Industrial & Labor Relations Review

Volume 60, *Issue* 4 2007 *Article* 5

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Ernesto Villanueva*

*Bank of Spain,

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Abstract

The author develops a model predicting that in a labor market that attaches a wage premium to jobs with a disamenity (a compensating wage differential), the premium's upper bound will be defined by the average wage change of voluntary job movers whose consumption of the disamenity rises as a result of their move; its lower bound, by the wage change of those whose consumption of the disamenity falls. These predictions will not hold if, as predicted by a "segmented" labor market model, the labor market attaches a wage penalty to workplace disamenities. Using longitudinal data on job characteristics and wages in Germany in 1984–2001, the author estimates the market returns to four workplace disamenities: heavy workload, job insecurity, poor hours regulation, and a mismatch between skills possessed and skills required. The results broadly support the existence of compensating differentials in the German labor market.

KEYWORDS: compensating wage differentials, voluntary job changes

ESTIMATING COMPENSATING WAGE DIFFERENTIALS USING VOLUNTARY JOB CHANGES: EVIDENCE FROM GERMANY

ERNESTO VILLANUEVA*

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Do labor markets place wage premia on jobs that require workers to consume disamenities? The answer to that question is a key to understanding the distribution of wages and welfare in the labor market. Economic theory predicts that if there is perfect information about job characteristics, workers with higher tolerance for disamenities will sort into firms with higher costs of provision of amenities (Rosen 1986). In equilibrium, jobs with disamenities have a wage premium.

This paper implements a new empirical strategy that bounds the market wage return to heavy workload, poor work hours regulation, skill requirements poorly matched to skills possessed by the new worker, and job insecurity. The strategy, at bottom, is to infer workers' preferences with regard to pay versus disamenities by examining the characteristics of jobs between which they move "voluntarily" (by their own account). An underlying premise, therefore, is that when workers voluntarily quit one job to take another, their decision to do so is based partly on how they weigh pay rates against disamenities.

The strategy in this paper has two important advantages over approaches used by most previous studies of compensating wage differentials. First, I overcome some of the problems associated with hedonic wage

*The author is Economist at the Research Department (Servicio de Estudios), Bank of Spain. For helpful comments, he thanks participants in seminar presentations at the University of the Basque Country, Universitat Pompeu Fabra, the Lower Conference at the London School of Economics (April 2004), the Winter Meetings of the Econometric Society (January 2005), and the Cost Conference on Applied Microeconometrics (June 2005). Joe Altonji, Dörte Domeland, Maia Guell, Sergi Jimenez, Francis Kramarz, Stephane Bonhomme, and Robert Valletta provided very useful feedback. Carlos Gonzalez and Tobias Pfütze provided outstanding research assistance.

A data appendix with additional results, and copies of the computer programs used to generate the results presented in the paper, are available from the author at Servicio de Estudios, Bank of Spain, 28014 Madrid, Spain; ernesto.villanueva@bde.es.

Industrial and Labor Relations Review, Vol. 60, No. 4 (July 2007). © by Cornell University.
0019-7939/00/6004 $01.00

regressions, which infer the market price of disamenities from cross-sectional regressions of (log) wages on indicators of the presence of a disamenity on the job.[1] Hedonic regressions are often criticized for not holding constant the worker's productivity, an omission that may bias the estimates against finding evidence for compensating wage differentials.[2] Using individual fixed effects to control for unobserved heterogeneity does not solve the problem, because moves across jobs mainly reflect sorting by workers into better jobs, and those moves induce spurious correlations between wage and amenity changes (see Solon 1988).[3] Instead, this paper exploits the sorting process of workers into jobs to bound the wage premia to several workplace disamenities.

I first show that in a labor market with compensating wage differentials (that is, a market where jobs with disamenities pay higher wages), the average wage change of the workers who voluntarily choose to consume a higher level of a disamenity provides an upper bound to the market return to the disamenity. The reason is that the distribution of observed wage changes is censored from below, as potential quitters reject those offers that entail a wage change that fails to compensate them for the utility loss due to more disamenities. A similar selection argument suggests that the average wage change experienced by workers who quit one job in order to take another with a lower level of a workplace disamenity provides a lower bound on the market return to the disamenity, as in this case we only observe quitters who voluntarily sacrifice a certain, presumably not-too-large amount of extra compensation in exchange for a decrease in their consumption of the disamenity. Conversely, in a labor market that places a wage *penalty* on jobs with disamenities (a "segmented" labor market), if a worker changes voluntarily to a job with higher amenity levels, his or her average wage must increase. Hence, the empirical strategy in this study allows me to assess if the labor market places a wage premium or a wage penalty on disamenities consumed in the workplace.

The second advantage is that this study uses the German Socioeconomic Panel (GSOEP), which is especially well suited for detecting market returns to workplace disamenities. First, Germany did not experience the increase in wage dispersion that affected the United States and United Kingdom during the 1980s (Abraham and Houseman 1995). An increase in wage inequality can change the relationship between wages and disamenities through income effects (Weiss 1976; Hamermesh 1999). Second, the GSOEP contains longitudinal information on motives for job-to-job changes, wages in each job, and job-specific indicators of workplace disamenities. The dataset allows us to estimate wage changes for voluntary job changers, and to relate those changes to changes in the consumption of workplace disamenities.

1. Dispersion of Wages and Disamenities

This section defines the parameter of interest and provides the background for the empirical strategy. I assume that there are search frictions in the job market, and that workers do not observe all available positions. Following Rosen (1986), each job is characterized by a wage level w and by a level of a nonpecuniary disamenity D. A job in which D equals 1 involves consuming the disamenity (a "dirty" job), whereas D is 0 if the job is "clean." With an exogenous probability of

[1]See Lucas (1977) for an early application of this procedure in the United States. In France, Daniel and Sofer (1998) found mixed evidence of compensating differentials associated with environmental conditions on the job, like noise, physical effort, and exposure to vibration. Daniel and Sofer found evidence that in strongly unionized industries there is a positive relationship between wages and amenities. For the German case, Lorenz and Wagner (1989) found that job requirements of physical effort affect wages negatively, contrary to the predictions of the theory.

[2]See Hwang et al. (1992) for a discussion of the consequences of mismeasuring workers' productivity. Hwang et al. (1998) discussed other biases associated with the estimation of hedonic models.

[3]Brown (1981) applied worker fixed-effect models to U.S. data and found no systematic effect of characteristics of jobs that are likely to enter the utility function of the worker on wages, once unobservable ability is controlled for. Duncan and Holmlund (1983) applied worker fixed-effect models to Swedish data and also found mixed evidence for the wage effect of environmental conditions.

arrival, a worker receives a job offer (w, D), as in Hwang, Mortensen, and Reed (1998). The preferences of a worker are

$$u_i(w, D) = w - Z_i D.$$

A subscript i indexes individuals. Z_i is the worker-specific marginal willingness to pay to avoid the consumption of a disamenity. Workers who have lower tolerance for disamenities have a high value of Z_i.[4]

I assume that the offers received by each worker are independent of his or her marginal willingness to avoid the disamenity. The rationale supporting this assumption is that firms do not observe Z_i and cannot target their offers to a specific kind of worker. Hwang, Mortensen, and Reed (1998) showed that in a labor market with search frictions, a constant arrival rate of offers, and heterogeneity in the firm's cost of provision of amenities, an equilibrium distribution of offer wages and disamenities in the market exists (namely, a distribution of wages for each level of amenities offered by firms). I augment the equilibrium wage distribution in Hwang, Mortensen, and Reed (1998) by including an unobserved individual-specific component μ, related to the ability of the worker:

$$w = \delta D + \mu + \varepsilon. \qquad (1)$$

δ denotes the market monetary return to the presence of a disamenity, and ε denotes the difference between the match-specific productivity and the average wage of workers with the same unobserved ability and job conditions. By definition, $E(\varepsilon | D,\mu) = 0$. My assumption about offers being independent of Z_i implies that $E(\Delta\varepsilon | Z_i) = 0$ (a worker's receipt of job offers does not depend on the worker's level of tolerance). The search models I am aware of that look at workplace disamenities do not include heterogeneity in individual ability, μ, so I make no assumptions about the correlation between μ and the level of the disamenity.

Z_i is the worker's marginal willingness to pay to avoid the presence of a disamenity on the job (MWP). The parameter δ is the market return to the presence of a disamenity on a job. Rosen (1986) showed that in a labor market without search frictions, δ and Z_i differ because of heterogeneity across firms in the marginal cost of reducing a disamenity. Hwang et al. (1998) and Lang and Majumdar (2004) showed that even if all workers dislike the presence of a disamenity on the job (that is, even if Z_i is positive for all workers), search frictions can lead to a negative δ.

This study focuses on the wage premia to workplace disamenities δ and makes no attempt to estimate Z_i.[5] While the magnitude of the MWP is crucial to evaluate public interventions in specific markets, search theoretic models suggest that the Z_i alone is not sufficient to resolve the question investigated in the present paper: does the labor market place a wage premium on jobs with workplace disamenities? Instead, δ is an important parameter to assess if wage differentials compensate for the amenities consumed on the job and, more generally, to understand the distribution of wages and welfare in the labor market.

The literature on wage differentials has used several strategies to estimate δ. The first is to use hedonic wage regressions, whose drawbacks are discussed in the introduction. The second strategy is to use a less selected group of job changers, like displaced workers (in another context, see Gibbons and Katz 1992). Unfortunately, I cannot implement this strategy, as many laid-off workers in my sample experienced unemployment spells before finding a new job. That complicates the interpretation of the association between wages and amenities with biases induced by heterogeneity in workers' reservation wages. A third option is to estimate (1) including a correction term accounting for selection

[4]Note that a linear specification in C and D is not generally restrictive, as one can define the utility function over consumption and disamenities $u(C, D)$ (where D can take values 0 and 1, $\partial u(C, D) / \partial C > 0$, and $u(C, 1) < u(C, 0)$ for a given C). If we denote C_0 as the wage level in a position without disamenities, Z can be defined as the extra consumption required to accept a job that involves consuming disamenities:

$$u(C_0, 0) = u(C_0 + Z, 1)$$

[5]Bonhomme and Jolivet (2005) and Gronberg and Reed (1994) provided alternative strategies for estimating the marginal willingness to pay for several workplace disamenities.

into jobs. It is not always straightforward to find variables that affect the consumption of disamenities D and are uncorrelated with wages (for an example of one success, see Olson 2002).

My strategy to estimate δ exploits the implicit preference about wages and disamenities that is revealed when workers change jobs voluntarily. I follow McLaughlin (1990) and assume that a worker changes jobs voluntarily ($Q = 1$) only if he or she attains a higher level of utility after the change (ignoring mobility costs). That is,

$$u_i(w_1, D_1) = w_1 - Z_i D_1 > w_0 - Z_i D_0 = u_i(w_0, D_0),$$

where a 1 subscript indexes the new job and a 0 the old job. A worker who currently has a "clean" job ($D_0 = 0$) willingly switches to a "dirty" job only if the wage difference $\Delta w = w_1 - w_0$ is sufficient to compensate for the utility decrease Z_i. In other words, voluntary changes from "clean" to "dirty" jobs are only observed if

$$\text{(2.1)} \qquad w_1 - w_0 = \delta(D_1 - D_0) + \varepsilon_1 - \varepsilon_0 \geq Z_i(D_1 - D_0) = Z_i.$$

That is, a worker quits one job to take another job with higher disamenities if $\varepsilon_1 - \varepsilon_0 > Z_i - \delta$. Taking averages over the distribution of $\Delta\varepsilon$ in (2.1), one finds that the average wage increase of voluntary job changers involving an increase in the consumption of disamenities is given by

$$\text{(2.2)} \qquad E(\Delta w | Q = 1, D_1 > D_0, Z_i) = \delta + E_{\Delta\varepsilon}(\Delta\varepsilon | \Delta\varepsilon \geq Z_i - \delta, Z_i).$$

(I assume that δ is positive, and defer the discussion of a zero/negative δ—a "segmented" labor market—to subsection 1.1.) Then, the average wage increase following a voluntary increase in the consumption of disamenities equals the (positive) market return to the presence of a disamenity δ and a positive selection term that arises because the sample of voluntary changers only includes wage increases that exceed the workers' monetary valuation of consuming disamenities.[6]

Similarly, workers who change to a job with lower levels of disamenities experience wage drops lower on average than δ, because we never observe job changes that entail a wage fall larger in absolute value than the utility gain Z_i. In more formal terms, reversing the values of D_1 and D_0 in (2.1), we obtain an expression for that wage change:

$$\text{(2.3)} \qquad E(\Delta w | Q = 1, D_1 < D_0, Z_i) = -\delta + E_{\Delta\varepsilon}(\Delta\varepsilon | \Delta\varepsilon \geq \delta - Z_i, Z_i).$$

The wage change of job quitters who change to jobs with lower levels of D is the sum of a negative term ($-\delta$) and a positive selection term and its sign is ambiguous. Nevertheless, it is easy to see that if $E(\Delta w | D_1 < D_0, Z_i)$ is negative or zero, then δ is bounded below by $|E(\Delta w | Q = 1, D_1 < D_0, Z_i)|$ and above by $E(\Delta w | Q = 1, D_1 > D_0, Z_i)$.

A way to obtain the bounds empirically is through the following regression (which does not include a constant):

$$\text{(2.4)} \qquad E(\Delta w | Q = 1, D_1 \neq D_0, Z_i) = d_1 1(D_1 > D_0) + d_2 1(D_1 < D_0).$$

d_1 equals (2.2), and d_2 equals (2.3). Section 2 discusses my strategy to recover (2.2) and (2.3) using panel data.

Proposition 1 summarizes the conclusions thus far:

Proposition 1: If, on average, the market rewards the presence of a disamenity on the job ($\delta > 0$), then (i) d_1, the average wage change following a voluntary job change that increases the consumption of disamenities, is positive and greater than the market return δ, and (ii) the sign of d_2 (the average wage change following a voluntary job change that decreases the consumption of disamenities) is ambiguous. Yet, if d_2 is negative, the market return δ is bounded between $|d_2|$ and d_1.[7]

[6] $E(\Delta\varepsilon | \Delta\varepsilon \geq Z_i - \delta, Z_i)$ is positive because $E(\Delta\varepsilon | \Delta\varepsilon \geq Z_i - \delta, Z_i) \geq E(\Delta\varepsilon | \Delta\varepsilon \geq -\infty, Z_i) = E(\Delta\varepsilon | Z_i) = 0$. The first inequality arises by the definition of a censored random variable. The second equality holds by our assumption of job offers being independent of Z_i.

[7] Altonji and Paxson (1988) used similar results to assess whether or not jobs in the U.S. economy are tied packages of hours and wages. However, they did not focus on equilibrium market relationships, but on the estimation of parameters of the utility function, such as the compensated elasticity of the hours of work to an increase in the wage. Usui (2006) also exploited wage changes between male- and female-dominated jobs to ascertain whether or not "male-dominated jobs" carry a

1.1 Labor Markets Penalize the Presence of a Disamenity ($\delta \leq 0$)

This subsection examines whether the results in proposition (1) hold if δ is zero (the case of a negative δ is very similar). Results (2.2) through (2.3) still hold because of sample selection arguments. The regression of wage changes on changes in the consumption of amenities now takes the form

$$E(\Delta w | Q = 1, D_1 \neq D_0) = c_1 1(D_1 > D_0) + c_2 1(D_1 < D_0).$$

The following result can easily be derived.

Proposition 2: If there is no average market return to the presence of a disamenity (if $\delta = 0$), (i) the average wage increase following a voluntary increase in the consumption of disamenities, c_1, is positive, and (ii) the average wage increase following a voluntary decrease in the consumption of disamenities, c_2, is also positive.

The intuition behind the second result is that quitting one job to take a second job with lower levels of disamenities does not imply a wage fall, because the market does not reward the presence of workplace disamenities, but some potential quitters will refuse changes that do not entail a sufficiently high wage increase (in other words, $E_{\Delta\varepsilon}(\Delta\varepsilon | \Delta\varepsilon \geq -Z_i, Z_i)$ is positive). Thus, in a "segmented" labor market the relative wage change following a change to a job with lower levels of disamenities is strictly positive.

Thus, this model has two testable implications:

- In a labor market in which wages are lower when a disamenity is present on the job (a "segmented" labor market), voluntary job changes from "dirty" to "clean" jobs result in wage increases.
- Conversely, if the market places wage premia on the presence of disamenities, and $d_2 \leq 0$, the (average) market return to the presence of a disamenity is bounded above by (2.2) and below by (minus) (2.3).

wage premium above the MWP to pay for (unobserved) disamenities associated with male jobs. Her identification strategy differed from the present one in two respects. First, Usui relied on the comparison of voluntary and involuntary job changers. Second, unlike Usui's empirical strategy, mine relies on the asymmetry of wage changes between job moves in which the destination job involves (a) higher consumption of disamenities and (b) lower consumption of disamenities.

2. Data and Empirical Specification

I test the implications described above by using a sample drawn from the 1984–2001 waves of the German Socio-Economic Panel (GSOEP). The GSOEP started by interviewing the members of 5,921 households in 1984, and it has continued following members of the households since then (see Wagner, Burkhauser, and Behringer 1993 for a description of the dataset). Three features of the GSOEP make it an specially suitable survey for implementing the methodology of this study. First, it has yearly longitudinal information that allows the construction of worker-specific changes in hourly wages. Second, workers are asked if they changed their job in the last year and, for those who changed, what was the reason. Responses to those questions allow the identification of "voluntary" job changes. Finally, an unusual aspect of this survey is that movers are asked to explicitly compare several characteristics of their new job to the characteristics of the previous one. That information allows us to exploit variation in nonmonetary characteristics of jobs at the individual level. Many papers in the compensating wage differentials literature impute disamenities to particular jobs based on out-of-sample occupation or industry characteristics, like the Dictionary of Occupational Titles in the United States (see, for example, DeLeire and Levy 2004; Gronberg and Reed 1994; Usui 2006). The accuracy of the imputation of the characteristics depends on, among other things, the quality of the report of occupational and industry codes, which is not always reliable (Mellow and Sider 1983). The problem gets worse when one uses individual-specific changes in characteristics, as biases due to measurement error are then exacerbated. Worker-specific reports of changes in disamenities avoid those problems (footnote 13 discusses some disadvantages of those measures).

The exact wording of the question I use is the following:

How do you view your current position compared to your previous one? In which of the following points has your new job improved or worsened your status? Or has it stayed about the same?

(c) workload?

(e) working schedules / work hours regulation?

(g) job security?

Pushing the inquiry a little beyond those three disamenities, the GSOEP asks job changers, "Can you use your knowledge and skills more, the same or less than in your last job?"

I focus on this particular set of disamenities based partly on previous literature on compensating wage differentials and partly on informal results from regressions of change in subjective satisfaction on self-assessed indicators of changes in consumption of disamenities when workers change jobs (see Villanueva 2004).[8]

The sample contains only observations of male workers in West Germany from 18 to 60 years of age who were not self-employed in the year of the interview. The GSOEP contains information on 3,305 workers who reported having resigned from their last position. I define a *quit* as occurring when a worker "resigned" from his last position. I further restrict the sample to "voluntary" transitions between full-time jobs for which information is available on contractual weekly hours in the origin job and the destination job. Contractual weekly hours in the origin and destination jobs must lie between 15 and 60. Job changes in which the worker reported having been in the last position for less than a year are also dropped. Finally, job changes that took place before the individual completed his military service or his education are also discarded. These restrictions left information on 743 job changes made by 653 individuals (see Appendix 1 for details).

My hourly wage measure, discussed in the next subsection, is the ratio between the previous month's earnings and standard weekly hours, multiplied by 4.33. The means, standard deviations, and minimums and maximums of the variables used for the sample are described in Table 1. The average age of quitters was 32.3 years. On average, the hourly wages of job changers increased by 8.5% in the year of the change. In general, a larger fraction of workers reported improved conditions in the new job than worsened conditions. For example, only 7% of changes involved an increase in job insecurity. A decrease in workload after a voluntary job change was reported by 32% of all movers, and an improvement in their working schedule was reported by 43%.

Table 2 presents the mean and standard deviation of hourly wage changes by changes in the consumption of disamenities. Voluntary job changes that involved an increase in workload were followed by real wage increases of about 14% (row 1, column 1). Job changers whose job characteristics did not change experienced wage increases of about 8% (row 1, column 2). Finally, improvements in the workload were associated with wage increases of only 6% (row 1, column 3).

Rows 2 through 4 of Table 2 document that a job change that involved a worsening of hours regulation was associated with a 12.6% increase in hourly wages (row 2, column 1, Table 2), an increase 6.5 percentage points larger than that experienced by workers whose regulation of hours did not change after a voluntary job change. The wage increase for workers whose hours regulation improved after a job change was 2.7 percentage points larger than that for workers whose hours regulation stayed the same. The wage patterns associated with the rest of the characteristics are similar: I document wage increases of about 13% when the change involved consuming more disamenities, and lower wage gains when job characteristics either improved or stayed constant.

2.1 Empirical Specification

The unit of observation in the analysis is a job change made by worker i from job $j-1$ to job j in moment t for an hourly wage w_{ijt}. The wage growth model is the following:

[8]The GSOEP asks about other disamenities, like commuting time. I chose not to report the market return to jobs that involve long commutes because I found the result hard to interpret. I did compute the bounds for that disamenity, and I report them in a footnote below.

Table 1. Descriptive Statistics of the Sample of Voluntary Changers across Firms.

Variable	*Mean*	*Std. Deviation*	*Minimum*	*Maximum*
Log Hourly Wage Increase	.085	.237	–.9876	.9861
Age	32.53	7.43	20	58
Single	.35	.48		
Years of Schooling	11.93	2.51	7	18
Vocational School	.58	.49		
No Vocational School	.12	.32		
College Degree	.16	.36		
Education Missing	.14	.35		
Satisfaction with Health—Old Job	7.49	1.91	0	10
Reported Change in Characteristics				
Wage Increases	.66	.47		
Wage Decreases	.10	.30		
Better Workload	.36	.48		
Worse Workload	.21	.41		
Better Hours Regulation	.44	.50		
Worse Hours Regulation	.16	.37		
More Secure Job	.38	.49		
Less Secure Job	.06	.24		
More Use of Skills	.41	.491		
Less Use of Skills	.16	.364		

Notes: 743 observations on 653 individuals. Hourly wage is defined as net monthly income divided by weekly agreed hours times 4.33, and is not corrected for overtime or short hours. Changes to jobs with agreed monthly hours below 15 hours or above 60 are excluded. Job changes resulting in a new wage more than 2.71 times or less than 0.36 times the previous wage are also excluded. Monetary magnitudes are in 1995 DM.

$$\log(w_{ijt}) - \log(w_{i,j-1,t-1}) = d_0 + \sum_{k=1}^{4} d_1^k 1(D_{jt}^k > D_{j-1,t-1}^k) + \sum_{k=1}^{4} d_2^k 1(D_{jt}^k < D_{j-1,t-1}^k) + \gamma_1 X_{it} + \varepsilon_{ijt} - \varepsilon_{ij-1,t-1}. \tag{3.1}$$

$1(D_{jt}^k > D_{j-1,t-1}^k)$ denotes the event "disamenity k occurs more frequently in the new job than in the previous one." The ordering of the disamenities presented in the following tables is increased workload ($k = 1$), bad hours regulation/working schedules ($k = 2$), mismatch between the skills required by the job and the worker's knowledge ($k = 3$), and job insecurity ($k = 4$). d_0 is the expected wage growth of a person who voluntarily leaves a job for another with the same observed level of disamenities. d_1^k (d_2^k) measures the average log-wage increase associated with a job change that increases (decreases) the consumption of disamenity k relative to a job change that leaves the consumption of disamenities unaffected.

There are a number of differences between expression (3.1) and expression (2.4).[9] First, (3.1) includes a constant, while (2.4) does not. The constant d_0 in (3.1) captures the average log-wage change when workplace disamenities stay constant. Such wage change includes a pure wage gain due to sorting (that is, quitters may change into jobs with better wages without varying their consumption of amenities) and time effects that impinge on all workers regardless of whether they change jobs. If d_0 captures mainly wage gains due to sorting, to obtain an estimate of the parameters $E(\Delta w_{ijt} | D_{jt}^k > D_{j-1,t-1}^k, Z_i)$ and $E(\Delta w_{ijt} | D_{jt}^k < D_{j-1,t-1}^k, Z_i)$ one should add the constant to the estimated parameters d_1^k and d_2^k. If, on the contrary, the "sorting" component is close to zero, then we will not need to add the constant to d_1^k and d_2^k to obtain the parameter of interest. Appendix 2 discusses

[9] I thank an anonymous referee for bringing to my attention the discrepancy between the theoretical model and the empirical implementation. That discrepancy motivates the following paragraph and the discussion in Appendix 2.

Table 2. Average Wage Changes by Change of Disamenities.

Disamenity	*Disamenity Worsens in Destination Job* (1)	*Disamenity Stays the Same* (2)	*Disamenity Improves in Destination Job* (3)	*T-Test* (1)–(2)	*T-Test* (2)–(3)
Workload					
1. Mean	.139	.079	.056	2.71	1.21
Standard Deviation	(.24)	(.22)	(.241)		
Number of Observations	158	316	269		
Hours Regulation/Working Schedule					
2. Mean	.126	.061	.088	2.62	−1.44
Standard Deviation	(.25)	(.22)	(.246)		
Number of Observations	120	297	326		
Mismatch between Worker's Skills and Knowledge Required in the Job					
3. Mean	.125	.055	.099	2.82	−2.37
Standard Deviation	(.256)	(.22)	(.245)		
Number of Observations	117	325	301		
Job Insecurity					
4. Mean	.13	.074	.091	1.45	−.93
Standard Deviation	(.245)	(.249)	(.22)		
Number of Observations	46	417	280		

Notes: Sample size = 743 observations on 653 individuals. Hourly wage is defined as net monthly income divided by contracted hours, and is not corrected for overtime. Job changes in which the destination job had agreed monthly hours below 15 hours or above 60 are excluded. Job changes in which the final wage was more than 2.71 times or less than 0.36 times the previous wage are also excluded. Each row in the fourth column shows the result of a T-test of the equal mean log-wage increase among changers who increased disamenities and the mean log wage changes of workers whose consumption of disamenities was unchanged. Variances are assumed to be known and equal across groups. The fifth column presents a test of equal average wage increases among workers for whom

evidence consistent with the idea that d_0 in (3.1) mainly reflects wage growth common to all workers, rather than quit-specific wage growth. Thus, in what follows I interpret d_1^k and d_2^k as $E(\Delta w_{ijt}|D_{jt}^k > D_{j-1,t-1}^k, Z_i)$ and $E(\Delta w_{ijt}|D_{jt}^k < D_{j-1,t-1}^k, Z_i)$, respectively (see Appendix 2 for a more detailed discussion).

Second, some authors, like Daniel and Sofer (1998), have estimated industry-specific compensating wage differentials based on the idea that unions may have different bargaining power in different industries, leading to variation in the link between disamenities and wages. Nevertheless, those are static models that rule out workers' mobility across industries (mobility *per se* would tend to erode bargaining-driven wage differentials). The model I test implicitly makes the opposite assumption that there is an economy-wide compensating wage differential, but this model does allow for industry effects on wage growth. Also, my sample size (less than 800 cases) is too small to allow estimation of industry-specific wage premia.

Finally, all moments in equation (2.4) hold constant the worker-specific marginal willingness to pay Z_i. For (3.1) to provide the empirical counterpart of (2.4), we need to condition on the unobservable variable Z_i (possibly interacted with the indicators of changes in the consumption of disamenities). In previous versions of the paper, I used workers' satisfaction with the previous job as a proxy for Z_i. The present version includes demographic indicators like the presence of children, marital status, and (lagged) health indicators to proxy for risk tolerance (DeLeire and Levy [2004] suggested that marital status and number of children are indicators of how great a risk of death workers are likely willing to expose themselves to). Again, sample size considerations led to unstable estimates when I interacted the indicators of disamenity changes with those demographics, so the results shown do not include those sample partitions.

Following the discussion in Section 1, if there exists a positive market return to the presence of a disamenity, d_2^k should be either zero or a negative number and d_1^k should be positive. If both conditions hold, the market return to the presence of a disamenity on the job is bounded between $|d_2^k|$ and d_1^k.

A final problem with specification (3.1) is that we do not observe the hourly wage w_{ijt}. The GSOEP asks respondents to report the amount of their net earnings last month, including overtime, but not the exact number of hours worked in that month. There are two questions about hours worked. The first one asks about the average number of hours worked in a regular week (not necessarily in the last month), including overtime. The second one asks about the agreed number of work hours, or standard weekly hours. Previous authors working with the GSOEP, such as Hunt (1999), have constructed a measure of hourly wages by dividing monthly wages by standard weekly hours (multiplied by 4.33) and then including an adjustment for overtime hours in wage regressions. I chose not to adjust for overtime in my baseline estimates, and I measure w_{ijt} as the ratio between monthly net earnings and contractual weekly hours (multiplied by 4.33). Below, I discuss what happens when (3.1) includes an adjustment for the change in the number of hours of overtime in the regression. Log-wage increases exceeding 1 or falling below –1 are screened out to avoid the influence of a small set of large wage changes on the estimates.[10] Finally, X_{it} contains a number of worker- and job-specific covariates discussed below.[11]

[10]Trimming the sample results in a dramatic reduction (of more than 25%) in the standard errors of the coefficient of interest. The point estimates are not very sensitive to trimming.

[11]An obvious difference between (2.4) and (3.1) is that (3.1) has log-wages as a dependent variable, rather than wage levels. A way of reconciling the empirical model with the theoretical discussion is to use $U(w, D) = \log(w) - Z_i D$ as the worker's utility function and express the market equilibrium wage in terms of logs.

3. Wage Regressions

Table 3 presents coefficients of regression (3.1) for the subsample of workers who reported having changed jobs voluntarily.[12] The first column of Table 3 reports the signs of the covariates that are consistent with the theory of compensating wage differentials; drops in the consumption of disamenities should not involve wage increases, and increases in the consumption of disamenities should be associated with wage increases.

Model I of Table 3 presents a specification that includes as covariates the indicators of changes in the consumption of disamenities, age minus 30, and age minus 30 squared to capture life-cycle effects, time dummies, and a constant. Model II adds the number of years of schooling of the individual and a separate intercept for individuals who did not have vocational training, to capture the differential growth in wages predicted by basic human capital models. Model III includes indicators of other nonmonetary attributes that change when the job changes: commuting time, availability of fringe benefits, and type of work. The rationale for including those characteristics is that we have only identified a subset of the job characteristics (and amenities) that change when individuals change jobs. Model III also includes 10 industry dummies. Finally, Model IV in Table 3 adds firm size indicators to control for permanent wage differences related to costs of monitoring workers' effort and working schedules (as stressed by the efficiency wage hypothesis). All standard errors reported in

[12]My strategy identifies the self-report of a "resignation" from a job as a voluntary job change, rather than as a lay-off (job terminated by the employer), for example. I used the 1984–2001 waves of the GSOEP to examine the patterns of labor market participation of both types of workers. The results, which are available on request, suggest that (self-reported) quitters were 30 percentage points less likely to experience a transition into unemployment or non-employment than were laid-off workers. Using an event-study analysis, I found that laid-off workers' reported satisfaction with the job decreased after the job change, while the satisfaction of workers who said they changed jobs voluntarily increased. Thus, the two types of workers seem observationally different.

Table 3. The Effect of Nonpecuniary Characteristics on Hourly Wages.
(Dependent Variable: Change in Logarithm of the Contracted Hourly Wage)

Independent Variable	*Sign Predicted by CD*	*Model I*	*Model II*	*Model III*	*Model IV*
1. Workload Worsened (1 if Worsened, 0 Otherwise)	Positive	.047 (.024)**	.041 (.024)*	.046 (.024)*	.048 (.024)**
2. Workload Improved (1 if Improved, 0 Otherwise)	Zero/Neg.	–.041 (.020)**	–.038 (.020)*	–.035 (.020)*	–.035 (.020)*
3. Worse Hours Regulation (1 if Worsened, 0 Otherwise)	Positive	.049 (.027)*	.050 (.027)*	.049 (.028)*	.051 (.028)*
4. Better Hours Regulation (1 if Improved, 0 Otherwise)	Zero/Neg.	.035 (.020)*	.035 (.020)*	.032 (.020)	.033 (.021)
5. Use Less Knowledge (1 if Worsened, 0 Otherwise)	Positive	.058 (.026)**	.061 (.026)**	.064 (.026)**	.061 (.026)**
6. Use More Knowledge (1 if Improved, 0 Otherwise)	Zero/Neg.	.038 (.019)*	.034 (.019)*	.031 (.020)	.031 (.020)
7. Job Security Worsened (1 if Worsened, 0 Otherwise)	Positive	0.017 (0.040)	.014 (.040)	.048 (.041)	.040 (.041)
8. Job Security Improved (1 if Improved, 0 Otherwise)	Zero/Neg.	0.016 (0.018)	.018 (.018)	–.007 (.018)	–.008 (.019)
(Age – 30)/10		–.034 (.020)	–.048 (.022)**	–.032 (.020)*	–.027 (.023)
(Age – 30)∗(Age – 30)/100		.020 (.012)*	.028 (.013)**	.023 (.012)*	.022 (.012)*
Years of Education			0.011 (.004)**		
Destination Firm Had More Than 2,000 Employees					.01 (.024)
Previous Firm Had More Than 2,000 Employees					–.01 (.030)
Constant		.066 (.037)*	.059 (.037)	0.068 (0.049)	.0626 (.048)
Industry Dummies		No	No	Yes	Yes
Other Job Characteristics		No	No	Yes	Yes
Number of Observations		743	743	740	740
R-Squared		0.09	0.09	0.16	0.17

Notes: The sample contains 743 voluntary job changes by 653 individuals staying in the origin job for at least a year. Standard errors (in parentheses) are corrected for heterogeneity in rates of growth across workers, and worker-autocorrelation. Wave dummies are used in all models. Other job characteristics include commuting time, type of job, fringe benefits, and possibilities for advancement.

*Statistically significant at the .10 level; **at the .05 level.

the tables are corrected for heteroskedasticity and arbitrary correlation between observations of the same individual.

The coefficient on "worse workload" in the first row of Model I is .047 (.024). This implies that higher work strain in the new job was associated with 4.7% higher wages, on average, than was unchanged work strain (holding constant the rest of the disamenities we observe). The coefficient on "better workload" in the second row of column (1) is –.041 (.020), negative and significantly different from zero at the 5% confidence level. This estimate implies that workers who quit one job and took another with a lighter workload experienced a 4.1% decrease in wages relative to job changers whose workload did not change. The magnitude of the wage increase following an increase in the workload (.047) exceeds (but is similar to) the magnitude of the (log) wage increase following a voluntary job change that involved a decrease in the workload. The latter result is consistent with the hypothesis of a wage premium for jobs with a heavy workload, and this return is bounded between 4.1% and 4.7% of the hourly wage.[13]

The third row of Model I in Table 3 presents the estimate for "worse hours regulation": .049 (.027), which is positive and significantly different from zero at the 10% confidence level. The wage for a worker who changed to a job with worse hours regulation (or worse working schedules) was 4.9% higher than that for a similar worker whose work schedule was unaffected by the job change. The coefficient on "better regulation of hours" (row 4, Model I in Table 3) is .035 (.020), which is relatively large, positive, and significantly different from zero at the 10% confidence level. The hypothesis that there is a return to jobs with bad hours regulation is not clearly supported in this specification.

The coefficient on "worse match between skills of the worker and skills required on the job" is presented in row 5, Model I in Table 3: .058 (.026). That is, to be induced to make a job change that resulted in a poorer match between their own skills and the skills called for on the job, workers required wages 5.8% higher than those received by job movers for whom this dimension did not change. An estimate that contradicts the theory of compensating wage differentials is presented in row 6, Model II in Table 3: workers whose job change improved the matching of their skills with their job responsibilities experienced wage increases of about 3.8%. If workers prefer jobs that require skills close to theirs, and the market puts a premium on jobs with required skill sets that poorly match those of typical workers—for example, jobs that call for unusual skills that most workers could acquire only through difficult training, or jobs with skill requirements that offer too little challenge and stimulation—there should not be a wage premium associated with moving to jobs with a better match between skills possessed and skills demanded. I discuss this finding below.

The coefficient on "worsened security on the job" in row 7, Model I, Table 3 is .017 (.04), which is positive but small and very imprecise. The coefficient on improved job security is .016 (Model I, row 8)—also positive, relatively small, and not significantly different from zero. Thus, the estimates in rows 7 and 8 in Model I do not support the idea that job changers made a trade-off between wages and job security.

Models II–IV examine the robustness of the foregoing results by adding covariates related to both workplace amenities and wages, like industry, firm size, and education. The preferred specification is Model IV, which controls for firm size, industry, and changes in other nonmonetary characteristics (fringe benefits, commuting time, type of job, and possibilities for advancement). Adding these covariates does not generally change the magnitudes of the "worsened amenity variables" with respect to the results in Model I. Nevertheless, the magnitudes of the estimates for "better hours regulation" and "better match between the skills of the worker and those required by the job" become smaller and not significantly different from zero, in line with the predictions of the model of compensating wage differentials. Specifically, job changes resulting in an improvement in the regulation of hours result in a .033 increase in wages (standard error, .021), not significantly different from zero at the 10% confidence level (row 4 in Model IV of Table 3). The other coefficient that rejects the compensating wage differentials hypothesis in Model I is the coefficient on "better skill match." Again, once we control for industry dummies and other job characteristics, the coefficient decreases to .031 (standard error, .020), shown in row 6 of Model IV in Table 3. It is not significantly different from zero at the 10% confidence level. Thus, the

[13]Note that downward wage rigidity is unlikely to generate the asymmetry in the absolute value of wage changes: adding the constant in Model I of Table 3 (.066) to the coefficient on "better workload" in row 2 of Model I in Table 3 (–.041), we still obtain a .025 log-wage increase for job changers who decreased their consumption of disamenities.

Table 4. Bounds on Compensating Wage Differentials for Selected Disamenities.

Disamenity	*Lower Bound*	*Upper Bound*
1. Workload	.035	.048
2. Regulation of Hours	0 [–.033]	.051
3. Mismatch between Required Skills and Knowledge	0 [–.031]	.061
4. Job Insecurity	0 [.008]	0 [.044]

Notes: A negative sign in any entry indicates a rejection of the Compensating Wage Differentials Hypothesis. Entries correspond to estimates in column (4), rows 1–8 of Table 3. A zero entry means that the estimate for the upper or lower bound is not statistically different from zero at the 10% confidence level. When the estimate in Table 3, Model IV is not significantly different from zero, the corresponding point estimate is included in brackets.

evidence presented in Table 3, Model IV is in line with the predictions of the theory of compensating wage differentials.[14]

3.1 Estimated Bounds

Table 4 lists the bounds on the market return to the presence of disamenities on the job I have estimated. The point estimates are obtained from the preferred specification, Model IV in Table 3. When the estimate in that specification is not statistically different from zero at the 10% confidence level, I list zero as the magnitude of the bound and bracket the magnitude of the point estimate.

Row 1 of Table 4 presents the lower and upper bounds on the market return to jobs with a heavy workload, 3.5% and 4.8%, respectively (rows 1 and 2 of Model IV in Table 3). In this case, the bounds are relatively close to each other (the hypothesis that they do not differ from each other cannot be rejected at the 5% confidence level).

Row 2 of Table 4 presents the lower and upper bounds on the market premium to jobs with bad hours regulation. We cannot reject the null that the lower bound is zero (the 3.1% estimate is not significantly different from zero at the 10% confidence level). The upper bound is 5.1% (row 3 of Model IV in Table 3). These estimated bounds for hours regulation are slightly less informative than those for workload, but they still broadly support the compensating wage differentials hypothesis.

Row 3 of Table 4 bounds the market return to jobs with skill requirements poorly matching the skills workers typically possess. The upper bound is 6.1%, and the lower bound is again zero. A possible interpretation of the bounds in row 3 of Table 4 is that there are jobs requiring skills that workers do not typically acquire in the formal educational system. The labor market places a wage premium on those jobs, as workers find it costly to learn the new skills. My results contradict those of Teulings and Gautier (2004), who proposed a model with search frictions and imperfect matching between skills required by the job and workers' knowledge. In that model, jobs vary over the skill requirements and workers over their human capital, with workers who possess a higher level of human capital having higher productivity. Teulings and Gautier showed that search frictions make workers accept jobs that do not perfectly match their skill set, creating a concave relationship between wages and worker skills, holding job requirements constant.

[14]One may argue that because all the estimates in Table 3 use self-reported data on the change in characteristics, they suffer from subjectivity bias. For example, a worker who left a job that was a bad match and took a job that was a better match may have tended to exaggerate the level of amenities in the new job with respect to the former. While I do not correct for this source of error, note that such biases would generate a positive correlation between wage changes and reported amenity increases, biasing d_2 toward a positive number. That is, subjectivity biases would tend to produce estimates that reject the compensating wage differential hypothesis.

My results provide little support for such a relationship, perhaps because workers care about the match between their skills and those required by the job for reasons other than the wage.

Row 4 presents estimates of the market return to positions with low job security. I document a zero return to that disamenity (albeit the point estimates are noisy). Perhaps surprisingly, the result does not support the segmented markets hypothesis, as I do not detect wage increases when job security improves. The introduction of fixed-term contracts in Germany, Italy, France, and Spain has raised discussion of the relationship between wages and insecurity (Winkelmann and Zimmermann 1998; De la Rica 2003). A worker with a fixed-term (low firing cost) contract has a higher risk of losing his or her job than does another worker with a permanent (high firing cost) contract. Using the hedonic approach, De la Rica documented a negative relationship between a low-hiring-cost contract and the worker's wage. The bounds I estimate, while noisy, suggest that, given the omitted variable biases in hedonic wage regressions, such a result cannot safely be interpreted as the market return to job insecurity.[15]

One way to put the magnitude of the lower bounds in perspective is to test the strongest prediction of the model. The theory of compensating wage differentials predicts either a zero or a negative relationship between amenity increases and wage increases, while what I term the "segmented market hypothesis" predicts a positive relationship between those variables. To discriminate between the two hypotheses, I test to see whether the estimates of improvements in nonmonetary conditions presented in Table 3 are jointly equal to zero. I cannot reject that hypothesis at the 10% confidence level. Overall, the results lend more support to the hypothesis that the labor market places a wage premium on jobs that involve consumption of disamenities than to the segmented markets hypothesis. Interestingly, the hedonic regression method applied to GSOEP yields estimates opposite in sign to those predicted by the theory of compensating wage differentials (Lorenz and Wagner 1989).

How robust are the results reported in Table 4? The upper bounds on the market return to jobs with a heavy workload, bad hours regulation, and poor matching between skills demanded and skills possessed are quite robust with respect to the inclusion of industry dummies or human capital indicators (see Table 3, rows 1, 3, and 5). The lower bounds on "hours regulation" and "better match between skills required and skills possessed by the worker" vary a bit more across specifications, and they become negative in the absence of controls for industry and firm size (rows 4 and 6, Model II in Table 3). I believe that industry and firm size should be held constant in the analysis of wage changes. I experimentally included a correction term in Model IV of Table 3 that adjusts for overtime hours along the lines of Hunt (1999)—see Villanueva (2004) for details. When such a correction term is included, the market return to jobs with a heavy workload is bounded between .036 (standard error, .019) and .04 (standard error, .024). The market return to jobs with a "poor match between skills required and skills possessed by the worker" has bounds similar to those reported in Table 4: 0 and .062 (.026). However, the return to jobs with "bad hours regulation" is bounded between –.04 (.020) and .048 (.027), a relatively broad band that includes negative numbers. Job security changes never explain wage changes, although the magnitudes are consistent with a wage premium between 0 and .04 (.042). Overall, the bounds presented in Table 4 are relatively robust with respect to specification changes for most disamenities. If anything, once a crude adjustment for overtime is included, the evidence as to whether markets place a premium on jobs with bad hours regulation becomes mixed.

[15]There are amenities like commuting time that I have not analyzed. The focus of the paper is the market return to workplace disamenities, and I had trouble interpreting a wage return to jobs that are far away from the place where workers typically live. In any case, in Model IV (Table 3) I did include indicators for a better commute and a worse commute; the resulting estimates were –.030 (.021) and .028 (.021), respectively.

3.2 Is the Analysis Capturing Promotions?

There is a literature relating job mobility to a process in which firms learn about the (unobserved) productivity of their workers. MacLeod and Malcomson (1988) suggested that firms provide their workers with incentives to exert effort by offering position-related wages. The most productive workers self-select into these positions because the cost of effort is lower for them than for other workers. These models predict that, early in their careers, high-ability workers self-select into higher-ranked jobs and increase their wages by exerting high levels of effort. In such a model, characteristics of the jobs and wages co-vary in ways that do not necessarily reflect trade-offs. A possible interpretation of the results in Table 3 is that the variation in workload across positions is related to increasing effort and the positive correlation between increases in workload and wage gains documented in Table 3 is caused by firm or market learning mechanisms (rather than by market returns to a disamenity).

To qualitatively assess whether the results are driven by promotions, I run regression (3.1) on a sample of workers who changed positions within the same firm. Following McCue (1996), I assume that position changes within the firm are promotions and do not necessarily reveal workers' preferences for job amenities. If the same pattern of correlations between wages and this study's measure of disamenities is manifest both for a sample of voluntary changers (who evince preferences with regard to wages versus amenities) and for a sample of within–firm changers (who are basically promoted workers), it is reasonable to conclude that the empirical strategy is really capturing promotions.

Table 5 presents the results of regression (3.1) for a sample of 316 job changes within the firm, and the summary statistics for the sample, in the fourth column. The coefficient on "workload increased" in row 1 of Model I in Table 5 is .007 (.02). A move to a new position within the firm that involves an increased workload basically involves no hourly wage change. The coefficient on "workload decreased" in row 2 of Model I in Table 5 is –.025 (standard error, .024), which is smaller but less precise than the corresponding coefficient in Table 3. These results reject the hypothesis in Section 1 that the average wage change following an increase in workload is higher in absolute value than the wage change following an improvement in conditions. The same hypothesis is rejected for the variable "regulation of hours," as the coefficient on "worse hours regulation" in row 3 of Model I in Table 5 is .006 (standard error, .025) and the coefficient on "better hours regulation" in row 4 of Model I of Table 5 is –.053 (standard error, .028). In the case of the variable "matching between skills required and skills possessed by the worker" (rows 5 and 6, Model I of Table 5), the signs are reversed with respect to Table 3. Overall, the results in Table 5 give little qualitative support to the hypothesis that the bounds documented in Table 4 are generated by promotions.[16]

Conclusion

An assumption in this paper has been that workers who change jobs voluntarily are, in so doing, implicitly expressing preferences between packages of wages, on the one hand, and nonmonetary job characteristics, on the other, in the origin and destination jobs. Under that assumption, if the labor market offers a trade-off between wages and amenities (that is, if there are compensating wage differentials in the labor market), the average wage change of workers who choose to take another job that requires increasing (decreasing) the consumption of a disamenity provides an upper (lower) bound on the market return to the disamenity. I use the results of my empirical analysis to bound the market return to the consumption of four workplace disamenities: heavy workload, bad regulation of working schedules (or hours), job insecurity, and the degree of mismatch between skills required by the job and skills possessed by the worker.

[16] A formal test would pool both samples of voluntary job changers and within–firm job changers and test for equality of slopes among groups. Given the magnitude of the standard errors in Tables 3 and 5, I chose not to present those results.

Table 5. Nonpecuniary Characteristics and Wages, within-Firm Changers.
(Dependent Variable: Change in Logarithm of
Contracted Hourly Wage after a Job Change within the Firm)

Variable	*Model I*	*Model II*	*Model III*	*Mean (Std. Dev.)*
1. Workload Worsened	.007	.004	.003	.397
(1 if Worsened, 0 Otherwise)	(.022)	(.022)	(.023)	(.49)
2. Workload Improved	–.025	–.007	–.024	.19
(1 if Improved, 0 Otherwise)	(.025)	(.025)	(.027)	(.39)
3. Hours Regulation Worsened	.006	.006	.006	.18
(1 if Worsened, 0 Otherwise)	(.025)	(.025)	(.027)	(.38)
4. Hours Regulation Improved	–.053	–.052	–.058	.16
(1 if Improved, 0 Otherwise)	(.028)*	(.029)*	(.031)*	(.37)
5. Use Less Knowledge	–.067	–.065	–.046	.14
(1 if Worsened, 0 Otherwise)	(.028)**	(.031)**	(.033)*	(.35)
6. Use More Knowledge	–.003	–.002	–.018	.45
(1 if Improved, 0 Otherwise)	(.020)	(.020)	(.020)	(.36)
7. Job Security Worsened	.048	.056	.089	.05
(1 if Worsened, 0 Otherwise)	(.057)	(.058)	(.056)	(.22)
8. Job Security Improved	.032	.035	.024	.21
(1 if Improved, 0 Otherwise)	(.024)	(.024)	(.027)	(.41)
(Age – 30)/10	–.045	–.054	–.056	.58
	(.032)	(.032)	(.031)*	(.862)
(Age – 30)*(Age – 30)/100	.014	.018	–.019	
	(.016)	(.016)	(.016)	
Years of Education		.005	.004	12.52
		(.004)	(.004)	(2.59)
Satisfaction with Health Prior to Change		.001	–.0001	7.31
		(.004)	(.005)	(2.00)
Not Married	–.022	–.028	–.032	.32
	(.025)	(.025)	(.026)	(.47)
Constant	.132	.136	.12	
	(.040)***	(.039)***	(.108)	
Industry Dummies	No	No	Yes	
Wave Dummies	Yes	Yes	Yes	
Other Job Characteristics	No	No	No	
Number of Observations	316	316	316	
R-Squared	0.15	0.16	0.20	

Notes: The sample consists of 274 individuals, for whom 316 changes are observed within the firm. Wages are defined as the ratio of income last month divided by agreed hours. Standard errors (in parentheses) are corrected for heterogeneity in rates of growth across workers, and worker-autocorrelation. The reference group consists of individuals who changed jobs in 1990, who had completed vocational education, and for whom nonpecuniary characteristics were reported to be the same across the new and previous jobs.

*Statistically significant at the .10 level; **at the .05 level.

I find that the market return to jobs with a heavy workload lies between 3.5% and 4.8%. The market return to bad hours regulation is between zero and 5.1% (although some specifications show a negative lower bound for hours regulation). Jobs whose skill requirements poorly match the skills possessed by the typical worker pay a wage premium bounded by 0 and 6.1%. I find little evidence of a relationship between wages and job security. Overall, the results lend more support to the compensating wage differentials hypothesis than to the opposite extreme of a “segmented” labor market.

Among the questions that researchers might pursue in the future, two are especially important. First, a substantial literature has documented wage gaps between men and women and, within the latter group, between mothers and non-mothers. There are reasons to think that mothers may sort into jobs with flexible hours and a light workload. One could combine the methodology followed in this paper with data on wages and workplace amenities to understand what fraction of the wage gap associated with gender or motherhood can be attributed to equalizing differentials. A second, "revealed preference" argument could be applied to the estimation of the marginal willingness to pay for different workplace amenities.

Appendix 1
Sample Criteria

I first use the question, "Has your job situation changed since the beginning of (last year)?" Possible answers are:

1. Changed position.
2. Job with new employer.
3. Became self-employed.
4. Gone back to work after a break.
5. Took up a job for the first time in my life.

The first phase of the selection process screens out all but those individuals who have a job with a new employer (answer 2). I identify quits using the question, "Why did you leave this job? Which one of the following points applies to you?"

1. Given notice.
2. Job ended automatically/time limit agreed upon before.
3. Training over.
4. Resigned.
5. Business relation ended.
6. By my own request, transferred within the firm.
7. Sent to another position by the firm.
8. Own business given up/family business dissolved.

I consider as a "quit" the answer "resigned." 3,502 male individuals between 18 and 60 years of age responded that they "resigned" their job between 1984 and 2001. I follow Winkelmann and Zimmermann (1998) and adjust for the possible double-counting of quits induced by the wording of the first question, and thus obtain 3,305 observations of quits. 1,212 of these quits involved a position in which the worker was not in a full-time job, and 610 quits were from positions with tenure less than one year. Hence, we are left with 1,423 quits. I was able to define hourly wages for only 898 cases, because the other cases either had missing values for income in the present or last year, or they lacked information on standard hours in the present or in the last year. I drop quits to or from self-employed positions, and quits that involved wage changes higher than 2.71 times the previous wage or smaller than 0.37 times the previous wage, reducing the sample size to 856 cases. Removing workers whose origin or destination jobs had contractual hours agreements below 15 hours a week or above 60 hours further reduces the number of cases to 838. Finally, I drop 95 quits that involved positions in East Germany. 575 workers each contributed one quit to the sample; 68 workers, two quits; 8 workers, three quits; and 2 workers, 4 quits. The maximum number of quits (96 cases) occurred in 1992, and the minimum number (17 cases) occurred in 1985.

Appendix 2
Regression Using Sample of Stayers

Here I discuss why I interpret the coefficients in expression (3.1) as moments (2.2) and (2.3). First, I rewrite (3.1) and rename d_1 and d_2 as d_1^c and d_2^c to make explicit that (2.4) and (3.1) differ because a constant is included in (3.1):

$$\Delta\log(w_{ijt}) = d_0 + d_1^c 1(D_{jt}^k > D_{j-1,t-1}^k) + d_2^c 1(D_{jt}^k < D_{j-1,t-1}^k) + \Delta\varepsilon_{ijt}. \quad \text{(A.1)}$$

The constant in model (A.1) captures wage growth among quitters who keep their consumption of disamenities constant. That is, d_0 may capture both quit-specific wage growth due to sorting and other time changes common to all workers. Ignoring for a moment the second component, the first one is due to quitters sorting into strictly better jobs. Under the assumptions of the model in Section 1, the first ("sorting") component is $E(\Delta\varepsilon_{ijt}|Q = 1, \Delta\varepsilon_{ijt} > 0)$. By definition, d_1^c in (3.1) equals

$$E(\Delta\ln w_{ijt}|Q = 1, D_1 > D_0) - E(\Delta\varepsilon_{ijt}|Q = 1, D_1 = D_0, \Delta\varepsilon_{ijt} > 0).$$

A similar expression for d_2^c can be obtained, reversing the inequality $D_1 > D_0$. I have not been able to sign d_1^c and d_2^c, as those parameters depend on the properties of the distribution of $\Delta\varepsilon_{ijt}$. Note, however, that d_1^c and d_2^c can be interpreted as the parameters in (2.2) and (2.3) when the "sorting" component $E(\Delta\varepsilon_{ijt}|Q = 1, D_1 = D_0, \Delta\varepsilon_{ijt} > 0)$ equals zero. To get a sense of the magnitude of the "sorting" component, I ran a regression using a sample of job stayers and quitters:

$$\begin{aligned}\Delta\log(w_{ijt}) = {}& a_0 + d_0 QUIT_{ijt} + \sum_{k=1}^{4} d_1^k 1(D_{jt}^k > D_{j-1,t-1}^k | QUIT_{ijt}) \\ &+ \sum_{k=1}^{4} d_2^k 1(D_{jt}^k < D_{j-1,t-1}^k | QUIT_{ijt}) + \gamma_1 X_{ijt} QUIT_{ijt} + \gamma_2 X_{ijt} + \Delta\varepsilon_{ijt}.\end{aligned} \quad \text{(A.2)}$$

a_0 is the expected wage growth of workers who stay in the same job and captures, among other factors, aggregate wage growth related to the business cycle. d_0 is the expected wage growth of a person who voluntarily leaves one job for another with the same observed level of disamenities, and is the parameter of interest in this Appendix. The interpretation for the other coefficients is similar to that for the coefficients in model (3.1), because the change in the level of disamenities is only observed for job changers.

I briefly summarize the results of that specification. When I do not include any control for disamenity changes, the estimate of $QUIT_{ijt}$ is .04 (.008). When indicators of disamenity changes are included, the coefficient on $QUIT_{ijt}$ drops to zero: –.002 (standard error, .0018). Selecting other years as the base year for the dummies or interacting time dummies with the quit intercept to account for differential wage increases among quitters (Devereux and Hart 2006) does not change the zero estimate of the coefficient on $QUIT_{ijt}$. Thus, I conclude that d_1^c and d_2^c can be interpreted as the empirical counterparts of (2.2) and (2.3).

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