Long-term Illness and Wages

The Impact of the Risk of Occupationally Related Long-term Illness on Earnings

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ABSTRACT

Long-term illness (LTI) is a more prevalent workplace risk than fatal accidents but there is virtually no evidence for compensating differentials for a broad measure of LTI. In 1990 almost 3.4 percent of the U.K. adult population suffered from a LTI caused solely by their working conditions. This paper provides the first estimates of compensating differentials for a broad measure of work-related LTI. Using data on self-reported illnesses we find significant CDs for male manual workers but none for male nonmanual workers. These results are robust to the addition of variables for the risk of accidental at-work deaths.

"In trades that are known to be unwholesome, the wages of Labour are always remarkably high"[1]

I. Introduction

Adam Smith recognized that the risk of an occupational illness was one of a set of factors that would command a high wage. According to Smith, excessive hard work was a feature of every manual occupation and was associated in each

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1. Smith, p. 123.

case with a particular infirmity (Smith 1976, p. 91). In evidence, Smith cited a book on occupational diseases by an eminent Italian physician.[2] Long-term occupational illnesses are far more common than deaths at work. Yet while a large number of studies find substantial compensating differentials for the risk of death at work Smith's occupational-illness high-wage hypothesis has never been properly tested. In this paper we show that, though the nature of the illnesses contracted at work have changed since Smith's days, 230 years after Smith wrote the risk of an occupational illness is still an important determinant of wages. We reveal significant compensating differentials for male manual workers for a broad measure of work-related long-term illness.

The term "workplace illness" refers to diseases or disabilities that are contracted in the course of work as distinct from the term "workplace injury," which typically refers to traumas such as cuts, burns, or broken limbs, caused by a workplace accident. Both the Bureau of Labor Statistics (BLS) and the U.K. Health and Safety Executive (HSE), whose data we use here, distinguish a workplace illness from a workplace injury by the presence of a causal accident in the latter case. Thus, a case of radiation poisoning due to a specific incident such as a single leak of radiation would be considered a work-related injury while a case of leukemia due to long-term exposure to benzene would be considered a workplace illness.[3]

Illness risk differs from injury and death risk at the workplace in two important respects. First, illness risk is cumulative; illness risk in any one year likely increases with the number of years of prior exposure. For most serious occupational illnesses, there are thresholds of exposure to hazards or repeated motions that have to be exceeded before the symptoms emerge. Hearing loss, respiratory diseases, repetitive strain syndrome, and most occupational cancers are examples of such illnesses. In contrast death and accident risks in a given year are not sensitive to the number of prior years of exposure. Second the experience of those who retired at the normal age as well as early retirees is an important source of information on the risks from long-term exposures. This is because in order to estimate their life-time illness risk, workers must consider the illnesses and the exposures of both their co-workers and retirees, for the latter have the longest exposures.

Occupational illnesses would be expected to create measurable wage premiums if the incidence of illnesses is both nontrivial and well known. The greater incidence and longer duration of long-term illnesses, together, perhaps, with the higher level of pain associated with a serious illness might generate higher economy-wide wage premiums than are found for risks of death and injury. Moreover, if the older workers in an occupation have readily observable symptoms of occupational illnesses while accidental deaths in the same occupation are infrequent, younger workers in that

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2. Bernadino Ramazzini, *De Morbis Artificum Diabriba,* 1700, (translated into English as *A Treatise on the Diseases of Tradesmen,* R. James, 1740); Smith misspelled the author's name as Ramuzzini, which can cause difficulties in finding the book.

3. The Bureau of Labor Statistics definitions (URL: http://stats.bls.gov/oshdef.htm) are: Occupational injury is any injury such as a cut, fracture, sprain, amputation, etc., which results from a work-related event or from a single instantaneous exposure in the work environment. Occupational illness is any abnormal condition or disorder, other than one resulting from an occupational injury, caused by exposure to factors associated with employment. It includes acute and chronic illnesses or disease which may be caused by inhalation, absorption, ingestion, or direct contact.

occupation might be much more aware of their risks of illnesses than their risks of death. It is therefore possible that compensating differentials for occupational illnesses are a more important explanation of variation in wages than compensation for risk of death or injury. Showing that the economy-wide compensation for illness risk is greater than compensation for fatal risk requires estimates of the entire risk-compensation gradient and the application of numeric methods to simulate total risk compensation. The wage-risk gradient for fatal accident risk is probably highly nonlinear with most of the compensation going to the small fraction of workers facing severe risks. Below we are able to compare compensation for fatal accident risk and illness risk for manual workers at the mean levels of these respective risks. We show that, at least at the mean levels, the illness-risk compensation is much higher.

There are now a large number of studies of compensating differentials (hereafter CDs) for deaths at work and some for injuries at work but very few of these assign risk to workers according to their occupation. Among these studies only a very small number attempt to measure CDs for illness due to work.[4] Moreover, most of these studies combine illness and injury and they do not distinguish between the two types of risk. They measure the risk by the percentage of workers having at least one lost work day in a year due to illness or injury and the great majority of employer-reported lost work days are the result of injuries.[5] Thus these studies tell us little about compensation for work-related illness. Only one study by Lott and Manning (2000) has achieved this objective but this study focuses exclusively on cancer risk. Lott and Manning found a statistically significant premium for exposure to cancer risk after controlling for injury risk. Their results suggest that a study entailing a broad measure of serious illnesses caused by work also should find positive compensation for exposure to such risk.

This paper is the first study of CDs for a broad measure of occupational long-term illness using a direct measure of reported illnesses. The data come from a unique survey conducted in the United Kingdom in the second quarter of 1990 for the Health and Safety Executive as an addition to the regular quarterly Labor Force Survey (LFS). In the second quarter of each year the LFS sample incidence is boosted and in 1990 it produced almost 50,000 household interviews from a representative sample of the U.K. adult population. The survey sought details of both accidents and illnesses at work. It distinguished carefully between accidents causing an injury at work and work-related illnesses. Among the illnesses reported by those surveyed it distinguished between those "caused by work" and those "made worse by work." The interviewers asked a series of detailed questions about the most serious illness. The link between the illness and the occupation that was the cause of that illness is tightly drawn. Interviewers were instructed to probe for details of the job that caused the illness and a fine occupational coding was used to classify jobs. In this analysis we concentrate on those illnesses caused by work and on only the most serious among these.

4. Viscusi (1996) contains a summary of the more recent studies

5. In the United States in 1998 the proportion of injuries out of all lost work-day incidents was more than 93 percent. BLS, *Workplace Injury and Illness Summary,* 1999 (URL: *http://stats.bls.gov/news.release/osh.nws.htm*)

II. Issues in Measurement

Identifying work-related illness and measuring the severity of an illness present substantial challenges. Only a small number of published papers have addressed any aspect of CDs for occupational illnesses. Fishback and Kantor (1992) and Leigh (1981) use employer-reported illnesses and injuries that result in at least one day of lost work to identify the combination of injuries and/or illnesses. Meng (1991) uses the records of a regulatory agency for compensable illnesses that result in death while Lott and Manning (2000) use a measure of exposure to carcinogens. With the exception of Lott and Manning these measures all have substantial shortcomings.

The studies by Fishback and Kantor (1992) and Leigh (1981) both used employer-reported lost workdays as a measure of occupational illness and injury risk. Fishback and Kantor (1981) used lost workdays over the period 1882 to 1903, and Leigh (1981) used days lost from work during 1973 and 1974. Neither study found a CD for work-related illnesses. Using lost workdays data to identify either injury risk or illness risk will result in substantial measurement error, and although the reasons for this differ between injury and illness data, the problems are likely to be greater in the case of illness data. Measurement error appears to provide at least part of the explanation for the failure of all but two studies to identify significant CDs for illness risk.

Measurement error arises because many illnesses, such as a gradual hearing loss, carpal tunnel syndrome, and emphysema, have a slow onset and may initially result in no lost workdays. In the early stages of the illness the worker may remain employed although the illness may eventually force an early retirement. Measurement error also arises because the number of lost workdays reported by employers is sensitive to reporting incentives. Employers may face incentives to reduce the number of workdays that are reported as lost and may be able to do so by assigning light duties to employees who are injured or ill. However, this is likely to be more possible in the case of an injury than in the case of a long-term illness. In the case of an illness it may be difficult for the employer to reduce an employee's exposure to hazards such as dust and chemical particles in the air that are general features of the workplace and which were the causes and exacerbants of the illness. The seriously ill may therefore have to take early retirement or switch occupations and lost workdays will not capture this. In contrast, in the case of an injury, such as a broken limb, from which the worker is likely to recover, the worker could be moved to light duties for a period after few if any lost workdays. Finally the accuracy of employer-reported lost days of work also may be sensitive to union presence, for where the union provides greater job security the presence of unions may affect the decisions of workers to take time off for illness and injury.

As well as missing any illnesses that did not cause lost workdays in the previous year, a measure such as workdays lost also does not capture work-related illnesses among people who retired early or died due to an occupational illness. Illnesses resulting in gradual debilitation and early retirements should be included in any study of the effect of occupational illnesses on wages. Any method of identifying occupational illnesses that relies on lost days of work is unlikely to offer any real insight into overall CDs for illnesses.

The study by Meng (1991) used data on deaths that a regulatory agency, in this case Labour Canada, recorded as due to "long-term" hazards, by which they meant deaths due to disease. These data were extracted from claims with the provincial Canadian

Workers' Compensation Boards but these data also are problematic. Meng found a much higher CD for risks of death from compensable occupational diseases than for risks of death from accidents. However such a measure of occupational illness is too limited because it excludes illnesses that do not result in deaths. Moreover, deaths from illnesses that are classified as compensable vary in a highly arbitrary manner across legal jurisdictions and tend to cover only cases from a restricted set of well-defined occupational diseases.

The most recent paper on CDs for illnesses focuses exclusively on cancer risk (Lott and Manning 2000). They employ a risk measure based on the toxicity of the sets of known carcinogens to which workers are exposed in two-digit U.S. industries as measured by the Hickey-Kearney carcinogen exposure indicator. The authors found a statistically significant premium for exposure to cancer risk after controlling for injury risk. The present value in 1984 dollars of the work-life premium at the mean level of cancer exposure was approximately $25,000. The implicit value of saving a life by avoiding a work-related cancer death was approximately six million dollars. These results were sensitive to a change in U.S. liability law. Once workers or their estates could successfully sue for damages due to work-related cancers these wage premiums dropped dramatically.

Both Meng's and Lott and Manning's studies suggest that a wider measure of occupational illness should yield a compensating differential. One substantial advantage of a study of the United Kingdom at this time is that we avoid problems of changing liability regimes and local variations in the rules for workplace compensation. In the United Kingdom in 1990 workers had very little opportunity to sue their employers over workplace illnesses. Thus in the United Kingdom, differential risks of workplace-related illness should, if they are compensated at all, be compensated with higher wages.

It must by now be evident that identifying work-related illness presents researchers with a challenge. This is made no easier because the origins of some illnesses are less clear than those of either accidental deaths at work or workplace injuries: they can develop slowly and may be exacerbated by outside-of-work factors such as smoking, exposure to carcinogens in the home, and loud noises, as at rock concerts. Furthermore, where workers switch occupations during their working life, this can obscure the connection between a specific occupation and a specific illness if the interviewers do not probe for the occupation which gave rise to the illness. More fundamentally, the data difficulties facing the researcher may result from the limitations of the information available to workers. Risk data generated from a retrospective survey of worker's experiences with occupational illnesses depends on the workers' abilities to connect their illnesses with their occupation. Contrary to the argument earlier these labor-market information deficiencies might be thought to make it less likely that illness would generate CDs than would risk of death.

It seems clear that the failure of earlier researchers to reveal positive CDs for illness risk other than cancers is due to substantial data problems and that measurement errors in data have plagued research in this area. There have been no estimates of CDs for a broad measure of work-related illness-risk due to these extensive data problems.[6]

6. According to Viscusi, "In the case of some job risks, particularly those involving illness and disease, we lack the extensive data needed to make the link between the risk and the wage compensation." (Viscusi 1992, p 8).

Data difficulties explain why studies of wages and workplace risk have focused on deaths and injuries at work for these are perhaps believed to be more immediately visible and can more easily be identified as caused by work. Our data, described in the next section, surmount these problems.

III. Data and Specification of Variables

A. Illness Data

In Smith's time, occupational illnesses were both common and obvious. Some examples with which he would have been familiar were matchmakers whose skin was yellow and scabrous from standing over vats of boiling sulfur and boy chimney sweeps whose growth was stunted from breathing the tar residues while climbing through chimneys. The nature of occupational illnesses has changed since Smith's time and it might be thought that severe occupational illnesses are now historical curiosities and that present occupational illnesses are so minor or so difficult to connect to a specific occupation that no CDs arise. This paper reveals that such a view would be mistaken and that long-term occupational illnesses are still sufficiently prevalent and known to workers to generate CDs.

The present study relies on data from a household survey of self-reported and self-defined occupational illnesses in the United Kingdom provided to the researchers by the HSE. The survey was administered in the second quarter of 1990 as a supplement to, a "trailer" to, the U.K. LFS, which is carried out quarterly and at that time by the Office of Population Censuses and Surveys (OPCS) on behalf of the Employment Department. It is described in Appendix 1.

We drew the remaining labor-market data we needed from the main part of the LFS. The LFS is a household survey designed to collect data from a random sample of households in the United Kingdom. It seeks data on individuals' economic activity and on their household and personal characteristics. In addition, it records their earnings, hours of work, their occupation and industry, and a number of other attributes of their jobs. In 1990 the LFS coded occupations into only 12 broad categories which were insufficiently detailed to construct our measure of risk. We therefore used a later version of the LFS for this other labor-market data. The LFS only began using the same fine occupational coding used to classify the illness data from late 1992 onward. The LFS accepts interview responses from proxy respondents but to minimize coding errors on the respondents' occupations we used only the data from self-respondents. This resulted in a sample size of 11,331 male respondents across 1993 and 1994. In addition to the LFS data we employed earlier data from the OPCS to construct measures of the risks of accidental deaths at work that are available for male workers only.

A potential source of weakness of self-reported illness data is the chance that the answers might not be thoughtful and the respondents may not have reflected sufficiently on what they believed to be the causes of their illnesses. However, the illness rates in this data set were constructed from a survey which contained detailed questions designed to elicit thoughtful, complete, and accurate questions, and the survey was administered by professional interviewers. Furthermore the illness rates that this survey produced were corroborated by a number of independent sources.

In *Self-Reported Work-Related Illness* (Hodgson et al. 1993) the HSE provided an initial description of the survey findings. They state that respondents in the LFS reported high rates of work-related illness for diseases that have a known medical cause connected to work such as loss of hearing and asbestosis. Moreover, attributions of diseases as caused by work were much less common for those diseases that have multiple causes or a less obvious connection to work such as heart disease and cancer.

The HSE also took several steps to verify the self-reported illness data. The HSE compared the LFS "trailer" population estimates to the numbers of people having a disabling complaint caused by an "industrial disease" in the 1985 to 1988 Disability Survey. (Hodgson et al., p 22). Allowing for the higher threshold set by the Disability Survey, the pattern of types of diseases and the rates of diseases in the two surveys are consistent. A second external corroboration came from the pattern of illnesses reported in visits by the adult population to General Practice (GP) physicians who are the initial medical contact under Britain's public health care system. Almost 7 percent of such visits were for work-related illnesses. (Hodgson et al., p 22). This figure is slightly above the 6 percent of the estimated population that had an illness that was either caused or made worse by their work in the LFS "trailer." However, the distribution of different illnesses in the GP survey, such as the high percentage of musculoskeletal conditions, matched the relative incidence in the LFS trailer.

The HSE also tested the reliability of the LFS trailer survey data by checking for internal consistency. For illnesses said to be caused by work the HSE checked whether: (a) the respondents knew of other similar cases in their workplace; (b) the employer was said to accept that the illness was work-related; and (c) whether the reported rates of the disease increased as a function of the number of years in the occupation. We judge that this is the most robust data yet to be made available for a study of this nature.

B. The Reported Incidence of Illness

Hodgson et al. 1993 reveal that nearly six per cent of the adult population reported suffering from an illness that was either caused by or made worse by their work. Around one third of this number were retirees or those unemployed for a year or more who reported being affected by the longer term consequences of work-related illness in the previous 12 months. Only half of all the illnesses reported by the currently employed, defined as those who worked within the year before the survey, resulted in lost days of work during the previous year. Thus in the United Kingdom a measure of occupational illness based solely on lost-days-of-work would miss two thirds of all reported illnesses.

The illness data are separated into cases caused by and cases made worse by work. The percentage of the adult population reporting illnesses solely caused by their work was 3.4 percent and these contained the most serious and long-term illnesses. In this analysis we rely exclusively on the cases "caused by" rather than cases "made worse" by work.

Table 1 shows the rates of illness caused by work for those males who are currently or were previously employed as manual workers when they contracted the illness. The table shows the type of illness contracted and classifies the jobs that caused the illness into broad occupational groups. The figures shown are rates per 10,000 workers and

Table 1
Occupationally Caused Illness for Male Manual Workers by Type of Illness and Occupation from the 1990 LFS Trailer Survey, as Rates per 10,000 Workers[a]

	Catering, Cleaning, Hair-Dressing	Farming, Fishing and Forestry	Processing (other)	Processing (Metal and Electrical)	Painting	Repetitive Assembly, Inspective	Construction	Coal Mining	Transport and Materials Moving	Other (Mainly Labourers)
Musculo-skeletal conditions (excluding RSI)[h]	205	269	196	255	259	166	368	1237	304	294
Musculo-skeletal Disorders of back	96	101	106	153	120	89	227	436	161	137
Musculo-skeletal Disorders of upper limbs and neck	20		37	34	34				53	26
Musculo-skeletal Disorders of lower limbs	7	78			52	13		301	40	
Skin disease	14	18				65				
Repetitive strain injury	7	17				50				
Stress/depression	48		80							
Trauma and poisoning (long term)	35			61	34	39	76	209	85	81
Deafness and ear conditions			57	130		53		801	40	
Upper limb RSI			15							
Eye conditions			29							
Lower respiratory disease		56		50	59	14	58	430		162
Pneumoconiosis								424		
Vibration white finger				13			35			
Asthma							14	143		
Other diseases[c]			19			40	38	100		
Percentage of total cases in this occupation	83	84.9x	74.6	78.3	74.1	90.1	80.2	96.6	75.8	84

Source: Calculated from Tables E-11 to E19, pages 80 to 84, in Hodgson et al. 1993.
a. Diseases listed account for 5 percent or more of total of self-reported work-related illness within the occupation.
b. Those shown in italics are a component of Musculo-skeletal (excluding RSI)
c. Other diseases refers to other circulatory diseases, neoplasms, endocrine and metabolic diseases, psychosis, nervous system disease, ulcer, other digestive disease, genito-urinary disease, and all unknown illnesses

are extracted from *Self-Reported Work-Related Illness* (Hodgson et al. 1993). The data is the same as that used in the present study except that, as described below, we employ a much more detailed occupational classification. The table records all those types of illnesses that were reported by at least 5 percent of the males in each of the manual occupations shown and it shows the percentage of the total number of illnesses experienced by the males in these occupations accounted for by the illnesses listed. The listed illnesses account for between 74.1 percent and 96.6 percent of the total illnesses experienced by workers in these manual occupations.

It will be clear from an inspection of the illnesses reported in the table that the data we use represent long-term or permanent illnesses rather than minor illnesses such as colds, upper-respiratory illnesses, and headaches. Table 1 shows that the broad occupational categories reporting the highest rates of work-related illnesses were coal mining and construction, and that muscular-skeletal illnesses were the most frequent illnesses in all occupations.

The calculations of the HSE at the broad occupational level reveal that men and women differed in both the incidence of occupational illness and in the types of illnesses they reported. Averaged over the entire population, men reported 70 per cent more work-related illnesses than women. According to the HSE report, this difference is "very largely because males are employed in the higher risk occupations . . ." (Hodgson et al., p. 6). The HSE also reports that ". . . *within* occupations the sex differences are much smaller [than those between occupations]; and the overall male to female rate ratio adjusted for occupation is 1.17" (Hodgson et al., p. 6, italics in the original).[7] It is interesting to note that men also have higher injury rates than women (Hersch 1998).

The study by Hersch (1998) excepted there has been little previous work on compensating differentials for women workers. This is likely because measurement problems are more pronounced for women workers than for men because they have spent long periods out of the labor force and have shorter durations of exposure within each occupation. For these reasons this paper focuses exclusively on males.

There is a marked difference in the types of illnesses reported by manual and nonmanual male workers and the incidence of illness is highest among manual men. In this first test for compensating differentials for a broad measure of occupational illness we therefore focus on male workers and ultimately on the group with the highest reported rates of the most severe illnesses, male manual workers.

C. Calculating Illness Risk

Conceptually, the illness incidence rate we would like to identify is the fraction of a cohort of workers who entered an occupation in the same year who now either have an occupational illness or died from an occupational illness. This incidence rate could then be converted into a probability by adjusting for years of exposure. The denominator for this rate would be the number of workers in the cohort. The numerator would include all workers in the cohort who are still working in spite of an occupational ill-

7. The HSE adjustments for occupation mix were made at the broad 20-occupation level rather than the 371 "unit" occupations.

ness, as well as the early and the regular retirees with an occupational illness,[8] together with the workers who died from the occupational illness. Our data fall short of providing enough information to meet this ideal. We have a snap shot of occupational illnesses among current workers, early retirees and regular retirees taken in 1990. Workers who died of an occupational illness before 1990 were unavailable for interview.

However, the formula we use provides for an indirect representation of the deceased workers by including the regular retirees who are still suffering from an occupational illness. Some of these could be thought to "stand in" for the workers who died from a long-term occupational disease before they would have retired. Of course, by including all regular retirees we may overestimate the incidence of some long-term irreversible illnesses. This is because when we count the prevalence of the illness, some retired workers with an illness could represent the same exposure to risk as is captured by current workers.[9] However, this is much less of a problem than first might appear. First because a high fraction of the respondents with work-related illnesses were early retirees. Moreover, given the severity of many of these illnesses, it is likely that a substantial number of workers died of an occupational illness before the date of the survey and there therefore need to be a substantial number of "stand-ins." Finally, the rapid declines in the number of workers in the most unhealthy manual occupations reduces the potential for double counting illness because the stocks of current workers in these occupations include relatively few workers who have had long exposures. Our risk measure would clearly be much less accurate if we based the measure on only the currently employed.

Though Hodgson et. al (1993) and Table 1 above report the incidence of illness by broad occupational categories more detailed data on the numbers of illnesses caused by work within each of 371 fine or "unit" occupational categories was provided for the researchers by the HSE. We used this data to construct a measure of illness risk proceeding in two stages. First we calculated a measure of the incidence of illness for each of the 371 occupations. We did this for two groups: first, the "currently and recently employed" and, second, those who had not worked in the past three years.

8. For diseases that are the product of long-term exposures or repetition of the same motions throughout a worker's working life, many young workers may appear to be free of any symptoms. Yet, if they are aware of early retirees in their occupation with work-related illnesses, the wage rate in these situations must be high enough to attract workers willing to face these risks. The stock of currently employed workers will not therefore provide an accurate base for determining the rate of illness caused by an occupation; it must include retirees and others who have long-term occupational illnesses.

9. A numerical example can illustrate the double counting. Suppose 1 percent of the workers in an occupation became hard-of-hearing after 20 years. If the occupation had a constant number of workers with half having 20 years experience, the true incidence of hearing loss among current workers would be 0.5 times 1 percent, or 0.5 percent. In a steady-state world adding any retirees who were hard-of-hearing via the term $y_j^t/N_j^{t-10}\, D_j$ in Equation 1 would overstate the true incidence among current workers. However, if most of the current workers had less than 20 years of experience their illness rate would understate the steady-state risk. If most of the current workers had more than 20 years of experience, their illness rate would overstate the true incidence for "steady-state" work force. It seems sensible to include a substantial fraction of the workers with long-term illnesses who have stopped working because we know that 40 percent of these are below retirement age and many of the others "stand-in" for workers who died of an occupational illness before reaching retirement age. Thus as a first approximation, our formula includes all of the early and regular retirees with an occupational illness.

For the first group this was done by dividing the reported count of illnesses among those who were either currently employed or who had worked in the last three years by the number currently or most recently employed in the occupation in question as reported in the LFS. The main LFS asks respondents either their current job or, for those not currently employed, their most recent job within the last three years. For the second group the calculation is less straightforward. This is because while the number of illnesses is known the number of people in the occupation that they described as the cause of their illness is likely to have fallen since they last worked.[10] In particular in the United Kingdom there has been a rapid shift away from manual occupations over the period studied here. We thus follow the procedure used by the HSE and utilize as the denominator for the second term the number of workers in an occupation ten years before the survey. This date is chosen because it was the date of the previous national census and thus these numbers were reported with great accuracy.

The incidence rates for the two groups were thus calculated using the same method as the HSE. However, the incidence rates do not measure the probability that a worker will contract an illness and thus do not measure the risk of working in different occupations. They do not do so because they do not adjust for exposure. Two occupations could have the same incidence of current and former workers reporting an illness but one occupation could be much riskier than the other if the average years of exposure in the two occupations differed.[11] If the workers in every occupation had the same average duration or years in the occupation, then our illness incidence measure would represent the probability of an occupational illness for this uniform exposure. However, there is no reason to believe that the average years spent in each occupation looking back from the LFS trailer survey of 1990 were the same and we therefore adjusted for years of exposure.

The LFS has no retrospective data on the duration in each occupation. To cover this gap we employed a further U.K. data set, the New Earnings Survey (NES). This data set is the annual employer-reported record of the earnings, hours, occupation, industry, sex, and age of a 1 percent sample of the employed U.K. labor force where the occupations are coded into 422 categories. It is a longitudinal sample that began in 1974. Workers drop out of the sample permanently when they retire, emigrate, or die but if they are unemployed during the survey week they drop out for that year and get picked up again in subsequent years. By 1990 data set had records on close to a half million workers. We estimated the average duration of employment for manual men in each occupation by 1990 under the assumption that a single missing year for an individual surrounded by years in the same occupation represented an unbroken string in that occupation. When there was more than one missing year we assumed that the worker had been in some other occupation. Under these assumptions we could calculate the number of years that each male in the sample employed in a manual occupation had been in the each occupation for up to 16 years prior to 1990. This enabled us to calculate an average duration for each occupation. The overall average duration was 8.8 years.

10. Some 60 percent of the respondents who had an occupational illness and had not worked in the last three years described themselves as retired. The balance are either unemployed or although not working too young to consider themselves retired.

11. We are grateful to John Ruser for emphasizing this point.

Our measure of the rate of illness in an occupation always refers to an illness present in the 12 months before the survey. Our measure is the sum of the fraction of current and recently employed workers (those employed within the last three years) who said that they had an illness caused by that occupation plus the fraction of the former workers (those who have not worked in the past three years) who have a long-term illness caused by that occupation where both denominators are adjusted for the mean duration in an occupation. Thus we have calculated an illness probability per worker year and we call this *Illmprob*. *Illmprob* can be thought of as approximating the midcareer risk of contracting an occupational illness. It does so under the following assumptions: that the gradient of risk per year of exposure is linear, that the annual flows of workers in or out of an occupation in between census years are equal, and that the number of workers who died from an occupational illness before 1990 is equal to the number of early retirees with long-term occupational illnesses.

The formula for *Illmprob* is:

$$(1) \quad r_j^t \equiv \frac{x_j^t}{N_j^t D_j^t} + \frac{y_j^t}{N_j^{t-10} D_j^t}$$

where:

r_j^t is the illness rate for the *j*th occupation at time t

x_j^t is the estimated number of currently and recently employed workers who said that they had an illness in the year before time t that was caused by occupation j

N_j^t is the estimated number of currently and recently employed workers in occupation j at time t

y_j^t is the estimated number of former workers who said that they had an illness in year before t that was caused by occupation j and who have not worked in the past three years

N_j^{t-10} is the estimated number of workers in occupation j at time t minus 10

D_j^t is the estimated duration of workers in occupation j at time t.

Thus the first term calculates the rate for the currently and recently employed and the second term the fraction of workers who permanently stop working and have long-term occupational diseases.

To check the appropriateness of the three assumptions underpinning this formula we did two things. First, to test the sensitivity of our results to the implicit assumption that the risk of contracting an illness increases linearly in years of exposure we ran a series of simulations under alternative assumptions. The procedures and results of this exercise are detailed in an Appendix available as supplementary material on the JHR website at http://www.ssc.wisc.edu/jhr/

Our simulations show that the *illmprob* sampling distributions are not sensitive to the gradient in risk, provided that the gradient is linear. The *illmprob* formula we constructed generated the same distribution of values over the range of gradients we simulated. Thus, in two occupations with the same midcareer risk of an occupational illness, the mean and standard deviations of the distributions of values for *illmprob* would be identical even if one of them had a flat gradient in risk and the other a gradient from zero risk in the initial year of employment to twice the mean risk in the year that is twice the mean duration of employment in the occupation.

The simulated distributions were always approximately normal. The implication of normality is that the observed number of illnesses and the calculated illness risk would be within two standards deviations of the true mid-career risk in about 95 percent of the occupations. One sampling distribution from the simulation, for carpenters and jointers, is shown in Figure 1 in the Appendix on the journal website.

Second, to check the appropriateness of the procedure in which the retirees "stand-in" for those who died of an occupational illness before they reached retirement age we analyzed death rates by cause of death and age. We were not able to simulate the impact on our results of having different fractions of ill workers die before they retired because we do not have any U.K. estimates of how many workers die from occupational illness before the age of 65. We do however know the number of deaths by occupation by age of the worker and by the broad medical cause of death. To check the reasonableness of the second part of our formula we compared the actual number of deaths in all occupations that could be due to occupational illness to the number of "stand-ins" generated by the formula.

The number of deaths from all causes over the period 1979 to 1990 can be obtained from (Drever 2002, Table 7). Over these years there were 3,746,554 illness deaths among U.K. workers in the age range 20 to 64. Our "stand-ins," workers who are suffering from a long-term occupational illness in the last 12 months but have not worked for three years and are under the age of 65, were estimated to number 300,000. The number of stand-ins thus represents 8 percent of all illness deaths in this period. We could not find a U.K. estimate of the proportion of all illness deaths as being caused by occupational illnesses, but one U.S. estimate places the ratio at 10 percent (Leigh 2000). Thus our formula yields an economy-wide percentage close to this estimate for the United States, the one estimate we have. For the individual occupations the imputed percentage illness deaths varies from zero to up to 40 percent of all illness deaths. It seems likely that occupations with high proportions of early retirees suffering long-term illnesses also have high proportions of occupation illness deaths. We conclude that the illness risk measure must take account of preretirement deaths from occupational illnesses. Limiting the risk measure to only current workers would imply that no one had died from an occupational illness and this would be highly implausible.

D. Other variables

The dependent variable in the model is the natural logarithm of the usual hourly wage rate where the overtime premium has been set at 1.33 as proposed by Hart and Ruffell (1993). This matched the most common specification in other studies of the LFS. To control for the effect of differences in hours worked on the wage rate one of the right-hand variables is a dummy for full-time work (30 hours or more) as distinct from part-time work status. There are also dummy variables for seven of the eight quarters used. Experience is proxied by subtracting the average age for attaining an educational qualification from the respondent's current age. There are also variables capturing trade union membership and the size of the firm where this latter is measured by three bands; under 25; 25 and under 50; or over 50. We also have measures of the risk of death and the risk of major injury that are discussed later in the paper. We use these additional risk measures to check if our illness risk measure are robust to their inclusion and thus to measures of other aspects of workplace risk. The definitions of all variables are given in Appendix 2. The descriptive statistics are in Appendix 3.

IV. Results

We estimate labor-market CDs for risk of illness by fitting standard human-capital wage equations to cross-section data. The coefficients on the risk variables are reduced-form estimates of the market equilibrium locus for CDs for risk of illness.

A. OLS Estimation

The results of OLS estimation of illness risk on wages and a range of controls for the whole of the male labor force, for union and nonunion and manual and nonmanual males, are reported in Table 2. The top half of the table is for male manual workers and the lower half for male nonmanual workers. Each regression starts with the same basic specification. Model 1 controls for education, private versus public sector employment, marital status, working full-time, years of work experience and its square, and the quarter in which the survey was taken. Model 2 then adds 17 dummy variables for industries and two dummy variables for firm size to the variables included in Model 1.

Because all individuals in the same occupation are assigned the same level of illness risk the residuals of the OLS regression may be correlated for respondents in the same occupation. This will reduce the standard errors on the illness risk variable. (Moulton 1990) The standard errors reported below are estimated by a robust procedure in STATA Version 8.0 which accounts for the within-group correlation. The *t* scores with this procedure are about 15 percent lower than the conventional *t* scores.

Dorman and Hagstrom (1998) have demonstrated that estimates of CDs for death and injury risk are highly sensitive to the inclusion of dummy variables for firm size and industry. Their results lead them to conclude that there are no compensating wage differentials for risks of death or injury, at least for the United States. However this conclusion is unwarranted for the following two reasons. First the industry dummy variables capture some of the differences in risk together with a host of other industry-specific factors. Viscusi (2004) has argued that there is a substantial variation in risk within an occupation across industries. To check whether firm size and industry are related to illness risk we regressed our male illness probability variable, *Illmprob,* on the industry and firm size dummies. Seven of the dummy variables were significant at a 0.05 level and the coefficient on the smallest firm size band, which was the most significant variable, had a *t*-ratio of 4.7. Second, the firm size and industry dummies also may be capturing unobserved worker ability that is in turn related to worker's choices about risk. Safety is typically thought to be a normal good and therefore we would expect high-ability workers, other things equal, to choose safer jobs. A recent paper using a large French panel data set of workers who switched industries or switched the size of their firms found that controlling for differences in worker ability reduced the firm size or industry wage effects by an order of magnitude compared to cross-sectional estimates (Abowd, Kramarz, and Margolis 1999). Thus the variations in illness risk that are embedded in the firm size and industry dummies together with the effects of unobserved ability lead us to conclude that the coefficients in Models 1 and 2 probably bound the true effect of illness risk on earnings.

The results in the lower half of Table 2 provide no support for CDs for illness risk for male nonmanual workers. It is noteworthy that Hersch (1998, p. 605) also found no evidence of CDs for nonmanual workers in her study using a combined measure of illness and injury. The upper half of Table 2 reports the coefficient for all manual workers in Column 1 and 2. Model 1 Column 1 reveals a significant coefficient on illness risk and a corresponding *t* score of 2.63. However the results for Model 2, Column 2 show this level of significance is not robust to the inclusion of the industry and firm size dummy variables. The reason appears to be that union and nonunion workers have completely different risk compensation environments. Columns 3 and 4 reveal that while the coefficients for the union workers are not significant Columns 5 and 6 show that those for the nonunion workers are positive, highly significant and are fairly robust to the inclusion of the industry and firm size dummy variables.

Table 2
Coefficients on Illness Risk for Male Workers in the United Kingdom

	Dependent Variable: LNWAGE					
	Manual Workers					
	All Employers		Union Sector		Nonunion Sector	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient	**1.58**	0.74	–0.10	–0.59	**2.68**	**1.72**
on illness risk	**(2.63)**	(1.24)	(0.13)	(0.74)	**(3.05)**	**(2.01)**
Adjusted R^2	0.29	0.24	0.11	0.16	0.19	0.25
N	5,191	5,191	2,138	2,138	3,053	3,053
	Nonmanual Workers					
	All Employers		Union Sector		Nonunion Sector	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient	–0.79	–0.35	–0.31	0.59	–0.91	–0.63
On illness risk	(1.27)	(0.63)	(0.38)	(0.80)	(1.15)	(0.91)
Adjusted R^2	0.23	0.31	0.18	0.23	0.25	0.32
N	6,140	6,140	2,050	2,050	4,090	4,090

Note: Absolute values of the *t* scores in parentheses. Significant coefficients in bold type
Controls made up of:
Model 1
Indicator variables for educational attainment (five categories), employment in the private sector, marital status, full-time status, and quarter in which the survey was taken (eight categories), years of work experience, years of work experience squared.
Model 2
As above together with indicator variables for establishment size (three categories) and industry (17 categories).

This difference between union and nonunion labor compensation mechanisms parallels that found in studies of CDs for fatal risk. Several U.K. studies have found a higher CD for the risk of death for nonunion workers where fatal risk is precisely measured using occupational risk data. Thus, in the United Kingdom, Marin and Psacharopoulus (1982) and Sandy and Elliott (1996) found higher CDs for nonunion workers while in Canada (Meng 1989) found similar results. There are however some studies using occupational risk data which find the opposite results for fatal risk (see Thaler and Rosen 1975; Gegax, Gerking, and Schulze 1991; Cousineau, Lacoix, and Girard 1992). Whether the CDs are higher in the union or the nonunion sector is however an empirical issue for there are plausible arguments for either outcome. Union and nonunion workplaces may have entirely different processes to mediate workplace risks.[12] Unions might police risks directly or use a seniority mechanism to allocate risks among workers. Alternatively, unions might negotiate greater risk premia if they have better information about risks. The above results for illness risk for the United Kingdom are in line with those for fatal risk, both find a higher nonunion risk premium for nonunion workers.

B. The Endogeneity of Risk

A common adjustment to estimates of CDs for labor-market risk is to use instrumental variables to correct for the endogeneity of risk. Some examples are Garen (1988); Siebert and Wei (1994); Sandy and Elliott (1996). Endogeneity-of-risk corrections can have a substantial effect on the implicit value of saving a life, often doubling them. The argument proffered is that risk is endogenous first because workers who are the most productive in risky environments—those workers who can keep a cool head and can nimbly avoid dangers—would self-select into the riskier jobs and second because workers with high unobserved ability would trade some of their potential earnings for more safety, that is that safety is a normal good. It is of course clear that workers who heavily discount the future are more likely to accept jobs with long-term health risks. However, the usual factors that are associated with a high discount of future benefits, such as low education, a criminal record, or the use of illegal drugs, also will have a direct effect on wages. Thus they cannot be used to identify self-selection into jobs with a high risk of illness. The primary focus of this paper is on that group of workers with the highest reported rates of the most severe illnesses, male manual workers. We attempted adjusting for the endogeneity of workplace risk for the group among these with the highest illness-risk premium, the nonunion manual workers; however, none of our candidate instruments were usable.

A very recent study for the United States (Viscusi and Hersch 2001) analyzes this issue of the effects of worker heterogeneity in risk preferences in some detail. They distinguish between smokers and nonsmokers and reveal that the former group is both less averse to accepting jobs with high levels of injury risk and more prone to accidents both at work and away from work. They find that smokers and nonsmokers face different market equilibrium offer curves for injury risk with a resulting lower wage

12. Dillingham and Smith (1984) offer a comprehensive discussion of why union and nonunion risk premia might differ.

for smokers, who are otherwise observationally identical to nonsmokers, at every level of risk. Smokers value avoiding an injury that caused a least one day lost from work at $14,000 while nonsmokers value it at $31,000. This is an important result. It confirms the need to adjust for worker heterogeneity while also suggesting that smoking is an excellent candidate as an instrument for preferences over workplace risk. Of greatest relevance for our study of a broad measure of long term illness risk is that smoking is likely to have a profound effect on an individual's overall illness risk and therefore their willingness to accept workplace illness risk. The results of Viscusi and Hersch's analysis suggest that the CDs for smokers and nonsmokers ought to be estimated separately. Unfortunately, no sufficiently large U.K. survey has both labor-market data and smoking behavior.

We therefore ran regressions of illness risk on some of the instruments previously used for willingness to accept a risk of death: the number of children living at home, wife's earnings, home ownership, and a dummy variable for the presence of any non-labor income. Only one of them had a significant effect on the level of illness risk, home ownership. This is because in the early 1990s a large proportion of the U.K. housing rental market was still provided by the state in what is called "council housing" and the division between home ownership and renting represents a much greater divide in lifetime wealth than it does in the United States. With the home ownership variable as an instrument the illness risk variable clearly passed a simple test for identifying an endogenous variable. When the residuals of a regression of illness risk on home ownership were added to the regressions, which were labeled Model 1 and Model 2 for manual nonunion workers in Table 2 the t-scores on these residuals were both above seven. However, Bound, Jaeger, and Baker (1995) argue that the use of weak instruments causes more harm than no adjustment for endogeneity because weak instruments are extremely noisy and biased toward the OLS estimates. Staiger and Stock (1997) argue that ten is an appropriate value for the F-statistic for a test of the quality of the instrument in the first-stage regression. In another paper on using weak instruments, Stock, Wright, and Yogo (2002) conclude that F statistics above nine are acceptable for a single instrument. The highest F statistic for the home ownership variable in any of the specifications for nonmanual workers in Table 2 was 1.2. With either rule-of-thumb our best candidate for an instrument is still too weak. Thus, although our housing tenure variable is statistically significant in an illness-risk regression, using it as an instrument would be inappropriate.

If the coefficients in Models 1 and 2 in Table 2 bound the true estimate of the CD for illness risk, then the two estimates for the nonunion manual workers can be used to calculate the implicit value of avoiding an occupational illness. The resulting figures are approximately 15,000 to 28,000 pounds in 1995 which are approximately 1.5 to 2.7 times mean annual earnings for nonunion male manual workers in 1993. The U.K. estimates of the implicit value of saving a life are many multiples of this but one would expect a large group of workers to be willing to pay much more to reduce their risk of death by one expected death than to reduce their risk of illness by one expected illness. The published U.K. implicit value of life estimates based on labor-market data are by Marin and Psacharopoulus (1982); Siebert and Wei (1994); Sandy and Elliott (1996). A more interesting comparison to the implicit value of avoiding one workplace illness is the value for avoiding one workplace injury. Viscusi (1993, p 1932–33) produces a table of such estimates. Two of the estimates are based on willingness-to-pay generated from labor-market data that also include annual earnings. These implicit values of avoiding an injury range vary from 0.8 to 3.7 times annual earnings. The implicit value of avoiding one workplace illness therefore is within the range of the estimates of the implicit value of avoiding a workplace injury.

An additional issue is whether these results are stable if either risk of death or risk of injury are added to the wage regressions. We have a risk of death variable based on five years of coroners' records of deaths that were determined to have occurred at work along with the coroner's description of the worker's occupation. The details of this variable are described in an earlier paper (Sandy and Elliott 1996). We also have a measure of the risk of a major injury. The HSE has two injury series, one for injuries that cause three or more consecutive days of absence from work and the other, for major injuries, which records any instance of an injury from a list drawn up by the HSE. There are substantial penalties on employers who fail to report one of these major injuries. In contrast, the three-lost-days series is highly problematic because of inconsistencies in treating absences that carry over a weekend, as well as the reporting problems we described above. Thus we do not employ the lost workdays series only the major injury data.

The simple correlation between major injury risk and illness risk for nonunion manual workers is 0.045. The correlation between the risk of death and illness risk for nonunion manual workers is 0.086. Adding these two variables to OLS version of Model I raises the coefficient on illness risk from 2.68 to 2.73 while lowering the *t*-score from 3.18 to 3.02. For Model II the coefficient on illness risk goes from 1.72 to 1.86. The respective *t*-scores are 2.01 and 2.12. Thus we conclude that the results for the illness risk variable are robust to the inclusion of other types of occupational risk. The major injury risk variable had a negative sign and was not significant. We attribute this result to the lack of occupational detail. It is coded at the two-digit level (71 occupations among the workers in our data set, of which 38 occupations include male manual workers), while the fatal accident risk (254 occupations, 150 with male manual workers) and the illness risk (210 occupations, 189 with male manual workers) are at the three digit level. Dividing manual workers into just 38 occupations probably provides too little detail on variations in injury risk to pick up a compensating differential.

The one published study with an estimate of compensation for injury risk for the United Kingdom (Siebert and Wei 1994) utilized a risk measure for one-digit industry crossed with one digit occupation cells. Their injury risk measure was both not significant and negative in most of their specifications. Our fatal risk injury variable had a positive sign in regressions that included the other two risk measures but it was not significant in the Model II specification with the industry and firm size variables. It was marginally (at 10 percent) significant in the Model I specification. This result was stable under different specifications, for example, running the fatal-injury risk variable without the other risk measures. The same or a similar fatal injury risk variable had much higher significance levels in papers using older data sets (Sandy and Elliott 1996; Marin and Psacharopoulus 1982). In this paper the labor-market data are from 1993 and 1994 while the death risk data are from 1979 through 1983. There was also a change between the fatal injury risk data and in the occupational classification that sharply reduced the number of manual occupations. Cross-walks between occupation classifications inevitably add noise to a risk measure. The most recent HSE fatal injury risk data are at the two-digit level so they are even worse. Although our *accrisk* variable is getting to be "long in the tooth," it is still the best measure available.

We also calculated the annual risk premium at the mean level of fatal injury risk and the mean level of illness risk for nonunion manual workers. The annual payment for such workers at the mean level of illness risk relative to a worker facing zero risk was 374 pounds or 2.39 percent of earnings. The annual payment for fatal accident risk at the mean level for this risk was 2.26 pounds or 0.01 percent of annual earnings per worker. This comparison does not prove that the economy-wide ex ante payments for illness risk are higher than for death risk because the average male manual worker in the United Kingdom faces almost imperceptibly low levels of at-work fatality risk. However, this comparison suggests that for the typical male manual worker compensation for illness risk is much more important than compensation for fatal accident risk.

V. Conclusion

The present study provides the first estimates of compensating differentials for a broad measure of work-related long-term illness. The other substantive studies of this topic established that there was a compensating differential for fatal cancer risk and compensable fatal illness risks. Our analysis reveals significant compensating differentials for male manual workers for a broad measure of work-related long-term illness. Using data on self-reported illnesses, the analysis finds significant CDs for male manual workers but none for male nonmanual workers. The results are robust to the addition of variables for the risk of accidental at-work deaths and for major injuries at work. Long-term work-related illness is more pervasive than either work-related injuries or work-related deaths. As a result long-term illness is likely to have both a more substantial effect on economy-wide productivity and a substantial impact on wages.

This study complements the existing literature on CDs for occupational injuries and for work-related deaths. In previous research both these risks have most frequently been measured in a broad way. One criticism of all such broad measures of risk is that workers may place different valuations on different types of illness, injuries, and deaths. They may dislike some types of illness, injuries, or even deaths more than others. Clearly it is important to distinguish the differing magnitudes of compensation for different types of each of these risks but it is equally important to obtain estimates of willingness-to-pay to avoid broadly measured risks. Public policies, such as workers compensation schemes or systems of inspections and fines, are generally aimed at reducing all work-related deaths or accidents or serious illnesses. For these policies to be effective they need to work in harmony with the labor-market mechanisms that induce employers to reduce workplace risk. Thus it is important to have estimates of the CDs for a broad measure of illness risk.

The results of the analysis in this paper have important implications for public policy. According to Viscusi, the widely used contingent valuations based on hypothetical questions about willingness-to-pay to avoid an illness need to be corroborated by labor-market data. (Viscusi 1992, p. 74). Reducing workplace illnesses is now a matter of high priority in both the United States and the United Kingdom. If there are substantial CDs for workplace illnesses, then public policy might be most effective if it seeks to complement the safety incentives already present in the labor market rather than seeking other remedies. They might appropriately provide more information on illness rates by occupation or provide training that could increase worker's mobility out of risky occupations. An accurate description of how the labor market treats illness risk should be the foundation of any public policy. As the first paper to measure the CDs for long-term occupational illness this paper contributes to this policy debate.

Appendix 1

The LFS Accident and Ill-Health Trailer Questionnaire

The LFS "trailer" survey is published as Appendix A1 in Hodgson et al. (1993). It very clearly distinguishes occupational illness from occupational injuries and accidents. This is of fundamental importance to this study and is a unique advantage of this survey; no other data set does this. The LFS *Accident and Ill-Health Trailer Questionnaire* devotes the first 35 questions to accidents and injuries. To make the distinction between illness and accidents and injuries very clear, the third question among these 35 provides a list of the types of injuries that are the focus of this part of the questionnaire. After the questions about accidents and injuries, respondents were then asked at Question 37, "In the past 12 months [apart from the accident you have just told me about], have you suffered from any illness, disability or other physical problem that was caused by or made worse by your work? Please include any work you have done in the past." This question is then followed by a question asking if the respondent had more than one illness and a further set of questions, Questions 39 to 46, asking about the nature of the most serious illness, the job which caused the illness, and whether it was caused or made worse by the job. Respondents reporting an illness also were asked to record their doctor's diagnosis or if they could not recall it to describe the illness in their own words. Using this information each illness was coded by HSE staff familiar with the International Classification of Diseases together with some additional categories used by the HSE. The responses also were coded into 371 different occupational categories and the illness data were separated into cases caused by and cases made worse by work.

Appendix 2

Definitions of Variables

ACCRISK probability of an at-work death

ALEVEL a dummy variable for other higher education qualification below the degree level, RSA higher diploma, A-level or equivalent, RSA advanced diploma, OND/ONC, BTEC etc. National, City or Guilds advanced craft, Scottish Sixth year certificate or equivalent, SCE higher or equivalent

BA	a dummy variable for either a first university degree, some other university degree, a diploma in higher education, HND-HNC, BTEC etc. Higher, Teaching-further education, teaching-secondary, teaching primary, teaching-level not stated, or a nursing degree
EMPSMALL	an indicator variable indicating the firm has fewer than 25 employees
EMPMED	an indicator variable indicating the firm has between 25 and 50 employees
EXPER	a proxy for years of work experience. It equals age –16, or (if postgrad = 1) equals age −23, or (if BA = 1) equals age −21, or (if Alevel = 1) equals = age −18, or (if trade = 1 exper) equals age − 17
EXPER2	the square of *EXPER*
FULLTIME	an indicator variable for working 30 or more hours per week
PRIVATE	an indicator variable for working in the private sector
ILLMPROB	probability of an illness caused by work for a male worker
IND1 to *IND16*	indicator variables for major industries
Q2.94 to *Q4.95*	a set of indicator variables for each quarter running from the second quarter of 1994 to the fourth quarter of 1995
LNWAGE	natural logarithm of the usual hourly wage rate
MARRIED	an indicator variable that equals one if married
OLEVEL	an indicator variable for O-level or equivalent
POSTGRAD	an indicator variable for postgraduate degree
PRIVATE	an indicator variable indicating that the employment is in the private sector
TRADE	an indicator variable for AS level or equivalent, trade appenticeship, RSA diploma, City and Guilds craft, BTEC first or general diploma

Appendix 3

Means of the Data

	All Manual	Union	Nonunion	All Nonmanual	Union	Nonunion
ACCRISK	5.40E-05	5.18E-05	5.57E-05	1.46E-05	1.61E-05	1.38E-05
ALEVEL	0.141	0.147	0.137	0.181	0.159	0.192
BA	0.063	0.057	0.068	0.431	0.450	0.422
EMPMED	0.120	0.087	0.143	0.119	0.122	0.117
EMPSMALL	0.256	0.136	0.337	0.189	0.117	0.227
EXPER	21.472	23.969	19.776	18.404	20.609	17.272
EXPER2	602.404	696.080	538.738	452.267	519.241	417.877
FULLTIME	0.978	0.993	0.968	0.968	0.978	0.962
ILLMPROB	1.32E-02	1.33E-02	1.31E-02	8.75E-03	8.86E-03	8.69E-03
INI1	0.020	0.010	0.027	0.004	0.002	0.005
IND2	0.000	0.000	0.000	0.000	0.000	0.000
IND3	0.010	0.010	0.009	0.005	0.002	0.006
IND4	0.409	0.450	0.381	0.225	0.138	0.269
IND5	0.021	0.033	0.012	0.016	0.028	0.001
IND6	0.086	0.071	0.097	0.040	0.035	0.043
IND7	0.109	0.050	0.149	0.107	0.028	0.147
IND8	0.024	0.004	0.037	0.008	0.003	0.010
IND9	0.162	0.225	0.119	0.059	0.069	0.053
IND10	0.004	0.001	0.005	0.083	0.074	0.087

Appendix 3 (*continued*)

Means of the Data

	All Manual	Union	Nonunion	All Nonmanual	Union	Nonunion
INI11	0.044	0.024	0.058	0.125	0.060	0.158
INI12	0.030	0.028	0.031	0.133	0.233	0.081
IND13	0.018	0.020	0.016	0.122	0.237	0.064
IND14	0.026	0.033	0.022	0.045	0.065	0.035
IND15	0.036	0.039	0.033	0.028	0.029	0.028
IND16	0.001	0.000	0.001	0.000	0.000	0.000
LNWAGE	1.704	1.826	1.621	2.101	2.188	2.057
MARRIED	0.594	0.671	0.542	0.611	0.703	0.565
OLEVEL	0.137	0.106	0.158	0.137	0.116	0.148
POSTGRAD	0.002	0.000	0.004	0.089	0.115	0.075
PRIVATE	0.837	0.754	0.894	0.679	0.380	0.802
Q1.95	0.125	0.129	0.123	0.123	0.142	0.113
Q2.94	0.123	0.144	0.109	0.123	0.152	0.107
Q2.95	0.123	0.127	0.119	0.123	0.120	0.125
Q3.94	0.118	0.132	0.109	0.117	0.130	0.110
Q3.95	0.123	0.151	0.104	0.121	0.150	0.105
Q4.94	0.132	0.147	0.122	0.125	0.123	0.127
Q4.95	0.118	0.130	0.111	0.128	0.130	0.127
TRADE	0.292	0.307	0.282	0.008	0.085	0.082
N	5,598	2,306	3,292	6,250	2,217	4,133

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