and hence raises minority wages.

Can the racial wage gap persist in the long run? Black assumes an exogenous distribution of entrepreneurial ability for all (potential) firms in the economy. Echoing Becker's earlier logic, low-ability prejudiced firms are driven out of the market by unprejudiced employers. High-ability prejudiced firms, however, can trade off some of their rents to exercise a preference for discrimination. Yet, if the assumption of an exogenous distribution of firm ability is relaxed, competition again

[4] For the full solution to the model see Black (1995).

eliminates the racial wage differential. Unprejudiced high-ability firms expand at the expense of high-ability prejudiced and low-ability unprejudiced employers.

4.2.3 Statistical Discrimination

Employers, in general, have only limited information regarding the true skill of job applicants. There is then an incentive to use indicators of worker quality to differentiate, statistically discriminate, between workers. In a simple static model we assume employees are equally productive, and that productivity is normally distributed; $\upsilon \sim N(\mu, \sigma_\upsilon^2)$. Whilst population productivity is common knowledge, the actual productivity of any one individual is unknown and the employer observes a noisy signal of ability, s, where $s = \upsilon + \varepsilon$; and ε is a normally distributed random error term, $\varepsilon \sim N(0, \sigma_\varepsilon^2)$.

In a competitive labour market with risk-neutral employers, wages are equal to expected productivity, conditional on the observed signal:

$$w = E(\upsilon/s) = \left(\frac{\sigma_\varepsilon^2}{\sigma_\upsilon^2 + \sigma_\varepsilon^2}\right)\mu + \left(\frac{\sigma_\upsilon^2}{\sigma_\upsilon^2 + \sigma_\varepsilon^2}\right)s \qquad (5)$$

If non-white qualifications are, for some reason, less informative signals of ability ($\infty > \sigma_{\varepsilon_b} > \sigma_{\varepsilon_a} > 0$) minority workers experience lower returns to human capital investment, s, and have less incentive to invest in education. Returns to population productivity, μ, are, however, greater and on average there is no racial pay gap. Implicitly this assumes education does not increase productivity. If this is relaxed, the lower levels of education predicted for minority workers may induce lower levels of productivity, and hence pay. Alternatively, if match quality is increasing in the precision of the ability signal, and productivity is itself determined by match

quality, non-white wages will be lower. These mechanisms may generate a racial differential in pay despite the same distribution of innate ability.[5]

Oettinger (1996) extends this framework to incorporate a job-matching model, along the lines of Jovanovic (1979). Individuals live for two periods (t=1, 2), work in both, and maximise the expected value of lifetime earnings. Each period workers receive a single job offer, with match productivity $\upsilon_t \sim N(\mu, \sigma_\upsilon^2)$. This distribution is common knowledge and identical for both racial groups, but individual productivity is observed only as a noisy signal.

The first period wage is defined as the weighted sum of the expected and true productivity values (the latter achieved by piece rate pay):

$$w_1 = \tau\upsilon_1^e + (1-\tau)\,\upsilon_1 \qquad \text{where } \upsilon_t^e = E(\upsilon_t/s_t) \qquad (6)$$

Where τ is the weight upon *ex ante* expected productivity in first period pay. After the first period the true productivity of the worker is revealed to both parties. In the second period, the worker will move to a new job if the expected wage exceeds their certain wage in the current job.[6] Second period pay is then:

$$w_2 = \upsilon_1 \qquad \text{if } \upsilon_1 \geq \upsilon_2^e \qquad \text{"stayers"} \qquad (7a)$$

$$w_2 = \tau\upsilon_2^e + (1-\tau)\,\upsilon_2 \qquad \text{if } \upsilon_1 < \upsilon_2^e \qquad \text{"movers"} \qquad (7b)$$

The model then offers predictions regarding how minority wages evolve over time, relative to white workers. As their signals are noisier, non-white employees are more likely to be erroneously viewed as low ability. Minority wages will then, on average, grow more slowly with general labour market experience but more quickly within jobs, where true productivity is revealed over time.

[5] The model's predictions become starker if minority workers are, on average, less productive.
[6] Essentially there is random assignment in the first period. The probability of switching jobs in the second period, for a given wage gain, is assumed the same for white and minority employees.

4.2.4 *Occupational Crowding*

Some authors have suggested the racial wage gap reflects a lack of occupational attainment of minority workers (e.g. Stewart, 1983b). This may reflect prejudicial tastes that are more severe in certain jobs, historical constraints upon worker access, differences in worker skills, or worker choice. This hypothesis is then less a model than a manifestation of discrimination.

Johnson and Stafford (1998) examine the effects of discrimination and skill differences upon occupation and pay. Whilst they study discrimination on grounds of gender, the model can straightforwardly be applied to racial discrimination. The economy is assumed to produce one good using two occupations in identical firms. Job 1 is predominantly white and non-white workers are 'crowded' into job 2. Employment in each race-occupation group is n_{gl}; where g = {a, b} and l =1, 2. The productivity of type b workers, relative to type a workers in job l is λ_l (= $\upsilon_{bl}/\upsilon_{al}$), and non-white workers enjoy a comparative advantage in the second job ($\lambda_1 < \lambda_2$).

Discrimination is modelled as a prejudicial taste (δ_l) which is more pronounced in the white occupation ($\delta_1 > \delta_2$).[7] To abstract from issues of segregation or long-run sustainability all firms have identical preferences. Firms profit maximise and wages are equal to the marginal product adjusted for the disamenity of hiring a minority worker:

$$w_{a1} = \upsilon_1 \qquad w_{b1} = (1-\delta_1)\lambda_1\upsilon_1 \qquad (8a)$$

$$w_{a2} = \upsilon_2 \qquad w_{b2} = (1-\delta_2)\lambda_2\upsilon_2 \qquad (8b)$$

Rearranging terms implies, in equilibrium, the ratio of white to non-white wages, the racial wage differential, will be greater in the white occupation ($w_{a1}/w_{b1} > w_{a2}/w_{b2}$).

[7] This discrimination coefficient is a transformation of the earlier term: $d_l = w_{al}.\delta_l/(1-\delta_l)$.

This follows from the comparative advantage of white workers and the greater degree of discrimination. Johnson and Stafford then demonstrate, as minority workers enter 'white' jobs, as the degree of discrimination (δ_1) falls and as non-white workers become more proficient in the first occupation, white wages fall and minority wages rise. We examine a modified version of this framework, where non-white individuals are crowded into low-pay establishments.

4.3 Existing Empirical Evidence

4.3.1 UK Evidence

The UK evidence has, to date, principally documented the existence of a racial wage differential using the Oaxaca (1973) mean wage decomposition methodology. Stewart (1983b), in one of the earliest UK studies, uses data from the National Training Survey of 1975 to examine the occupational attainment (defined by mean earnings in reported occupation) of male ethnic minority immigrants. The author finds statistically significant evidence of a racial differential on occupational wages of around -12 percent. When actual wages are examined the figure rises to -17 percent, suggesting two thirds of the wage gap is attributable to a lack of occupational attainment. Both the productivity and discrimination based explanations are, however, consistent with such a result.

These estimates combine the effects of race and immigration. The latter potentially affects wages independently of race through, discrimination, differing language skills, the quality of schooling in the country of origin, and the unobserved heterogeneity of immigrants. Shields and Wheatley-Price (1998) find non-white immigrants experience lower returns to schooling than UK-born ethnic minorities. Education received abroad is less valued than that received in the UK, especially

where it is likely English is not the first language. To make inferences concerning, purely, racial discrimination is then difficult where first generation migrants form a large proportion of the ethnic minority population. As the non-white population of Britain is predominantly a result of post-war immigration, and only a third of non-white individuals of working age are UK born (source Labour Force Survey, 1997), most UK samples will suffer from this bias to some extent. This caveat must be borne in mind when examining British data.

Differences in pay between racial groups are unlikely to have remained static in the last 20 years, with changes in attitudes and the composition of the ethnic minority population, and with the passing of the Race Relations Act in 1976. Blackaby et al (1994) examine how the male racial wage differential changed over the 1970s and 1980s using pooled General Household Survey data. For the period 1973-1979 the authors estimate a differential, after controlling for observed characteristics and immigrant status, of -12 percent. For the period 1983-1989 the comparable figure is -19 percent. Blackaby et al (1998) offer more contemporary evidence, using the Quarterly Labour Force Survey of winter 1992. The male racial wage differential is estimated to be -11 percent. This result is, however, shown to mask asymmetries between ethnic groups. The earnings gap is -10 percent for both Black and Indian workers and -16 percent for those of Pakistani origin.

4.3.2 US Evidence

Possibly the most influential US study is that of Neal and Johnson (1996). The studies chief contribution is to examine the impact of a racially unbiased ability score. Using the National Longitudinal Survey of Youth (NLSY) a single measure of skill is considered, scores from the Armed Forces Qualification Test (AFQT) when respondents were aged 18 or below. Parsimonious wage equations are estimated,

upon race, age and test score, when individuals are in their late 20s. The AFQT score is found to enter positively and statistically significantly and to explain a considerable proportion of the observed wage differential. Upon its introduction the estimated black-white wage differential falls from -24 to -7 percent for males and from -16 to +4 percent for females, and is no longer statistically significantly different from zero in the last case. This provides strong evidence, for the US at least, that lower black wages to a large extent reflect differences in basic skills acquired prior to entry to the labour market.

The validity of this evidence rests upon the assumption that the AFQT is racially unbiased. Evidence is documented that the test predicts (military) job performance equally well for black and white candidates, and that the key determinants of scores are family background and school characteristics. Other evidence, however, suggests there are differential returns to components of the test, with the verbal component more important for black respondents (see Altonji and Blank, 1999). This does raise some issues of bias upon the test.

Hellerstein et al (1999) arrive at a similar conclusion to Neal and Johnson, but via a very different approach. Using the Worker Establishment Characteristics Database (WECD), which matches individual data to the employing workplace, the authors estimate plant-level production function equations that account for the demographic composition of the workforce. Workers are assumed to have different characteristics, and potentially different marginal products, but to remain perfectly substitutable labour inputs. The productivity of black workers, relative to white, is then estimated. Plant-level wage equations are also estimated. If the relative productivity of black workers is significantly different from the relative wage, this provides evidence potentially indicative of racial discrimination. The authors discern no such evidence.

Concerns do, however, remain regarding the reliability of results. The composition of the workforce is likely to be endogenously related to production technology. Also, in contrast to the negative wage differential observed in the individual data, black workers are estimated to be both more productive and highly paid than white employees. This may reflect that estimates are identified by cross-plant variation and do not capture within establishment differences in pay.

Other studies have sought to test the predictions of the models of prejudicial tastes. Black (1995) presents a limited test of his model using a sample of small businesses. Black employees are found to be significantly less likely to be awarded above average pay, controlling for observed skill. Yet, given the ability controls are a discrete approximation of true skill, unobserved productivity differences may remain. Also the model cannot distinguish between employer and statistical discrimination.

Holzer and Ihlanfeldt (1999) examine a model of consumer discrimination and use a survey of employers in four major US cities (Atlanta, Boston, Detroit, and Los Angeles) to estimate equations for the race of the last employee hired. The effects of the racial composition of customers and the applicant pool, the race of the hiring manager, and whether the job requires customer contact, are used to test the consumer discrimination model.

The number of black customers has a positive and statistically significant effect upon the probability of hiring a black worker, but a robust negative effect upon hiring a white individual. These effects are more pronounced in workplaces with customer contact. Whilst this is supportive of consumer discrimination, customer contact is not an important determinant of the race of the last hire in white customer establishments, but is in black consumer plants. The evidence in support of consumer discrimination is then mixed.

Recent tests of models of statistical discrimination have sought to explore the dynamic properties of wages, utilising the assumption that worker productivity is revealed to employers over time, and firms update their beliefs.

Oettinger (1996), assuming equally productive black and white employees and statistically discriminating employers, predicts first period pay to be independent of race and, subsequently, minority wages to exhibit greater returns to tenure and lower returns to experience. Using the NLSY the author finds, consistent with the model, the male racial wage differential to be small and statistically insignificant on entry to the labour market, and returns to experience significantly larger for white men. There is, however, no evidence of a steeper wage-tenure profile for black employees and, inconsistent with the model, the wage gap is observed to grow over time.

Altonji and Pierret (1997) assume race is negatively correlated with productivity. They show if employers statistically discriminate, the racial wage differential will not vary over time, as firms subsume the lower productivity of non-whites into their expectations. If firms do not statistically discriminate, they only learn of this relationship as productivity is revealed in the labour market, and the racial wage differential will grow with experience. Using a sample of non-hispanic males from the NLSY, and in contrast to the predictions of the model, the racial wage gap widens markedly with experience, again supporting a productivity explanation for the US.

Carrington and Troske (1998) study the racial composition of plants using the WECD and a survey of small business owners, the Characteristics of Business Owners (CBO). Using a Gini coefficient methodology, they find no evidence of inter-firm racial segregation when detailed controls for region are included. Suggesting segregation is a geographic, rather than labour market, phenomenon.

Black workers are, however, disproportionately employed within firms whose owners, managers or customers are also black. These two seemingly contradictory findings are reconciled by the fact there are insufficient black employers to generate segregation. Establishments with large black minorities are also found not to pay low wages on average, white wages are here higher, but it is within these plants that the racial wage gap is largest. Finally, the majority of the black-white wage differential is attributed to individual, rather than employer, characteristics.

4.3.3 Racial differences in worker well-being

Bartel (1981) offers an interesting analysis of racial differences in job satisfaction. For a constant wage, non-white job satisfaction is predicted to be lower if, as a result of discrimination, minority workers are more poorly matched, or if discrimination occurs on non-pecuniary benefits. Yet such discrimination as exists may also lower black employees' aspirations and expectations, for the same wage satisfaction may then be higher. *A priori* the effect is indeterminate. For a sample of middle-aged American men taken in 1966, 1969 and 1971 the latter effect is found to dominate, with satisfaction higher amongst black respondents within this cohort. In contrast, Blanchflower and Oswald (2000), using US General Social Surveys from 1972 to 1998, observe blacks and other non-white ethnic minorities to report statistically significant lower levels of happiness for both men and women.

Evidence for the UK is more limited. Both Clark (1996), for the British Household Panel Survey, and Brown and McIntosh (1998), using a sample of three low wage sectors, observe lower levels of ethnic minority job satisfaction. Results are not, however, well determined, possibly due to the small number of non-white workers observed (139 and 94 respectively).

4.4 Data

The data studied in this chapter come from two sources, the Workplace Employee Relations Survey (WERS) and the Quarterly Labour Force Survey (QLFS).

The QLFS is a nationally representative rolling panel survey, where each quarter almost 60,000 households, containing 150,000 individuals, are interviewed. Each household is followed for one year (five consecutive quarters) whereupon they exit the sample. Earnings are surveyed upon entry and exit. Attention is here restricted to those individuals who entered the QLFS, aged 16 to 64, between 1997 quarter one and 1999 quarter four, with non-missing pay data. The final sample contains some 150,000 observations from 110,000 individuals.

The WERS is a random sample[8] of around 2,200 British establishments with ten or more employees, completed between October 1997 and June 1998. Within these workplaces 25 worker questionnaires were randomly allocated amongst the employees, for establishments with less than 25 employees the population of workers was sampled. This yielded approximately 28,000 individual responses matched to 1,800 workplaces (see Appendix for fuller discussion). The employee data includes questions on earnings, education, workplace characteristics and a rich source of information on worker attitudes. These data are augmented by a management questionnaire regarding establishment characteristics.

Given the ethnic minority population of Britain is relatively small, forming only 6.4 percent of the total (Regional Trends, 1999), even random samples may not be reliable if the number of respondents are small. This may be especially true when

[8] The survey population in fact excludes establishments in the following Standard Industrial Classification (1992) divisions: A (Agriculture, hunting and forestry), B (fishing), C (Mining and quarrying), P (Private households with employed persons), and Q (Extra-territorial organisations).

sub-samples are analysed. The WERS includes around 950 ethnic minority employees. The QLFS, with its larger sample size, some 5000. The QLFS is here assumed to be representative and used as a base from which results, using the WERS data, are compared.

4.4.1 The Job Satisfaction Data

Within the WERS all respondents are asked to rate their level of satisfaction with respect to four aspects of their employment: influence over the job, total pay, sense of achievement, and, the respect received from supervisors. Each of these questions was answered on a 1 to 5 scale, where 1 corresponded to the highest level of satisfaction and 5 the lowest. This scale was reversed so 1 represents the lowest level of well-being (very dissatisfied), 5 the highest (very satisfied) and 2 to 4 intermediate values. Unfortunately there is no overall job satisfaction question within the WERS data, and analysis focuses upon these four questions.

4.4.2 Comparison of Satisfaction Responses: Interpersonal and over time

Job satisfaction reflects both objective circumstances, working conditions, and subjective factors, aspirations and expectations. This subjectivity has led some economists to be sceptical of the concept's worth. Scores may be random draws and interpersonal comparisons meaningless. Yet one may then not expect to observe the systematic patterns of correlation, between job satisfaction and observed events and actions, that have been documented. Satisfaction has been found to influence subsequent labour market behaviour. It is a significant predictor of quits (Freeman, 1978) and is negatively related to absenteeism, non- and counter-productive work. Furthermore, it is related, in the expected direction, with other indicators of well-being: poor mental health, length of life and coronary heart disease (see Clark and

Oswald, 1996). Such a pattern of results can probably not be reconciled with a purely idiosyncratic variable.

A more rigorous argument in favour of the ability of the researcher to make use of satisfaction data is found in Kahneman et al (1997) who argue that functions that relate subjective intensity to physical variables are similar for different types of people. They suggest the well-being of any event have a basic scale, pleasant, neutral, and unpleasant. Other scales may expand the positive or negative categories to a finer degree but the neutral case is a constant. It is argued the distinctiveness of this neutral value provides a focal point that allows some confidence in matching subjective experiences across time for a given individual and to support interpersonal comparisons.

Whilst it has been suggested that the satisfaction data do allow us to infer the relative well-being of different types of individuals, they nevertheless remain imperfect. They are qualitative not quantitative, often banded and there is considerable potential for measurement error, though this would be less easily handled if satisfaction were to be used as an independent variable.

4.5 Results

4.5.1 Summary Statistics

The comparability of the data sets is investigated in Tables 4.1a and 4.1b, and summary statistics for white and non-white employees presented for QLFS and WERS respectively.

Within the QLFS, for both males and females, the ethnic minority sample is observed to have lower levels of potential experience (age - years of schooling - six) and employer tenure, to be more likely to be in temporary employment, and to have

greater years of schooling. This greater schooling runs counter to the under-education prediction of the statistical discrimination model. If non-white individuals are subject to discrimination in employment this may reflect selectivity bias, only the most educated may acquire employment. However non-white education levels remain markedly higher when *all* individuals, including those not in paid employment, were examined. In the raw data, the average hourly pay of ethnic minority men is lower than that for white males. For women the reverse is true, minority pay is observed to be greater. The latter finding may reflect the greater education of minority workers and their concentration in, and around, London.

Within the WERS data, and as with the QLFS, minority employees, both male and female, are observed with lower levels of job tenure, less general labour market experience (here captured by age), greater levels of education, and are more likely to be in temporary employment. Non-white workers in the WERS are, on average, more highly paid, for both men and women. Again, this may reflect the higher education and metropolitan location of ethnic minorities in the UK. Finally, non-white individuals work in establishments with more non-white co-workers.

4.5.2 Estimation strategy

To investigate these patterns in more detail we turn to regression analysis. Wages are here modeled as a function of personal characteristics (such as education, experience, gender and race) and employer characteristics (e.g. establishment size, industry and the ethnic composition of the workforce). Hourly pay for individual i in time period t and employer j, is then expressed in its log-linear form as:

$$w_{itj} = x_{itj}'\beta + z_{tj}'\gamma + \varepsilon_{itj} \qquad i = 1, \ldots, n \qquad (9)$$

$$t = 1, \ldots, T$$

$$j = j(i, t) = 1, \ldots, m.$$

where, w is the dependent variable, log hourly pay,[9] x the vector of worker characteristics, z the vector of employer characteristics,[10] ε the conformable error term with mean zero and constant variance, and β and γ the vectors of parameters to be estimated.

For the WERS data, models are also estimated which account for the, potential, unobserved heterogeneity of employers, by including an establishment effect upon wages (f_j) common to all workers within a plant:

$$w_{ij} = x_{ij}'\beta + z_j'\gamma + f_j + \varepsilon_{ij} \qquad (10)$$

(As the WERS data are a cross-section the time subscript is dropped.) Implicitly this assumes the difference in wages between any two workers in the same establishment is solely attributable to individual characteristics. Parameters are then estimated by taking within plant mean deviations (subtracting plant averages):

$$(w_{ij} - \overline{w}_j) = (x_{ij} - \overline{x}_j)'\beta + (\varepsilon_{ij} - \overline{\varepsilon}_j) \qquad (11)$$

Whilst employer heterogeneity is controlled for individual heterogeneity, both observed and unobserved, remains as deviations from plant means.

4.5.3 Results by Ethnic group

Tables 4.2 and 4.3 estimate wage equations for the white and ethnic minority samples, by gender, for the QLFS and WERS respectively. Estimates generally match standard earnings equation predictions and attention is focussed upon examining differences between racial groups.

[9] The WERS pay data are observed only as a grouped variable. Here mid-points are taken and pay proxied as continuous. Estimates of pay equations using these mid-point and the more robust Grouped regression method of Stewart (1983a) yield practically identical predictions.

[10] Industry is coded at the one-digit level due to the limited number of non-white respondents in the WERS. Unless explicitly stated occupation is not included as a control variable in regression analysis, due to the possibility of discriminatory barriers upon occupational attainment.

For both males and females the returns to schooling are estimated to be smaller for non-whites. This is consistent with statistical discrimination, but may also reflect a lower quality of ethnic minority education. Within the QLFS, non-whites are observed to experience faster wage growth with respect to tenure and slower growth with respect to potential experience. Estimates may, however, be complicated by racial differences in the time spent outside the labour market. With respect to union recognition, within the WERS a positive and statistically robust union pay effect is observed for non-white male employees, but not for white males, white females or ethnic minority females.[11] This provides some support for the hypothesis that unions compress the (male) racial wage differential.

To what extent can the differences in coefficients explain the differences in wages between the racial groups? Using the Oaxaca (1973) mean wage decomposition technique we estimate the portion of the log wage gap that cannot be explained by observed characteristics. This unexplained differential captures the effects of both discrimination and unobserved differences in characteristics. For the QLFS, the (mean) wage differential between ethnic minority and white workers is estimated to be -0.191 for males and -0.119 for females.[12] For the WERS, the comparable figures are -0.207 and -0.083.[13] These results suggest the two data sets offer broadly comparable estimates of the racial differential in wages.

4.5.4 Immigration and the wage differential

The British ethnic minority population comprises a large number of first generation immigrants. Estimates may then combine the effects of race and immigration, with

[11] See Metcalf et al (2000) for an analysis of the role of unions in compressing pay differentials.
[12] The lower racial wage differential for females is itself a pervasive empirical regularity (see Altonji and Blank, 1999).
[13] The decomposition is calculated using WERS weighted means.

potentially misleading inferences. This issue is investigated in Table 4.4. Regressions now estimate restricted models, where coefficients are held constant across racial groups, and an ethnic minority indicator captures the difference in wages between otherwise similar white and minority workers.

Columns one and two estimate wage equations for men and women, the racial wage gap is estimated to be -0.182 and -0.112 respectively. These estimates are very similar to those obtained from the Oaxaca decomposition in Table 4.2. As the majority of non-white individuals born in the UK are aged 40 or below, attention is restricted to this age range in columns three and four. Parameter estimates upon the minority indicator are identical to those observed previously.[14] Attention is further restricted to individuals born in the UK in columns five and six of Table 4.4. The estimated non-white parameters fall by around a half, to -0.100 for males and -0.054 for females, but remain statistically well determined.

Immigrant status here explains approximately half of the racial pay differential. This may result from different language skills, a lower market valuation upon foreign education, the occupational choices of migrants, or discrimination. It is not, however, common to native-born minority workers. This should be borne in mind when examining estimates, as for the WERS, where no controls for immigrant status are available.

4.5.5 The Ethnic composition of the workforce

The effect of the ethnic composition of the plant (defined as the proportion of the workforce who are from an ethnic minority) upon pay is analysed in Table 4.5, using the WERS data. Coefficients are held constant across racial groups and a non-white

[14] Results are not dependent upon this age restriction.

indicator captures the racial differential in wages. This is estimated to be -0.199 for men and -0.092 for women. These estimates are comparable to those obtained using the Oaxaca technique, in Table 4.3.

The establishment's ethnic minority employment share[15] is entered in the wage equation in columns two and five of Table 4.5. For men, the non-white employment share enters negatively, and is statistically significantly different from zero, with a coefficient of -0.199. A 10 percent increase in the proportion of non-white workers in the establishment is predicted to reduce wages by approximately 2 percent.[16] For women, the workforce ethnic composition enters negatively with a parameter of -0.073, but is not statistically well determined. The estimated racial wage differential declines from -0.199 to -0.178, for men, and from -0.092 to -0.082, for women. Pay is then, on average, lower in establishments with a large non-white presence, but this is found to explain only a small part of the ethnic wage gap.

Columns three and six, of Table 4.5, examine whether the plant's ethnic composition affects white and minority pay differently, by including an interaction term between minority status and the non-white employment share. The wages of white males are observed to be lower, white females higher, within plants with a greater proportion of minority staff. Neither effect is statistically different from zero. The effect of the workplace's ethnic composition upon non-white wages, both male and female, is, however, both negative and statistically robust. A 10 percent increase in the ethnic minority employment share is estimated to reduce log wages by -0.047 for ethnic men and by -0.049 for ethnic women, over and above

[15] Unfortunately this variable cannot be broken down by gender.

[16] Pudney (2000) finds, to the contrary, plants with a greater proportion of non-white workers pay higher wages. However, he has no controls for region, as present in this analysis, so picks up the larger presence of minority workers in the South East. Blanchflower (1984, 1986) using the predecessor WIRS surveys found mixed evidence that typical manual pay levels were lower in plants with a higher proportion of non-white workers.

the effects observed for white males and females.[17] Indeed, the lower pay of non-white women in minority plants, here, largely accounts for the female racial pay gap.

Similar evidence has been observed for the US. Carrington and Troske (1998) find white wages are increasing in the black share of plant employment, black wages declining. Hirsch and Schumacher (1992) observe both white and black wages to be declining in the proportion of black employees in the labour market (characterised by industry-occupation-region cells). The finding that, for some reason, non-white wages are lower in establishments with more minority employees then appears robust. The behaviour of white wages appears less clear cut.

One concern with the estimates in Table 4.5 is that the UK's ethnic minority population is relatively concentrated in large metropolitan cities. Regions or plants where there is little or no contact with non-white employees may then drive results, especially for white workers. Equations are then estimated for the Greater London region in Tables 4.6. The non-white population here constitutes around a quarter of the cities total population, and approximately half of all Britain's ethnic minority residents (Regional Trends, 1999). The racial wage differential is estimated to be larger in London than for Britain as a whole, -0.306 compared to -0.199 for men and -0.180 compared to -0.092 for women, in a region with a much higher degree of racial integration than the national norm.

With respect to the plant's ethnic composition, results for females are similar to those observed previously. White females suffer no penalty from working in plants with more minority workers whilst non-white pay is statistically significantly lower. The wages of white males in London are, however, found to be statistically significantly lower within plants with more non-white co-workers. A 10

[17] Unrestricted models, where all parameters are allowed to differ by race, yield similar results.

percent increase in the minority share of establishment employment is associated with a fall in white wages of -0.033 percent. For minority males the analogous fall in wages is -0.030 percent, over and above the effect upon white pay.[18]

Finally, Table 4.7 estimates how the racial wage differential varies by the ethnic minority share of establishment employment. Logically, given previous results, the racial wage gap is estimated to be greater in plants with a higher proportion of ethnic minority employees.[19]

How do these results relate to the predictions of the models outlined earlier? Whilst not providing a complete test, the evidence does allow an extensive examination of the predictions of the competing theories.

Black's (1995) model of employer discrimination predicts that as more non-white workers enter the (local) labour market the probability of a minority individual finding a good job match will rise. In turn, non-white wages increase and the racial wage gap falls. We observe non-white wages to be lower within workplaces with larger proportions of ethnic minority staff. Yet this can only test Black's model if a larger proportion of minority employees reflect a greater presence of non-white workers within the labour market. This need not be the case. An increase in the number of prejudiced employers will crowd minority workers into fewer unprejudiced firms, raising the ethnic composition of these plants, whilst reducing minority wages and increasing the pay gap.

Potentially more convincing evidence is that for London, where the ethnic minority population is much larger, the racial wage differential is observed to be

[18] This measures the *marginal* effect of the plant's ethnic minority employment share upon non-white pay, and is relative to the effect upon white pay. The *total* effect is the sum of the effects upon white and non-white pay. For minority males, this is -0.063 in London and -0.056 for the national sample.

[19] The addition of controls for occupation attenuates the coefficients upon race but leaves results substantially unchanged.

greater than for the rest of the country. This was also observed in the QLFS, both for all individuals and those aged 40 and below and born in the UK. The geographic clustering of non-white individuals in London may lead one to expect the opposite result. Ethnic minority workers' job opportunities are here likely to be superior, hence the racial wage gap lower, due to the greater likelihood of encountering a non-white employer and the reduced opportunities for prejudiced firms to survive in equilibrium. Nevertheless, a more rigorous test would examine the change in wages, over time, as a region's labour force changes in ethnic composition. This is not possible with these data.

A model of customer discrimination predicts white pay to be increasing in the plant's ethnic minority employment share, as consumer prejudice implies white productivity is greater in plants with more non-white staff. Non-white pay is expected to be lower, as minority workers have to compensate the employer for the lower prices necessary to attract prejudiced white consumers. The racial wage gap is then predicted to rise with the non-white employment share. The evidence is supportive as regards ethnic minority pay and the racial wage differential, but does not match predictions with respect to white wages. One would also expect such factors to be less important in London, where the larger pool of minority customers will act to offset the impact of prejudiced white consumers.

A model of employee prejudice would predict white pay to be greater in workplaces with more non-white staff, as white workers require a compensating differential to work with minority co-workers. White pay is, however, observed to be largely unrelated to the ethnic minority employment share. Alternatively, non-white workers may be more likely to suffer from racial abuse in 'white' plants and hence require a compensating differential to work in such establishments. Yet given the relatively small size of the ethnic minority population in the UK, non-white

workers are unlikely to be compensated with higher pay than white workers. Instead, minority workers may trade off lower pay for working with more non-white co-workers. This would explain why non-white pay is higher, and the racial pay differential smaller, in 'white' plants. Yet, if employers are unprejudiced we would expect them to hire whichever labour is cheaper, and for competition for labour to equalise wages between segregated plants.

Plants who hire more non-white workers may be expected to have a greater understanding of non-white individuals' abilities. Ethnic minority workers may then achieve superior job matches in such establishments and, if productivity is increasing in match quality, earn higher wages. Alternatively, if plants with more ethnic minority staff have a greater awareness of minority skills, they will have less recourse to use race as an indicator of productivity. If we assume firms are risk averse, rather than risk neutral, non-white wages will then be higher in more ethnically mixed plants. The reverse is observed to be true. The evidence as to statistical discrimination explaining the racial wage gap is then, here, scant.

The crowding model of Johnson and Stafford (1998), utilising the assumptions of differential discrimination and comparative advantage between jobs, predicts the racial pay gap to be lower within 'minority' plants. The evidence above is not supportive. Whether the racial wage gap is due to white workers filling more senior positions, within the plant, is examined, in section 4.5.6, below.

Finally, if ethnic minority workers are, for some reason, less productive, the plant's ethnic composition may act as an indicator of a low-skill labour force. Then high-skill, high-wage, non-white employees will work in establishments with fewer ethnic minority co-workers. This would support the finding of a negative effect of the non-white employment share on minority pay. One may also then expect white wages to be lower in such establishments. Some limited support is found for this

hypothesis for males, but very little for females. A true test of this hypothesis would, however, require a racially unbiased ability measure. Unfortunately such a measure is not available within these data.

4.5.6 Occupation and Establishment Effects

The impact of occupation upon the racial wage differential is examined in Table 4.8.[20] Column one estimates the male racial wage differential at -0.201. Controls for occupation are added in column three, and the estimated racial pay gap falls by 30 percent to -0.140. For females the estimated parameter upon ethnic minority status falls by around 20 percent, from -0.091 to -0.072 (Table 4.8, columns five and seven). Occupational differences do then explain a significant proportion of racial differences in pay, yet whether these differences reflect worker skill, individual choice or discrimination remains unclear. The unexplained racial wage differential does, however, remain large and statistically significantly different from zero.

The question remains to what extent the racial wage differential can be explained by employer characteristics, or by the matching of high-skill workers to high-wage firms. Table 4.8 indicates the answer is little. The addition of establishment effects, which capture all plant characteristics and the mean characteristics of employees in the workplace, reduces the estimate of the ethnic wage differential from -0.201 to -0.165 for men and from -0.091 to -0.086 for women. When controls for worker occupation are present a similar picture emerges, the male wage differential is estimated to decline from -0.140 to -0.126, whilst for women the differential increases slightly, from -0.072 to -0.082. All estimates remain statistically robust. Establishment effects here constitute a relatively modest

[20] Estimates are conditional upon observing at least three workers, by gender, within the plant.

contribution to the racial pay gap. Minority workers are paid significantly less than otherwise comparable whites in the same broad occupational group, and within the same workplace.

The primary avenue for the racial wage differential is not that non-whites are employed in low-pay establishments, rather they are, here, paid lower wages, on average, in any given plant. This is apparently the first such evidence for the UK, and parallels that found for the US (Carrington and Troske, 1998).

4.5.7 Job satisfaction

Whether there exist racial differences in reported job satisfaction levels is now analysed. A racial satisfaction differential, holding pay constant, could reflect discrimination upon non-pecuniary compensation, different working conditions or different expectations. If discrimination occurs, and was not fully anticipated, non-white workers may be less satisfied than equally paid whites. Yet if discrimination was expected, equally paid minority employees will have surpassed expectations and satisfaction may be higher.

Satisfaction is assumed a function of personal characteristics (such as education, experience, gender and race), employer characteristics (e.g. establishment size, industry, the ethnic composition of the workforce) and variables associated with the labour contract (income, hours of work). Job satisfaction for individual i in time period t and employer j, is then expressed as:

$$s^*_{itj} = y_{itj}'\varphi + x_{itj}'\beta + z_{tj}'\gamma + u_{itj} \qquad i = 1, \ldots, n \qquad (12)$$

$$t = 1, \ldots, T$$

$$j = j(i,t) = 1, \ldots, m.$$

Where, s* is the satisfaction variable, y the vector of pay and hours variables, x the vector of worker characteristics, z the vector of employer characteristics, u the

conformable error term with mean zero and constant variance, and φ, β and γ the vectors of parameters to be estimated.[21]

The satisfaction data are observed as ordered categorical responses (on a scale 1, 2, ..., K). These map latent well-being (s*) into discrete space (s) as below:

$$s_{itj} = k \qquad \text{if } \mu_{k-1} < s^* \leq \mu_k \; \forall \; k = 1, \ldots, K \qquad (13)$$

Estimation is then by the Ordered Probit technique of McKelvey and Zavoina (1975). This imposes the restrictions; $\mu_o \leq \mu_1 \leq \ldots \leq \mu_K$; $\mu_o = -\infty$; $\mu_K = \infty$; and the normalisation $\mu_1 = 0$ and $\sigma_U = 1$. The probability of observing a response within a category, k, is then:

$$\Pr(s = k) = \Phi(\mu_k - y_{itj}'\varphi - x_{itj}'\beta - z_{tj}'\gamma) - \Phi(\mu_{k-1} - y_{itj}'\varphi - x_{itj}'\beta - z_{tj}'\gamma) \qquad (14)$$

Where, $\Phi(.)$ is the standard normal distribution function and parameters are estimated by maximum likelihood. Larger coefficients denote higher levels of satisfaction are more likely.

Table 4.9 investigates the effect of the ethnic composition of the workforce upon employee job satisfaction using the WERS data. For white employees, both male and female, working in a plant with more ethnic minority co-workers is found to reduce satisfaction with respect to all four measures of well-being (satisfaction with: pay, respect from managers, influence over job, and sense of achievement). However, only for satisfaction with influence for men, and satisfaction with pay and achievement for women, are coefficients statistically well determined. With respect to minority males, results are mixed. Non-white men employed in plants with more minority co-workers are more satisfied with their pay and the amount of influence they have over their job, and less satisfied with their sense of achievement and the respect of managers. However, no effect is statistically robust. In contrast, for non-

[21] As with all models of job satisfaction this implicitly assumes responses are cardinal.

white females the proportion of the plant from an ethnic minority enters negatively in all four cases, and effects are on the border of statistical significance for satisfaction with pay and satisfaction with the respect from managers.

Plants where ethnic minority workers form a large proportion of the workforce are then, here, associated with lower levels of job satisfaction. This is true for white males, white females and ethnic minority women. Results for minority males are, however, mixed.[22] Results appear most consistent with workplaces with more minority staff offering inferior working conditions. Whilst a preference for segregation could explain the lower levels of white satisfaction in 'minority' plants, it cannot easily rationalise why minority satisfaction levels are not higher in such workplaces. In the presence of discrimination, one may further expect higher levels of non-white well-being in 'minority' plants, given the plausible assumption that such employers are less prejudiced.

Are non-white individuals less satisfied than otherwise comparable whites? This issue is examined in Table 4.10a. Amongst male employees, minority workers are *more* satisfied with their influence, achievement and respect from management. This may reflect lower expectations of non-white men, possibly due to discrimination in the workplace or prior to labour market entry. For the same wage, non-white satisfaction may then be higher. Nevertheless, effects are not statistically well determined. Satisfaction with pay, however, is statistically significantly lower for non-white men. For women, all satisfaction questions exhibit a negative race effect, with a statistically robust effect upon both pay and achievement. Table 4.10b reports the marginal effects of the estimates. Non-white men are 4.8 percent more likely to respond as dissatisfied or very dissatisfied with their pay and -4.2 percent

[22] Conclusions are unchanged if controls for pay are omitted.

less likely to be satisfied or very satisfied, than a similarly paid white male. For non-white women, the comparable figures are 6.2 percent and -6.0 percent respectively, relative to a similarly paid white female.

Whether the relationship between satisfaction and income is similar for white and non-white workers is examined in Table 4.11, and the ethnic minority indicator interacted with income. Results are restricted to the male sample, as no differential effect of income upon satisfaction was observed for females. Ethnic minority males are found to report higher levels of well-being at low income levels, but as earnings rise non-white satisfaction grows at a slower rate than that for white men. This differential effect is, however, statistically robust only for satisfaction with influence and satisfaction with the respect received from management. Indeed, here the satisfaction of non-white males is relatively flat across the pay distribution. This may reflect more moderate aspirations of low-skilled non-white men, for whom relative achievement is greater. For more highly paid minority workers, however, feelings of dissatisfaction become more pronounced. In contrast, for satisfaction with pay very similar income effects are observed for white and non-white men.

Whether the source of non-white individuals lower satisfaction with pay (Table 4.10a) occurs within or between plants is investigated in Table 4.12. Row one reports the racial parameter from satisfaction equations estimated by the Ordered Probit technique. Row two reports OLS estimates. Results are substantially the same. Employer specific differences in satisfaction levels are captured, in row three, by the inclusion of an establishment effect. For men, the addition of these controls leaves the estimated racial parameter upon satisfaction with pay largely unaltered. For women, the estimated racial differential is attenuated but remains statistically significantly different from zero. Satisfaction with pay is here lower for ethnic minority employees, both male and female, compared to otherwise similar

white workers in the same plant, holding pay constant. In row four of Table 4.12 pay is omitted. For both men and women, the estimated differential in satisfaction with pay increases. Conditional upon observed characteristics, lower non-white pay accounts for over half of the male racial differential in satisfaction with pay, and around a quarter of the female differential.

Interestingly, within the same plant, ethnic minority men are observed to be *more* satisfied with their sense of achievement, influence over the job, and the respect they receive from management. The latter two effects are also statistically robust. Again this may reflect lower expectations of non-white workers, and hence greater satisfaction. The question remains why diminished expectations should affect satisfaction with influence but not satisfaction with pay. A possible explanation is that influence over the job, the sense of achievement, and feeling respected by ones employer, are intrinsic concepts. In contrast, wage information is more readily available and directly comparable, and expectations as to ones comparable worth may be more quickly revised.

In summary, ethnic minority workers are found to work within plants with lower levels of job satisfaction. Perhaps the most convincing evidence that this reflects inferior working conditions is that the plant's ethnic minority employment share exerts a negative influence upon the large majority of measures of job satisfaction, for both white and ethnic minority men and women. Results are, however, not always well determined. This evidence is potentially consistent with both discrimination (possibly related to crowding) and unobserved differences in worker skill.

Are non-white workers less satisfied with their pay than observationally equivalent white employees? The evidence suggests so. For both men and women we estimate a statistically significant negative effect of race upon satisfaction with

pay, within the same workplace and holding pay constant. This finding is in line with what would be expected if non-white employees face discrimination. An alternative explanation is that non-white workers have higher expectations, and so are less satisfied for the same wage. Yet this does not seem convincing given widespread perceptions of racism and discrimination. More concretely, Table 4.10a and Table 4.12 suggest the relationship between satisfaction with pay and pay itself is similar for minority and white employees. Whilst a negative race effect upon satisfaction may be evidence of discrimination it does not necessarily imply wages are discriminatory. Ethnic minority workers may be subject to harassment and prejudice even if wages are set competitively. Yet, for this to cause the negative effect of race upon satisfaction with pay, we would expect similar strong negative effects upon the other three satisfaction measures. This is not the case.[23]

4.5.8 Turnover, Tenure and Absenteeism

An alternative to analysing subjective measures of worker well-being is to examine indicators of working conditions. Using the WERS establishment-level data, which is representative of all plants with ten or more employees, we investigate the impact of the plant's ethnic composition upon quits, turnover and absenteeism.

Columns one and two of Table 4.13 examine the determinants of the establishment's quit rate. In practice the distinction between quits and dismissals may not be clear, as there are potential benefits from a separation being in one form rather than the other. Individuals may face penalties, in terms of benefit eligibility, following a voluntary quit. Whilst employers turnover costs may be lower if they are able to induce quits on the part of employees, rather than resorting to dismissals. In

[23] A similar argument suggests this does not simply reflect perceptions of discrimination.

columns three and four, of Table 4.13, we then estimate equations upon total separations from the plant.[24] Turnover rates are here defined as the number of quits, or separations, within the last year relative to the total number employees last year. Rates may then exceed the unit interval, and equations are estimated by OLS.[25]

Table 4.13 shows plants with more ethnic minority employees are associated with statistically significantly higher rates of quits and separations.[26] A 10 percent increase in the ethnic minority employment share is predicted to increase the rate of quits by 1.2 percent, the rate of separations by 1.3 percent. The proportion of full-time employees, within the establishment, earning less than £9,000 per annum and the proportion earning more than £22,000 are entered in columns two and four of Table 4.13, and parameters are relative to intermediate pay levels. Whilst imperfect, these should broadly proxy the effect of pay.

As would be expected, the rate of quits and separations are negatively related to pay.[27] Turnover is increasing in the proportion of low pay workers, and decreasing in the proportion of employees paid £22,000 or more. This is despite the potential endogeneity bias, running from quits onto pay. The estimated parameters upon the racial composition of the plant are, however, largely unaltered. Finally, more skilled plants, as captured by the time it takes a new hire to become as proficient as an incumbent worker, are found to experience lower rates of turnover.

The higher rate of turnover in plants with more minority workers may combine employer and employee effects. Minority plants may offer inferior working conditions, with a higher rate of quits for both white and non-white employees. Or

[24] To account for possible measurement error and outliers, observations above the 99th percentile were trimmed. Results are not dependent upon this sampling condition.
[25] The (weighted) means of the quit and separation rates are respectively 0.163 and 0.228.
[26] Knight and Latreille (2000), analysing the same data set, find a higher rate of dismissals within plants with more ethnic minority employees.
[27] Weiss (1984), Leonard (1987), Campbell (1993), and Benito (1997), observe similar results.

plants with high rates of turnover may be more willing to hire minority workers.[28] Alternatively, ethnic minority workers may be more likely to quit than are similar white employees. Here previous evidence is mixed. Weiss (1984) observes, to the contrary, black workers in the US to have a lower rate of quits. Zax (1989), in contrast, finds a positive effect of race upon quits, once racial differences in responses to commuting time and local unemployment are controlled for.

Evidence is presented in Table 4.14, for the WERS individual-level data, that non-white workers have lower levels of establishment tenure, compared to otherwise similar whites.[29] Results are, however, statistically significant only at the ten percent level. Columns two and four, of Table 4.14, examine the relationship between tenure, race, and the racial composition of the plant. The workplace tenure of white employees, both male and female, is observed to be statistically significantly lower in plants with a higher non-white employment share. For ethnic minority workers, both male and female, the reverse is true; tenure is statistically significantly greater in plants with a greater proportion of non-white co-workers.[30] A 10 percent increase in the non-white employment share is predicted to increase tenure by, on average, 5 months for minority men and women, compared to white employees.

This evidence is consistent with employees expressing a preference for segregation; minority tenure is greater, white tenure lower, in plants with more non-white co-workers. The hypothesis that establishments with a larger proportion of minority workers are associated with inferior working conditions finds support in the lower levels of white tenure, but is contradicted by the higher levels of minority tenure. It is possible, however, that non-white employees trade off potential gains in

[28] The racial composition of the plant is observed only after turnover has taken place.
[29] Mumford and Smith (2000), using the same data, similarly find a negative effect of race upon job tenure. Moreover, this effect is found to occur for workers within the same establishment.
[30] Results are essentially the same if controls for pay are included.

working conditions to work with more ethnic minority co-workers. Alternatively, if high-skill ethnic minority employees work in 'white' plants the greater non-white tenure in 'minority' plants may reflect the reduced set of outside options of less skilled workers. The opposite effect is, however, observed for whites in these plants.

Table 4.15 investigates the relationship between the plant's ethnic minority employment share and the proportion of days lost to absenteeism.[31] As the dependent variable is a proportion, and bounded between zero and one, standard least squares analysis is inefficient (for reasons analogous to the linear probability model in binary data). An alternative technique, corresponding to a logit model of behaviour, is to transform the dependent proportion variable (p) into its log-odds ratio form and to estimate by weighted least squares on the transformed variable.

$$\mathrm{Ln}\left(\frac{p_j}{1-p_j}\right) = x_j'\beta + \varepsilon_j \qquad j = 1, \ldots, m. \qquad (15)$$

$$E(\varepsilon_j) = 0 \text{ and } V(\varepsilon_j) = n_j\Lambda_j(1-\Lambda_j)$$

where, n is plant employment, Λ the logistic cumulative distribution function, and weights account for the heteroskedastic error variance (see Greene, 2000).[32]

Plants with more ethnic minority employees are found to experience statistically significantly higher rates of absenteeism, both with and without conditioning upon pay within the establishment. A 10 percent increase in the minority employment share is predicted to reduce the proportion of days lost due to absences by 0.37 percent, relative to the mean of 3.9 percent.[33] Similar results were observed when equations were estimated, by OLS, on the proportions.

[31] Observations are trimmed at the 99th percentile. Results are not dependent upon this restriction.
[32] Where the proportion is zero, and the transformation undefined, a small constant is added.
[33] To calculate the marginal effect of x_j on p_j (i.e. $\partial p_j / \partial x_j$) multiply coefficients by $p_j(1-p_j)$, which at the mean equals 0.0375.

Absenteeism is negatively related to pay. The number of days lost is increasing in the proportion of low pay workers, and decreasing in the proportion of employees paid £22,000 or more. The skill of the plant, as measured by the time it takes to become proficient at the largest occupation, does not, however, exert a statistically robust effect upon absenteeism. These results echo findings for the US (Leigh, 1983).

Nevertheless, estimates may again capture workplace and individual effects. Moreover, it is not clear *a priori* whether the higher rates of absenteeism, in plants with large non-white workforces, reflects dissatisfaction or labour supply adjustments in the face of constraints on work time (Allen, 1981).

4.5.9 The determinants of the ethnic composition of the workforce

The effect of the racial composition of the workforce upon pay and well-being has been examined above. The determinants of the plant's ethnic minority employment share are now analysed using the WERS establishment data. Some 40 percent of plants, within the WERS, report employing no minority workers, hence estimation is by the Tobit technique. Table 4.16a reports parameter estimates corresponding to the latent variable. Marginal effects are reported in Table 4.16b.

The effect of aptitude tests, upon the workforce's ethnic composition, is examined in row one. Such tests may leave less scope for individual prejudice in hiring decisions, and hence induce a larger stream of non-white applicants. If aptitude tests provide an accurate signal of worker quality they may also surmount the statistical discrimination problem, that qualifications of minority workers are less informative.[34] We would then predict more minority workers to be hired. A worker

[34] The lines of causality are here, and for the other parameters, open to debate.

skill explanation, however, suggests no strong relation between tests and hiring. Workers will be assigned to jobs commensurate with their skill, irrespective of race. Aptitude tests are here estimated to increase the proportion of the workforce from ethnic minorities by 0.9 percent, parameters are not, however, well determined.

The statistical discrimination model is further explored by the inclusion of variables indicating whether an employer is especially concerned with qualifications or personal references when hiring. Where they are, statistical discrimination may be more likely. In both cases we observe a statistically significant lower proportion of non-white staff. Where a personal reference is important, the non-white employment share is estimated to be -1.9 percent lower, for qualifications the comparable figure is -2.1 percent. As sample statistics show the ethnic minority population to be more educated, it is difficult to rationalise the latter result without recourse to an employer belief that minority qualifications are less informative or inferior, or that workplaces with large minority workforces are, here, less skilled.

As Holzer (1997) observed for the US, large establishments hire a statistically significant larger number of non-white employees. Whether this reflects differential discrimination, worker choice or other factors cannot though be ascertained. Finally, results are largely invariant to the introduction of controls for pay and skill, themselves likely to be endogenously related to workforce composition.

The sensitivity of the estimates is examined in columns three and four of Table 4.16a. Heteroskedasticity has been observed to be a particular problem for the Tobit model. Johnston and DiNardo (1997) report Monte Carlo evidence that, in the presence of heteroskedasticity, OLS may have a smaller bias and variance. OLS estimates are then reported in column five. Column six follows the Censored Least Absolute Deviations (CLAD) procedure suggested by Powell (1984), this

provides consistent estimates in the face of heterokedasticity and non-normality. Results remain, largely, consistent for the Tobit, OLS and CLAD[35] estimates.

4.6 Conclusion

This chapter has investigated the relationship between race, the racial composition of establishments, pay and job satisfaction. Ethnic minorities in Britain are observed to earn less than, observationally equivalent, white employees. The racial wage differential is estimated to be approximately -20 percent for men and -10 percent for women. Around half of these differentials can be attributed to immigrant status. Whilst this may reflect discrimination, it cannot be associated purely with race.

Using new data, with detailed information about both the employer and the employee, we find non-white workers earn substantially lower wages in plants with more ethnic minority co-workers. White wages, on the other hand, are only weakly related to the racial composition of the plant. The gap between ethnic minority and white pay is hence larger in establishments with more non-white staff. In addition, the racial wage differential remains for employees in the same occupation and workplace. Consequently, the primary source of the observed racial wage gap is not that ethnic minority workers are employed in low-pay plants, rather they are less well paid in any given workplace.

The evidence does not then lend strong support to the hypothesis that non-white workers are 'crowded' into low-pay establishments. Workplace effects are found to explain little of the racial wage differential. The model of crowding predicted the racial wage gap to be smaller in plants where minority employment is

[35] Implemented in STATA by code provided in Deaton (1997).

greater, assuming these employers are less prejudiced. In fact, the racial wage differential was here larger. Occupational attainment is found to account for a significant proportion of the racial pay differential. Yet large unexplained, and statistically significant, differences in pay remain. Moreover, it is not clear whether racial differences in occupational status reflect discrimination or worker skill differences.

Statistical discrimination is not found to offer a good explanation of the behaviour of wages. Employers who hire more minority workers are likely to have a greater awareness of the reliability of minority individuals' abilities, and less recourse to use race as an indicator of productivity. Yet it is in these plants where the wage gap is observed to be greatest. Evidence is, however, found consistent with statistical discrimination in hiring. The plant's non-white employment share is observed to be lower where qualifications are an important factor in recruitment.

The model of employer discrimination investigated predicted a positive relationship between the number of non-white workers in the labour market and non-white pay. This is because, in the model, a larger ethnic minority labour pool reduces the number of prejudiced firms who can survive in equilibrium, improving employment opportunities for minority workers, hence raising non-white pay. The limited evidence here is not supportive. Within London, where ethnic minority residents form a much larger share of the total population than the national norm, the racial pay gap was estimated to be *larger* than for the country for as a whole.

Job satisfaction data are used to test whether worker well-being is lower for non-white employees. Ethnic minority workers are observed to be less satisfied with their pay, even when pay is held constant. Non-white men are 4.8 percent more likely to respond as dissatisfied or very dissatisfied with their pay and -4.2 percent less likely to be satisfied or very satisfied, than a similarly paid white male. For non-

white women, the comparable figures are 6.2 percent and -6.0 percent respectively, relative to a similarly paid white female.

Workplaces that employ more non-white staff are also found to have lower levels of job satisfaction, for white males, white females and for ethnic minority women. Results are, however, more mixed for non-white men. Other evidence, consistent with the hypothesis well-being is lower in 'minority' workplaces, is found in the higher rates of quits, separations, and absenteeism in these plants.

The lower levels of employee well-being in workplaces with a greater non-white employment share are, potentially, consistent with discrimination and crowding. More robust evidence, in favour of discrimination upon pay, is that ethnic minority employees are observed to be less satisfied with their pay, than otherwise similar white workers, even when pay is held constant.

A preference for segregation would explain the lower levels of white satisfaction and tenure in 'minority' plants, but then cannot easily rationalise why non-white satisfaction is not greater in such establishments. Prejudiced white employees would also be expected to demand a compensating differential, for working with ethnic minority co-workers, yet white wages are estimated to be independent or falling with respect to the plants ethnic composition.

Alternatively, non-white workers may be more likely to encounter racism in a 'white' plant. Ethnic minority employees may then trade off lower pay for working with more minority co-workers. This is consistent with the lower pay and greater tenure of non-white workers in plants with a greater ethnic minority employment share, but it cannot explain why non-white satisfaction is not higher in such plants. Whilst there is some evidence in support of a preference for segregation, given the model provides no strong rationale for the existence of a pay differential, it seems more likely to work in conjunction with other explanations for the racial wage gap.

The unobserved productivity hypothesis, that ethnic minority employees are for some reason less productive, offers a potentially convincing explanation for observed behaviour. Workplaces that hire more ethnic minority employees are, under this hypothesis, liable to be less skilled and to offer inferior working conditions, explaining the lower pay and satisfaction within these plants. The lower racial wage differential in plants with less minority workers would then reflect a positive selection of the most able minority employees to work in 'white' establishments. Whilst this hypothesis cannot be rejected, a true test requires a racially unbiased ability measure. Unfortunately such a measure is not available within these data.

In summary, non-white workers are, here, employed in workplaces with lower levels of worker well-being. This may, however, reflect discrimination or unobserved worker quality differences. Results are also presented which suggest ethnic minority employees may trade off lower pay to work with more minority co-workers. Nevertheless, ethnic minority employees are found to be less satisfied with their pay, compared to otherwise similar white workers, even when pay is held constant. This provides new evidence potentially indicative of racial discrimination, on pay, within the workplace.

TABLE 4.1a
QLFS Sample Means (1997-99)

	MALE		*FEMALE*	
	WHITE	*ETHNIC*	*WHITE*	*ETHNIC*
Hourly Pay	8.92 (6.22)	8.10 (4.51)	6.60 (5.06)	6.96 (4.52)
Potential Experience	22.22 (12.21)	18.02 (10.86)	22.23 (11.92)	18.53 (10.86)
Tenure	9.00 (9.12)	6.25 (6.49)	7.01 (7.05)	5.85 (6.49)
Years of Schooling	11.19 (2.55)	13.01 (2.99)	11.15 (2.32)	12.50 (2.98)
Qualification: Degree	0.18 (0.38)	0.24 (0.43)	0.14 (0.35)	0.18 (0.39)
Workplace size 25 or more	0.72 (0.45)	0.71 (0.45)	0.63 (0.48)	0.71 (0.45)
Temporary Job	0.05 (0.22)	0.09 (0.29)	0.07 (0.25)	0.10 (0.30)
Public Sector Employee	0.21 (0.41)	0.20 (0.40)	0.36 (0.48)	0.39 (0.49)
Number of Individuals	55647	2487	56868	2461
Number of Observations	77029	3182	79302	3205

- Standard deviations are in parentheses. Pay is deflated to January 1997 values.

TABLE 4.1b
WERS Sample Means (1998)

	MALE		*FEMALE*	
	WHITE	*ETHNIC*	*WHITE*	*ETHNIC*
Hourly Pay	7.88 (5.02)	8.14 (9.87)	6.03 (4.01)	7.01 (5.43)
Proportion of Plant Ethnic	0.04 (0.08)	0.18 (0.19)	0.04 (0.08)	0.22 (0.22)
Age: 40 or more	0.48 (0.50)	0.37 (0.48)	0.48 (0.50)	0.37 (0.48)
Tenure: 5 years or more	0.52 (0.50)	0.38 (0.49)	0.45 (0.50)	0.39 (0.49)
Qualification: Degree	0.21 (0.40)	0.38 (0.49)	0.19 (0.39)	0.30 (0.46)
Workplace size 25 or more	0.90 (0.30)	0.91 (0.29)	0.84 (0.36)	0.83 (0.37)
Temporary Job	0.06 (0.24)	0.09 (0.29)	0.08 (0.27)	0.10 (0.31)
Public Sector Employee	0.24 (0.43)	0.29 (0.46)	0.38 (0.49)	0.39 (0.49)
Union Recognised at workplace	0.62 (0.49)	0.61 (0.49)	0.59 (0.49)	0.60 (0.49)
Number of Workplaces	1577	300	1591	301
Number of Observations	11685	448	11880	508

- Standard deviations are in parentheses. Statistics use WERS sample weights.
- Total number of plants is 1705, of which 486 are observed with a sampled non-white employee.

TABLE 4.2
Regression Results by Ethnic Group (QLFS)
Dependent Variable: Ln(wage)

	MALE		*FEMALE*	
REGRESSOR	*WHITE*	*ETHNIC*	*WHITE*	*ETHNIC*
Experience	0.041	0.031	0.026	0.023
	(0.001)	(0.003)	(0.001)	(0.003)
Experience^2/100	-0.074	-0.061	-0.049	-0.046
	(0.001)	(0.006)	(0.001)	(0.006)
Employer Tenure	0.017	0.022	0.022	0.025
	(0.001)	(0.004)	(0.001)	(0.004)
Employer Tenure^2/100	-0.025	-0.021	-0.038	-0.048
	(0.002)	(0.015)	(0.003)	(0.014)
Workplace Size: 25 plus	0.137	0.177	0.098	0.067
	(0.005)	(0.023)	(0.004)	(0.020)
Years of Schooling	0.041	0.022	0.042	0.016
	(0.001)	(0.004)	(0.001)	(0.004)
Other Qualification	0.077	0.137	0.090	0.096
	(0.007)	(0.031)	(0.006)	(0.030)
O-Level or equivalent	0.180	0.214	0.159	0.238
	(0.007)	(0.033)	(0.005)	(0.033)
A-Level or equivalent	0.239	0.283	0.218	0.303
	(0.007)	(0.032)	(0.006)	(0.038)
Other Degree	0.420	0.384	0.444	0.417
	(0.009)	(0.044)	(0.008)	(0.038)
Degree or above	0.552	0.581	0.576	0.659
	(0.010)	(0.041)	(0.009)	(0.041)
Public Sector	-0.022	0.003	0.107	0.045
	(0.008)	(0.037)	(0.006)	(0.026)
Oaxaca Unexplained Wage differential	-0.191		-0.119	
Observations				
Individuals	55647	2487	56868	2461
Panel Total	77029	3182	79302	3205
Adjusted R^2	0.43	0.43	0.42	0.38

1. All regressions include controls for industry (SIC code at the one-digit level), region, marital status, temporary employment and time period.
2. Standard errors are in parentheses and are robust to arbitrary heteroskedasticity and the repeat sampling of individuals.
3. The education qualification variables are with reference to the omitted category, no qualification.

TABLE 4.3
Regression Results by Ethnic Group (WERS)
Dependent Variable: Ln(wage)

	MALE		*FEMALE*	
REGRESSOR	*WHITE*	*ETHNIC*	*WHITE*	*ETHNIC*
Age:20-24	0.191	0.310	0.134	0.159
	(0.029)	(0.161)	(0.022)	(0.117)
Age:25-29	0.383	0.363	0.301	0.311
	(0.028)	(0.159)	(0.022)	(0.117)
Age:30-39	0.534	0.474	0.384	0.382
	(0.028)	(0.170)	(0.022)	(0.116)
Age:40-49	0.642	0.471	0.396	0.437
	(0.030)	(0.178)	(0.022)	(0.129)
Age:50-59	0.626	0.454	0.374	0.311
	(0.031)	(0.182)	(0.023)	(0.131)
Age:60 or more	0.438	0.302	0.251	0.608
	(0.036)	(0.213)	(0.032)	(0.241)
Workplace Tenure:1-2 years	0.007	-0.054	0.022	-0.103
	(0.016)	(0.073)	(0.014)	(0.063)
Workplace Tenure:2-5 years	0.055	0.057	0.062	-0.008
	(0.014)	(0.069)	(0.012)	(0.057)
Workplace Tenure:5-10 years	0.093	0.111	0.129	0.104
	(0.016)	(0.081)	(0.013)	(0.061)
Workplace Tenure:10 years plus	0.145	0.131	0.174	0.092
	(0.016)	(0.088)	(0.013)	(0.071)
Ln(workplace size)	0.027	-0.017	0.023	0.054
	(0.006)	(0.026)	(0.005)	(0.016)
CSE or equivalent	0.115	0.122	0.120	0.023
	(0.014)	(0.093)	(0.013)	(0.097)
O-Level or equivalent	0.250	0.175	0.257	0.246
	(0.013)	(0.094)	(0.012)	(0.091)
A-Level	0.360	0.289	0.360	0.368
	(0.015)	(0.077)	(0.014)	(0.098)
Degree	0.639	0.502	0.670	0.394
	(0.016)	(0.087)	(0.015)	(0.096)
Post-graduate degree	0.754	0.707	0.790	0.728
	(0.021)	(0.103)	(0.021)	(0.127)
Public Sector	-0.020	-0.083	0.093	0.090
	(0.023)	(0.078)	(0.022)	(0.072)
Union recognised at workplace	0.016	0.208	0.015	-0.048
	(0.018)	(0.075)	(0.016)	(0.063)
Oaxaca Unexplained Wage differential	-0.207		-0.083	
Observations				
Workplaces	1577	300	1591	301
Individuals	11685	448	11880	508
Adjusted R^2	0.49	0.32	0.45	0.41

1. All regressions include controls for industry (SIC code at the one-digit level), single establishment enterprise, region, marital status and temporary employment.
2. Standard errors are in parentheses and are robust to arbitrary heteroscedasticity and the repeat sampling of employees within establishments.
3. The education qualification variables are with reference to the omitted category, no qualification. Age is relative to less than 20 years old, workplace tenure to less than 1-year tenure.

TABLE 4.4
The Effect of Immigration (QLFS)
Dependent Variable: Ln(wage)

	ALL		*AGE ≤ 40*		*AGE ≤ 40 UK BORN*	
REGRESSOR	*MALE*	*FEMALE*	*MALE*	*FEMALE*	*MALE*	*FEMALE*
Ethnic	-0.182	-0.112	-0.182	-0.112	-0.100	-0.054
	(0.010)	(0.009)	(0.012)	(0.011)	(0.015)	(0.014)
Observations						
Ethnic Workers	2487	2461	1683	1681	724	810
All Individuals	58134	59329	33058	33453	30764	31017
Panel Total	80211	82507	44050	44761	41137	41650
Adjusted R^2	0.43	0.41	0.46	0.42	0.46	0.43

1. Regressions include all the controls examined in Table 4.2 but are suppressed in the presentation of results. Coefficient estimates are very similar to those for the white sample.
2. Standard errors are in parentheses and are robust to arbitrary heteroskedasticity and the repeat sampling of individuals.

TABLE 4.5
Wages and the Racial Composition of the Plant (WERS)
Dependent Variables: Ln(wage)

	MALE			*FEMALE*		
REGRESSOR	*ALL*	*ALL*	*ALL*	*ALL*	*ALL*	*ALL*
Ethnic	-0.199	-0.178	-0.105	-0.092	-0.082	0.005
	(0.026)	(0.023)	(0.029)	(0.022)	(0.020)	(0.026)
Proportion of Plant Ethnic		-0.199	-0.089		-0.073	0.070
		(0.092)	(0.096)		(0.079)	(0.081)
Ethnic * Plant Ethnic			-0.472			-0.493
			(0.135)			(0.116)
Observations						
Workplaces	1588	1588	1588	1596	1596	1596
Ethnic workers	448	448	448	508	508	508
All Individuals	12133	12133	12133	12388	12388	12388
Adjusted R^2	0.49	0.49	0.49	0.45	0.45	0.45

1. Regressions include all the controls examined in Table 4.3. Parameter estimates are not reported.
2. Standard errors are in parentheses and are robust to arbitrary heteroskedasticity and the repeat sampling of individuals.

TABLE 4.6
Wages and the Racial Composition of the Plant (WERS)
The London Sample
Dependent Variables: Ln(wage)

	MALE			*FEMALE*		
REGRESSOR	*ALL*	*ALL*	*ALL*	*ALL*	*ALL*	*ALL*
Ethnic	-0.306	-0.248	-0.180	-0.180	-0.156	-0.066
	(0.043)	(0.038)	(0.058)	(0.031)	(0.030)	(0.046)
Proportion of Plant Ethnic		-0.418	-0.332		-0.151	-0.017
		(0.100)	(0.123)		(0.099)	(0.114)
Ethnic * Plant Ethnic			-0.300			-0.351
			(0.176)			(0.151)
Observations						
Workplaces	219	219	219	217	217	217
Ethnic workers	182	182	182	256	256	256
All Individuals	1632	1632	1632	1587	1587	1587
Adjusted R^2	0.43	0.44	0.45	0.43	0.43	0.43

1. See notes to Table 4.5.

TABLE 4.7
The Ethnic Wage Differential by the Ethnic Composition of the Plant (WERS)
Dependent Variable: Ln(wage)

	MALE			*FEMALE*		
REGRESSOR	*PLANT <10% ETHNIC*	*PLANT 10-24% ETHNIC*	*PLANT 25%+ ETHNIC*	*PLANT <10% ETHNIC*	*PLANT 10-24% ETHNIC*	*PLANT 25%+ ETHNIC*
Ethnic	-0.180	-0.206	-0.282	-0.004	-0.118	-0.194
	(0.032)	(0.040)	(0.056)	(0.030)	(0.034)	(0.044)
Observations						
Workplaces	736	138	75	719	146	81
Ethnic Workers	165	125	112	167	146	165
All Individuals	6038	981	427	5631	1123	494
Adjusted R^2	0.49	0.56	0.39	0.45	0.57	0.43

1. See notes for Table 4.5.
2. Plants were the manager reports there are no ethnic minority workers are here excluded.

TABLE 4.8
The Impact of Occupation and Establishment Effects (WERS)
Dependent Variable: Ln(wage)

	MALE				*FEMALE*			
REGRESSOR	*3 Plus*	*3 Plus*	*3 Plus*	*3 Plus*	*3 Plus*	*3 Plus*	*3 Plus*	*3 Plus*
Ethnic	-0.201	-0.165	-0.140	-0.126	-0.091	-0.086	-0.072	-0.082
	(0.027)	(0.020)	(0.022)	(0.018)	(0.022)	(0.019)	(0.020)	(0.018)
Occupation	No	No	Yes	Yes	No	No	Yes	Yes
Workplace Effects	No	Yes	No	Yes	No	Yes	No	Yes
Observations								
Workplaces	1293	1293	1293	1293	1312	1312	1312	1312
Ethnic Workers	425	425	425	425	488	488	488	488
All Individuals	11703	11703	11703	11703	11997	11997	11977	11977
Adjusted R^2	0.49	0.61	0.59	0.69	0.45	0.56	0.55	0.62

1. See notes for Table 4.5.
2. At least 3 gender-person observations must be observed within a plant for that establishment to be included in the sample.

TABLE 4.9
Job Satisfaction and Race (WERS)
Dependent Variables: Job Satisfaction Scores

	MALE							
REGRESSOR	*Influence* WHITE	*Pay* WHITE	*Achievement* WHITE	*Respect of Boss* WHITE	*Influence* ETHNIC	*Pay* ETHNIC	*Achievement* ETHNIC	*Respect of Boss* ETHNIC
Proportion of Plant Ethnic	-0.440	-0.329	-0.297	-0.274	0.425	0.044	-0.187	-0.024
	(0.189)	(0.186)	(0.177)	(0.188)	(0.324)	(0.281)	(0.335)	(0.302)
Ln(pay)	0.370	0.770	0.337	0.285	0.009	0.706	0.184	0.066
	(0.029)	(0.035)	(0.027)	(0.029)	(0.118)	(0.118)	(0.129)	(0.123)
Observations								
Workplaces	1569	1569	1569	1569	295	295	295	295
Individuals	11374	11374	11374	11374	429	429	429	429
Log-L	-15120.9	-15747.5	-15263.4	-16178.9	-554.3	-586.4	-551.9	-588.1
Pseudo R^2	0.065	0.140	0.059	0.069	0.151	0.172	0.214	0.188

	FEMALE							
REGRESSOR	*Influence* WHITE	*Pay* WHITE	*Achievement* WHITE	*Respect of Boss* WHITE	*Influence* ETHNIC	*Pay* ETHNIC	*Achievement* ETHNIC	*Respect of Boss* ETHNIC
Proportion of Plant Ethnic	-0.217	-0.565	-0.470	-0.248	-0.238	-0.581	-0.457	-0.859
	(0.159)	(0.181)	(0.163)	(0.157)	(0.309)	(0.298)	(0.306)	(0.331)
Ln(pay)	0.181	0.483	0.160	0.111	0.023	0.335	0.010	-0.008
	(0.027)	(0.030)	(0.027)	(0.027)	(0.122)	(0.131)	(0.116)	(0.131)
Observations								
Workplaces	1587	1587	1587	1587	290	290	290	290
Individuals	11467	11467	11467	11467	471	471	471	471
Log-L	-14470.4	-15814.9	-14509.2	-15719.7	-627.4	-629.8	-610.3	-637.6
Pseudo R^2	0.030	0.073	0.068	0.044	0.073	0.184	0.115	0.168

1. Estimation is by the Ordered Probit technique. The Pseudo R^2 is calculated using the method of McKelvey and Zavoina (1975)
2. Regressions also include all the controls examined in Table 4.3 and also normal hours. Parameter estimates are not reported.
3. Standard errors are in parentheses and are robust to arbitrary heteroscedasticity and the repeat sampling within the establishment.

TABLE 4.10a
Job Satisfaction and Race (WERS)
Dependent Variables: Job Satisfaction Scores

	MALE			
	Influence	*Pay*	*Achievement*	*Respect of Boss*
REGRESSOR	*ALL*	*ALL*	*ALL*	*ALL*
Ethnic	0.082	-0.122	0.050	0.042
	(0.057)	(0.055)	(0.057)	(0.056)
Ln(pay)	0.353	0.767	0.331	0.280
	(0.028)	(0.035)	(0.027)	(0.028)
Observations				
Workplaces	1580	1580	1580	1580
Ethnic Individuals	429	429	429	429
All Individuals	11803	11803	11803	11803
Log-L	-15715.7	-16359.1	-15856.0	-16794.1
Pseudo R^2	0.061	0.139	0.058	0.069
	FEMALE			
	Influence	*Pay*	*Achievement*	*Respect of Boss*
REGRESSOR	*ALL*	*ALL*	*ALL*	*ALL*
Ethnic	-0.049	-0.160	-0.152	-0.070
	(0.060)	(0.055)	(0.054)	(0.056)
Ln(pay)	0.174	0.471	0.154	0.108
	(0.026)	(0.029)	(0.027)	(0.027)
Observations				
Workplaces	1594	1594	1594	1594
Ethnic Individuals	471	471	471	471
All Individuals	11938	11938	11938	11938
Log-L	-15114.3	-16496.5	-15147.5	-16391.4
Pseudo R^2	0.030	0.072	0.068	0.044

1. See notes to Table 4.11.

TABLE 4.10b
Marginal Effects of Race upon Job Satisfaction

	MALE				
Dependent variable	*Satisfaction Score*				
Satisfaction with respect to:	*1*	*2*	*3*	*4*	*5*
Influence over Job	-0.006	-0.014	-0.012	0.015	0.016
Pay	0.027	0.021	-0.006	-0.036	-0.006
Sense of achievement	-0.005	-0.007	-0.007	0.008	0.011
Respect get from supervisors	-0.007	-0.006	-0.004	0.008	0.008
	FEMALE				
Dependent variable	*Satisfaction Score*				
Satisfaction with respect to:	*1*	*2*	*3*	*4*	*5*
Influence over Job	0.003	0.008	0.008	-0.010	-0.009
Pay	0.030	0.032	-0.003	-0.050	-0.010
Sense of achievement	0.012	0.022	0.022	-0.024	-0.032
Respect get from supervisors	0.009	0.010	0.008	-0.011	-0.016

1. Marginal effects are based upon Table 4.10a above and are calculated, at the mean, as the difference in the predicted probability, of satisfaction score k, for a ethnic minority employee relative to a white worker.

TABLE 4.11
Job Satisfaction, Race and the Effect of Pay (WERS)
Dependent Variables: Job Satisfaction Scores

	MALE			
	Influence	*Pay*	*Achievement*	*Respect of Boss*
REGRESSOR	*ALL*	*ALL*	*ALL*	*ALL*
Ethnic	1.825	0.392	0.847	1.554
	(0.430)	(0.565)	(0.532)	(0.469)
Ethnic * Ln(pay)	-0.310	-0.091	-0.142	-0.269
	(0.075)	(0.099)	(0.093)	(0.082)
Ln(pay)	0.364	0.770	0.336	0.290
	(0.029)	(0.035)	(0.027)	(0.028)
Observations				
Workplaces	1580	1580	1580	1580
Ethnic Individuals	429	429	429	429
All Individuals	11803	11803	11803	11803
Log-L	-15708.6	-16358.5	-15854.5	-16788.8
Pseudo R^2	0.062	0.139	0.058	0.070

1. See notes to Table 4.11.

TABLE 4.12
Job Satisfaction and Race (WERS)
The Estimated Effect of Race by Different Estimation Technique
Dependent Variables: Job Satisfaction Scores

	MALE			
	Influence	*Pay*	*Achievement*	*Respect of Boss*
Estimated by	*ALL*	*ALL*	*ALL*	*ALL*
Ordered Probit	0.064	-0.116	0.049	0.041
	(0.058)	(0.057)	(0.059)	(0.058)
OLS – No establishment Effect	0.057	-0.108	0.040	0.034
	(0.053)	(0.056)	(0.056)	(0.062)
OLS – Establishment Effect	0.129	-0.110	0.088	0.122
	(0.057)	(0.060)	(0.058)	(0.063)
OLS – Establishment Effect	0.061	-0.244	0.030	0.062
Pay term omitted	(0.057)	(0.061)	(0.058)	(0.063)
	FEMALE			
	Influence	*Pay*	*Achievement*	*Respect of Boss*
Estimated by	*ALL*	*ALL*	*ALL*	*ALL*
Ordered Probit	-0.041	-0.163	-0.131	-0.059
	(0.061)	(0.056)	(0.055)	(0.058)
OLS – No establishment Effect	-0.049	-0.177	-0.116	-0.069
	(0.053)	(0.055)	(0.051)	(0.060)
OLS – Establishment Effect	-0.025	-0.126	-0.071	0.007
	(0.054)	(0.057)	(0.056)	(0.059)
OLS – Establishment Effect	-0.043	-0.172	-0.088	-0.006
Pay term omitted	(0.055)	(0.058)	(0.056)	(0.058)

1. See notes for Table 4.11.
2. At least 3 gender-person observations must be observed within a plant for the establishment to be included in the sample.
3. Male sample: 1278 establishments, 406 ethnic individuals, and 11359 individuals in total.
4. Female sample: 1300 establishments, 450 ethnic individuals, and 11515 individuals in total.

TABLE 4.13
Workforce Ethnic Composition and Turnover (WERS)
Plant-Level Data
Dependent Variables: Quit Rate/Separation Rate

	Quits		*Separations*	
REGRESSOR	*ALL*	*ALL*	*ALL*	*ALL*
Proportion of Plant Ethnic	0.116	0.115	0.132	0.130
	(0.048)	(0.047)	(0.054)	(0.052)
Proficiency: 1-6 months		-0.017		-0.023
		(0.011)		(0.012)
Proficiency: 6 months or more		-0.025		-0.038
		(0.010)		(0.012)
Proportion FT paid £9k or less		0.086		0.100
		(0.030)		(0.033)
Proportion FT paid £22k plus		-0.037		-0.039
		(0.019)		(0.026)
Observations	1628	1628	1628	1628
Adjusted R^2	0.30	0.31	0.24	0.26

1. Regressions also include controls for the composition of the establishment by age, gender, part-time employment and occupation, and for workplace size, single establishment organisation, union recognition, industry (SIC code at the one-digit level), employing sector and region. Parameter estimates are not reported. Standard errors are in parentheses.
2. PROFICIENCY is a manager-derived variable that measures the amount of time required for a new employee, in the largest occupational group, to become proficient at their job. The omitted category is less than 1 month.
3. Equations are estimated by OLS.
4. Mean quit rate 0.163. Mean separations rate 0.228.Mean proportion of plant ethnic 0.041.

TABLE 4.14
Workplace Tenure and the Racial Composition of the Plant (WERS)
Dependent Variables: Years of Workplace Tenure

	MALE		*FEMALE*	
REGRESSOR	*ALL*	*ALL*	*ALL*	*ALL*
Ethnic	-0.496	-0.989	-0.429	-1.130
	(0.300)	(0.383)	(0.227)	(0.313)
Proportion of Plant Ethnic		-2.313		-1.351
		(1.058)		(0.851)
Ethnic * Plant Ethnic		4.131		4.076
		(1.673)		(1.238)
Observations				
Workplaces	1588	1588	1596	1596
Ethnic workers	448	448	508	508
All Individuals	12133	12133	12388	12388
Log-L	-17873.5	-17867.9	-18514.6	-18508.5
Pseudo R^2	0.161	0.162	0.166	0.167

1. Regressions include all controls examined in Table 4.3 and also occupation (at the one-digit level). Parameter estimates are not reported.
2. Tenure is identified in one of 5 bands. Equations are estimated by maximum likelihood interval regression (Stewart, 1983a) and robust standard errors are in parentheses.
3. Pseudo R^2 is calculated using the method of McKelvey and Zavoina (1975).

TABLE 4.15
Plant-Level Data
Workforce Ethnic Composition and Absenteeism (WERS)
Dependent Variable: Proportion days lost to absences

REGRESSOR	*ALL*	*ALL*
Proportion of Plant Ethnic	0.987	0.983
	(0.190)	(0.190)
Proficiency: 1-6 months		0.075
		(0.051)
Proficiency: 6 months or more		0.052
		(0.061)
Proportion FT paid £9k or less		0.072
		(0.141)
Proportion FT paid £22k plus		-0.300
		(0.155)
Observations	1417	1417
Adjusted R^2	0.17	0.17

1. Regressions also include controls for the composition of the establishment by age, gender, part-time employment and occupation, and for workplace size, single establishment organisation, union recognition, industry (SIC code at the one-digit level), employing sector and region. Parameter estimates are not reported. Standard errors are in parentheses.
2. PROFICIENCY is a manager-derived variable that measures the amount of time required for a new employee, in the largest occupational group, to become proficient at their job. The omitted category is less than 1 month.
3. Equations are estimated by weighted least squares and correspond to minimum χ^2 estimates.
4. Mean absenteeism rate 0.039. Mean proportion of plant ethnic 0.041.
5. Coefficients here show the percentage change in $p_i / (1-p_i)$ for a one-unit change in the independent variable x_i. To calculate the marginal effect of x_i on p_i (i.e. $\partial p_i / \partial x_i$) multiply the coefficients by $p_i (1-p_i)$, which at the mean equals 0.0375.

TABLE 4.16a
Determinants of Workforce Ethnic Composition (WERS)
Plant-Level Data
Dependent Variable: Establishment Ethnic Minority Employment Share

REGRESSOR	*TOBIT ALL*	*TOBIT ALL*	*OLS ALL*	*CLAD ALL*
Aptitude Tests	0.009	0.009	0.007	0.002
	(0.007)	(0.007)	(0.005)	(0.004)
Personal Reference	-0.019	-0.019	-0.006	-0.009
	(0.008)	(0.008)	(0.005)	(0.006)
Qualifications	-0.021	-0.021	-0.009	-0.014
	(0.008)	(0.008)	(0.006)	(0.005)
Ln(workplace size)	0.031	0.031	0.008	0.014
	(0.003)	(0.003)	(0.002)	(0.002)
Proficiency: 1-6 months		0.011	0.005	0.006
		(0.008)	(0.005)	(0.006)
Proficiency: 6 months plus		-0.005	-0.004	-0.007
		(0.009)	(0.005)	(0.008)
Proportion FT Paid £9k or less		-0.024	-0.016	-0.045
		(0.023)	(0.015)	(0.011)
Proportion FT Paid £22k plus		-0.053	-0.047	-0.023
		(0.020)	(0.014)	(0.015)
Observations	1709	1709	1709	755
Log-L	332.3	338.0		
Pseudo R^2	0.24	0.24		0.16
Adjusted R^2			0.27	

1. Regressions also include controls for the composition of the establishment by age, gender, part-time employment and occupation, and for workplace size, single establishment organisation, union recognition, industry (SIC code at the one-digit level), employing sector and region. Parameter estimates are not reported.
2. Robust standard errors are in parentheses. Standard errors are bootstrapped in column four.
3. For the Tobit estimates the Pseudo R^2 is calculated using the method of McKelvey and Zavoina (1975). For the CLAD model it is calculated by 1-(Σ absolute deviations/Σ raw deviations).
4. For the workforce ethnic composition 706 values are censored at zero with 1003 positive observations. Its unconditional mean is 0.0407, conditional on being uncensored it is 0.110.

TABLE 4.16b
Tobit Marginal Effects

Variable	*I*	*II*
Aptitude Tests	0.0033	0.0033
Personal Reference	-0.0065	-0.0065
Qualifications	-0.0076	-0.0076
Ln(workplace size)	0.0110	0.0111
Proportion Paid £9k or less		-0.0084
Proportion Paid £22k plus		-0.0190

1. Marginal effects are based upon the Tobit estimates, columns one and two of Table 4.16a above, and are calculated for the unconditional expected value.
2. The marginal effect for the test dummy is calculated as the change in predicted probability of moving from a plant that does not test to one that does. Similarly for the reference, qualification, and proficiency dummies.
3. Marginal effects for log workplace size, and the proportion of workers paid less than £9,000 or more than £22,000 are calculated at the mean.

APPENDIX: Sample Selectivity within the WERS

The WERS survey is a cluster stratified random sample, where the sampling fraction is increasing in establishment size and varies by industry code. Large establishments are then over-sampled. Secondly, the sampling strategy of interviewing a maximum of 25 workers per establishment implies that a plant with 25 employees can contribute the same number of employee data points as a workplace with 100 or 1000 workers. Hence, within the sample of WERS plants, the individual data over-samples respondents within small establishments. The WERS data then suffers from sample selectivity in two opposite directions. However, since the chief selection mechanisms, establishment size and industry, are explanatory variables in all analyses it is not clear as to why results should be biased.

To check whether coefficients suffer from selection bias they are compared to estimates using the WERS sample survey weights (see Airey et al, 1999, and Cully et al, 1999). DuMouchel and Duncan (1983) discuss the merits and uses of sample survey weights. Weighting is unnecessary where the model holds independent of the stratification, where parameters are the same for each stratum. Or where we include amongst the explanatory variables the variables upon which selection is based, intuitively we then control for selection. Both weighted and unweighted estimates are then consistent, and the use of sample weights should be avoided. Weighting the variance covariance matrix, when unnecessary, inflates standard errors and introduces random variation in coefficients. This is potentially problematic here, as results are identified by examining a relatively small number of non-white employees (approximately round 450 minority males and 500 non-white females).

Table A1 estimates hourly pay equations for males and females, both with and without sample weights. For men, results are similar for both the unweighted

and weighted estimates, with an ethnic minority coefficient of -0.199 and -0.169 respectively. For women, the analogous estimates are -0.092 and -0.045, with the latter not statistically well determined.

As a further check results are compared to those from the QLFS. The male racial wage differential is estimated, for the QLFS, to be -0.182. When an analogous sample and time period to the WERS data are examined the estimate is -0.196. The comparable estimate for the, unweighted, WERS data is -0.199 in column one of Table A1. Results are then of a similar magnitude for male employees. For females in the QLFS, the estimated non-white wage gap is -0.112. For the sample that corresponds to the WERS data, the female racial wage differential is -0.131. The estimate for the WERS itself is slightly lower at -0.092 (column two, Table A1). For females the unweighted estimates then appear comparable to those observed for the QLFS, and if anything understate the differential.

These results suggest the unweighted estimates do not suffer unduly from selection bias. The estimation strategy adopted is to include establishment size and industry as control variables in all regressions. To account for potential variation in coefficients, by observed characteristics, all standard errors are robust to arbitrary heteroskedasticity. The potential non-independence of errors within the same plant is also corrected for, as ignoring the clustering of individuals within workplaces can potentially significantly underestimate standard errors (see Moulton, 1986).

TABLE A1
The Impact of Sample Weights upon the WERS
Dependent Variable: Ln(wage)

	UNWEIGHTED		*WEIGHTED*	
REGRESSOR	*MALE*	*FEMALE*	*MALE*	*FEMALE*
Ethnic	-0.199	-0.092	-0.169	-0.045
	(0.026)	(0.022)	(0.040)	(0.029)
Observations				
Workplaces	1588	1596	1588	1596
Ethnic Individuals	12133	12388	12133	12388
All Individuals	0.49	0.45	0.47	0.44
Adjusted R^2	448	508	448	508

1. See notes to Table 4.5.

Chapter Five

Does Money Buy Happiness? A Longitudinal Study using Data on Windfalls

This chapter is based upon joint work with Professor Andrew Oswald.

Abstract

One of the most fundamental ideas in economics is that money makes people happy. This chapter constructs a test. It studies longitudinal information on the psychological health and reported happiness of approximately 9,000 randomly chosen people. In the spirit of a natural experiment, the chapter shows that those in the panel who receive windfalls – by winning lottery money or receiving an inheritance – have higher mental well-being in the following year. A windfall of 50,000 pounds is associated with a rise in well-being of between 0.1 and 0.3 standard deviations.

5.1 Introduction

A central tenet of economics is that money makes people happy. Using deduction, rather than evidence, economists teach their students that utility must be increasing in income.[1] In this chapter we construct one of the first empirical tests. Our results, using two measures of mental well-being, show that the economist's textbook view is correct. We also estimate the size of the effect of a windfall on well-being.

To make persuasive progress on this problem, data with three special features are required. First, it is necessary to have a panel of people, that is, longitudinal rather than purely cross-sectional information. Second, measures of psychological well-being are needed. Third, it is necessary to observe, whether by an actual or natural experiment, a random assignment of money amongst individuals. We have a data set that approximates these conditions. As far as we know, previous investigators in economics or psychology have been unable to implement such a test. Diener and Biswas-Diener (2000) argue that this form of research design is required.

Individuals' survey responses to questions about well-being are used in the chapter. Such responses have been studied before. They have been used intensively by psychologists[2], examined a little by sociologists and political scientists[3], and

[1] A common approach would be to argue that more income simply must make people happier because it opens up extra choices that are denied those with less money; yet in principle human beings might find it costly to make decisions about how to spend the greater income. Another argument might be that people seek more income whenever they can, so that it necessarily makes them happier; yet in principle they could be mistaken about how they will feel *ex post*. However, the best reason to want empirical evidence is that it is dangerous for any subject to reach the point where it cannot be conceived that a familiar assumption might be wrong.

[2] Earlier work includes Andrews (1991), Argyle (1989), Campbell (1981), Diener (1984), Diener et al (1999), Douthitt et al (1992), Fox and Kahneman (1992), Larsen et al (1984), Mullis (1992), Shin (1980), Veenhoven (1991, 1993), and Warr (1990).

[3] For example, Inglehart (1990) and Gallie et al (1998). There is also a related literature on interactions between economic forces and people's voting behaviour; see for example Frey and Schneider (1978).

studied to a small, but growing, extent by economists[4]. Some economists may emphasise the likely unreliability of subjective data – perhaps because they are unaware of the large literature by research psychologists that uses such numbers, or perhaps because they believe economists are better judges of human motivation than those researchers. A recent literature on the border between economics and psychology, however, has attempted to understand the patterns in happiness and stress data.

5.2 Well-being Patterns

One definition of happiness is the degree to which an individual judges the overall quality of life in a favourable way (Veenhoven, 1991, 1993).

Self-reported well-being measures are thought to be a reflection of at least four factors: circumstances, aspirations, comparisons with others, and a person's baseline happiness or disposition (e.g. Warr, 1980, Chen and Spector, 1991). Konow and Earley (1999) describes evidence that recorded happiness levels have been demonstrated to be correlated with:

1. Objective characteristics such as unemployment.
2. The person's recall of positive versus negative life-events.
3. Assessments of the person's happiness by friends and family members.
4. Assessments of the person's happiness by his or her spouse.
5. Duration of authentic or so-called Duchenne smiles (a Duchenne smile occurs when both the zygomatic major and obicularus orus facial muscles fire, and human beings identify these as 'genuine' smiles).

[4] Recent research papers include: Blanchflower and Freeman (1997), Blanchflower and Oswald (1998, 1999), Clark (1996), Clark and Oswald (1994), Di Tella and MacCulloch (1999), Di Tella

6. Heart rate and blood pressure measures responses to stress.
7. Skin-resistance measures of response to stress.
8. Electroencephelogram measures of prefrontal brain activity.

Rather than summarise the psychological literature's assessment of well-being data, this chapter refers readers to the checks on self-reported happiness statistics that are discussed in Argyle (1989) and Myers (1993), and to psychologists' articles on reliability and validity, such as Fordyce (1985), Larsen, Diener, and Emmons (1984), Pavot and Diener (1993), and Watson and Clark (1991).

We assume a reported well-being function:

$$r = h(u(y, z, t)) + e \qquad (1)$$

where r is some measure of psychological stress or self-reported number or well-being level (perhaps the integer 4 on a satisfaction scale, or "very happy" on an ordinal happiness scale), u(...) is to be thought of as the person's true well-being or utility, h(.) is a continuous non-differentiable function relating actual to reported well-being, y is real income, z is a set of demographic and personal characteristics, t is the time period, and e is an error term. It is assumed, as seems plausible, that u(...) is a function that is observable only to the individual. Its structure cannot be conveyed unambiguously to the interviewer or any other individual. The error term, e, then subsumes among other factors the inability of human beings to communicate accurately their happiness level (your 'two' may be my 'three').[5] The measurement error in reported well-being data would be less easily handled if well-being were to be used as an independent variable. This approach might be viewed as

et al (2001), Frank (1985, 1997), Frey and Stutzer (1998, 1999) and Ng (1996, 1997).

[5] This recognises the social scientist's instinctive distrust of a single person's subjective 'utility'.

an empirical cousin of the experienced-utility idea advocated by Kahneman et al (1997).

It is possible to view some of the self-reported well-being questions in the psychology literature as assessments of a person's lifetime or expected stock value of future utilities. Equation 1 would then be rewritten as an integral over the u(...) terms. This chapter, however, will use stress and happiness questions on the assumption they describe a flow rather than a stock.

Easterlin's seminal research (1974, and more recently 1995) examined the reported level of happiness in the United States. The author viewed people as getting utility from a comparison of themselves against others; this is the idea that happiness has a large relative component. Hirsch (1976), Scitovsky (1976), Layard (1980), Frank (1985, 1999) and Schor (1998) have argued a similar thesis; a different tradition, with equivalent implications, begins with Cooper and Garcia-Penalosa (1999) and Keely (1999).

5.3 Data

The data used in this study come from the British Household Panel Survey (BHPS). The BHPS is a nationally representative sample of more than 5,000 British households, containing over 10,000 adult individuals, conducted between September and Christmas of each year from 1991 to 1998. Respondents are interviewed in successive waves; if an individual splits off from their original household, all adult members of their new household are also interviewed. Children

are interviewed once they reach 16. The sample has remained representative of the British population throughout the 1990s.[6]

The BHPS contains a standard mental well-being measure, a General Health Questionnaire (GHQ) score. This is a variable used by medical researchers and psychiatrists as a measure of stress or psychological distress. It is unfamiliar to some economists, but GHQ is probably the most widely used, questionnaire-based, method of measuring mental stress. In the spirit favoured by psychologists, it amalgamates answers to the following list of twelve questions, each one of which is, itself, scored on a four-point scale from 0 to 3:

Have you recently:

1. Been able to concentrate on whatever you are doing?
2. Lost much sleep over worry?
3. Felt that you are playing a useful part in things?
4. Felt capable of making decisions about things?
5. Felt constantly under strain?
6. Felt you could not overcome your difficulties?
7. Been able to enjoy your normal day-to-day activities?
8. Been able to face up to your problems?
9. Been feeling unhappy and depressed?
10. Been losing confidence in yourself?
11. Been thinking of yourself as a worthless person?
12. Been feeling reasonably happy all things considered?

[6] See Nathan (1999).

We use the responses to these so-called GHQ-12 questions. For the *first measure of mental well-being*, we take the simple sum of the responses to the twelve questions, coded so that the response with the lowest well-being value scores 3 and that with the highest well-being value scores 0. This approach is sometimes called a Likert scale and is scored out of 36.[7] The GHQ measure of stress, or lack of well-being, thus runs from a worst possible outcome of 36 (all twelve responses indicating very poor psychological health) to a minimum of 0 (no responses indicating poor psychological health). In general, medical opinion is that healthy individuals will score typically around 10-13 on the test. Numbers near 36 are rare and usually indicate depression in a formal clinical sense.

A second measure is used in the chapter. We also study a direct happiness question. This is question 12 above, denoted GHQH; so our happiness measure is in fact one twelfth of the GHQ measure. We assume that this is a sufficiently small proportion to be ignored without re-calibrating GHQ on only eleven questions.

We therefore employ a measure of (un)happiness as well as the mental stress measure described earlier. The GHQH question is: have you been feeling reasonably happy all things considered? This is the *second measure of mental well-being*. It is coded so that high numbers denote more unhappiness.

A key requirement for a test is that something approximating a random drop of money occurs. In a giant laboratory setting, this could be created experimentally. Aside from any ethical considerations, such an experiment at the start of the 21st century is probably infeasibly expensive to run. An equivalent is needed.

This chapter relies on a natural experiment created by windfalls. The data contains two sources of these – lottery wins and inheritances. These figures refer to

[7] Likert is 12 times a number from zero to three. An alternative is the Caseness score, which counts the number of times that an individual answers in one of two negative response categories.

windfalls 'within the last year', as assessed by the respondents. Lottery wins throughout the chapter include other gambling wins, such as on the soccer 'pools'. A huge percentage of the British population play the national lottery, and small wins are common. Hence for simplicity, because they dominate the data, we talk primarily of the lottery. The inheritance variable includes both bequests and inherited property (it excludes receipts of gifts or other private income transfers).

Despite the potential usefulness of lottery data to economists and psychologists, the literature exploiting lottery information is still a comparatively small one. Most work has looked at how consumption and work choices are affected by winning (for example, Bodkin 1959, Holtz-Eakin, Joulfaian and Rosen 1993, Imbens, Rubin and Sacerdote 2000, Kaplan 1985, Kreinin 1961, Landsberger 1963, and Sacerdote 1996). One well-known study in the psychology literature is Brickman, Coates and Janoff-Bulman (1978). This uses only a tiny cross-section sample of lottery winners, and concludes that winners are slightly happier than those who do not win, but that the difference is not statistically significant. Smith and Razzell (1975) examined a cross-section of those who won on football betting (the 'pools'), and found that there was some evidence of higher recorded happiness; but individuals also reported lower well-being in other spheres of life.

There is an important disadvantage to our data set. Although the British panel itself goes back to the start of the 1990s, questions on windfalls are relatively new. Information on the size of windfalls is known only for the 1997 and 1998 survey years. Analysis is therefore restricted to that sample period.[8] These data are augmented with people's GHQ scores from prior waves, so as to allow the examination of how windfalls affect both the level of well-being and how it changes

[8] There is one other piece of information. In 1995, people were asked whether they had received a windfall. This is used as a control variable in some of the regression equations.

over time. In other words, we are able to examine long lags on the dependent well-being variables, but have only two years with which to examine the effect of windfall gains.

5.4 Results

Table 5.1 presents the simplest results. In these bivariate regressions, money does buy greater happiness and lower measured stress.

Rises in well-being, to be clear about the choice of units and definitions, are given by declines in GHQ mental stress and in GHQH unhappiness. This follows the standard usage in the psychology and medical literature. Hence if money buys happiness, that shows up in the chapter's tables as negatives on windfall coefficients.

In general, windfalls are associated with a statistically well-determined improvement in well-being. Mental stress (GHQ) and unhappiness (GHQH) both decline in the year after a windfall. This effect is found in the cross-sectional levels and in the longitudinal changes.

In the cross-section equations, a windfall dummy (that is, whether the individual had either an inheritance or lottery win) enters negatively in both a mental stress equation and an unhappiness equation. In the first columns of Tables 5.1a and 5.1b, the t-statistics are, respectively, 2.83 and 1.24. Entering the amount of windfall gives, predictably, results that are better determined. This is column 2 of Tables 5.1a and 5.1b. When only windfall recipients are studied, in column 3, the size of the windfall enters with the expected negative sign and it is possible in both Table 5.1a and Table 5.1b to reject the null of zero at normal confidence levels.

The longitudinal effect of a windfall is picked up in the first-difference equations in the last three columns of Tables 5.1a and 5.1b. Here the two dependent variables are the change in mental stress (GHQ) and the change in reported unhappiness (GHQH). Person fixed-effects, therefore, have been removed. In five of the six equations, it is possible to reject the null of zero on the windfall variables. In the sixth case, in column four of Table 5.1a, the t-statistic is 1.64.[9]

How large are these improvements in well-being? The cross-section estimates predict that subsequent to a windfall of 50,000 pounds sterling the level of GHQ improves by -0.709. This is approximately 0.13 of a standard deviation in GHQ (5.44). For the sample of windfall recipients, the gain in GHQ is -1.11 or around 0.21 of the relevant standard deviation (5.28).[10] For GHQH, the predicted gain in well-being is -0.042 amongst all respondents, and is -0.114 amongst the sub-sample of windfall recipients. These are relative to a standard deviation of 0.59.

When the change in well-being is instead examined (in columns four to six of Tables 5.1a and 5.1b), a 50,000 pounds windfall is predicted to improve GHQ by -0.446, or in other words 0.08 of a standard deviation. For the sample of recipients, the relevant figure is -1.09, or 0.21 of the relevant standard deviation. When we examine the change in GHQH unhappiness, we predict a welfare gain of approximately 0.1 of a standard deviation for the sample of all respondents, and 0.2 of a standard deviation within the sample of windfall recipients.[11]

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[9] If equations are, instead, estimated by Ordered Probit or similar methods almost identical results are produced.

[10] The change in well-being is calculated for windfalls of 50,000 pounds relative to the minimum windfall in the sample. For the sample of all individuals this is 0.1 (a small constant replaces zero wins). For the sample of windfall recipients 1 pound. The predicted change in well-being is then calculated and compared to the standard deviation in the dependent variable. Where the change in well-being is examined we use the standard deviation in the differenced variable.

[11] As an illustrative way to think about the size of this effect, if the estimated number is 0.2 then a windfall of 1 million pounds would move a person by 4 standard deviations – or in other words from approximately close to the bottom of a well-being distribution to close to the top.

There are two sources of windfalls in our data – lottery wins and inheritances. For the rest of the chapter, we examine their impact upon well-being separately, and add explanatory variables. Although this reduces the size of the regression samples and tends to weaken the standard errors, it has the advantage of providing transparency. Having data on inheritances provides a useful check on the results for lottery wins, because people choose to play the lottery, whereas they presumably have less control over their probability of receiving bequests.

The aim of the remainder of the chapter is deliberately not to present equations with, necessarily, the highest t-statistics. Rather, it is to provide a feel, by studying lottery wins and inheritances separately, even when standard errors become poorly determined, for the ubiquity of the expected negative sign on windfalls. Later tables find that in all but 2 of 70 occasions – across a variety of settings – the windfall coefficient has the expected sign.

It is natural to begin in a simple way by examining whether, in a cross-section, those who obtain such windfalls are happiest. Table 5.2a provides evidence consistent with this hypothesis. In the second column of Table 5.2a, the mean GHQ stress score among those who are not lottery winners is 11.22. Among winners it is 10.91.[12] The same pattern is observed for the GHQH unhappiness score in the third column of Table 5.2a, though the raw effect is much less pronounced. The mean score for winners is 2.00 whilst amongst non-winners it is 2.01.

These cross-tabulations are consistent with the idea that money and well-being are positively correlated. Yet, these findings are raw cross-section results without controls. Further evidence, in the same spirit, would be provided if

[12] We are unable to distinguish between those who do not gamble and those who do gamble but do not win.

individuals longitudinally report themselves with higher levels of well-being subsequent to a lottery windfall. This issue is investigated in the second panel of Table 5.2a (so-called Sample 2), and summary statistics reported for those individuals where we observe the change in GHQ score. For this sample, the mean lottery win, conditional on being a winner, is observed to be considerably lower than that observed in the cross-section, respectively 118.5 and 200.0 pounds. Investigation revealed this difference to be chiefly attributable to the dropping of a small number of large lottery wins from the sample. Whether this selectivity reflects coincidence, or a more systematic bias, is not here possible to ascertain. The direction of bias is not clear *a priori* and will depend upon whether there are diminishing returns to well-being at very large windfalls.

Despite these concerns, the mean GHQ and GHQH scores for both winners and non-winners are remarkably similar to those observed previously. In the lower half of Table 5.2a, column 2, the mean GHQ stress score among lottery winners is 10.93, compared to 10.91 for the full sample (called Sample 1). Among non-winners it is 11.25, as opposed to 11.22. Both samples appear to capture similar patterns in well-being.

When the data are differenced, and changes over time in a person's well-being studied, we observe lottery winners to show, on average, increased levels of well-being (more precisely a reduced lack of well-being). In the second half of Table 5.2a, individuals who record a lottery win experience an average decrease in GHQ mental stress of -0.096 points (see column four of Table 5.2a, Sample 2). Amongst non-winners, GHQ worsens on average by approximately 0.020. For the GHQH unhappiness question the respective figures for winners and non-winners are -0.010 and 0.006. The observed rise in well-being subsequent to a lottery windfall appears

pronounced when contrasted with the fall in well-being for non-winners in the period.

Inheritances also work in the way that would be predicted. In Table 5.2b, the GHQ mental stress scores of inheritors are on average better than the scores of those who do not inherit any cash; they are 10.93 as opposed to 11.15 (see column two of Table 5.2b, Sample 1). For the GHQH unhappiness question, the mean response for inheritors is 1.95 (in Table 5.2b), whilst for those who do not receive a bequest 2.01. Panel two of Table 5.2b, which uses the so-called Sample 2, restricts attention to those individuals where we can observe the change in well-being over time. Both for those who inherit and those who do not, this (smaller) sample appears to be representative of that observed for the pooled cross-section. Furthermore, this selection does little to alter the tenor of the results.

The most noticeable finding in Table 5.2b, Sample 2, is that there is a marked drop in mental stress and unhappiness among those people who inherit. Amongst inheritors, there is an average GHQ mental stress decline of -0.429 compared to a mean rise of 0.0002 amongst non-inheritors. For GHQH unhappiness, the relevant figures are -0.097 and 0.006 respectively. As with winning the lottery, inheritances are associated with greater psychological well-being.

These numbers are averages across rather heterogeneous outcomes. It is likely that more information, in the statistical sense, is conveyed by the *size* of the inheritance or lottery win. Tables 5.3 and 5.4 explore such data.

Table 5.3 reveals, in its second column, a strong pattern in which the worst mental well-being scores (mean of GHQ is 11.22) are found among those who did not receive a lottery win. This accords with intuition. Largish wins are nicer than tiny wins. For those individuals who received small winnings, of less than 100 pounds, there is slightly higher well-being (mean 11.05). For those individuals who

win between 100 and 1000 pounds, GHQ scores are observed to be noticeably better (mean 10.18). Although the sample size here is not a large one, the stress levels of big lottery winners, 1000 pounds or more, seem paradoxically in Sample 1 of Table 5.5 to rise slightly (mean 10.28). For GHQH, we observe in the second column of Table 5.3 a similar relationship, and in this case the effect of winnings upon unhappiness is monotonically negative.

Consider the sample where we observe the change in well-being, namely, panel two of Table 5.3. The issue of selectivity can here be seen more clearly: mean lottery wins for those individuals who receive more than 1000 pounds is 2868.9 in Sample 2 as opposed to 6766.6 for the full sample. Whilst we do not know the largest lottery winners, the same distribution of GHQ and GHQH scores is observed. Examining changes in scores, Table 5.3 reveals in Sample 2 that GHQ stress levels improve with the size of lottery windfall. On average, GHQ worsens over the year 1998-97 by 0.020 for non-winners, but improves by -0.081 for small winners, -0.109 for medium winners, and -0.655 for the largest winners. For the change in GHQH unhappiness levels, the most marked effect is of an improvement in happiness of large winners (mean -0.109).

The same issue can be pursued for individuals who receive an inheritance. Table 5.4 reports the data. A consistent and intriguing cross-section pattern is revealed in both GHQ and GHQH scores: a smallish inheritance of less than 2500 pounds is associated with the highest level of well-being. An inheritance of between 2500 and 10,000 pounds on average improves welfare relative to not receiving an inheritance but is associated with lower well-being than the smallest level of inheritance. Individuals who receive the largest inheritances, over 10,000 pounds, are however those with significantly *worse* cross-section levels of well-being, both for stress (GHQ) and unhappiness (GHQH). This is true in the full sample and in the

sample where we observe the change in well-being. Yet, when we instead examine the change in well-being in response to a bequest, both GHQ stress levels and GHQH unhappiness levels are observed to have improved for all categories, relative to a decline observed for non-inheritors. In the longitudinal changes, then, observed behaviour matches intuition. The largest windfalls produce the greatest gains in GHQH well-being (column 5 of Table 5.4, Sample 2).

The summary statistics thus support the hypothesis that money is welfare improving. Windfalls of cash are associated with higher levels of well-being. This is, in the main, observed independent of how the data are cut, for both GHQ mental stress and GHQH unhappiness scores, both when examining the level of well-being and its change over time.

This evidence is fairly compelling. The recipients of windfalls have, on average, higher levels of well-being. For such summary statistics to provide conclusive evidence, however, would require the receipt and size of windfall to be randomly distributed across individuals. Whilst windfalls may be unanticipated, this is unlikely always to be true. The decision to gamble and the intensity of play are likely to be correlated with observed and unobserved characteristics. Indeed early tables demonstrate a positive correlation between lottery winnings and income. Moreover, if happier people are more (or less) likely to play, and thus win, the correlation between winnings and well-being could be due to some subtle self-selection of players rather than any welfare-enhancing effects. Similarly, inheritances may be positively associated with parental wealth, which is likely to be correlated with recipient income (as seen in Tables 5.2 and 5.4).

To investigate these issues in more detail we turn to regression analysis, and throughout the remainder of the chapter we examine the robustness of the negative sign on windfall gains.

5.4.1 *Estimation strategy*

The regression equation estimated is an empirical version of equation 1. Well-being is assumed a function of the monetary windfall, personal characteristics (age, education, gender, race, and region) and the time period. On occasion it is also examined whether results are robust to the inclusion of income as an explanatory variable. Well-being for individual i in time period t is then expressed as:

$$r_{it} = w_{it}'\beta + y_{it}'\delta + z_{it}'\gamma + \varepsilon_{it} \qquad i = 1, \ldots, n \qquad (2)$$

$$t = 1, \ldots, T$$

where r is the dependent variable that captures individual well-being, w is the amount of windfall (lottery win or inheritance), y is family income, z is a vector of individual characteristics and time dummies, ε is the conformable error term with mean zero and constant variance, and β, δ and γ the parameters to be estimated. The well-being function is approximated as linear and equations for the two measures of well-being, the overall GHQ score (on a 0 to 36 scale) and the GHQH unhappiness question (on a 0 to 3 scale), estimated by OLS.[13] Alternative specifications include a lagged dependent variable or instead adopt the change in well-being as the dependent variable.

5.4.2 Lottery Wins

A simple regression-equation test of whether winning money improves well-being is contained in Table 5.5. Here, and in all subsequent tables, panel A contains analysis of the GHQ mental stress score, panel B the GHQH unhappiness score. For comparison, column one of Table 5.5 reports the estimated effect of family income upon well-being. As expected, richer people are happier. GHQ is estimated to

improve by -0.117, and GHQH by -0.005, for an increase in income of ten thousand pounds sterling. The controls here, and throughout, are a quadratic in age, and dummies for gender, ethnic minority status, educational qualifications, region, and year. This cross-section result is, however, likely to confound various influences and cannot be presumed to capture causation.

Columns two and three of Table 5.5 do a regression test of the hypothesis that lottery winners are happier. Similarly to the sample statistics observed previously, well-being is observed to be higher for those who receive winnings and it is increasing in the amount of windfall. The monotonicity in column 3 is encouraging. Coefficient estimates are negative but not usually independently well determined. For people who win a small amount, such as less than 100 pounds, there is only a negligible difference in well-being relative to non-winners. This suggests that the pleasure associated with being a winner *per se* is largely trivial, at least for the measures of well-being studied here, and should not greatly influence results.

Column four of Table 5.5 instead enters the amount of winnings as the explanatory variable. This gives a strong result. Both for GHQ mental stress and GHQH unhappiness, the amount of winnings enters negatively – thus improving well-being – and is statistically significant. A windfall of 10,000 pounds improves GHQ mental well-being by -0.686 with a t-statistic above 6 and the GHQH unhappiness score by -0.032 with a t-statistic of 2.01. These effects are of a magnitude approximately 6 times as large as those estimated for income; it is not easy to know why.

The impact of a 50,000 pounds lottery windfall is estimated from Table 5.5 to improve GHQ mental stress by -3.43 points or over 0.6 of a standard deviation.

[13] This implicitly assumes responses are cardinal.

The improvement in GHQH is slightly less marked at -0.16, but still constitutes approximately 0.3 of a standard deviation. Nevertheless, if 'gamblers' in general have high (low) levels of well-being, independent of any monetary gain, we shall overestimate (underestimate) the effects of a windfall upon welfare. If gambling behaviour is characterised by such selection, coefficient estimates may be different when we restrict attention to a sample of winners only. All individuals are then gamblers and, moreover, they are also likely to be the more intensive players. In this case, selection bias should be reduced. Column five of Table 5.5 checks this and reveals that both for GHQ and GHQH the estimated effects of winnings are similar, for the sample of all individuals and the sample of winners. This is reassuring and suggests that the impact of winnings upon well-being is broadly independent to the selection of gamblers.[14]

Walker (1998) and Farrell and Walker (1999) provide evidence that lottery expenditure is a form of inferior good, that is, increasing in income but at a declining rate. Our amount-won variable may then capture the effect of income and be prone to similar problems of status and selection. Table 5.6 examines whether the effect of a lottery windfall is robust to the inclusion of a control for income. Column one restates the basic result. Column two, of Table 5.6, adds family income as an explanatory variable. For both the GHQ mental stress and the GHQH unhappiness measures of well-being, the estimated coefficient upon the amount won is essentially unaltered – indicating that the psychological benefits of winnings occur largely independently of income.

[14] Ideally one would wish to instrument winnings by a variable correlated with play but uncorrelated with well-being. As we analyse the effect of the *amount* won, this requires an instrument that identifies gambling propensity, conditional on income. No such variable was here available. A large degree of random variation is, however, introduced subsequent to participation.

There is potential for non-linearity. This is checked in column three of Table 5.6, where quadratics in the amount of winnings and income are examined. The first and second-order terms for the amount won enter with the expected signs, consistent with diminishing returns, but are not statistically significantly different from zero – neither for stress (GHQ) or reported feelings of unhappiness (GHQH).

An alternative approach to the self-selection of gamblers is followed in Table 5.7. This assumes the effects of selection are stable over time, and examines the change in well-being associated with a windfall.

As seen previously, the sample of individuals where such data are observed omits some of the largest windfalls. This has the effect of increasing the magnitude of the estimated effect of winnings upon mental stress (GHQ) and reducing the estimated effect upon unhappiness (GHQH) and in both cases reduces the precision of estimates. Nevertheless, when a lagged dependent variable is included in column three of Table 5.7, the effect is to increase the estimated gain in well-being subsequent to a lottery win. In contrast, the coefficient upon income is driven towards zero and is no longer well determined.

In column three of Table 5.7 a control for previous gambling behaviour is added – whether the individual received a lottery windfall in 1995.[15] Again the estimated coefficient upon amount won is more pronounced whilst the income parameter is unaffected. Moreover, previous gambling exerts a positive, though not statistically well-determined, effect upon both GHQ mental stress and GHQH unhappiness scores.[16] This evidence suggests that any differences in well-being levels between gamblers and non-gamblers do not crucially shape results.

[15] The amount won is not known.

[16] This result holds if lottery winnings in the *current* year are omitted.

A similar conclusion is reached when we examine the change in well-being scores over time in columns four and five of Table 5.7. For GHQ mental stress scores, a windfall of 10,000 pounds is predicted to improve well-being, relative to the previous year, by -1.976. This effect is statistically significant only at the 10 percent level. By comparison, in column one of Table 5.7, where the dependent variable is the level of GHQ the predicted improvement in well-being is -0.826. Similar results are observed for GHQH unhappiness, although coefficient estimates are again not well-determined. Interestingly, high-income individuals are observed in Table 5.7 column five to have experienced, on average, a decline in well-being levels over this period, both for GHQ and GHQH.

Hence, whilst, due to the characteristics of the sample, care must be taken in interpretation, results seem robust to the inclusion of a lagged dependent variable, to controlling for previous gambling success, and to examining the change in well-being over time. If anything, such checks magnify the improvement in well-being from a lottery windfall.

5.4.3 Inheritances

A potential difficulty with the examination of lottery wins is that the act of gambling, and winning, may bring pleasure independent of monetary gain. Table 5.8 therefore explores the impact upon well-being of receiving an inheritance. This event is likely to occur with the death of a close friend or relative and hence, in contrast, often be associated with reductions in well-being.

Column one of Table 5.8 estimates the effect of income upon well-being for this sample. Results are close to those in column one of Table 5.5. Table 5.8's column two examines a simple test of whether a windfall increases happiness. Receiving a bequest is found to improve well-being for both GHQ mental stress

and GHQH unhappiness scores. For GHQ the estimated coefficient is -0.235, for GHQH -0.061, though only the latter effect is statistically robust. Column three extends this analysis by instead entering dummies for the amount of inheritance. For both measures of well-being it is predominantly small inheritances, of less than 2500 pounds, that are observed to reduce mental stress and unhappiness. The effect upon GHQ is estimated at -0.488. For GHQH the parameter estimate is -0.102. Again only the latter effect is statistically significantly different from zero. Medium sized bequests are observed to improve well-being, whilst the largest inheritances are estimated to increase GHQ mental stress, though reduce GHQH unhappiness.

Column four of Table 5.8 examines the effect of the amount of inheritance, in tens of thousands of pounds, upon well-being. Both GHQ and GHQH scores are shown to be improving in the size of the bequest, despite the non-linearity observed above. A bequest of 10,000 pounds is predicted to improve the GHQ mental health score by -0.075 points and the GHQH unhappiness score by -0.014 points. When analysis is conditional upon only those individuals who do inherit, in column five of Table 5.8, both GHQ and GHQH coefficients are attenuated and are less precisely estimated but remain negative.

McGarry (1999) examines data on intended bequests and finds that the major determinant of the size of bequest is parental wealth. A significant role is, though, found also for the closeness of family relations.

With respect to the data studied here, recipients of the smallest category of inheritance (less than 2500 pounds) may include grandchildren rather than children and individuals with weaker parental links. They may then be more distant from the deceased benefactor and thus likely to suffer less distress. As the amount of inheritance increases, we potentially observe individuals with closer ties to the deceased. Also, larger inheritances may be in the form of property or other assets,

which themselves may, possibly, induce greater levels of stress in disposing of. The improvement in well-being observed for small inheritances may be being offset for larger bequests by the mental stress associated with bereavement.

Alternatively, those who inherit are themselves likely to be more affluent, due to linkages in family wealth, potentially with *higher* levels of well-being. Yet for such a mechanism to explain the behaviour observed here would require this effect to be felt for small inheritances but not for large. Such a relation seems doubtful, especially as the age, gender, race, education and region of the recipient are held constant.

Table 5.9 seeks to investigate these issues. It uses the sample of individuals where past (i.e. lagged levels of) GHQ and GHQH data are available. Column one replicates the result for column four of Table 5.8. Parameter estimates are found to be similar, though less well determined. In column two of Table 5.9, family income is added as an explanatory variable. If the observed effect of the size of inheritance upon well-being reflects the wealth of inheritors, then the addition of this variable should drive the estimated coefficient towards zero. In fact, the estimated relationship between well-being and bequests seems to be independent to the inclusion of an income control. Furthermore, when we add a lagged dependent variable or instead examine the change in well-being,[17] in columns three and four of Table 5.9 respectively, the estimated beneficial effect of a bequest increases. Thus results do not depend upon the wealth of inheritors.

Any gains in well-being associated with an inheritance are potentially contaminated by distress associated with the death of a close relative. The estimates so far may then form a lower bound upon the true effect. Assuming stress levels are

[17] This will capture the effect of selectivity if it remains stable over time.

liable to be high both pre- and post-bereavement, it seems natural to examine how well-being changes over time in response to an inheritance. Columns one and two of Table 5.10 indicate an improvement in both GHQ mental health scores and GHQH unhappiness scores that is increasing in the size of the bequest. This is true both within the sample of all individuals and the sample restricted to inheritors only. A 50,000 pounds inheritance is predicted to produce an improvement in GHQ mental stress of -0.99 and GHQH unhappiness of -0.15, both of which are approximately 0.2 of a standard deviation.

These results may reflect heightened distress pre-bereavement and a subsequent return to 'normal' well-being levels. If so, we spuriously overestimate the effect of a windfall. If the bequest is anticipated, consumption patterns may change in advance, improving welfare, and, in contrast, we may underestimate the true gain in well-being. Hence we next examine the change in well-being over longer time periods, namely, two-year and three-year gaps. Columns three to six of Table 5.10 show that the results are robust to such considerations; indeed the gains in well-being from an inheritance appear to be amplified. A bequest of 10,000 pounds improves the GHQ mental stress score by -0.520 and the GHQH unhappiness scores by -0.083, compared to the well-being levels that prevailed three years prior. The latter effect is found to be statistically significant at normal confidence levels.

5.5 Conclusions

Economists assume, without detailed evidence, that a person who becomes richer becomes happier. This chapter shows that what is arguably the central tenet of economics is supported by the data.

While it is known from recent cross-sectional work that reported happiness is positively correlated with income, that is not a persuasive reason to believe that more money leads to greater well-being. Cross-section patterns are at best suggestive because their causal implications are hard to interpret. Constructing a compelling test is difficult because of the stringent requirements of an ideal data set. Our approach seems to have three advantages. First, we follow a group of individuals longitudinally, and thus can measure the same person's well-being and income level at different points in time. Second, the data set provides information on financial windfalls (inheritances and lottery wins). These are probably as close as can be achieved to randomly occurring events in which some individuals have money showered upon them while others, in a control group, do not. Third, information is available on two ways to measure well-being: mental stress using a standard psychological health measure, and happiness using a simple four-point question.

We find that, as theory would predict, a windfall of money in year t is followed by lower mental stress and higher reported happiness.[18] At a conservative estimate, a windfall of 50,000 pounds improves mental well-being by between 0.1 and 0.3 standard deviations.

[18] Because we have data on both windfalls *and* well-being only for two years right at the end of our sample, it is not possible to assess whether people adapt psychologically to a windfall (perhaps returning eventually to some baseline happiness level). But the longitudinal data collection is continuing, so eventually it should be possible to address this question.

The Effects of Windfalls upon Two Measures of Well-being in a Panel

TABLE 5.1a
Mental Stress Equations
Dependent Variable: GHQ Stress Scores

Regressor	*All GHQ*	*All GHQ*	*Windfall GHQ*	*All ΔGHQ*	*All ΔGHQ*	*Windfall ΔGHQ*
Windfall dummy	-0.299			-0.156		
	(0.106)			(0.095)		
Ln(Windfall amount)		-0.054	-0.103		-0.034	-0.101
		(0.016)	(0.045)		(0.015)	(0.048)
Observations						
Individuals in Panel	9588	9588	2932	8620	8620	2722
Panel Total	17556	17556	3737	16075	16075	3478
Mean GHQ stress score	11.14	11.14	10.90	11.16	11.16	10.93
	(5.44)	(5.44)	(5.28)	(5.43)	(5.43)	(5.27)
Mean windfall amount	388.7	388.7	1825.9	376.0	376.0	1737.6
	(5655.3)	(5655.3)	(12151.5)	(5344.5)	(5344.5)	(12387.8)

TABLE 5.1b
Unhappiness Equations
Dependent Variable: GHQH Unhappiness Scores

Regressor	*All GHQH*	*All GHQH*	*Windfall GHQH*	*All ΔGHQH*	*All ΔGHQH*	*Windfall ΔGHQH*
Windfall dummy	-0.014			-0.026		
	(0.011)			(0.013)		
Ln(Windfall amount)		-0.003	-0.011		-0.005	-0.013
		(0.002)	(0.005)		(0.002)	(0.006)
Observations						
Individuals in Panel	9588	9588	2932	8696	8696	2737
Panel Total	17556	17556	3737	16201	16201	3499
Mean GHQH unhappiness	2.01	2.01	2.00	2.01	2.01	2.00
	(0.59)	(0.59)	(0.59)	(0.59)	(0.59)	(0.59)
Mean windfall amount	388.7	388.7	1825.9	376.0	376.0	1737.6
	(5655.3)	(5655.3)	(12151.5)	(5344.5)	(5344.5)	(12387.8)

1. Standard errors are in parentheses and are robust to arbitrary heteroscedasticity and the repeat sampling of individuals. All estimates are from least squares bivariate regressions. Data are for 1997 and 1998.
2. GHQ is a measure of mental stress on a 36-point scale. GHQH is a measure of unhappiness on a 4-point scale.
3. 'All' refers to the whole sample. The heading 'Windfall' refers to the sub-sample of those people who receive a non-zero windfall. Windfalls refer to cumulative gains, from lottery winnings plus inheritances, within the last year. They are deflated to 1997 values.
4. The log of windfall corrects the zero-windfall terms by adding a small constant (0.1).
5. The first three columns are cross-sections. The second three columns are differences.
6. Where sample means are reported, standard deviations are in parentheses.

Summary Statistics by Source of Windfall

TABLE 5.2a
Summary Statistics by Whether Had a Win on the Lottery
Sample 1: The Pooled Cross-section

Lottery Win	*Lottery Amount*	*GHQ*	*GHQH*	*ΔGHQ*	*ΔGHQH*	*Income*	*Frequency*
No		11.22	2.01			21532	14079
		(5.48)	(0.59)			(18730)	
Yes	200.0	10.91	2.00			23439	3334
	(2859.2)	(5.29)	(0.59)			(17234)	
Total	38.3	11.16	2.01			21897	17413
	(1253.4)	(5.45)	(0.59)			(18467)	

Sample 2: The sub-sample with Lagged GHQ Scores available

Lottery Win	*Lottery Amount*	*GHQ*	*GHQH*	*ΔGHQ*	*ΔGHQH*	*Income*	*Frequency*
No		11.25	2.02	0.020	0.006	21445	11558
		(5.46)	(0.58)	(5.41)	(0.71)	(18395)	
Yes	118.5	10.93	2.01	-0.096	-0.010	23483	2831
	(565.6)	(5.19)	(0.58)	(5.27)	(0.70)	(17378)	
Total	23.3	11.19	2.02	-0.003	0.003	21846	14389
	(255.2)	(5.41)	(0.58)	(5.38)	(0.71)	(18217)	

TABLE 5.2b
Summary Statistics by Whether had an Inheritance
Sample 1: The Pooled Cross-section

Bequest	*Bequest Amount*	*GHQ*	*GHQH*	*ΔGHQ*	*ΔGHQH*	*Income*	*Frequency*
No		11.15	2.01			21811	16953
		(5.45)	(0.59)			(18113)	
Yes	14547.6	10.93	1.95			24621	422
	(32582.4)	(5.16)	(0.61)			(27959)	
Total	353.3	11.15	2.01			21898	17375
	(5544.4)	(5.44)	(0.59)			(18418)	

Sample 2: The sub-sample with Lagged GHQ Scores available

Bequest	*Bequest Amount*	*GHQ*	*GHQH*	*ΔGHQ*	*ΔGHQH*	*Income*	*Frequency*
No		11.19	2.02	0.0002	0.006	21797	13306
		(5.40)	(0.58)	(5.36)	(0.70)	(17827)	
Yes	14137.0	11.10	1.96	-0.429	-0.097	24465	340
	(30362.3)	(5.34)	(0.62)	(5.59)	(0.78)	(28793)	
Total	352.2	11.18	2.02	-0.010	0.003	21864	13646
	(5268.9)	(5.39)	(0.58)	(5.37)	(0.70)	(18184)	

1. Sample 2 denotes those in the data set for whom we have some observations on well-being for earlier periods. Lottery winnings and inheritances refer to cumulative gains within the last year. The amount of lottery winnings, bequests and income variables are deflated to 1997 values.
2. Standard deviations are in parentheses.
3. ΔGHQ refers to the one period change in GHQ score ($GHQ_t - GHQ_{t-1}$). ΔGHQH is defined analogously.

Summary Statistics by Amount of Windfall

TABLE 5.3
Summary Statistics by Amount of Win on Lottery

Sample 1: The Pooled Cross-section

Lottery Win	*Lottery Amount*	*GHQ*	*GHQH*	*ΔGHQ*	*ΔGHQH*	*Income*	*Frequency*
No		11.22	2.01			21532	14079
		(5.48)	(0.59)			(18730)	
1-99	28.6	11.05	2.01			23098	2769
	(24.1)	(5.34)	(0.59)			(16684)	
100-999	256.6	10.18	1.98			23921	497
	(195.4)	(4.98)	(0.56)			(17278)	
1000 plus	6766.6	10.28	1.94			33776	68
	(19009.5)	(4.67)	(0.57)			(30829)	
Total	38.30	11.16	2.01			21897	17413
	(1253.4)	(5.45)	(0.59)			(18467)	

Sample 2: The sub-sample with Lagged GHQ Scores available

Lottery Win	*Lottery Amount*	*GHQ*	*GHQH*	*ΔGHQ*	*ΔGHQH*	*Income*	*Frequency*
No		11.25	2.02	0.020	0.006	21445	11558
		(5.46)	(0.58)	(5.41)	(0.71)	(18395)	
1-99	28.9	11.07	2.02	-0.081	-0.011	23073	2353
	(23.9)	(5.22)	(0.58)	(5.28)	(0.71)	(16667)	
100-999	259.7	10.24	1.98	-0.109	0.009	24156	423
	(200.2)	(5.02)	(0.57)	(5.30)	(0.66)	(17707)	
1000 plus	2868.9	10.31	1.98	-0.655	-0.109	35878	55
	(2865.8)	(4.53)	(0.59)	(4.25)	(0.66)	(33319)	
Total	23.3	11.19	2.02	-0.003	0.003	21846	14389
	(255.2)	(5.41)	(0.58)	(5.38)	(0.71)	(18217)	

1. Lottery winnings and inheritances refer to cumulative gains within the last year. The amount of lottery winnings, bequests and income variables are deflated to 1997 values.
2. Standard deviations are in parentheses.
3. ΔGHQ refers to the one period change in GHQ score ($GHQ_t - GHQ_{t-1}$). ΔGHQH is defined analogously.

TABLE 5.4
Summary Statistics by Amount of Inheritance

Sample 1: The Pooled Cross-section

Bequest	*Bequest Amount*	*GHQ*	*GHQH*	*ΔGHQ*	*ΔGHQH*	*Income*	*Frequency*
No		11.15	2.01			21811	16953
		(5.45)	(0.59)			(18113)	
1-2499	881.7	10.58	1.89			24269	189
	(629.3)	(5.04)	(0.56)			(19974)	
2500-	5351.7	10.95	1.97			23270	118
9999	(2150.1)	(5.19)	(0.61)			(14219)	
10,000 +	46442.8	11.50	2.02			26588	115
	(49917.4)	(5.34)	(0.68)			(44894)	
Total	353.3	11.15	2.01			21880	17375
	(5544.4)	(5.44)	(0.59)			(18418)	

Sample 2: The sub-sample with Lagged GHQ Scores available

Bequest	*Bequest Amount*	*GHQ*	*GHQH*	*ΔGHQ*	*ΔGHQH*	*Income*	*Frequency*
No		11.19	2.02	0.0002	0.0061	21797	13306
		(5.40)	(0.58)	(5.36)	(0.70)	(17827)	
1-2499	908.4	10.76	1.90	-0.2800	-0.0933	23489	150
	(641.5)	(5.20)	(0.58)	(5.42)	(0.74)	(19709)	
2500-	5477.4	11.04	1.99	-0.8242	-0.0769	24366	91
9999	(2177.6)	(5.25)	(0.61)	(5.43)	(0.67)	(14723)	
10,000 +	42140.3	11.68	2.04	-0.2929	-0.1212	26033	99
	(45324.0)	(5.62)	(0.68)	(6.00)	(0.91)	(45544)	
Total	352.2	11.18	2.02	-0.0105	0.0035	21864	13646
	(5268.9)	(5.39)	(0.58)	(5.37)	(0.70)	(18184)	

1. Lottery winnings and inheritances refer to cumulative gains within the last year. The amount of lottery winnings, bequests and income variables are deflated to 1997 values.
2. Standard deviations are in parentheses.
3. ΔGHQ refers to the one period change in GHQ score ($GHQ_t - GHQ_{t-1}$). ΔGHQH is defined analogously.

The Effect of a Lottery Win upon Well-being

TABLE 5.5a
Mental Stress Equations
Dependent Variable: GHQ Stress Scores

Regressor	*All 1997-98*	*All 1997-98*	*All 1997-98*	*All 1997-98*	*Win 1997-98*
Amount of Income	-0.117				
	(0.025)				
Lottery Win		-0.199			
		(0.110)			
Lottery Win: 1-99 pounds			-0.077		
			(0.118)		
Lottery Win: 100-999			-0.796		
			(0.237)		
Lottery Win: 1000 or more			-0.825		
			(0.603)		
Amount of Lottery Win				-0.686	-0.666
				(0.110)	(0.124)
Observations					
Individuals in Panel	9493	9493	9493	9493	2607
Panel Total	17413	17413	17413	17413	3334
Adjusted R^2	0.03	0.03	0.03	0.03	0.03

TABLE 5.5b
Unhappiness Equations
Dependent Variable: GHQH Unhappiness Scores

Regressor	*All 1997-98*	*All 1997-98*	*All 1997-98*	*All 1997-98*	*Win 1997-98*
Amount of Income	-0.005				
	(0.003)				
Lottery Win		-0.005			
		(0.012)			
Lottery Win: 1-99 pounds			0.000		
			(0.013)		
Lottery Win: 100-999			-0.026		
			(0.026)		
Lottery Win: 1000 or more			-0.078		
			(0.069)		
Amount of Lottery Win				-0.032	-0.031
				(0.016)	(0.018)
Observations					
Individuals in Panel	9493	9493	9493	9493	2607
Panel Total	17413	17413	17413	17413	3334
Adjusted R^2	0.01	0.01	0.01	0.01	0.02

1. Standard errors are in parentheses. See notes to Table 5.1.
2. The lottery win dummies are relative to the omitted category of zero winnings. The amount variables are measured in £10,000's (deflated to 1997 values).
3. All regressions are estimated by OLS and include controls for age, gender, race, highest educational qualification, region of residence, and year.

The Effect of a Lottery Win upon Well-being: Non-linear Income Effects

TABLE 5.6a
Mental Stress Equations
Dependent Variable: GHQ Stress Scores

Regressor	*All 1997-98*	*All 1997-98*	*All 1997-98*
Amount of Lottery Win	-0.686	-0.664	-1.510
	(0.110)	(0.104)	(1.305)
(Amount of Lottery Win)^2/100			0.917
			(1.371)
Amount of Income		-0.117	-0.172
		(0.025)	(0.035)
(Amount of Income)^2/100			0.041
			(0.014)
Observations			
Individuals in Panel	9493	9493	9493
Panel Total	17413	17413	17413
Adjusted R^2	0.03	0.03	0.03

TABLE 5.6b
Unhappiness Equations
Dependent Variable: GHQH Unhappiness Scores

Regressor	*All 1997-98*	*All 1997-98*	*All 1997-98*
Amount of Lottery Win	-0.032	-0.031	-0.142
	(0.016)	(0.016)	(0.148)
(Amount of Lottery Win)^2/100			0.121
			(0.155)
Amount of Income		-0.005	-0.006
		(0.003)	(0.004)
(Amount of Income)^2/100			0.001
			(0.001)
Observations			
Individuals in Panel	9493	9493	9493
Panel Total	17413	17413	17413
Adjusted R^2	0.01	0.01	0.01

1. Standard errors are in parentheses. See notes to Table 5.1.
2. The amount variables are measured in £10,000's (deflated to 1997 values).
3. All regressions are estimated by OLS and include controls for age, gender, race, highest educational qualification, region of residence, and year.

The Effects of a Lottery Win upon Well-being: Robustness Checks

TABLE 5.7a
Mental Stress Equations
Dependent Variable: GHQ Stress Scores

Regressor	*GHQ All 1997-98*	*GHQ All 1997-98*	*GHQ All 1997-98*	*ΔGHQ All 1997-98*	*ΔGHQ All 1997-98*
Amount of Lottery Win	-0.826	-1.391	-1.449	-1.976	-2.012
	(1.357)	(1.127)	(1.135)	(1.174)	(1.181)
Amount of Income	-0.123	-0.020	-0.020	0.088	0.088
	(0.029)	(0.022)	(0.022)	(0.024)	(0.024)
GHQ (t-1)		0.491	0.491		
		(0.011)	(0.011)		
Lottery Win in 1995			0.059		0.037
			(0.084)		(0.078)
Observations					
Individuals in Panel	7515	7515	7515	7515	7515
Panel Total	14389	14389	14389	14389	14389
Adjusted R^2	0.03	0.26	0.26	0.00	0.00

TABLE 5.7b
Unhappiness Equations
Dependent Variable: GHQH Unhappiness Scores

Regressor	*GHQH All 1997-98*	*GHQH All 1997-98*	*GHQH All 1997-98*	*ΔGHQH All 1997-98*	*ΔGHQH All 1997-98*
Amount of Lottery Win	-0.019	-0.059	-0.073	-0.181	-0.177
	(0.141)	(0.134)	(0.135)	(0.157)	(0.157)
Amount of Income	-0.003	-0.000	-0.001	0.006	0.006
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
GHQH (t-1)		0.247	0.247		
		(0.012)	(0.012)		
Lottery Win in 1995			0.014		-0.004
			(0.011)		(0.010)
Observations					
Individuals in Panel	7515	7515	7515	7515	7515
Panel Total	14389	14389	14389	14389	14389
Adjusted R^2	0.01	0.07	0.07	0.00	0.00

1. Standard errors are in parentheses. See notes to Table 5.1.
2. The amount variables are measured in £10,000's (deflated to 1997 values).
3. All regressions are estimated by OLS and include controls for age, gender, race, highest educational qualification, region of residence, and year.
4. ΔGHQ refers to the one period change in GHQ score ($GHQ_t - GHQ_{t-1}$). ΔGHQH is defined analogously.

The Effect of an Inheritance upon Well-being

TABLE 5.8a
Mental Stress Equations
Dependent Variable: GHQ Stress Scores

Regressor	*All 1997-98*	*All 1997-98*	*All 1997-98*	*All 1997-98*	*Inherit 1997-98*
Amount of Income	-0.135				
	(0.030)				
Inheritance dummy		-0.235			
		(0.265)			
Inheritance: 1-2499 pounds			-0.488		
			(0.387)		
Inheritance: 2500-9999			-0.232		
			(0.474)		
Inheritance: 10,000 or more			0.176		
			(0.519)		
Amount of Inheritance				-0.075	-0.057
				(0.059)	(0.073)
Observations					
Individuals in Panel	9488	9488	9488	9488	392
Panel Total	17375	17375	17375	17375	422
Adjusted R^2	0.03	0.03	0.03	0.03	0.04

TABLE 5.8b
Unhappiness Equations
Dependent Variable: GHQH Unhappiness Scores

Regressor	*All 1997-98*	*All 1997-98*	*All 1997-98*	*All 1997-98*	*Inherit 1997-98*
Amount of Income	-0.004				
	(0.003)				
Inheritance dummy		-0.061			
		(0.031)			
Inheritance: 1-2499			-0.102		
			(0.042)		
Inheritance: 2500-9999			-0.035		
			(0.056)		
Inheritance: 10,000 or more			-0.019		
			(0.065)		
Amount of Inheritance				-0.014	-0.008
				(0.008)	(0.008)
Observations					
Individuals in Panel	9488	9488	9488	9488	392
Panel Total	17375	17375	17375	17375	422
Adjusted R^2	0.01	0.01	0.01	0.01	0.01

1. Standard errors are in parentheses. See notes to Table 5.1.
2. The inheritance dummies are relative to the omitted category of not receiving a bequest. The amount variables are measured in £10,000's (deflated to 1997 values).
3. All regressions are estimated by OLS and include controls for age, gender, race, highest educational qualification, region of residence, and year.

The Effect of an Inheritance upon Well-being: Robustness Checks

TABLE 5.9a
Mental Stress Equations
Dependent Variable: GHQ Stress Scores

Regressor	*GHQ All 1997-98*	*GHQ All 1997-98*	*GHQ All 1997-98*	*ΔGHQ All 1997-98*	*ΔGHQ All 1997-98*
Amount of Inheritance	-0.075	-0.078	-0.136	-0.198	-0.196
	(0.079)	(0.079)	(0.118)	(0.178)	(0.178)
Amount of Income		-0.113	-0.014		0.089
		(0.029)	(0.022)		(0.024)
GHQ (t-1)			0.492		
			(0.011)		
Observations					
Individuals in Panel	7262	7262	7262	7262	7262
Panel Total	13646	13646	13646	13646	13646
Adjusted R^2	0.03	0.03	0.25	0.00	0.00

TABLE 5.9b
Unhappiness Equations
Dependent Variable: GHQH Unhappiness Scores

Regressor	*GHQH All 1997-98*	*GHQH All 1997-98*	*GHQH All 1997-98*	*ΔGHQH All 1997-98*	*ΔGHQH All 1997-98*
Amount of Inheritance	-0.009	-0.009	-0.014	-0.030	-0.030
	(0.009)	(0.009)	(0.012)	(0.023)	(0.023)
Amount of Income		-0.001	0.001		0.007
		(0.003)	(0.003)		(0.003)
GHQH (t-1)			0.248		
			(0.012)		
Observations					
Individuals in Panel	7262	7262	7262	7262	7262
Panel Total	13646	13646	13646	13646	13646
Adjusted R^2	0.01	0.01	0.07	0.00	0.00

1. Standard errors are in parentheses. See notes to Table 5.1.
2. The amount variables are measured in £10,000's (deflated to 1997 values).
3. All regressions are estimated by OLS and include controls for age, gender, race, highest educational qualification, region of residence, and year.
4. ΔGHQ refers to the one period change in GHQ score ($GHQ_t - GHQ_{t-1}$).

The Effect of an Inheritance upon Well-being: First Differences

TABLE 5.10a
Mental Stress Equations
Dependent Variable: GHQ Stress Scores

Regressor	*ΔGHQ All 1997-98*	*ΔGHQ Inherit 1997-98*	*Δ2GHQ All 1997-98*	*Δ2GHQ Inherit 1997-98*	*Δ3GHQ All 1997-98*	*Δ3GHQ Inherit 1997-98*
Amount of Inheritance	-0.196	-0.235	-0.363	-0.450	-0.520	-0.683
	(0.178)	(0.220)	(0.296)	(0.358)	(0.413)	(0.484)
Amount of Income	0.089	-0.031	0.087	-0.251	0.054	-0.605
	(0.024)	(0.085)	(0.038)	(0.161)	(0.064)	(0.284)
Observations						
Individuals in Panel	7262	316	7262	316	7262	316
Panel Total	13646	340	13646	340	13646	340
Adjusted R^2	0.00	0.00	0.00	0.01	0.00	0.03

TABLE 5.10b
Unhappiness Equations
Dependent Variable: GHQH Unhappiness Scores

Regressor	*ΔGHQH All 1997-98*	*ΔGHQH Inherit 1997-98*	*Δ2GHQH All 1997-98*	*Δ2GHQH Inherit 1997-98*	*Δ3GHQH All 1997-98*	*Δ3GHQH Inherit 1997-98*
Amount of Inheritance	-0.030	-0.031	-0.060	-0.072	-0.083	-0.108
	(0.023)	(0.029)	(0.033)	(0.041)	(0.038)	(0.049)
Amount of Income	0.007	-0.002	0.008	-0.017	0.007	-0.036
	(0.003)	(0.012)	(0.005)	(0.021)	(0.008)	(0.038)
Observations						
Individuals in Panel	7262	316	7262	316	7262	316
Panel Total	13646	340	13646	340	13646	340
Adjusted R^2	0.00	0.00	0.00	0.01	0.00	0.02

1. Standard errors are in parentheses. See notes to Table 5.1.
2. The amount variables are measured in £10,000's (deflated to 1997 values).
3. All regressions are estimated by OLS and include controls for age, gender, race, highest educational qualification, region of residence, and year.
4. ΔGHQ refers to the one period change in GHQ score ($GHQ_t - GHQ_{t-1}$). Δ2GHQ is the two period change in GHQ score ($GHQ_t - GHQ_{t-2}$). Δ3GHQ is the three period change in GHQ score ($GHQ_t - GHQ_{t-3}$). Similarly terms are defined for GHQH.

Chapter Six

The Decline of Well-being amongst Britain's Public Sector Workers

This chapter is based upon joint work with Professor Andrew Oswald.

Abstract

Over the period from 1991 to 1998, British public sector employees are found to show substantially reduced well-being levels, both in absolute terms and relative to private sector employees. At the start of the 1990s, the levels of psychological health, as measured by a General Health Question score, for public sector workers were similar to those within the private sector, but by 1998 were noticeably worse. Consistent with this, a relative decline in public sector job satisfaction is also noted. It is not possible to be completely certain as to why stress within the public sector has risen. The evidence, however, suggests it cannot be adequately explained by the changing composition of public sector employment, by the relative decline in public sector pay, or by aggregate movements in economic conditions.

6.1 Introduction

In the early 1990s, the British government entered into an experiment. It embarked on a process of reform, with the objective of improving the provision of public services. The public sector was subjected to greater, and more formal, scrutiny. League tables on the performance of health and education authorities were published. Schools were subject to external inspections and newspapers printed the results. Market forces were introduced into the state sector, as contracting-out and the encouragement of competitive tendering forced public sector suppliers to compete with private sector firms. Workers such as college employees were required to reapply for their own jobs. In 1993 tough budgetary limits were imposed upon the public sector and pay awards recommended by public sector review bodies were not fully implemented.[1] Such a policy was sustained for the remainder of the 1990s. This experiment is of interest to other countries that may choose to follow suit.

Despite anecdotal evidence of high levels of mental distress in particular cross-sections (Kapur, Borill and Stride, 1998), and concern about recruiting (Machin, 1999), comparatively little attention has been paid to the well-being of representative samples of public sector workers. This chapter examines the well-being of these employees, and contrasts outcomes with those in the private sector.

The central finding of the chapter is that, over the 1990s, there has been a sharp increase in observed stress levels and a marked decline in job satisfaction within the public sector, relative to private sector employees. This effect is found whichever way the data are cut and irrespective of the estimation method used. The

[1] For a discussion of the change in public sector pay, see Blackaby et al (1999), Disney and Gosling (1998) and Elliott and Duffus (1996).

well-being advantage observed for public sector workers at the beginning of the 1990s is found to be negligible, if positive at all, by the end of the sample period.

The plan of the chapter is as follows. Section two discusses the use and validity of subjective measures of well-being. Section three outlines the data analysed, whilst section four discusses results. Finally, section five concludes.

6.2 Subjective measures of worker well-being

Individuals' survey responses to questions about well-being are analysed in this chapter. Whilst such responses have been studied extensively by psychologists[2] the use of such data by economists is relatively modest,[3] but growing. Some economists may emphasise the likely unreliability of subjective data – perhaps because they are unaware of the large literature by research psychologists that uses such numbers. A recent literature on the border between economics and psychology has, however, attempted to understand the patterns in well-being data.

Self-reported well-being measures are thought to be a reflection of at least four factors: circumstances, aspirations, comparisons with others, and a person's baseline happiness or disposition (e.g. Warr, 1980, Chen and Spector, 1991). Konow and Earley (1999) document evidence that recorded well-being levels have been demonstrated to be correlated, in the expected direction, with objective characteristics, such as unemployment, and with the person's recall of positive versus negative life-events. Well-being is also positively correlated with assessments of the person's happiness by friends or family members and his or her spouse.

[2] Earlier work includes Andrews (1991), Argyle (1989), Diener (1984), Diener et al (1999), Douthitt et al (1992), Fox and Kahneman (1992), Larsen et al (1984), Mullis (1992), Veenhoven (1991, 1993), and Warr (1990).

[3] Recent papers include: Blanchflower and Freeman (1997), Blanchflower and Oswald (1999, 2000), Clark (1996), Clark and Oswald (1996), Di Tella and MacCulloch (1999), Di Tella et al (2001), Frank (1985, 1997), Frey and Stutzer (1998, 1999) and Ng (1996).

Moreover, physiological responses – heart rate, blood pressure measures and skin-resistance measures of response to stress – are correlated with well-being responses in the expected way. Such a pattern of results can probably not be reconciled with a purely idiosyncratic variable.[4]

A more rigorous argument in favour of the ability of the researcher to make use of well-being data is found in Kahneman et al (1997). The authors argue that functions that relate subjective feelings to physical variables are similar for different types of people. They suggest the well-being of any event has a basic scale: pleasant, neutral, and unpleasant. Other scales may expand the positive or negative categories to a finer degree but the neutral case is a constant. It is argued the distinctiveness of this neutral value provides a focal point that allows some confidence in matching subjective experiences across time for a given individual and to support interpersonal comparisons.

Here we assume a reported well-being function:

$$r = h(u(y, z, t)) + e \qquad (1)$$

where r is some self-reported number on an ordinal well-being scale, u(...) is thought to be an individual's true level of well-being, h(.) is a continuous non-differentiable function relating actual to reported well-being, y is real income, z is a set of demographic and personal characteristics, t is the time period, and e an error term. It is assumed, as seems plausible, that u(...) is a function observable only to the respondent. Its structure cannot be conveyed unambiguously to the interviewer or any other individual. The error term, e, then subsumes among other factors the inability of human beings to communicate accurately their well-being level.[5] This

[4] For alternative discussions of well-being data, and the issues of reliability and validity, see Argyle (1989) Fordyce (1985), Larsen et al (1984), Pavot and Diener (1993), and Watson and Clark (1991).

[5] This recognises the social scientist's instinctive distrust of a single person's subjective 'utility' and the likelihood that self-reported data, whilst informative, will be subject to error.

measurement error in reported well-being would, though, be less easily handled if well-being were to be used as an independent variable.

6.3 Data

The data used in this study come from the British Household Panel Survey (BHPS). The BHPS is a nationally representative sample of more than 5,000 British households, containing over 10,000 adult individuals, conducted late each year from 1991 to 1998. Respondents are interviewed annually. If an individual leaves their original household all adult members of their new household are also interviewed. Children are interviewed once aged 16. Together this should ensure the sample remains broadly representative of the British population.[6] These data include detailed information concerning earnings, education, employment characteristics and demographics, worker well-being and job satisfaction. Attention is here restricted to those individuals aged less than 65 and in employment at the survey date, approximately 5,000 respondents in any one year.

The BHPS contains a standard mental well-being measure, a General Health Questionnaire (GHQ) score. This is a variable used by medical researchers and psychiatrists as a measure of stress or psychological distress. It is likely to be unfamiliar to some economists, but the GHQ is probably the most widely used, questionnaire-based, method of measuring mental stress. It amalgamates answers to the following twelve questions:

[6] Nathan (1999) undertakes a systematic analysis of the effects of attrition, and compares the BHPS to Census data, the General Household Survey (GHS) and the Family Expenditure Survey (FES), with respect to age, sex, marital status, socio-economic group, ethnicity, employment status and household characteristics. The author concludes that cumulative attrition in the BHPS is limited and does not lead to serious bias in inference.

Have you recently:

1. Been able to concentrate on whatever you are doing?
2. Lost much sleep over worry?
3. Felt that you are playing a useful part in things?
4. Felt capable of making decisions about things?
5. Felt constantly under strain?
6. Felt you could not overcome your difficulties?
7. Been able to enjoy your normal day-to-day activities?
8. Been able to face up to your problems?
9. Been feeling unhappy and depressed?
10. Been losing confidence in yourself?
11. Been thinking of yourself as a worthless person?
12. Been feeling reasonably happy all things considered?

Each one of the responses to these questions is scored on a four-point scale, from 0 to 3, where the response with the lowest well-being level scores 3 and that with the highest scores 0. The responses to these twelve questions are then summed to form an overall measure of GHQ distress.[7] This approach is sometimes called a Likert scale and is scored out of 36. This measure of stress, or lack of well-being, thus runs from a worst possible outcome of 36 (all twelve responses indicating very poor psychological health) to a minimum of 0 (no responses indicating poor psychological health). In general, medical opinion is that healthy individuals will score typically around 10-13 on the test. Numbers near 36 are rare and usually indicate depression in a formal clinical sense.

[7] Responses are derived from a self-completion questionnaire. Some 95 percent of individuals answer at least one GHQ question and 94 percent all twelve. Amongst employed respondents the latter figure is approximately 96 percent.

A second measure of worker well-being is used in the chapter, employees' job satisfaction. Satisfaction has been found to influence subsequent labour market behaviour. It is a significant predictor of quits (Freeman, 1978) and is negatively related to absenteeism, non- and counter-productive work. Furthermore, it is related, in the expected direction, with other indicators of well-being: poor mental health, length of life and coronary heart disease (see Clark and Oswald, 1996).

Within the BHPS all working respondents are asked to rate their level of satisfaction with respect to seven aspects of their employment: promotion prospects, total pay, relations with supervisor, job security, ability to work on own initiative, the actual work itself, and, the hours of work. Each of these categories is assigned a rank between 1 and 7, 1 representing 'not satisfied at all', 7 indicating 'completely satisfied' and the numbers from 2 to 6 corresponding to intermediate levels of satisfaction (where 4 is 'neither satisfied or dissatisfied').[8] Finally, and subsequent to these seven questions, a question was asked:

"All things considered, how satisfied or dissatisfied are you with your present job overall using the same 1-7 scale?"

The method in which these questions were asked suggests individuals evaluated many attributes of their job package when responding. It seems probable this approach will elicit responses more closely approximating satisfaction at the workplace, than would a simple direct question. Responses to this last question then form the basis of analysis of job satisfaction within the BHPS. Unfortunately the method in which the questions were asked changed in 1998, with only a subset of the preliminary satisfaction questions retained. Analysis is here restricted to the

[8] In wave one the categories 1, 4 and 7 are given the descriptions outlined, whilst 2,3,5 and 6 are left unlabeled. From wave two onwards all values were given a label, with the descriptors 'mostly' and 'somewhat' added. The question itself was a constant. This issue is discussed later.

years 1991 to 1997, for which consistent satisfaction data are available. The GHQ data remain consistent in every wave and are analysed for the period 1991 to 1998.

These two measures, GHQ and job satisfaction, are viewed as describing the flow of worker well-being and are used to examine how well-being has changed over time in both the public and private sectors.

6.4 Results

Table 6.1 presents the simplest results and examines whether well-being levels, as measured by (Likert scaled) GHQ scores, have declined over time for public and private sector workers. Figure 6.1 plots the corresponding time trend.

To be clear, about the choice of units and definitions, rising well-being is here given by declining GHQ mental stress scores. This follows the standard usage in the psychology and medical literature. Hence if well-being is falling over time, GHQ will be rising.

In the summary statistics, public sector workers are observed to experience a pronounced increase in measured stress, relative to private sector employees. Between 1991 and 1998 the average GHQ score of public sector employees worsens by approximately 1.3 points, from 10.36 to 11.63. This is a large, and statistically significant, increase over a period of less than a decade. In comparison, the difference in mean GHQ stress scores between unemployed and employed individuals, in the BHPS over the period, is approximately 2 points, that between females and males 1.4 points.[9]

[9] Clark and Oswald (1994) instead study the alternative Caseness score version of the GHQ, this counts the number of times, out of twelve, that an individual answers in one of two negative response categories. Caseness scores are, on average, 1.5 points higher for the unemployed.

For private sector employees, average GHQ levels are initially, in 1991, similar to those observed in the public sector, at 10.14, and we cannot reject the null of equality of public and private sector scores at conventional significance levels.[10] By 1998 mean GHQ levels within the private sector have risen to 10.65. This marks a statistically significant deterioration in measured stress. However, from 1994 onwards we can reject the null of equality in mean GHQ scores between the sectors, for all reasonable p-values, in favour of higher public sector stress. For both public and private sector employees we thus observe a worsening of well-being in the 1990s.[11] This effect is particularly pronounced for those workers employed by the state.

These findings are, however, raw cross-section results without controls. Many factors shape, or are correlated with, GHQ. It could, in principle, be that the pattern in mean GHQ scores in Table 6.1 reflects changes in the composition of the public, and private, sectors. The growth in stress could, perhaps, capture the growth of part-time, female, and white-collar employment observed for the economy over the period. For the public sector this may be a particular concern, as privatisation, compulsory competitive tendering, and a shift towards more private sector style management may have amplified these general trends. To investigate issues more fully we turn to regression analysis.

6.4.1 Estimation strategy

The model estimated is an empirical version of equation 1. Well-being is assumed a function of personal characteristics (such as education, age, gender and race), employer characteristics (e.g. establishment size), variables associated with the

[10] Tests are here t-tests of the equality of means, allowing for potentially unequal variances.

labour contract (income, hours of work, occupation) and the time period. Well-being for individual i, in time period t, is then expressed as:

$$r_{it} = t_t'\lambda + y_{it}'\varphi + x_{it}'\beta + z_{it}'\gamma + \varepsilon_{it} \qquad i = 1, \ldots, n \qquad (2)$$

$$t = 1, \ldots, T$$

where, r is the dependent variable that captures individual well-being, t the time trend, y the vector of pay and hours variables, x the vector of worker characteristics, z the vector of employer characteristics[12], ε the conformable error term with mean zero and constant variance, and λ, φ, β and γ the vectors of parameters to be estimated.[13] The well-being function is approximated as linear and, where the GHQ score (on a 0 to 36 scale) is the dependent variable, equations estimated by OLS.[14]

Table 6.2a presents the regression equivalent to Table 6.1 and estimates the time trend in GHQ, after controlling for observed characteristics. Columns one and two estimate regressions separately for public and private sector employees, and report the coefficients upon year dummies (these represent the growth in GHQ relative to 1991). Column one indicates that GHQ mental stress in the public sector is higher by, on average, 1.009 points in 1998 relative to 1991. In column two, the estimated growth in mental stress in the private sector is 0.463 points. In both cases the null hypothesis of no change is rejected at normal confidence levels. This suggests public sector stress grew by 0.546 GHQ points relative to the private sector over the period.

Column three pools the data and includes an interaction term between public sector status and the year dummies. These interaction terms capture the

[11] Blanchflower and Oswald (2000) document evidence of falling well-being levels for the US and Britain, since the early 1970s.
[12] Industry dummies are not included due to the high degree of collinearity with public sector status.
[13] This approach implicitly assumes well-being responses are cardinal.
[14] Results are qualitatively unchanged if equations are estimated by the Ordered Probit technique.

growth in GHQ levels in the public sector, over and above that in the private sector.[15] Again we observe a statistically significant worsening of stress levels in the public sector. The growth in GHQ stress, between 1991 and 1998, is some 0.679 points greater than that observed for private sector employees. This effect is non-negligible. Examining coefficients from the GHQ regression, it is over twice as large as the stress differential estimated for non-white respondents' (0.299) and approximately half as large as the estimated gender differential (1.100 for women).[16]

Table 6.2b repeats the previous analysis, but instead includes a simple time trend.[17] In this case, GHQ is estimated to worsen by approximately 0.120 and 0.047 points per year, in the public and private sectors respectively. Over the eight-year period (1991 to 1998) this suggests public sector stress has worsened by 0.511 GHQ points, relative to the private sector. The interaction term in column three of Table 6.2b suggests public sector stress has grown, on average, by 0.093 points per year faster, over the period, than that in the private sector. This implies public sector stress has worsened, relatively, by 0.651 GHQ points between 1991 and 1998. All effects are statistically significantly different from zero.[18]

The results suggest a fall in the mental health of state sector workers of between 0.5 and 0.6 GHQ points. These estimates may, however, be an unreliable guide to the growth in GHQ, as Table 6.1 indicates there was a marked jump in reported GHQ between 1991 and 1992, within both sectors.[19] Indeed, examining column two of Table 6.2a, the difference in mental stress levels in the private sector between 1992 and 1998 is small and statistically insignificant. In comparison, both in columns one and three, for the public sector the null of equality of GHQ levels in

[15] The public sector dummy subsumes the effect of the public sector in 1991.
[16] Positive effects denote greater stress.
[17] Where the time trend equals 1 if the year is 1991, 2 if the year is 1992, etc.
[18] The use of more disaggregated occupation codes produced substantially similar results.
[19] This may, potentially, be due to the onset of recession.

1992 and 1998 can be rejected at normal confidence levels. Table 6.3 then examines the coefficients upon a time trend for the period 1992 to 1998. Whilst the estimates suggest a positive and statistically significant worsening of GHQ in the public sector, the private sector time trend is small and not statistically different from zero. The increase in stress levels in the private sector, over the period, is then largely a result of upward shift in GHQ between 1991 and 1992, with little increase thereafter. In contrast, public sector stress is observed to rise by 0.4 to 0.5 GHQ points between 1992 and 1998.[20]

6.4.2 The trend in public sector well-being and the business cycle

Pay is likely to be pro-cyclical in both the public and private sectors, however the greater volatility of pay in the latter generates an observed counter-cyclical public sector pay premium (see Disney and Gosling, 1998). Public sector employment may then be, relatively, more attractive in economic downturns. The relative fall in public sector well-being could then capture the improvement in economic conditions over the 1990s.

The BHPS covers the eight-year period 1991 to 1998 and so observes only a part of the business cycle. A complete test of whether public sector well-being is counter-cyclical is then not, here, possible. Instead, in column four of Table 6.2b, we examine whether conditions in the local economy can explain the increase in public sector stress. The county unemployment rate[21] is entered as a measure of local labour market conditions. This is interacted with the public sector indicator to allow for a differential impact of local unemployment upon GHQ stress for public sector employees.

[20] In the results that follow estimation is upon the 1991 to 1998 sample. The broad tenor of result is the same if we omit the 1991 survey data.

Where local unemployment is high, public sector employment is likely to be more attractive and stress lower. This would suggest a negative coefficient upon the public sector unemployment effect. The reverse is observed. The parameter is not, however, statistically different from zero. Moreover, the estimated time trend in public sector stress increases, and remains statistically robust. Local labour market conditions do not then explain the trend in public sector GHQ stress levels, nevertheless it remains possible national conditions are influencing results.

An alternative test is carried out in column four of Table 6.3. If the observed positive trend in public sector stress reflects a counter-cyclical movement in well-being, one may expect the rise in stress to occur predominantly towards the end of the 1990s, where economic conditions are improving. For the early part of the decade, where the economy is more depressed,[22] public sector well-being would be expected to be greater, and the trend in GHQ attenuated. We then analyse these data for the period 1991 to 1995. Public sector stress is here estimated to grow by 0.106 GHQ points per year faster than that amongst private sector employees, and remains statistically well determined. The deterioration in public sector well-being then seems to have begun to occur prior to the economic upturn of the late 1990s.

Whilst the movement in the aggregate economy is likely to play a role in the reduction in public sector well-being the evidence, here, is that it does fully explain the upward trend in stress levels.

6.4.3 The trend in public sector well-being by worker characteristics

The reforms to the public sector in the 1990s are unlikely to have affected employees uniformly. Tables 6.4a and 6.4b examine the trends in GHQ mental

[21] Source: Labour Market Trends (1999).

stress, in the public and private sectors, for different groups of individuals. Column one, of Table 6.4a, displays coefficient estimates for the pooled sample, where an interaction effect captures the trend in public sector GHQ. Disney, Gosling and Machin (1995) report evidence that in 1990 approximately 91 percent of public sector establishments recognise unions for manual workers and some 98 percent for non-manual employees. The respective figures in the private sector are 44 and 28 percent. The trend in public sector well-being may then, in part, pick up changes in well-being within unionised plants, not captured by a union dummy.[23] Column two restricts analysis to unionised workplaces, itself potentially an endogenous choice variable, and thus implicitly conditions upon union status. Results are essentially unchanged.

Columns three and four of Table 6.4a examine the trend in well-being for males and females respectively. Parameter estimates suggest mental stress among private sector females worsened, with a positive and statistically significant effect. For males the effect is smaller and not well determined. Within the public sector, both males and females are observed to have experienced a large increase in mental stress (fall in well-being) but again this is only statistically significantly different from zero for women. In both cases we cannot reject the equality of the time trend coefficients, between men and women, at normal confidence levels.

Column five of Table 6.4a estimates the time trend in well-being for the South of England (London, the South East and East Anglia), column six the North of England (North West and North East), Scotland and Wales. Within the South, public sector stress is estimated to increase, on average, by 0.064 GHQ points per year. This is slightly below that observed for the country as a whole, and is not

[22] The national (claimant) unemployment rate was 7.7 in 1991, 9.3 in 1992, 9.9 in 1993, 9.0 in 1994, 7.7 in 1995. 7.1 in 1996, 5.4 in 1997 and 4.6 in 1998 (Labour Market Trends, 1999).
[23] There may also be a problem of collinearity between public sector and union status.

statistically robust. Within the North of England, Scotland and Wales, the public sector time trend is estimated to be 0.102 GHQ points per year, and in this case is statistically well determined. The fall in well-being in the public sector is then, if anything, larger outside the South-East of England, in regions where the growth in the economy has been less pronounced.

Table 6.4b examines the time trend in mental stress by workers' highest academic qualification. Column one reports parameter estimates for individuals with no formal qualification, column two individuals with at least one O-level, column three workers with one A-level or more, and column four degree-qualified employees. In all four cases the growth in mental stress in the private sector is positive and similar, though not well determined. For all education groups the (relative) public sector time trend is observed to be positive, but only for those individuals with no formal qualification is it statistically robust. Here mental stress is predicted to grow, on average, by 0.171 GHQ points per year faster than that observed in the private sector. For individuals with degrees, where the public sector time trend is smallest, the estimated effect is 0.067 GHQ points per year. Despite the difference in coefficients, we cannot reject the null of equality of the public sector time trends.[24]

The evidence suggests the fall in well-being (rise in mental stress) within the public sector has been greatest for the less educated. This is potentially consistent with the greater impact that public sector reforms, such as compulsory competitive tendering and privatisation of services, have had upon relatively less-skilled employees. Indeed, Disney and Gosling (1998) find that the public-sector pay

[24] Results are similar when we examine the effects separately for males and females.

premium to have almost entirely been eroded for workers with no formal qualifications between the 1980s and 1990s.[25]

This decline in relative pay may help to explain the reduction in public sector well-being in the 1990s. Yet it does not appear to offer a complete explanation. Whilst a relative decline in public sector pay may be expected to reduce well-being it would seem plausible this would have greatest impact where pay levels lag those observed in the private sector. The decline in public sector well-being would then be predicted to be greatest amongst highly qualified employees, and in the South East.[26] This is not the case. The rise in mental distress in the public sector is greater for less qualified workers and larger outside the South. Furthermore, for women, for whom public sector pay premiums remain robustly positive, the rise in GHQ mental stress is observed to be greater. Finally, the results observed are robust to omitting controls for pay and/or the hours of work. Whilst declining (relative) public sector pay would seem to play a part in the observed fall in well-being it does not seem to tell the full story.

6.4.4 The changing composition of public sector employment

The decline in well-being may then be linked to the changing working conditions, of some, in the public sector. Estimates may, however, be biased due to the changing characteristics of public sector jobs. Reforms that have increased market pressure on low-skilled occupations may have caused a shift towards white-collar employment, as former public sector occupations have been reclassified as being within the private sector. This may explain the trend in GHQ stress if occupations transferred from the public to the private sector were associated with low levels of

[25] Elliot and Duffus (1996) observe a general decline in the public-sector wage premium, for all workers, once account is made for occupational structure dating from the early 1970s.

stress. The largest increases in mental stress in the public sector are, however, observed for the least educated individuals, whom are more likely to work within jobs transferred to the private sector. Alternatively, for occupations remaining within the public sector, changing recruitment patterns may produce composition change *within* jobs.

Table 6.5 investigates these issues by estimating the time trend in public and private sector well-being for those individuals who have remained in the same sector, over the samples eight-year period (1991 to 1998). The composition of public and private sector employees, within this sample, is then unchanged. Columns one and four report results for those individuals observed in every wave (the balanced sample), columns two and five those workers observed in the same sector in each period (the 'stayers'). Compared to the results in Table 6.2b, both the public and private sector time trends are estimated to be greater in the balanced sample (0.228 compared to 0.120 for the public sector, and 0.061 compared to 0.047 for the private sector). This suggests there may be issues of sample selection for those individuals observed in employment and who respond in every wave of the survey. This appears especially true for the public sector sample. Nevertheless, when we examine the sample of 'stayers' the estimated time trends are largely unchanged, at 0.206 for public sector employees and 0.061 for private sector workers. In all cases parameters are statistically robust.

An alternative test, of whether composition change can explain the trend in public sector well-being, is offered in column three of Table 6.5, where the trend in GHQ is estimated for a sample of public sector occupations that have experienced little or no reform (see Appendix). Within these occupations, with limited composition change, the estimated coefficient upon the time trend is again positive

[26] See Blackaby et al (1999) for a description of public sector pay premia.

and statistically significant, and estimated to be similar to that for all public sector workers at 0.102.

Table 6.6 examines longitudinal changes in GHQ well-being for workers followed over time. This will control for any person fixed-effects in GHQ well-being levels.[27] Column one of Table 6.6 examines the one-year change in GHQ scores, for the years 1992 to 1998 (one period is lost due to calculating the first difference). The public sector parameter here measures how much faster public sector GHQ has risen, on average, year-to-year relative to the private sector, between 1991 and 1998. The estimated coefficient, at 0.096, is similar to the public sector time trend observed in column three of Table 6.2b, though in this case not statistically well determined.

Column two of Table 6.6 analyses the change in well-being over the entire eight-year period, and examines how the same person's GHQ score changes between 1991 and 1998. The public sector parameter here captures the, average, growth in public sector stress over the eight-year period between 1991 and 1998, compared to workers in the private sector. The estimated public sector coefficient, at 0.689 GHQ points, is similar to previous estimates of the increase in public sector stress over the period, and is statistically different from zero. Columns three and four of Table 6.6 similarly analyse the change in well-being for the sample of 'stayers'. Results are, as in Table 6.5, even more pronounced and in both cases statistically robust.

A final issue with these estimates concerns endogenous job choice. Within a sector where the conditions of work are worsening it will be those who most dislike

[27] One may wish to identify the difference in well-being levels between the public and private sectors by examining the change in well-being for job switchers between the sectors. This is not here attempted due the relatively small number of individuals who move between the sectors. Moreover, observed mobility is likely to capture a large degree of endogenous job choices and classification error. Rather we focus upon the relative trend in GHQ over time.

a new regime who are prone to leave. Observing those who remain within the public sector then may underestimate the true decline in well-being, and the estimates above would be a lower bound on the true deterioration in public sector stress. This logic may, however, be mitigated by investment in sector-specific skills, which may make it unprofitable to switch jobs even as conditions deteriorate.

In summary, the estimates suggest between 1991 and 1998 the mental stress levels of British public sector employees worsened by 0.5 to 0.6 GHQ points, relative to the private sector. There is some evidence that this trend cannot be, adequately, explained by changes in the composition of public sector employment or relative-pay effects.

6.4.5 An alternative measure of Mental Distress

An alternative method of measuring GHQ mental distress, as opposed to the Likert score (on a 0 to 36 scale), is to form a dichotomous indicator of those likely to be at risk of psychiatric morbidity. This is commonly measured as being individuals who respond to four or more of the twelve GHQ questions negatively.[28]

Table 6.7 presents the sample proportions of individuals with high mental distress, within the public and private sectors, over time. The broad patterns in proportions are similar to that observed for the (Likert) overall GHQ score in Table 6.1. For both public and private sector employees there has been a statistically significant increase in the proportion of employees with high mental distress, between 1991 and 1998. Moreover by the end of the period the incidence of mental distress is statistically significantly greater in the state sector.

Table 6.8a examines whether public sector workers are more likely to report high mental distress scores, for each year in the 1990s. Estimation is by the Probit

technique and positive coefficients are associated with an increased likelihood of mental distress.[29] For the early 1990s the incidence of high mental distress, whilst generally more likely, in the public sector is not statistically significantly different from that in the private sector. However, after 1997 we do observe a positive and statistically robust public sector coefficient. By the end of the 1990s public sector employees are more likely to be characterised as being highly distressed.[30]

Estimates may, however, conceal changes in the composition of public sector jobs. Table 6.8b then examines the model for the sample of 'stayers'. A similar pattern in the estimated public sector coefficients, to that in Table 6.8a, is observed and if anything shows a more pronounced trend over time. The public sector effect is, however, only well determined in the last period, 1998.

Whilst a greater time span would be desired to analyse the permanence of these results, they usefully reinforce previous estimates and suggest the upward trend in mental stress in the public sector is non-trivial.

6.4.6 Job satisfaction

An alternative measure of worker well-being is now examined, and versions of equation (2) estimated where job satisfaction is the dependent variable. The job satisfaction data are observed as ordered categorical responses (on a 1 to 7 scale) and estimation is by the Ordered Probit technique of McKelvey and Zavoina (1975). Positive coefficients here denote higher levels of satisfaction are more likely.

Mean satisfaction scores are reported, for the public and private sectors, in Table 6.9 and Figure 6.2 plots the time trends. Consistent satisfaction data here only cover the period 1991 to 1997, and results are henceforth restricted to that period.

[28] See Bowling (1997) for a full discussion.
[29] Examining marginal effects suggests a similar qualitative interpretation.

Comparable to the rise in GHQ mental stress observed previously, we observe a sharp drop in mean job satisfaction levels in the early 1990s, but one which flattens out in 1994 and climbs slowly thereafter. Whereas public sector satisfaction was, on average, 0.18 points higher than that observed in the private sector in 1991, by 1997 the gap had all but disappeared, to 0.03. This fall in job satisfaction is of a similar magnitude to the difference in satisfaction levels observed, in the sample, between union and non-union workplaces (see Freeman, 1978, for the classic economics discussion of union voice and job satisfaction).

An additional issue with these satisfaction data is that in 1991 the categories 1, 4 and 7 were given descriptions whilst 2, 3, 5 and 6 were left unlabeled. This had the effect of providing focal points for responses at those categories with titles. As the job satisfaction data are positively skewed, with mean values of above 5 on a 1 to 7 scale, this may over-estimate job satisfaction in 1991. From 1992 onwards all categories were given a descriptive label. The question itself was a constant.

Whilst satisfaction in 1991 may be overstated, it is not clear why public sector workers should respond to this discrepancy in a systematically differently way to employees in the private sector. The question format in 1991 would then add noise, but not bias, into comparisons of public and private sector employees. Yet, the way the question, in 1991, provided focal points to responses, may lead to measurement error positively related to true satisfaction, overstating any satisfaction differential in that year.

Table 6.10a offers a simple attempt to analyse this issue, by examining cross-section snapshots of workers for each year between 1991 and 1997, and estimates the satisfaction differential between public and private sectors workers. In 1991 the

[30] Analogous to previously, when these data are pooled over time, we observe a positive and well-determined trend in public sector stress over and above that observed in the private sector.

public sector parameter is estimated to be 0.129, this effect remains relatively stable at approximately 0.100 for the years 1992 to 1995, but in 1996 and 1997 the estimated coefficient is attenuated and no longer statistically significantly different from zero. Table 6.10b presents the marginal effects associated with the estimates. In 1991 public sector employees are observed to be 5 percent more likely to be 'mostly' or 'completely' satisfied (categories 6 and 7). Between 1992 and 1995 the figure lies between 3.4 and 4.0 percent. In 1996 this falls to 2.0 percent, and in 1997 to 1.2 percent.

During the early 1990s, when job satisfaction was falling, the public-sector satisfaction differential remained positive and statistically significantly different from zero. This suggests any dissatisfaction resulting from public sector reforms was, at least partially, offset by growing dissatisfaction amongst private sector employees. Only in the late 1990s, when private sector satisfaction rose faster than that in the public sector, did the estimated differential fall.

Whilst the difference in the estimated public sector effect between 1991 and 1992 is relatively minor, we henceforth err on the side of caution and restrict attention to the sample period 1992 to 1997.[31] Table 6.11a pools the data over time and estimates the coefficient upon time trends in job satisfaction, for samples of public and private sector workers. Table 6.11b presents the marginal effects. We again observe a statistically significant worsening of worker well-being, here job satisfaction, for workers within the state sector between 1992 and 1997. Amongst private sector employees we observe a small and statistically insignificant fall in job satisfaction. Column three, where we pool the public and private sectors and capture the trend in the state sector by an interaction term, confirms these findings.

[31] Results are, however, qualitatively the same for the period 1991 to 1997.

Whilst these estimates are likely to mask the dip in observed job satisfaction in the middle of the decade (Figure 6.2) they suggest the satisfaction differential between public and private sector occupations narrowed between 1991 and 1998. Furthermore, the estimated time trend in public-sector job satisfaction is essentially the same when we omit controls for pay and/or the hours of work.

6.5 Conclusions

Well-being has worsened markedly among Britain's public sector workers. It is not possible to be completely certain as to why. The evidence suggests composition change within public sector employment and the relative decline in public sector pay are not sufficient explanations.

It would be desirable to examine further years of data to check the permanence of these results, and to see whether they remain during an economic downturn. Nevertheless, given that the downward trend in public sector well-being is observed to have occurred, in part, prior to the economic upswing of the late 1990s, and to be greatest in regions that have benefited least from this upturn, it is not clear that general economic conditions will explain the results.

Whilst these data are insufficient to fully isolate those workers for whom well-being has fallen within the public sector, it appears the rise in stress has disproportionately affected those workers with relatively lower-skills and women. What can be said, however, is that the evidence appears to point unambiguously to a non-negligible increase in GHQ mental stress, and fall in job satisfaction, among Britain's public employees over the 1990s.

GHQ MENTAL STRESS HEALTH SCORES

TABLE 6.1
Mean Scores in GHQ Mental Stress Health Scores over Time

Public and Private Sector Employees over Time

Year	*PUBLIC*	*PRIVATE*
1991	10.36 (4.55)	10.14 (4.31)
1992	10.94 (4.81)	10.58 (4.68)
1993	10.85 (4.78)	10.54 (4.87)
1994	11.13 (4.96)	10.69 (5.04)
1995	11.41 (5.48)	10.71 (4.89)
1996	11.29 (5.37)	10.71 (4.99)
1997	11.50 (5.60)	10.59 (4.96)
1998	11.63 (5.62)	10.65 (4.98)
Total	*11.12 (5.15)*	*10.57 (4.84)*

- Standard deviations are in parenthesis.
- The GHQ variable measures mental distress or **lack** of psychological well-being on a 36-point scale, with 0 being the lowest level of distress and 36 the highest.

FIGURE 6.1
GHQ Levels of UK Workers over Time
Public and Private Sector Employees

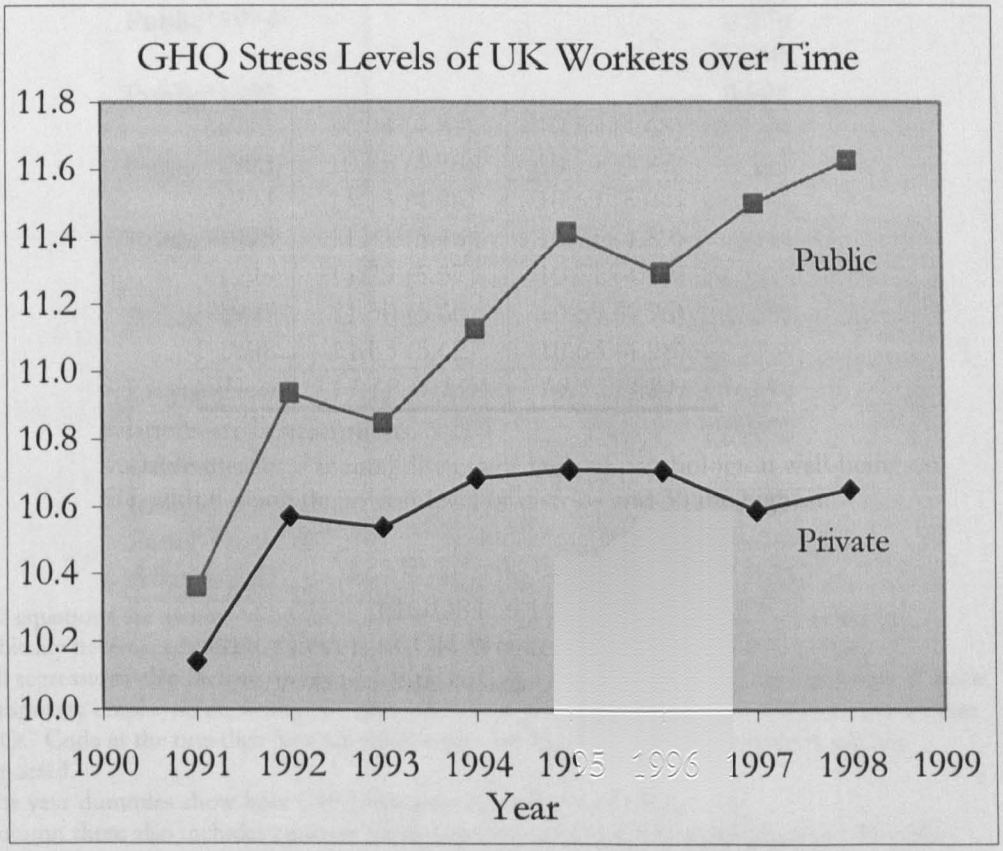

TABLE 6.2a
The Trend in GHQ Mental Stress Health Scores over Time (1991-8)
Coefficients upon Year Dummies
Dependent Variable: GHQ

REGRESSOR	*PUBLIC*	*PRIVATE*	*ALL*
1992	0.577	0.408	0.404
	(0.151)	(0.098)	(0.098)
1993	0.463	0.349	0.342
	(0.165)	(0.107)	(0.107)
1994	0.677	0.475	0.465
	(0.171)	(0.111)	(0.111)
1995	0.918	0.510	0.492
	(0.188)	(0.113)	(0.113)
1996	0.772	0.510	0.488
	(0.190)	(0.113)	(0.112)
1997	0.914	0.410	0.385
	(0.200)	(0.112)	(0.112)
1998	1.009	0.463	0.439
	(0.198)	(0.113)	(0.113)
Public Sector			-0.256
			(0.163)
Public*1992			0.190
			(0.179)
Public*1993			0.156
			(0.197)
Public*1994			0.270
			(0.204)
Public*1995			0.501
			(0.220)
Public*1996			0.357
			(0.219)
Public*1997			0.623
			(0.227)
Public*1998			0.679
			(0.225)
Ln(pay)	-0.234	-0.319	-0.350
	(0.204)	(0.107)	(0.092)
Observations			
Individuals (N_i)	2303	5883	7737
Panel Total (NT)	9221	22534	33000
Adjusted R^2	0.04	0.03	0.03

1. All equations are estimated by OLS. Standard errors are in parentheses and are robust to arbitrary heteroscedasticity and the repeat sampling of the same individuals over time.
2. All regressions also include quadratics in age and job tenure, and controls for the hours of work, temporary employment, workplace size, education, gender, race, union recognition, occupation (SOC Code at the one-digit level), marital status and region. Parameter estimates are not reported.
3. The year dummies show how GHQ has increased relative to 1991.
4. Column three also includes controls for non-government non-profit organisations. For this column year dummies capture the time trend in the private sector. The public-year interactions capture the difference in trend between the public and private sectors, the public indicator the difference in constant terms.

TABLE 6.2b
The Trend in GHQ Mental Stress Health Scores over Time (1991-8)
Coefficients upon a Time Trend
Dependent Variable: GHQ

REGRESSOR	*PUBLIC*	*PRIVATE*	*ALL*	*ALL*
Time trend	0.120	0.047	0.043	0.050
	(0.026)	(0.015)	(0.014)	(0.021)
Public Sector			-0.324	-0.466
			(0.162)	(0.449)
Public*(Time trend)			0.093	0.102
			(0.029)	(0.039)
Ln(pay)	-0.217	-0.321	-0.349	-0.349
	(0.203)	(0.107)	(0.092)	(0.092)
County Unemployment Rate				0.010
				(0.024)
Public*(Unemployment Rate)				0.013
				(0.040)
Observations				
Individuals (N_i)	2303	5883	7737	7737
Panel Total (NT)	9221	22534	33000	33000
Adjusted R^2	0.04	0.03	0.03	0.03

1. See notes for Table 6.2a.
2. The time trend here equals 1 if the year is 1991, 2 if the year is 1992,..., and 8 if the year is 1998.
3. In column three the time trend captures the trend in the private sector. The public-trend interaction captures the difference in time trend between the public and private sectors, the public sector indicator the difference in constant terms.

TABLE 6.3
The Time Trend in GHQ Mental Stress Health Scores over Time (1992-8)
Coefficients upon a Time Trend: The effect of dropping 1991
Dependent Variable: GHQ

REGRESSOR	*PUBLIC 1992-8*	*PRIVATE 1992-8*	*ALL 1992-8*	*ALL 1991-5*
Time trend	0.080	0.010	0.007	0.106
	(0.031)	(0.018)	(0.018)	(0.027)
Public Sector			-0.329	-0.293
			(0.205)	(0.189)
Public*(Time trend)			0.088	0.106
			(0.036)	(0.051)
Ln(pay)	-0.267	-0.312	-0.364	-0.278
	(0.221)	(0.113)	(0.099)	(0.106)
Observations				
Individuals (N_i)	2115	5354	7066	6621
Panel Total (NT)	7930	19441	28429	20614
Adjusted R^2	0.04	0.03	0.03	0.03

1. See notes for Table 6.2a.

TABLE 6.4a
The Time Trend in GHQ Mental Stress Health Scores over Time (1991-8)
All employees: By Union recognition, Gender and Region
Dependent Variable: GHQ

REGRESSOR	*ALL*	*UNION*	*MALE*	*FEMALE*	*SOUTH*	*NORTH*
Time trend	0.043	0.044	0.025	0.065	0.066	0.043
	(0.014)	(0.024)	(0.018)	(0.024)	(0.026)	(0.027)
Public Sector	-0.324	-0.363	-0.336	-0.173	-0.154	-0.333
	(0.162)	(0.195)	(0.236)	(0.234)	(0.290)	(0.284)
Public*(Time trend)	0.093	0.096	0.075	0.093	0.064	0.102
	(0.029)	(0.036)	(0.044)	(0.040)	(0.052)	(0.050)
Ln(pay)	-0.349	-0.442	-0.521	-0.247	-0.235	-0.529
	(0.092)	(0.138)	(0.135)	(0.143)	(0.160)	(0.161)
Observations						
Individuals (N_i)	7737	4351	3837	3900	2683	2438
Panel Total (NT)	33000	16969	16288	16712	11064	10278
Adjusted R^2	0.03	0.04	0.02	0.02	0.02	0.05

1. See notes to Table 6.2b.
2. UNION denotes a union recognised workplace.
3. SOUTH includes London, the South East of England and East Anglia.
4. NORTH includes the North East of England, the North West of England, Scotland and Wales.

TABLE 6.4b
The Time Trend in GHQ Mental Stress Health Scores over Time (1991-8)
All employees: By Education
Dependent Variable: GHQ

REGRESSOR	*NONE*	*O-LEVEL*	*A-LEVEL*	*DEGREE*
Time trend	0.048	0.044	0.051	0.032
	(0.030)	(0.023)	(0.029)	(0.048)
Public Sector	-0.620	-0.288	-0.403	-0.163
	(0.345)	(0.276)	(0.302)	(0.474)
Public*(Time trend)	0.171	0.093	0.074	0.067
	(0.071)	(0.049)	(0.054)	(0.074)
Ln(pay)	-0.238	-0.224	-0.350	-0.759
	(0.205)	(0.149)	(0.182)	(0.248)
Observations				
Individuals (N_i)	1908	2952	2174	1089
Panel Total (NT)	7525	12098	8883	4494
Adjusted R^2	0.04	0.04	0.04	0.05

1. See notes to Table 6.2b. Education refers to highest (formal) qualification, or equivalent.

TABLE 6.5
The Time Trend in GHQ Mental Stress Health Scores over Time (1991-8)
The Effect of Composition Change: By Sector
Dependent Variable: GHQ

	PUBLIC			*PRIVATE*	
REGRESSOR	*BALANCE*	*STAYERS*	*PUBLIC JOBS*	*BALANCE*	*STAYERS*
Time trend	0.228	0.206	0.102	0.060	0.061
	(0.040)	(0.043)	(0.043)	(0.025)	(0.026)
Ln(pay)	-0.390	-0.325	-0.408	-0.249	-0.303
	(0.361)	(0.423)	(0.303)	(0.220)	(0.237)
Observations					
Individuals (N_i)	607	427	903	1086	891
Panel Total (NT)	4156	3416	3406	7894	7128
Adjusted R^2	0.06	0.06	0.04	0.04	0.04

1. See notes to Table 6.2b.
2. BALANCE denotes a respondent observed within the sample for all eight waves (1991-1998). STAYER denotes an employee observed in the same sector in all eight waves.
3. PUBLIC JOB denotes an occupation considered that has remained largely untouched by privatisation and competitive tendering, and where composition change is limited (see Appendix).

TABLE 6.6
The Time Trend in GHQ Mental Stress Health Scores over Time (1991-8)
The Change in Well-being
Dependent Variable: *ΔGHQ/Δ7GHQ*

Regressor	*ΔGHQ ALL 1992-8*	*Δ7GHQ ALL 1998*	*ΔGHQ STAYERS 1992-8*	*Δ7GHQ STAYERS 1998*
Public	0.096	0.689	0.178	0.985
	(0.066)	(0.346)	(0.071)	(0.445)
Ln(pay)	-0.156	0.288	-0.089	-0.224
	(0.064)	(0.302)	(0.085)	(0.434)
Observations				
Individuals (N_i)	5696	2384	1335	1335
Panel Total (NT)	23380	2384	9345	1335
Adjusted R^2	0.00	0.03	0.00	0.04

1. See notes to Table 6.2b.
2. ALL denotes the unbalanced sample for of respondents for whom a change in GHQ is observed. STAYER denotes an employee observed in the same sector in all eight waves.
3. All equations are estimated by OLS. Standard errors are in parentheses and are robust to arbitrary heteroscedasticity and the repeat sampling of the same individuals over time.
4. ΔGHQ refers to the one period change in GHQ score ($GHQ_t - GHQ_{t-1}$). Δ7GHQ is the change in GHQ score over the full seven year period of the panel ($GHQ_t - GHQ_{t-7}$) = ($GHQ_{1998} - GHQ_{1991}$).
5. 1992-8 then denotes that *changes* in GHQ are only available for that period.

TABLE 6.7
Proportion of Respondents with High Mental Stress
Public and Private Sector Employees over Time

Year	*PUBLIC*	*PRIVATE*
1991	0.17 (0.37)	0.15 (0.35)
1992	0.20 (0.40)	0.19 (0.39)
1993	0.20 (0.40)	0.18 (0.38)
1994	0.21 (0.40)	0.18 (0.38)
1995	0.21 (0.41)	0.17 (0.37)
1996	0.21 (0.40)	0.18 (0.38)
1997	0.23 (0.42)	0.17 (0.38)
1998	0.25 (0.43)	0.17 (0.37)
Total	0.21 (0.41)	0.17 (0.38)

- Standard deviations are in parenthesis.
- The measure of high mental distress is a dichotomous indictor, taking the value 1 if an individual answers negatively to four or more of the twelve GHQ questions and 0 otherwise.
- This measure is commonly employed as an indicator of likely psychiatric disorder.

TABLE 6.8a
Yearly Cross-section High Mental Stress Regressions (1991-8)
The Public Sector Effect
Dependent Variable: High Mental Stress

REGRESSOR	*1991*	*1992*	*1993*	*1994*	*1995*	*1996*	*1997*	*1998*
Public Sector	0.097	0.023	0.059	0.036	0.014	-0.022	0.158	0.136
	(0.065)	(0.065)	(0.068)	(0.067)	(0.068)	(0.067)	(0.067)	(0.068)
Ln(pay)	0.031	-0.056	-0.089	-0.030	-0.076	-0.055	-0.071	-0.095
	(0.058)	(0.058)	(0.061)	(0.058)	(0.055)	(0.057)	(0.054)	(0.058)
Observations								
Individuals	4571	4141	3915	3968	4019	4125	4144	4117
Log-L	-1923.1	-1976.8	-1831.0	-1865.9	-1876.3	-1947.1	-1946.0	-1927.2
Pseudo R^2	0.035	0.040	0.044	0.042	0.046	0.053	0.051	0.071

1. All equations are estimated by the Probit technique. Standard errors are in parentheses and are robust to arbitrary heteroscedasticity.
2. All columns include the same controls as in Table 6.2a. Parameter estimates are not reported.
3. The public sector dummy is relative to those in the private sector, in the sample year.
4. The Pseudo R^2 is calculated using the method of McKelvey and Zavoina (1975).

TABLE 6.8b
Yearly Cross-section High Mental Stress Regressions (1991-8)
The Public Sector Effect: The 'Stayers'
Dependent Variable: High Mental Stress

REGRESSOR	*1991*	*1992*	*1993*	*1994*	*1995*	*1996*	*1997*	*1998*
Public Sector	-0.003	0.008	-0.122	-0.166	0.102	-0.045	0.187	0.232
	(0.129)	(0.118)	(0.119)	(0.117)	(0.123)	(0.119)	(0.121)	(0.121)
Ln(pay)	-0.091	-0.020	0.002	-0.162	-0.083	-0.124	0.110	-0.159
	(0.130)	(0.114)	(0.121)	(0.115)	(0.114)	(0.112)	(0.116)	(0.118)
Observations								
Individuals	1335	1335	1335	1335	1335	1335	1335	1335
Log-L	-494.4	-585.9	-584.7	-565.1	-562.9	-606.3	-600.9	-628.5
Pseudo R^2	0.108	0.075	0.080	0.101	0.108	0.067	0.082	0.124

1. See notes to Table 6.8b. STAYER denotes a respondent observed in the same sector in every period.

JOB SATISFACTION

TABLE 6.9
Mean Job Satisfaction Scores over Time

Public and Private Sector Employees over Time

Year	*PUBLIC*	*PRIVATE*
1991	5.58 (1.41)	5.40 (1.57)
1992	5.56 (1.30)	5.42 (1.41)
1993	5.45 (1.33)	5.35 (1.40)
1994	5.36 (1.39)	5.29 (1.43)
1995	5.39 (1.29)	5.30 (1.39)
1996	5.40 (1.28)	5.36 (1.34)
1997	5.43 (1.27)	5.40 (1.32)
Total	*5.46 (1.33)*	*5.36 (1.41)*

- Standard deviations are in parenthesis.
- The job satisfaction variable measures overall satisfaction on a 7-point scale, with 1 being the lowest level of satisfaction and 7 the highest. In 1991 the categories 1, 4 and 7 were given descriptions whilst 2,3,5 and 6 are left unlabeled. From 1992 onwards all values were given a label. The question itself was a constant. In 1998 the process by which the question is asked was changed.

FIGURE 6.2
Job Satisfaction Levels of UK Workers over Time
Public and Private Sector Employees

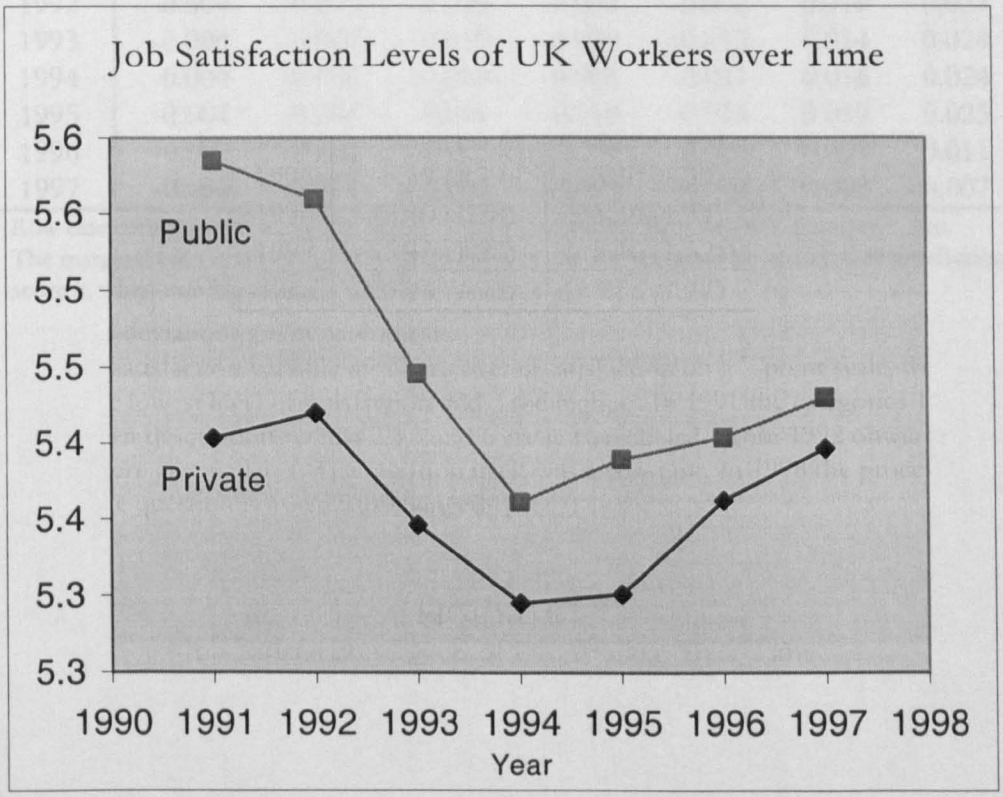

TABLE 6.10
Yearly Cross-section Job Satisfaction Equations (1991-7)
The Public Sector Effect
Dependent Variable: Overall Job Satisfaction

REGRESSOR	*1991*	*1992*	*1993*	*1994*	*1995*	*1996*	*1997*
Public Sector	0.129	0.099	0.100	0.102	0.115	0.052	0.031
	(0.045)	(0.047)	(0.048)	(0.048)	(0.049)	(0.049)	(0.048)
Ln(pay)	0.028	0.078	0.058	0.036	-0.020	-0.006	0.025
	(0.040)	(0.044)	(0.045)	(0.043)	(0.042)	(0.042)	(0.041)
Observations							
Individuals (N_i)	4551	4127	3897	3949	4009	4115	4132
Log-Likelihood	-7216.3	-6137.8	-5836.9	-6059.4	-6013.2	-6076.9	-6054.2
Pseudo R^2	0.089	0.097	0.095	0.091	0.102	0.087	0.087

1. All equations are estimated by the Ordered Probit technique. Robust standard errors are in parentheses.
2. All columns include the same set of controls as in Table 6.2a. Coefficients are not reported.
3. The public sector dummy is relative to those in the private sector, in the sample year.
4. Consistent Job satisfaction data cover only the period 1991-1997.
5. The Pseudo R^2 is calculated using the method of McKelvey and Zavoina (1975).

TABLE 6.10b
The Marginal Effect of the Time Trend upon Overall Job Satisfaction

	Overall Satisfaction Score						
Sample	*1*	*2*	*3*	*4*	*5*	*6*	*7*
1991	-0.008	-0.004	-0.008	-0.017	-0.013	0.005	0.045
1992	-0.004	-0.005	-0.009	-0.008	-0.012	0.010	0.027
1993	-0.004	-0.004	-0.010	-0.008	-0.012	0.014	0.024
1994	-0.004	-0.006	-0.010	-0.008	-0.011	0.016	0.024
1995	-0.004	-0.006	-0.011	-0.010	-0.014	0.019	0.025
1996	-0.002	-0.002	-0.005	-0.004	-0.006	0.009	0.011
1997	-0.001	-0.001	-0.003	-0.002	-0.004	0.005	0.007

1. Row one corresponds to column one of Table 6.10a above. Row two to column two, etc.
2. The marginal effects are calculated as the difference in the predicted probability, of satisfaction score k, when moving from the private to public sector.

TABLE 6.11a
The Trend in Job Satisfaction Scores over Time (1992-7)
Coefficients upon a Time Trend
Dependent Variable: Overall Job Satisfaction

REGRESSOR	*PUBLIC*	*PRIVATE*	*ALL*
Time trend	-0.026	-0.007	-0.006
	(0.008)	(0.005)	(0.005)
Public Sector			0.169
			(0.050)
Public*(Time trend)			-0.019
			(0.009)
Ln(pay)	-0.023	0.041	0.027
	(0.053)	(0.028)	(0.024)
Observations			
Individuals (N_i)	2015	5019	6712
Panel Total (NT)	6861	16477	24229
Log-likelihood	-9933.5	-25126.4	-36297.4
Pseudo R^2	0.102	0.082	0.087

1. See notes to Table 6.10 Standard errors are robust to the repeat sampling of individuals over time.
2. In column three the time trend captures the trend in the private sector. The public-trend interaction captures the difference in time trend between the public and private sectors. The public sector dummy shows the level of job satisfaction for the base person in the public sector.

TABLE 6.11b
The Marginal Effect of the Time Trend upon Overall Job Satisfaction

	Overall Satisfaction Score						
Sample	*1*	*2*	*3*	*4*	*5*	*6*	*7*
Public (1992-1997)							
Public sector trend	0.004	0.006	0.013	0.010	0.017	-0.020	-0.030
Private (1992-1997)							
Private sector trend	0.001	0.002	0.003	0.003	0.004	-0.005	-0.008
All (1992-1997)							
Private sector trend	0.001	0.002	0.003	0.003	0.004	-0.005	-0.007
Public sector trend	0.005	0.007	0.013	0.011	0.016	-0.021	-0.030

1. Row one corresponds to column one of Table 6.11a above. Row two to column two, etc.
2. The marginal effects are calculated as the difference in the predicted probability, of satisfaction score k, when moving from 1992 to 1997.

APPENDIX: Definition of Public Sector Occupations

The table below outlines occupations considered to be predominantly within the public sector. They must satisfy three criterions:

1. Sector has not been subject to privatisation or contracting out (e.g. eliminates British Rail, traffic wardens, refuse collectors)
2. Job itself is not subject to contracting out (e.g. rules out cleaning staff in hospitals).
3. Limited opportunities for transferring into the private or non-profit sector *within* the same occupation (e.g. eliminates dentists).

SOC Codes Corresponding to Public Occupations

SOC	*Description of Occupation*
150	Officers in UK armed forces
151	Officers in foreign & Commonwealth armed forces
152	Police officers
153	Fire service officers
155	Customs & excise, immigration service officers
191	Registrars & administrators of educational establishments
220	Medical practitioners
230	University & polytechnic teaching professionals
231	Higher & further education teaching professionals
233	Secondary education teaching professionals
234	Primary & nursery education teaching professionals
235	Special education teaching professionals
239	Other teaching professionals
240	Judges & officers of the court
293	Social workers, probation officers
340	Nurses
341	Midwives
342	Medical radiographers
600	NCOs & other ranks, UK armed forces
601	NCOs & other ranks, foreign & Commonwealth armed forces
610	Police officers (sergeant & below)
611	Fire service officers (leading fire officer & below)
613	Customs & excise officers, immigration officers
640	Assistant nurses, nursing auxiliaries
641	Hospital ward assistants
642	Ambulance staff
652	Educational assistants

Chapter Seven

Conclusion

This thesis has studied the forces that influence pay and well-being. We have used new data, with detailed information about both the employee and the firm, to investigate the relative importance of the employer in determining worker pay. This work adds to an emerging research field that studies matched worker-firm data. It also questions whether wage determination is, in some way, non-competitive. Chapter two tests whether more profitable firms pay higher wages, chapter three whether pay is greater in large employers. In both chapters the role of unobserved worker quality, and the sorting of employees into firms, is studied in detail. The role of the employer in explaining the racial wage differential is investigated in chapter four, with particular focus upon the relationship between pay and the ethnic makeup of the workforce.

This thesis has also examined the determinants of self-reported measures of well-being. It has used these variables to provide several new tests of economic hypotheses. In chapter three we use such data to test whether working conditions are inferior within large establishments or firms. The role of the employer in explaining racial differentials in job satisfaction is examined in chapter four. This provides a new approach to the analysis of racial disadvantage in Britain. Chapter five tested one of the most fundamental ideas in economics, that money makes people happy. It examined whether recorded measures of mental distress and happiness improved subsequent to receiving a windfall of money. Finally, the well-being levels of British public sector workers over the 1990s were examined in chapter six.

Chapter two studied whether more profitable firms pay higher wages. OLS estimates of the elasticity of wages with respect to (current) firm profits per employee were equal to 0.02, after controlling for observed worker and firm characteristics. This is not as small as first appears. Moving from a firm with

profitability one standard deviation below the mean to one with profits one standard deviation above the mean was predicted to increase wages by 8 percent. The positive effect of firm profitability was found to be robust to controlling for formal profit-sharing schemes, within occupations where labour supply difficulties should be limited, and with respect to past firm profitability. Moreover, the observed link between pay and profitability was not found only in the union sector.

Few previous investigators have had access to matched data. Such data were here used to more fully model worker and firm heterogeneity in pay. When controls for worker and firm fixed effects were added, we observed statistically significant evidence in support of rent-sharing upon weekly earnings, with an estimated elasticity of 0.01, but no robust positive effect upon hourly pay, in a sample that potentially favours the rent-sharing hypothesis. Estimates upon weekly earnings could, however, capture increased hours of work, if positively correlated with movements in firm profitability. A key issue in estimation is endogeneity. An increase in worker pay will reduce firm profitability, other things being equal. The estimated parameter upon firm profitability may then be biased downward. Chapter two instruments firm profitability by international product market shocks, captured by movements in US industry profitability. The estimated rent-sharing parameters, both for weekly and hourly pay, were, however, either incorrectly signed or statistically insignificant. Support for the rent-sharing hypothesis was then found to be, at best, modest and limited to weekly earnings, within a sample that is likely, if anything, to overstate the impact of profitability upon pay.

Chapter three examined four potential explanations for the observed positive relationship between employer size and worker pay: unobserved productivity differences, compensating differentials, rent-sharing, and differences in monitoring intensity. Using employer-employee data, we found the addition of

richer controls for employer characteristics (the firm's capital to labour ratio and profitability, and the workplace's monitoring intensity) to leave the estimated establishment size coefficients largely unperturbed. The evidence suggested neither rent-sharing nor monitoring costs could, here, explain the size-wage correlation.

The role of unobserved differences in labour productivity was analysed in two ways. Correlates of worker skill, such as the use of information technology and the skill of the establishment's workforce, were found to explain up to 15 percent of the plant size-wage relationship, and up to 30 percent of the firm size effect upon pay. Secondly, controls for person fixed-effects were found to reduce the estimated effect of workplace size upon wages by over a half. Nevertheless, wages remained statistically significantly greater in large establishments.

Whether the relationship between employer size and pay reflects a compensating differential, possibly for inferior working conditions, was tested in chapter three using well-being data. Job satisfaction was found to be superior in the smallest plants, but differences in satisfaction between medium-sized and large establishments were not pronounced. The evidence does not then offer a convincing route as to why pay is observed to be higher in the largest plants, relative to medium-sized establishments. Moreover, much of the dissatisfaction associated with workplace size was found to be attributable to the size of the parent company. Holding firm size constant we observed no robust independent influence of establishment size upon satisfaction. In contrast, wages were observed to be statistically significantly greater in large plants, after conditioning upon the size of the firm.

On this evidence, employee distaste for employer size does not offer a persuasive explanation for the existence of a plant size-wage premium, though it may help to explain the firm size-wage premium. Correlates of worker skill and

person fixed effects were, instead, found to offer the most convincing avenue from which to explain the establishment size-wage differential. A large unexplained wage premium, to those employees working in the largest workplaces, does though remain.

The influence of the employer upon racial differentials in pay and job satisfaction was examined in chapter four. Non-white workers were found to earn lower wages in establishments with more ethnic minority co-workers. White wages, in contrast, were only weakly related to the racial composition of the plant. The gap between ethnic minority and white pay was hence larger in establishments with more non-white workers. The racial differential in pay was also observed to be robust to controls for occupation, and for workers within the same workplace. The primary avenue for the racial wage gap is then not that ethnic minority workers are employed in low-pay plants, rather they are less well paid in any given workplace.

Job satisfaction data were used to test whether well-being is lower for non-white employees, and in plants which hire more ethnic minority workers. Establishments that employ a greater proportion of non-white staff were found to be associated with lower levels of job satisfaction, for white males, white females, and for ethnic minority women. Results were, however, more mixed for non-white men. Plants with a greater ethnic minority employment share were also observed to be associated with a higher rate of quits, separations, and absenteeism. The evidence suggests non-white workers are, here, employed in workplaces with lower levels of worker well-being. This could reflect either discrimination or unobserved worker quality differences, where non-white workers are for some reason less productive (possibly due to pre-labour market discrimination).

Evidence was also observed consistent with ethnic minority employees trading off lower pay to work with more minority co-workers. Non-white pay was

found to be lower, employer tenure higher, in plants with a greater ethnic minority employment share. Nevertheless, ethnic minority workers were found to be less satisfied with their pay, compared to otherwise similar white workers, even when pay is held constant. Non-white men are -4.2 percent less likely, than white males, to respond as satisfied or very satisfied with their pay, whilst non-white women are -6.0 percent less likely than similar white females. Results remained when the establishment's effect upon satisfaction was held constant. This provides new evidence that seems consistent with racial discrimination on pay.

Chapter five devised a test of what is probably one of the central tenets of economics, that a person who becomes richer becomes happier. Whilst recent work has documented a positive cross-section correlation between reported happiness and income, that is not a persuasive reason to believe that more money leads to greater well-being. Cross-section patterns are at best suggestive because their causal implications are hard to interpret. The analysis in chapter five has three advantages. First, individuals are followed longitudinally and the same person's well-being and income levels measured at different points in time. Second, two types of financial windfall (inheritances and lottery wins) are observed. In the spirit of a random experiment, these approximate random events in which some individuals have money showered upon them while others, in a control group, do not. Third, two measures of well-being are analysed: mental stress using a standard psychological health score, and happiness using a simple four-point question. Even the psychological literature appears not to have had a suitable longitudinal test.

The evidence did indeed suggest, as theory would predict, that money improves well-being. A monetary windfall in year t was found to be followed by lower mental stress and higher reported happiness in year t+1. At a conservative

estimate, a windfall of 50,000 pounds is predicted to improve mental well-being by between 0.1 and 0.3 standard deviations.

Chapter six studied how the reported well-being levels of British public sector workers changed over 1990s, and compared outcomes to those for workers in the private sector. Well-being was found to have notably deteriorated amongst Britain's public sector workers. At the start of the 1990s, levels of well-being, as measured by a standard psychological health score, for public sector workers were similar to those within the private sector, but by 1998 were markedly worse. Consistent with this, a relative decline in public sector job satisfaction was also observed.

We can not be completely certain as to why stress has risen with the public sector. The evidence of chapter six suggests it can not be satisfactorily explained by changes in the composition of public sector employment, by a decline in public sector pay relative to the private sector, or by aggregate movements in economic conditions. The rise in stress was, though, found to have disproportionately affected those workers with relatively lower-skills, and women. What is clear, however, is that the evidence shows a marked reduction in well-being amongst Britain's public employees over the 1990s.

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