Using twins to resolve the twin problem of having a bad job and a low wage*

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Abstract

We use twin data linked to register-based information on earnings to examine the long-standing puzzle of non-existent compensating wage differentials. The use of twin data with a panel dimension allows us to alleviate the impact of otherwise unobserved productivity differences that have been the prominent reason for estimation bias in prior studies. Using a twin design, we find some evidence for positive compensation of adverse working conditions in the labor market.

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1. Introduction

The idea that adverse working conditions should be compensated in the form of higher pay is an old idea that dates back to Adam Smith's Wealth of Nations. Although the notion is intuitively appealing, its verification is difficult. Empirical research has investigated this issue with conflicting results (see Rosen 1986, for a survey).

The main approach has been to estimate hedonic wage functions where earnings are explained by personal characteristics and some indicators of working conditions. In a competitive labor market with differing tastes and technologies, workers with a strong distaste for a negative attribute of work are matched with firms that have a low cost of avoiding it, and obtain a low wage. Correspondingly, workers with a weaker distaste for disamenity are matched with firms with a high cost of producing lower levels of it, and obtain higher pay. This forms the theoretical basis for the estimation of compensating wage differentials using wage equations augmented with job characteristics.

A variety of different characteristics of work have been investigated, including risk of death, physical and mental harm, different hazards, shift work, part-time work, night work, promotion prospects, and conflicts. Analysis is sometimes conducted at an aggregate level (e.g., using industry or occupation as the observation unit), but mostly individuals are used as the observation units. Even in the latter case, measurements of working conditions may be taken from a more aggregated level (firm or occupation). In the most appealing settings, the analyses use information on individual working conditions that workers face at workplaces (e.g., Duncan and Holmlund, 1983).

The clearest result is that occupations with a high risk of death are better compensated, conditional on individual characteristics (e.g., Dorman and Hagstrom, 1998). However, in many studies, no compensating differential has been found or even negative returns have appeared. The paradox has inspired studies that have attempted to explain the unexpected findings. Hwang *et al.* (1992) theoretically showed how unobservable worker productivity differences cause bias in the estimates. Persons who have more human capital (and hence higher productivity) choose jobs with both a higher wage and better working conditions, although with fixed human capital there is a tradeoff between wage and job quality. Hwang *et al.* (1998) demonstrated that an additional bias is caused by firms' unobserved productivity differences.

If unobservable individual characteristics are correlated with job characteristics, panel data can be used to eliminate time-invariant unobservables (e.g., Brown, 1980). Another alternative is to account for the endogeneity of the working conditions using instrumental variables or selection correction (e.g., Garen, 1988). However, these solutions are not always successful. It is difficult to find sufficiently strong and valid instruments that are significantly correlated with working conditions but are otherwise unrelated to earnings. Another issue is the measurement error in the working condition variables. The error may arise from the use of aggregate occupational measures or from the use of self-reported information. Classical measurement error tends to attenuate the estimates used for compensating differentials. The use of panel data with differencing has been suggested as a solution to the problem in cases of measurement error in self-reported (dis)amenities that are correlated with individuals over time (Duncan and Holmlund, 1983).

Other reasons for not finding compensating differentials (even with panel data) include wage bargaining institutions (e.g., Daniel and Sofer, 1998) or search and other frictions in the labor market (e.g., Manning, 2003). Both explanations imply that the actual labor markets differ from the competitive markets that underlie the standard theory.

Given the crucial role of unobservables in the failure to detect compensating wage differentials, we propose a novel approach to examine compensating differentials using data on twins with a panel dimension. The approach promises to complement the other empirical strategies used in the literature. The idea is simple. When wage differences within pairs of identical twins are explained by within-pair differences in working conditions, the unobservable characteristics similar to both twins are eliminated. The twin-difference effects are identified for twins who work under different conditions. Using data on identical twins accounts for both genetic and other family factors, such as parental investments that may predispose individuals to both poor working conditions and low earnings.[1] The approach is an alternative to the use of individual-level panel data that purges unobservables that are time-invariant for the individual. Both types of unobservables can be accounted for by using the panel dimension of our twin data.

The paper has two interlinked goals. The first goal is methodological, an evaluation of whether twin data are useful for estimating compensating differentials compared to individual-level panel data. We are unaware of any previous attempts to use twin data to estimate compensating differentials. Second, we attempt to obtain as accurate an estimate of compensation for poor working conditions as possible with the twin data available to us. Few previous estimates of compensating differentials have been reported for Finland, which has

[1] In essence, identical twins share 100% of their genes and fraternal twins share 50% of genes so that twin differencing removes the genetic effects for identical but not for fraternal twins. However, family background is eliminated for both types of twins.

collective wage bargaining similar to many other European countries. Therefore, exploring whether workers are compensated for poor working conditions in such an institutional environment is interesting.[2]

We use a large nationally representative sample of Finnish twins born before 1958 who were surveyed in 1975, 1981, and 1990. The surveys include work-related questions that we use to measure working conditions: assessment of the monotony of work, the physical demands of work, and opportunities to influence the work content. The data can be linked to register data on the same individuals by using personal ID codes. Thus, earnings from tax registers in 1975 and 1990 can be explained by the working conditions reported in the surveys, which allows us to control for both time-invariant person-specific unobservables and family-specific possibly time-varying effects. Our results support the idea that pooled OLS estimations and even fixed effect estimations may lead to an underestimation of the wage effects of adverse working conditions. Using twin differences, we find some evidence for compensating differentials when the employee has no influence on work content and/or when work is physically demanding.

[2] Finland has a fairly centralized wage bargaining system (see Vartiainen 1998, for a summary of labor market institutions in Finland). Union density is 70%. Collective negotiations have traditionally taken place either at the national level or at the industry level. Collective labor agreements already contain some pecuniary compensation for adverse working conditions, at least at the industry level, but the apparent heterogeneity of individual workplaces makes it difficult for collective labor agreements to take into account all relevant working conditions. Binding collective agreements establish only the wage floors, which individual firms may exceed on the basis of local negotiations. This provides room for compensating wage differences at the firm level, and actual wages are typically much higher than the task-specific minimum wages set in collective agreements. Collective bargaining also leads to wage compression that limits the individual-level variation in wages. Hence, employees' subjective valuations of their working conditions can differ from the valuations stipulated in collective agreements by the central organizations of employees and employers, or at the firm level.

2. Econometric Issues in Estimating Compensating Wage Differentials

To evaluate the potential usefulness of twin data, we first discuss what factors cause biases for the estimates and how they can be accounted for in different types of data sets. We combine insights from Duncan and Holmlund (1983) on measurement errors of working conditions and from Isacsson (2007) on the use of twin data and panel data to remove unobservables. For ease of exposition, we assume that there is only one job characteristic and that there are no control variables (or that their effect has been removed with a prior regression).

2.1. Accounting for Unobserved Effects Using Twin Data and Panel Data

It is assumed that

$$y_{ijt} = \beta_0 + \beta_1 J_{ijt} + u_{ijt}, \qquad (1)$$

where y_{ijt} is log earnings, J_{ijt} is a job characteristic, i=1, 2 denotes individuals within twin pairs, j=1, …, N denotes twin pairs and t=1, …, T refers to years. The characteristic J is assumed to be a positive attribute. Thus, its return β_1 is expected to be negative if compensating differentials exist.

The error term has the following structure:

$$u_{ijt} = \gamma_t A_{ij} + e_{ijt}, \qquad (2)$$

where A_{ij} is an individual-specific, time-invariant unobserved characteristic ("ability"). γ_t is the return to the unobserved ability, which we allow to vary over time (e.g., due to increasing skill bias). Isacsson (2007) has a similar setting where he allows the ability term to change over time but assumes that the change is the same for both twins. Our formulation is slightly

different but essentially similar and highlights that the return to ability may change over time even if the ability itself remains constant.

The cross-sectional OLS estimate is biased if A_{ij} correlates with J_{ijt}. For example, the negative compensating differential for a job amenity is biased towards zero if the more productive (able) persons choose more amenable jobs. The estimate can even become positive, reflecting the unobserved productivity (human capital) bias in estimating compensating differentials (Brown, 1980; Duncan and Holmlund, 1983; Hwang *et al.,* 1992).

The individual-level panel data fixed effects (time difference) estimator is consistent for β_1 only if the ability-related wage growth components are not correlated with the changes in job characteristics. However, if the higher wage growth of more able persons ($(\gamma_t - \gamma_s)A_{ij} > 0$) leads them to choose better job conditions ($(J_{ijt} - J_{ijs}) > 0$), the compensating differential estimate β_1^{FD} is biased towards zero (in the formulas, the subscript *s* refers to some previous year). The standard panel data assumption is that the return to ability is constant over time ($(\gamma_t - \gamma_s) = 0$), which eliminates the bias. However, this assumption may not be valid, due to higher wage growth for more able persons. If increasing skill bias or some other ability-related wage growth, such as stronger learning-by-doing effects, has important wage effects that individuals 'spend' on better working conditions, the individual-level FE estimates can become wrong-signed similar to the OLS estimates.

A fixed effects estimator based on twin data (twin differences) is given by

$$y_{1jt} - y_{2jt} = \beta_1(J_{1jt} - J_{2jt}) + \gamma_t(A_{1j} - A_{2j}) + (e_{1jt} - e_{2jt}) \qquad (3)$$

It is a standard assumption that the unobserved ability is equal for identical (MZ) twins (A_{1j} = A_{2j}) because they share exactly the same genes, in most cases are brought up in the same family environment and usually also share the same peer group in their youth. The validity of this assumption is discussed below, but, assuming it holds, the approach yields consistent estimates for compensating differential β_1 for MZ twins because the ability term is eliminated due to twin-differencing. However, the compensating differential estimates for DZ twins and other siblings are biased towards zero if sibling-differences in ability correlate positively with sibling-differences in job amenities.

A Difference-in-Differences (time difference – twin difference) estimator, henceforth DiD, is obtained from

$$\begin{aligned}(y_{1jt} - y_{1js}) - (y_{2jt} - y_{2js}) = \beta_1[(J_{1jt} - J_{1js}) - (J_{2jt} - J_{2js})] + (\gamma_t - \gamma_s)(A_{1j} - A_{2j}) \\ + [(e_{1jt} - e_{1js}) - (e_{2jt} - e_{2js})]\end{aligned} \tag{4}$$

The DiD estimator removes the ability effects if the ability-related wage growth components are fixed within the twin pairs (i.e., pair-specific; $A_{1j} = A_{2j}$ for MZ twins). Otherwise, for DZ twins a downward bias remains even in the DiD estimation if the twin with higher wage growth experiences larger improvement in job amenity over time. DiD also removes any unobservable, time-invariant differences between the twins that would not be purged in standard twin differencing.

The specification for ability effects clarifies the central assumption for the consistency of DiD. The wage growth component $(\gamma_t - \gamma_s)A_{ij}$ must be pair-specific as opposed to individual-specific for the DiD to be consistent. The formulation allows for wage growth to be different for two persons in general; however, when the equal ability assumption for the MZ twins is

made, the wage growth is the same for the twins. Thus, the wage growth term is eliminated in the DiD estimation for MZ twins, thereby avoiding ability bias. The DiD for MZ twins and the twin-differences for MZs are both unbiased with the same assumption of equal ability within MZ twin pairs, even with ability-related wage growth effects over time.[3] However, the individual-level FE estimates are biased with ability-related wage growth as shown above.

2.2. Measurement Error Bias with Correlated Errors and Job Characteristics

Assuming that the random measurement error in job characteristics is not correlated with true job characteristic *J* or with ability *A*, the cross-sectional OLS is (further) biased towards zero due to the well-known attenuation bias. Bias towards zero increases when the noise-to-signal ratio increases. It is often assumed that differencing increases the bias to the extent that the noise-to-signal ratio is higher compared to cross-sectional OLS.

As noted by Duncan and Holmlund (1983), if individuals have a tendency to persistently over- or underestimate their self-reported working conditions in surveys, job characteristic measures are strongly correlated over time. Time-differencing this time-invariant measurement error component reduces measurement error bias.[4]

However, true job characteristics should also be correlated over time. With unchanged preferences for working conditions and unchanged market price for job amenities, an

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[3] Generally, a DiD estimator is unbiased if $(A_{1jt} - A_{1js}) = (A_{2jt} - A_{2js})$ (i.e., the wage growth due to ability is the same for both twins even if abilities are not the same for the twins). In this case, the standard twin-difference estimator would be biased.

[4] Measurement error bias does not disappear even in the presence of perfect autocorrelation as claimed by Duncan and Holmlund (1983, p. 371). They state that "… in the special case with 'perfect' autocorrelation (ρ=1), the OLS estimate is consistent." However, we assume a correlated measurement error process $\varepsilon_t = \rho\varepsilon_{t-1} + v_t$, where $\mathrm{var}(\Delta\varepsilon_t) = \sigma_v^2$ when ρ=1, so the noise-to-signal ratio does not disappear altogether.

individual's optimum wage-amenity combination should be stable over time and captured by high autocorrelation.

Therefore, it is intuitive (and can be formally shown[5]) that high correlations in the signal (true *J*) and the measurement error have opposing effects on the fixed effects estimation bias. Persistence in the measurement error improves but persistence in the signal worsens the consistency of the fixed effects estimate for compensating differentials. The latter effect arises because the variation in the differenced explanatory variable is reduced by persistence in the working conditions over time. However, our use of long differences may lower the autocorrelation in *J*.

2.3. Comparison of Twin-Differences vs. Time-Differences in FE Estimators

The results for the panel FE (time-difference) estimator above extend analogously to the twin-difference estimator where the difference is taken between the two twins in the same pair. The correlations are now between the true job characteristics or the measurement errors of the two twins within the same pair.

Therefore, we make the following conclusions regarding the relative biases in the twin-difference and panel-difference FE estimators:

1) If the within twin-pair correlation in true *J* is smaller than the panel correlation within individuals over time, then the bias from the Twin FE is *smaller* than that from the Time-Difference FE if all other factors are equal. This situation should be the case,

[5] See the working paper version of this paper for the formal derivation of the result (Böckerman *et al.,* 2014).

because it is likely that preferences/choices for job characteristics vary more between twins than for one individual over time.[6]

2) If the twin correlation in measurement error is also smaller than the panel correlation, then the bias from the Twin FE tends to be *larger* than the bias from the Time-Difference FE if all other factors are equal. This should be the case, because it is less likely that the two twins over- or underestimate their working conditions in the same way as it is for the same individual to over- or underestimate his working conditions in different time periods.

As a result, we cannot make unambiguous predictions about the size of bias in the Twin FE and Time-Difference FE due to the measurement error effects.

Neither can we form any definitive conclusions about the size of the unobserved ability bias in these two estimators in Section 2.1. The unobserved ability bias in panel FE is removed if there are no wage growth effects from ability. The Twin FE is unbiased for MZ if their ability is equal. Both of these assumptions can in principle be questioned, so unbiasedness cannot be claimed indisputably. Furthermore, not even DiD is unambiguously consistent without further restrictions on ability effects. However, for MZ twins the DiD estimator is unbiased given the standard assumption of equal ability within MZ twin pairs even with the time-varying ability bias, whereas individual-level panel FE is biased in this case. It is the main reason why we believe that the panel data for twins to which we have access is useful in estimating compensating differences.

Sandewall *et al.* (2014) challenge the identification power of equal ability assumption for MZ twins. They argue that additionally controlling for IQ test scores reduces within-twin pair

[6] For example, Cesarini *et al.* (2009) show that genetic factors explain approximately 20% of the variation in behavior in their dictator experiments. Therefore, social preferences are arguably only explained by genetics in a small part.

estimates of returns to schooling by approximately 15%. However, because their within-twin pair estimates are over 50% smaller than the cross-sectional OLS estimates, it seems that the "equal ability" assumption for MZ twins accounts for the bulk of the ability variation across individuals. Thus, the explanatory power of IQ test scores for MZ twins is small compared to controlling ability in general using MZ twin differences in the estimations of returns to schooling.[7] There is also direct empirical evidence for a considerable genetic influence on cognitive abilities for twins even 80 or more years of age (see McClearn *et al.,* 1997), which supports the equal ability assumption for prime-age workers choosing wage-amenity bundles.

3. Data

The twin data used in this study are based on the Older Finnish Twin Cohort Study of the Department of Public Health at the University of Helsinki. The initial twin data gathered in 1974 contain almost all same-sex DZ (dizygotic) and MZ (monozygotic) twins in the Finnish population born before 1958 (see Kaprio *et al.,* 1979; Kaprio and Koskenvuo, 2002). The surveys were sent to the twins in 1975, 1981, and 1990. Identification of the DZs and MZs is based on the survey questions, but later this identification was confirmed for a small subsample using blood markers. The results based on the tests and surveys matched almost perfectly.

The twin sample was linked to the FLEED (Finnish Longitudinal Employer-Employee Data) maintained by Statistics Finland using personal ID codes attached to every person residing in Finland. This matching is exact, and there are no misreported ID codes. So we avoid problems

[7] The twin correlation for general cognitive ability and verbal ability is in the range of 0.7-0.8 for identical twins and approximately half that amount for non-identical twins (see McClearn *et al.,* 1997, p. 1562; Plomin and DeFries, 1998, p. 66). There are also personality differences between identical twins (Tellegen *et al.,* 1988). However, Helliwell *et al.* (2009, p. 93) found that compensating differentials were robust across personality differences.

associated with errors in record linkages (e.g., Ridder and Moffitt, 2007). FLEED is constructed from a number of different registers of individuals and firms maintained by Statistics Finland. FLEED includes information on individuals' labor market status and salaries and other income, taken directly from tax and other administrative registers that are collected and/or maintained by Statistics Finland. Thus, our earnings data do not suffer from underreporting or recall error and are not top coded. We concentrate on year 1990 in the FLEED data because a twin survey is available from the same year. For the survey year 1975, earnings data are available from the Longitudinal Population Census of Statistics Finland, but there is no income information available for the survey year 1981. The income measure is the logarithm of annual earnings from the tax register. Because the data on earnings contain some outliers, we have truncated the observations outside the 1st and 99th percentiles.

There are two types of attrition in the linked data that combine the Finnish twin data to register-based information from FLEED. First, there is some amount of attrition over time because not all twins in the original sample in 1975 are included in the later waves (1981 and 1990). However, Kaprio (2013) has argued that this type of attrition is not a major problem in this twin data.[8] Second, there is attrition arising from the fact that it is not possible to link register-based information from FLEED for all the twins in the original sample and later waves. However, this type of attrition is not a major issue in our context, because FLEED covers all workers in Finland. Attrition therefore arises only because of death or moving out of the country. Additionally, Hyytinen *et al.* (2013, p. 63) document that the lifetime labor market outcomes of the combined twin data are representative of the Finnish population.

[8] Prior studies document details about response rates, attrition and representativeness of the twin data (Kaprio *et al.,* 1979; Kaprio and Koskenvuo, 2002; Hyytinen *et al*., 2013).

The twin data contain information on individual-specific perceptions of working conditions. Working conditions are self-reported in the survey waves conducted in 1975 and 1990.[9] The measures that were used for working conditions were coded exactly as they were reported in the original twin data. Monotonous work (1975 and 1990) is measured using the question "your work can be characterized as" with the alternatives "very monotonous", "rather monotonous", "rather non-repetitive" and "very non-repetitive". To avoid imposing any restrictions on the effects of working conditions on earnings, we form three binary indicators for the degree of monotony of work using "very non-repetitive" as the reference category. Opportunities to influence work methods (1990) are measured with the alternatives "no influence", "some influence" and "substantial freedom in choosing work methods". Two indicators for the level of influence are used, with "substantial freedom" as the reference. Physically demanding work (1975 and 1990) is measured with the alternatives "my work is physically very demanding", "my work involves lifting and carrying objects in addition to standing and walking", "my work involves standing and walking but no other physical activity" and "my work requires hardly any physical activity". Again, we use binary indicators for the types of physical demands with "hardly any" as the reference category. Thus, the reference category in the empirical models is always the one that is considered best in each set.[10] Because the working condition variables describe negative attributes of work, the compensating differentials would imply positive coefficients (unlike Section 2 above where the job characteristic was a positive attribute).

[9] Duncan and Holmlund (1983) argue that self-reported measures of working conditions at the individual level contain less measurement error than the aggregated measures at the industry or occupational level because the aggregate measures do not capture the actual working conditions that employees face at workplaces. However, perceived working conditions may be affected by personality traits. Twins may also have different perceptions of the same working conditions. This would be included in the measurement error, which weakens the possibility of identifying compensating differences.

[10] Table A1 and Figures A1-A3 report basic descriptive statistics for the working condition variables.

The empirical specifications are estimated for individuals born after 1944 but before 1958. The twins were 18-30, 23-36 and 33-45 years old in 1975, 1981 and 1990, respectively.[11] Our analysis focuses on men because they are more strongly attached to the labor market (e.g., part-time work is much more common among females and we cannot control for part-time status). Male labor supply decisions are also much less complex compared with women because men are less affected by family and fertility choices. Because we examine working conditions, we require that both twins are employed. The total number of men in the dataset was 2824 after imposing the sample restrictions regarding age. Of these persons, 2423 (86%) were employed in 1990. The number of twins used in the models was further reduced because there were some missing observations for perceived working conditions. For example, using information on monotonous work the number of individual twins in the estimations is 1988 (70%). To make the estimates more comparable, cross-sectional OLS models are estimated only for the individual twins for whom it is possible to calculate the twin differences in working conditions.

Additional explanatory variables are potentially "bad controls" in our setting because they may be affected by characteristics in 1975 (or earlier). For example, unobservable productivity characteristics may affect a person's education choices. For this reason, we estimate the models without additional controls, but we briefly note also results with some (education) controls.

[11] Individuals were relatively young (between 18 and 30) the first time we observed workplace conditions in 1975. It might seem that this early information on working conditions would be less reliable regarding the equilibrium outcomes in the labor market. However, the observed wage-amenity combinations should reflect market conditions and therefore be the same regardless of the person's age. Additionally, variation over time in wages and amenities increases if the jobs held as a young person do not fully reflect the individual's long-term equilibrium behavior. This improves the precision of estimation.

4. Results

Before the presentation of the estimates for compensating wage differentials, it is useful to report the raw unconditional correlations of our measures for working conditions in the twin data. Table A2 documents that measured self-reported working conditions are somewhat less correlated between twins (Panel A) than within individuals over time (Panel B), even when the time difference is as long as from 1975 to 1990. This pattern provides some support for our notion above that the same would prevail for the correlations of true working conditions. The finding implies that the twin FE estimates are less biased than the panel FE estimates when all other factors are equal. The means of the absolute values of the twin differences (Table A3) show that there is a sufficient amount of within-twin pair variation in the data among MZ twins for most indicators for working conditions, which is necessary for model identification, with the exception of the indicator for very monotonous work.

We begin by presenting standard OLS estimates to obtain baseline results that are comparable to earlier empirical studies.[12] Then, we proceed to estimate three specifications that address unobservables using twin data with a panel dimension. First, we use twin differences to control for otherwise unobserved productivity/human capital effects. The estimations for DZs control for sibling effects (i.e., family background and some genetics because DZ twins share approximately 50% of their genes). The estimations using MZ data more completely control for family background, peer effects and genetics because MZ twins are genetically identical.[13]

[12] The OLS specification controls for age to be more comparable to the specifications estimated using the within-twin pair regressions that automatically account for this invariant within-twin variable.
[13] We focus on models in which each working condition measure is entered in a separate model because there is substantial correlation between the measures. Such correlation increases the standard errors of the estimates if the measures are included at the same time. We also report results from an alternative specification in which the measures are combined to one composite indicator.

Second, we estimate panel data fixed effects models using two of our three measures for working conditions available in the survey waves for both 1975 and 1990. Individual-level fixed effects models are estimated using the panel dimension of the data (i.e., long differences of 15 years). These models control for all time-invariant unobservable characteristics at the individual level.

Third, we estimate fixed effects specifications for twin differences (i.e., Difference-in-Differences models). In addition to the standard twin differences for DZs and MZs, these models also control for all otherwise unobservable time-invariant characteristics at the twin pair level that affect workers' sorting into workplaces. For example, differences in risk preferences between twins that have a constant impact over time on their wage-amenity choices are eliminated. Additionally, the effects of birth weight and birth order are controlled to the extent that their effect is time-invariant.

The standard OLS estimates show that the indicator for rather monotonous work and the indicators for somewhat physical and very physical work are significantly[14] *negatively* related to earnings (Table 1, Panels A and C, Column 1).[15] The result is in contrast to the expected positive compensating wage differential for adverse working conditions.[16] The pattern is likely to be driven by unobserved heterogeneity related to the sorting of workers into different

[14] To evaluate the joint significance of the coefficients for the job characteristics, we report F-test statistics for all specifications in Tables 1-3. Standard errors for the cross-sectional OLS estimates and individual-level FE models are clustered by twin pair. For twin difference models the observation unit is a twin pair and consequently we report heteroscedasticity-robust standard errors for these models in the tables.

[15] We have also estimated all models including the control for observed human capital using a linear term for the years of education based on the standard degree times used by Statistics Finland. The estimates are lower across the board when we control for education years. Qualitatively, the results remain intact (not reported in tables).

[16] To assess the external validity of our findings, we estimated wage models using the Quality of Work Life Survey (QWLS) from 2003 (Lehto and Sutela, 2005). QWLS is a cross-sectional random sample of all wage and salary earners in Finland that contains self-reported information on working conditions. We added perceived measures for working conditions to the set of standard covariates for earnings. The estimates show no significant relationship between physically demanding work and earnings (Table A4). The result supports our finding from the twin data that there is no evidence for compensating wage differentials using only cross-sectional variation in working conditions.

working conditions. The OLS estimations in other studies also often find wrong-signed premiums for adverse working conditions. Taken at face value, the quantitative magnitude of the cross-sectional OLS estimates is large. For example, we find that those who have rather monotonous work have 48% *lower* earnings compared to those who experience very non-repetitive work (Column 1 of Table 1).

===== Table 1 here =====

The use of twin differences changes the picture (Table 1, Columns 2-3). First, the significant negative effects observed in the OLS models either lose their significance or turn positive in the twin difference models. Second, we obtain some evidence for positive compensation for adverse working conditions. In particular, having no influence at work and being positioned in somewhat physical work both have a significant *positive* effect on earnings for MZs at the 10% level (Table 1, Panels A and B, Column 3). For DZs we obtain no significant effect for any of the working condition variables. For MZs, the point estimate of no influence at work is 55% with a 90% confidence interval of 6.5% to 107%. The result suggests that the unobserved ability bias considerably affects the OLS point estimates; however, the magnitude of the true effect remains imprecise in our estimations, due to the small sample size.

We do not obtain a significant positive compensation for monotonous work even using twin differences but we find a positive compensation for somewhat physical work of 65% (with a confidence interval from 6% to 124%). However, the positive compensation does not prevail for those who are in rather physical or very physical working conditions. This discrepancy is puzzling and, if true, it requires union power or monopsony effects as an explanation. Daniel and Sofer (1998) argue that the non-existence of wage compensation for those who work in

the worst working conditions may be due to the lack of workers' bargaining power, which is probably weakest in the most onerous low-wage jobs. They argue that a positive relationship between job amenities and earnings is possible along the contract curve from joint bargaining of job amenities and earnings. It is challenging to verify this explanation empirically because it is difficult to identify individual-level bargaining power in standard observational data. There are no such measures for bargaining power in our twin data. We are also unable to evaluate the monopsony explanation empirically, but as argued in Manning (2003, p. 220-224) monopsony power leads to less than full compensation for bad working conditions. If monopsony power affects working conditions negatively, the estimates for the worst conditions are more biased.

Next, we turn to the results that use the panel dimension of our data. Table 2 reports the estimates for individual-level panel fixed effects models. These results show that controlling for time-invariant unobservable characteristics at the individual level is not able to remove the negative compensating wage differentials for our measure of physical work (Table 2, Panel B). There is still a significant *negative* compensation for rather physical and very physical work that has the same size as the compensation in the OLS estimation. Thus, individual-level fixed effects models are unable to remove the ability bias. The ability bias would imply positive point estimates, which might be attenuated towards zero due to measurement error using 15-year changes in working conditions. Because we observe significant negative effects, our results cast doubt on this explanation. The result is similar to that of Duncan and Holmlund (1983, p. 373) who also did not find a positive compensating wage differential for physically demanding work in panel FE models. However, Duncan and Holmlund (1983)

found support for compensating wage differentials for dangerous work and stressful work in panel estimations.[17] One explanation may be the difference in the working conditions studied.

===== Table 2 here =====

The unique feature of our data is that we are also able to estimate DiD models (Table 3). These preferred specifications control for unobservable time-invariant characteristics at the twin pair level and for common wage growth for the twins. There is evidence for positive compensating wage differentials for both monotonous and physical work using the data on MZs (Table 3, Panels A and B, Column 3). For physical work, all three indicators are also jointly significant at the 10% level according to the reported F-test. The effects are significant only at the 10% level most likely because the sample size is even smaller for MZs when using information on working conditions and earnings in both 1975 and 1990. The quantitative magnitude of the estimates is again large in Table 3. For example, those with somewhat physical work have 59% higher annual earnings compared to those who have non-physical work (Column 3 of Table 3). Note that the standard error for the point estimate is also large, most likely reflecting the relatively small sample size for MZs.[18]

For physical work, the results in Table 3 are similar to the results that use twin differences for MZs in Table 1. For rather monotonous work, the point estimates are also similar but the effect obtains statistical significance only in Table 3. Notably, the point estimates of the significant effects are similar in Tables 1 and 3 and are in accordance with the discussion in

[17] In an earlier study of compensating wage differentials in the Finnish setting, Böckerman *et al.* (2011) obtained a positive compensating differential for job uncertainty using panel data.
[18] The earnings level for the sample that is used in the last column of Panel A in Table 3 does not differ dramatically from the earnings level for the sample that is used to estimate the model that is reported in the last column of Panel A in Table 1. The earnings levels are 9.3549 and 9.6452, respectively.

Section 2. Assuming that the equal ability assumption holds for MZs, the results support the notion that both twin difference estimates and DiD estimates are unbiased for MZs.

===== Table 3 here =====

An alternative possibility for the puzzling pattern of the effects is the relatively small number of non-zero twin differences for some of the working condition indicators. To alleviate this, we have used a composite indicator of working conditions. For each of the three working conditions, we form a binary indicator for bad conditions irrespective of the level of the condition. Thus, we sum the binary indicators to form a composite indicator with values from 0 to 3.

The results in Table 4 show that there is a significant negative coefficient for bad working conditions in the OLS model that uses cross-sectional variation in the data at the individual level (Column 1). Twin differences for DZs show no significant effect, but for MZ twin differences the effect of bad working conditions on earnings is marginally significantly positive at the 10% level. The estimates imply that earnings rise by 22% for each bad working condition with a 90% confidence interval ranging from 0% to 45%.

===== Table 4 here =====

Table 5 reports the panel difference and DiD models using the composite measures. The count of bad working conditions ranges from 0 to 2 because one working condition was not measured in the first twin survey in 1975. The individual-level panel FE obtains a negative and significant coefficient for bad conditions similar to the separate indicators, whereas the

fixed effects estimation for twin-differences (DiD) obtains a positive coefficient. Although the last effect is not significant, it has a magnitude similar to the standard twin-difference estimate in Table 4.[19] The pattern of estimated effects using the composite indicator supports our main results that use a separate indicator for each working condition.

===== Table 5 here =====

5. Conclusions

We used twin data linked to register-based information on earnings to examine the long-standing puzzle of non-existent compensating wage differentials. This novel approach provides a promising alternative for the removal of otherwise unobserved productivity differences that have been the prominent reason for estimation bias in earlier studies.

To compare our approach with previous studies, we also report standard OLS estimates and individual-level fixed effects estimates. The OLS results show negative or insignificantly positive compensating wage differentials similar to findings in earlier studies. Duncan and Holmlund (1983) found some support for positive compensating wage differentials in individual-level panel estimations. In contrast, our panel FE results are similar to the OLS estimates.

The use of twin differences changes the picture. Using twin differences for MZs, we find evidence for positive wage compensation for somewhat physically demanding work and for

[19] The non-significance of the composite indicator may be due to a limitation of the panel dimension of twin data. The measure for opportunities to influence work methods was not available in 1975, and the indicator for "no influence" was one of the two significant effects in twin difference models that used each work characteristic separately (Table 1, Panel B).

having no influence on work content. These results suggest that an unobserved ability bias considerably affects the OLS point estimates. The twin differencing alleviates the unobserved ability bias that seems to remain in the panel FE results in our data.

One reason for the difference between the panel FE and twin difference results might be the fairly long time period used in this study. For example, experience-related productivity changes may be significant over a 15 year period; therefore, using long differences in panel FE estimation is unlikely to purge their effect. The issue is not as relevant in twin differencing because the twins are at the same stage in their working career. It is also possible that panel FE works better for other working conditions than those studied here.

The unique feature of our data is that we are able to estimate Difference-in-Differences specifications that also control for unobservable time-invariant characteristics at the twin pair level (e.g., risk preferences) and the common wage growth for the twins. Using this preferred specification, we find evidence for positive compensating wage differentials for both monotonous and physical work using the MZ data. If the equal ability assumption holds for MZs, both twin difference estimates and Difference-in-Differences (combined twin difference – time difference) estimates are unbiased for MZs. The estimation results for somewhat physical work and rather monotonous work are of the same magnitude in these models, which provides some support for the validity of the equal ability assumption. One caveat is that the linked data have a relatively limited sample size for Difference-in-Differences specifications. Larger data would be beneficial in order to obtain more tightly estimated coefficients for compensating wage differentials. The pattern that a positive compensating wage differential does not prevail in the worst working conditions (i.e., “very monotonous work” and “very physical work”) may be due to the lack of control for workers’ bargaining power if bargaining

power is weakest in the most onerous low-wage jobs or due to firm monopsony power. Controlling these effects is challenging because it requires employer-employee data and firm-level measures of bargaining and/or monopsony power.

The central message of the paper is that twin data are useful when accounting for unobserved ability effects in the estimation of compensating wage differences and may be more useful than individual-level panel data under certain conditions. There is some evidence that poor working conditions are compensated in a labor market characterized by collective wage bargaining. The result suggests that the principle of compensation is strong and applies even in non-competitive markets. To confirm the external validity of our results, more research using different twin data with alternative measures of working conditions and a larger sample size is needed.

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Table 1. The Effect of Working Conditions on Earnings.

Panel A:	OLS	Twin differences: DZ	Twin differences: MZ
Very monotonous work	-0.3896 (0.3709)	0.0397 (0.2501)	-0.0460 (0.2944)
Rather monotonous work	-0.4803*** (0.1818)	-0.3911 (0.2865)	0.7420 (0.4923)
Rather non-repetitive work	-0.1393 (0.0920)	0.0403 (0.1482)	-0.0200 (0.2270)
F-test statistics	2.67 (0.0461)	0.89 (0.8615)	1.12 (0.3428)
N	1988	670	324
Panel B:	OLS	Twin differences: DZ	Twin differences: MZ
No influence	-0.5054* (0.2836)	0.0940 (0.4060)	0.5485* (0.2908)
Some influence	0.0527 (0.0837)	0.1705 (0.1137)	0.2531 (0.2221)
F-test statistics	2.54 (0.0798)	1.14 (0.3191)	1.82 (0.1642)
N	1872	625	311
Panel C:	OLS	Twin differences: DZ	Twin differences: MZ
Very physical	-0.7366*** (0.1856)	-0.3566 (0.3013)	-0.2653 (0.4341)
Rather physical	-0.3207*** (0.0994)	-0.2642 (0.2094)	0.0054 (0.2668)
Somewhat physical	-0.4076*** (0.1565)	-0.2522 (0.2453)	0.6496* (0.3557)
F-test statistics	7.69*** (0.0000)	0.74 (0.5268)	1.34 (0.2603)
N	2030	681	334

Notes: All working conditions were measured in 1990. In Panel A, the reference category is very non-repetitive work. In Panel B, the reference category is having substantial influence. In Panel C, the reference category is non-physical work. The cross-sectional OLS models control for age (squared and cubed), and the standard errors are clustered by twin pair. For twin difference models, heteroscedasticity-robust standard errors are reported in parenthesis. F-test statistics for the joint statistical significance of the working condition variables are reported (p-values in parentheses). Significant at *10%, ** 5%, and *** 1% levels.

Table 2. The Effect of Working Conditions on Earnings: Individual-Level Panel Fixed Effects Estimations.

Panel A:	FE All	FE DZ	FE MZ
Very monotonous work	-0.1322 (0.2600)	-0.1726 (0.3344)	-0.1067 (0.3297)
Rather monotonous work	-0.0350 (0.1661)	-0.2266 (0.1959)	0.4111 (0.3101)
Rather non-repetitive work	-0.0856 (0.1027)	-0.1347 (0.1249)	0.0227 (0.1783)
F-test statistics	0.26 (0.8511)	0.55 (0.6464)	0.76 (0.5147)
N	1232	855	377
Panel B:	FE All	FE DZ	FE MZ
Very physical	-0.6731*** (0.1770)	-0.6581*** (0.2163)	-0.7041** (0.3051)
Rather physical	-0.3849*** (0.1409)	-0.3698** (0.1758)	-0.4158* (0.2274)
Somewhat physical	-0.1608 (0.1685)	-0.1629 (0.2151)	-0.1614 (0.2581)
F-test statistics	6.18*** (0.0004)	4.10*** (0.0067)	2.24* (0.0829)
N	1303	906	397

Notes: Working conditions were measured in 1975 and 1990. In Panel A, the reference category is very non-repetitive work. In Panel B, the reference category is non-physical work. The standard errors are clustered by twin pair. F-test statistics for the joint statistical significance of the working condition variables are reported (p-values in parentheses). Significant at *10%, ** 5%, and *** 1% levels.

Table 3. The Effect of Working Conditions on Earnings: Fixed Effects Specification for Twin Differences (Difference-in-Differences).

Panel A:	FE All	FE DZ	FE MZ
Very monotonous work	0.3404 (0.2716)	0.3686 (0.3099)	0.4236 (0.4153)
Rather monotonous work	0.0855 (0.1975)	-0.1071 (0.2321)	0.6141* (0.3568)
Rather non-repetitive work	0.0277 (0.1042)	0.0643 (0.1191)	-0.0195 (0.2084)
F-test statistics	0.68 (0.5641)	0.59 (0.6213)	1.92 (0.1273)
N	806	554	252
Panel B:	FE All	FE DZ	FE MZ
Very physical	-0.1565 (0.2306)	-0.2078 (0.2965)	-0.0694 (0.3487)
Rather physical	0.0864 (0.1831)	-0.0721 (0.2308)	0.4199 (0.2824)
Somewhat physical	0.1354 (0.1648)	-0.0926 (0.1884)	0.5946* (0.3121)
F-test statistics	0.92 (0.4308)	0.20 (0.8931)	2.29* (0.0790)
N	874	600	274

Notes: Working conditions were measured in 1975 and 1990. In Panel A, the reference category is very non-repetitive work. In Panel B, the reference category is non-physical work. Heteroscedasticity-robust standard errors are provided in parenthesis. F-test statistics for the joint statistical significance of the working condition variables are reported (p-values in parentheses). Significant at *10%, ** 5%, and *** 1% levels.

Table 4. The Effect of Composite Indicator for Working Conditions on Earnings.

	OLS	Twin differences: DZ	Twin differences: MZ
Count of bad working conditions	-0.0978** (0.0491)	0.0624 (0.0914)	0.2239* (0.1367)
N	1988	670	324

Notes: All working conditions were measured in 1990. The construction of the composite indicator for the count of bad working conditions is explained in the text. The cross-sectional OLS models control for age (squared and cubed), and the standard errors are clustered by twin pair. For twin difference models, heteroscedasticity-robust standard errors are reported in parentheses. Significant at *10%, ** 5%, and *** 1% levels.

Table 5. The Effect of Composite Indicator on Earnings: Individual Panel FE and Twin-Difference FE (Difference-in-Differences) Estimations.

	Panel FE (All)	Twin-Difference FE (MZ)
Count of bad working conditions	-0.2042** (0.0830)	0.2073 (0.1800)
N	1209	248

Notes: Working conditions were measured in 1975 and 1990. The standard errors are clustered by twin pair in Column 1. Heteroscedasticity-robust standard errors are reported in Column 2. Significant at *10%, ** 5%, and *** 1% levels.

Table A1. Descriptive Statistics for the Indicators of Working Conditions.

	Mean	N
Very monotonous work	0.0136	1988
Rather monotonous work	0.1101	1988
Rather non-repetitive work	0.5236	1988
Very non-repetitive work	0.3526	1988
No influence	0.0385	1872
Some influence	0.3381	1872
Substantial freedom	0.6234	1872
Very physical	0.1241	2030
Rather physical	0.3921	2030
Somewhat physical	0.1340	2030
Hardly any physical	0.3498	2030

Notes: All working conditions were measured in 1990.

Table A2. Correlation Coefficients between the Measures of Working Conditions.

Panel A: within twin-pairs (1990)	DZ	MZ
Monotonous work	0.0915 (0.1056)	0.2216*** (0.0090)
Opportunities to influence	0.1144** (0.0428)	0.1334 (0.1189)
Physically demanding work	0.0696 (0.2188)	0.1911** (0.0248)
Panel B: within individuals (1975-1990)	DZ	MZ
Monotonous work	0.2258*** (0.0000)	0.2349*** (0.0000)
Physically demanding work	0.2571*** (0.0000)	0.2251*** (0.0000)

Notes: Spearman's rank correlation coefficients reported. Significant at *10%, ** 5%, and *** 1% levels.

Table A3. Twin Differences in Working Conditions.

	DZ	MZ
Very monotonous work	0.0358 [670]	0.0093 [324]
Rather monotonous work	0.1373 [670]	0.0988 [324]
Rather non-repetitive work	0.4597 [670]	0.4537 [324]
Very non-repetitive work	0.4104 [670]	0.3704 [324]
No influence	0.0800 [625]	0.0579 [311]
Some influence	0.4752 [625]	0.3505 [311]
Substantial freedom	0.4384 [625]	0.3441 [311]
Very physical	0.1968 [681]	0.1617 [334]
Rather physical	0.4170 [681]	0.3772 [334]
Somewhat physical	0.2438 [681]	0.2156 [334]
Hardly any physical	0.3436 [681]	0.2934 [334]

Notes: Absolute differences between twin pairs are reported. The number of observations is reported in squared brackets. All working conditions were measured in 1990.

Figure A1. The distribution of monotonous work.

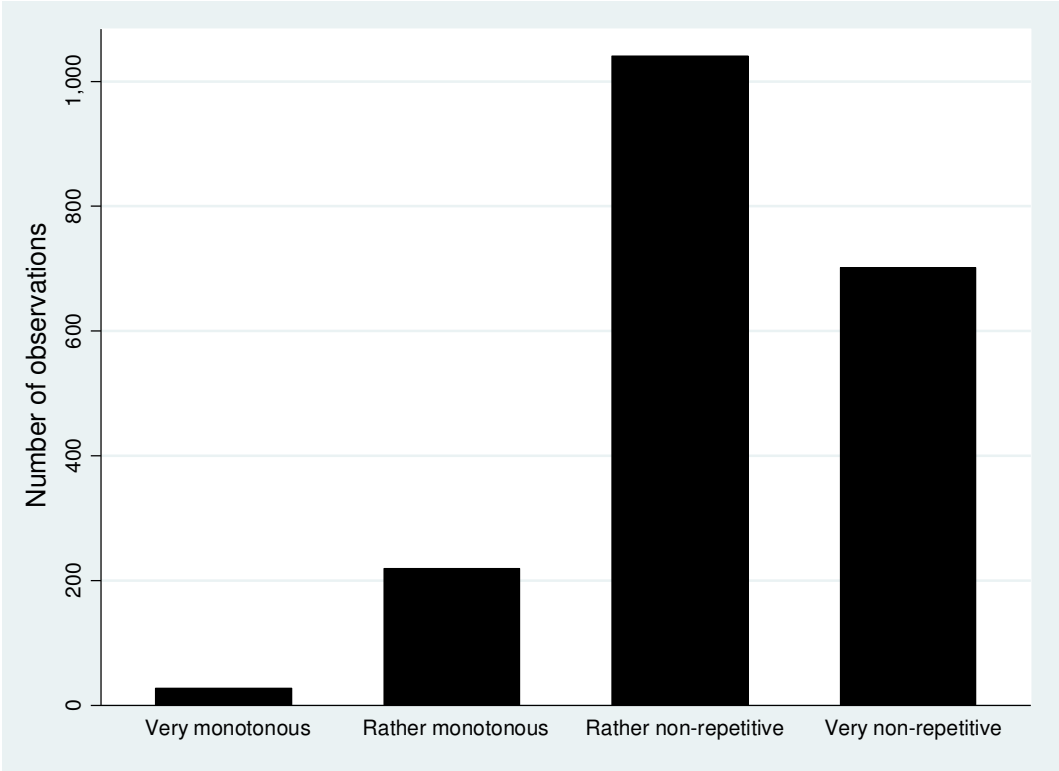

Figure A2. The distribution of influence at work.

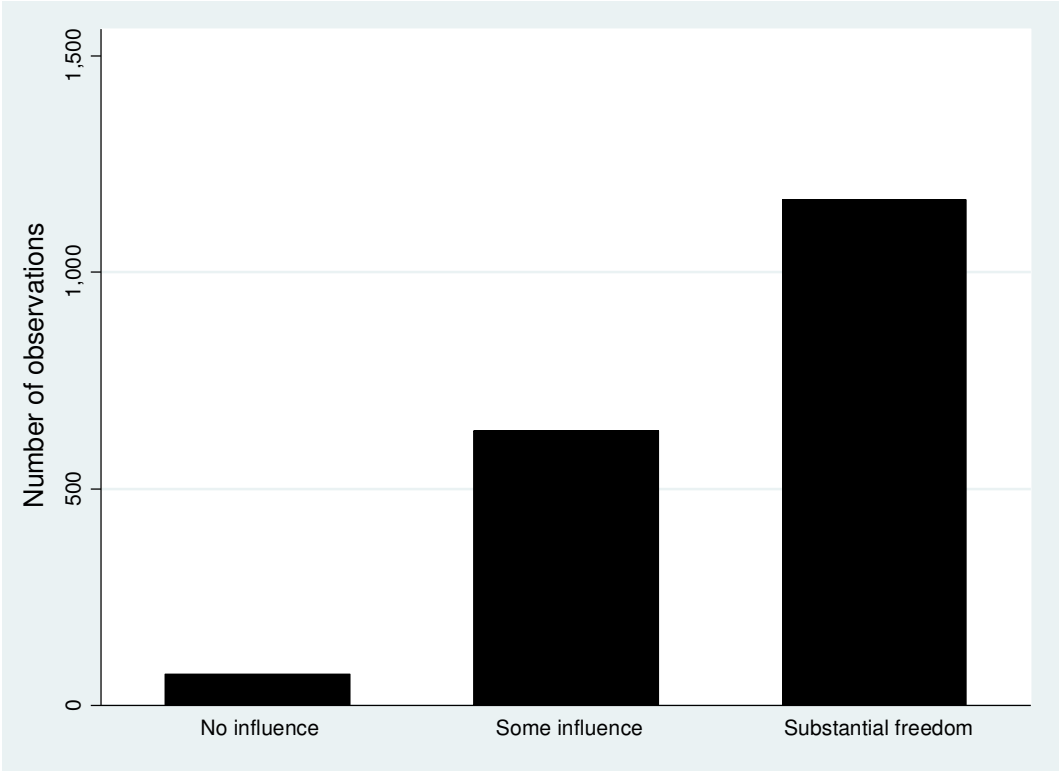

Figure A3. The distribution of physical work.

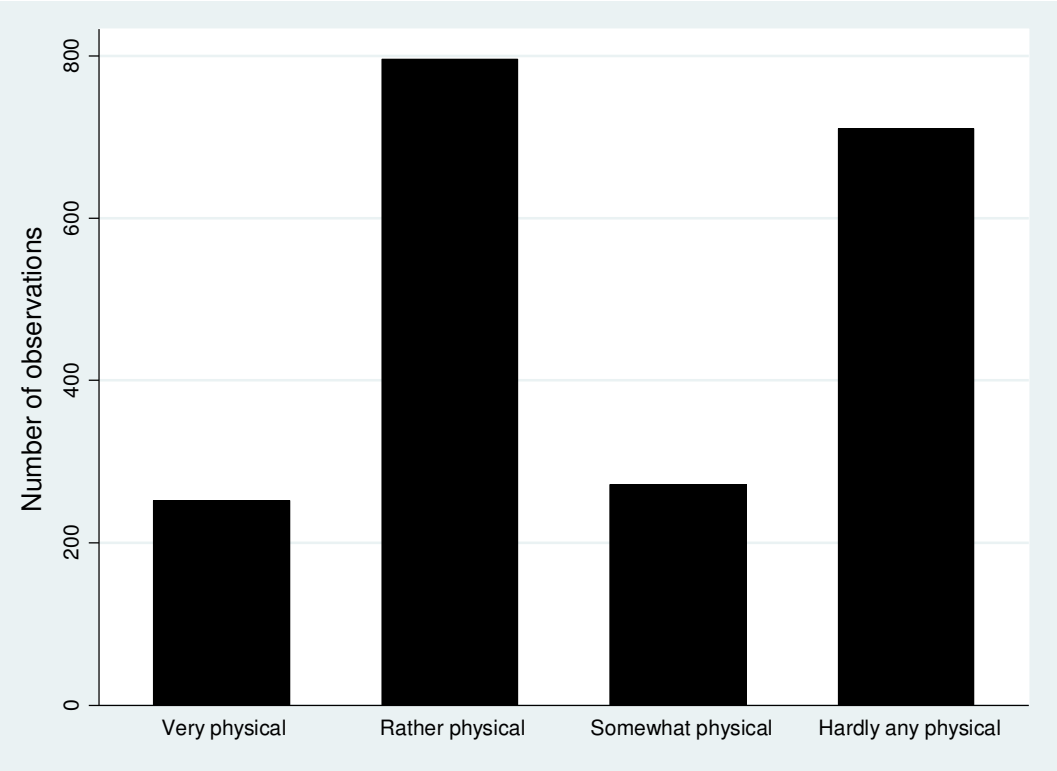

Table A4. The Cross-Sectional OLS Estimates for the Relationship between Perceived Working Conditions and Earnings Using the Quality of Work Life Survey.

	OLS
Physically heavy work	-0.0092 (0.0794)
Harm	-0.0079 (0.4000)
Hazard	0.0413 (0.0385)
N	2903

Notes: The estimates are based on the Quality of Work Life Survey by Statistics Finland from 2003. The dependent variable is the log of annual earnings (2003). Perceived working conditions are described in detail in Böckerman and Ilmakunnas (2006). The (unreported) controls include gender, age, tenure, temporary worker indicator, public sector indicator, perceived working capacity, plant size and 15 industry indicators. Heteroscedasticity-robust standard errors are in parentheses. Significant at *10%, ** 5%, and *** 1% levels.