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Discussion Papers

713

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Using Job Changes to Evaluate the Bias of the Value of a Statistical Life

Berlin, July 2007

IMPRESSUM

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http://www.diw.de

ISSN print edition 1433-0210
ISSN electronic edition 1619-4535

Available for free downloading from the DIW Berlin website.

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Abstract

This paper presents a new approach to obtain unbiased estimates of the value of a statistical life (VSL) with labor market data. Investigating job changes, we combine the advantages of recent panel studies, which allow to control for unobserved heterogeneity of workers, and conventional cross-sectional estimations, which primarily exploit the variation of wage and risk between different jobs. We find a VSL of 6.1 million euros from pooled cross-sectional estimation, 1.9 million euros from the static first-differences panel model and 3.5 million euros from the job-changer specification. Thus, ignoring individual heterogeneity causes overestimates of the VSL, whereas identifying the wage-risk tradeoff not only by means of between job variation (job-changer model) but also on the basis of noisy variation on the job (panel models) may lead to underestimates of the VSL. Our results can be used to perform cost-benefit analyses of public projects aimed at reducing fatality risks, e.g., in the domains of health, environmental or traffic policy.

JEL Classification: I10, J17, J28, K00

Keywords: Value of a statistical life (VSL), compensating wage differentials, work accidents, job changes

Acknowledgements:

We are grateful to Jason Shogren, Thomas Bauer, Thiess Büttner, Horst Entorf, Jochen Kluve, Patrick Puhani, and the participants of the session “Empirical Health Economics” at the annual meeting 2006 of the Verein für Socialpolitik in Bayreuth for useful comments. Furthermore we are grateful to Philip Savage, Daniel Langer, and Julia Bredtmann for research assistance as well as to Burkhard Hoffmann, Martin Butz, Wolfgang Jäger, Willi Standke, and Jürgen Strack for providing data on work accidents.

1. Introduction

Studies of the value of a statistical life (VSL) using labor market data are almost exclusively based on cross-sectional data and thus cannot account for unobserved heterogeneity of workers by exploiting the time dimension of the data set.[1] Only very recently, two panel studies - one for the US (Kniesner, Viscusi, Woock, & Ziliak, 2005) and one for Germany (Spengler, 2004) - have begun to fill this research gap. Although performed for different countries, the studies find very similar results: if unobserved heterogeneity is controlled for with suitable econometric techniques, such as fixed-effects or first-differences estimation, the VSL turns out to be at least 50% lower compared to pooled time series cross-section estimates. Not only do these results call the reliability of former VSL-studies into question, they also provide new evidence on the ongoing theoretical debate on the direction of the potential bias in VSL studies if unobserved individual heterogeneity or productivity is ignored. The existing evidence suggests that the influence of differences in workers' unobserved risk-related productivity, which Shogren and Stamland (2002) demonstrate to be a source of upward bias of the VSL, is empirically more important than general differences in unobserved productivity, which impose a downward bias (Hwang, Reed, & Hubbard, 1992).

Although making an important point concerning the direction of a po-

[1] Older studies investigating the VSL with US panel data controlling for unobserved heterogeneity failed to find significant effects of the fatal risk variable presumably because they have used too rough risk measures (an example is the study of Brown, 1980).

tential bias, the studies by Spengler (2004) and Kniesner et al. (2005) run the risk of potentially underestimating the VSL because a first-difference or within-transformation of the data leads not only to an elimination of time-constant individual heterogeneity, but also eliminates the cross-sectional variation, which reflects compensating wage differentials in a pure sense, e.g., the difference in wage and risk levels of a scaffolder and a secretary. What dominates the identification of the focal effect is the within-group variation, which stems from risk changes in a given occupation as time goes by and from job changes of a given worker to a riskier or more secure job. Whereas - putting aside selectivity problems of job changes - the latter variation is of the same quality as the eliminated cross-group information, the former is less likely to be suitable for identifying compensating wage differentials, because it cannot be simply assumed that for a given worker in a given job actual wage adjustments are made for small changes in risk (perhaps only observed by the researcher). More importantly, there might be spurious negative correlation between wage and risk if third variables (e.g. time dummies) do not fully capture the general trend towards higher real wages and less riskier jobs. Thus, focusing on job changes would be a promising approach to filter out that part of within-group variation which is presumably more suitable to identify compensating wage differentials and, at the same time, not to renounce controlling for unobserved heterogeneity.[2]

[2]To our knowledge the present paper is the first English language contribution assessing the VSL focusing on job changes. Job changes were first used by the authors to estimate the VSL in a German language paper based on different data (Schaffner & Spengler, 2005). In parallel working Kniesner, Viscusi, Woock, and Ziliak (2006) also present VSL estimates

With respect to our preferred (5-year) fatality risk measure we find point estimates for the VSL of 6.1 million euros on average from cross-sectional estimations, 1.9 million euros from the static first-differences panel model and 3.5 million euros from the job-changer specification. This result is in accordance with our assessment that focusing on job changers combines the advantages from cross-sectional and panel estimation. Moreover, the job-changer estimates are more robust with respect to the choice of the risk measure and, thus, less sensitive to measurement error in the main explanatory variable. Although we corroborate the findings of Spengler (2004) and Kniesner et al. (2005) that controlling for individual heterogeneity yields lower VSL-estimates compared to the conventional approach, we find smaller reductions than these studies.

The remainder of the paper is organized as follows: section 2 provides a brief introduction of the theory of compensating wage differentials, presents the econometric models employed in the empirical analysis and discusses misspecification issues. Section 3 presents our data sets and develops the fatality risk measures. The empirical results for conventional cross-sectional estimations, various panel-estimations and estimations for job changers follow in section 4. Section 5 concludes.

based on job changers and job changes.

2. Unbiased Estimation of the VSL with Labor Market Data

The labor market is the most prominent field to study tradeoffs between money and fatality risks in order to estimate the VSL. The underlying theory of compensating wage differentials (for details see Thaler and Rosen (1975) or Viscusi (1993)) is developed in a simple model in which the wage is exclusively determined as a function of the occupational fatality risk, with workers' indifference curves having a (usually increasing) positive slope indicating that higher risks are only accepted in exchange for higher wages, and firms' iso-profit curves having a (decreasing) positive slope indicating that higher levels of safety are only provided in exchange for wage reductions. In such a model the VSL equals the (positive) slope of the expansion path of points of tangency between workers indifference curves and firms iso-profit curves.[3]

The slope of the expansion path can be estimated using information on wages and risk available in suitable individual labor market data sets. In order to identify the wage-risk tradeoff, all other relevant factors have to be controlled for in a hedonic (i.e. quality-adjusted) wage regression. Such a

[3]This equality can be demonstrated with the following example: Assume the expansion path is linear (i.e., that the wage-risk tradeoffs are the same in all points of tangency between indifference and iso-profit curves) and contains the points ($p_1 = 0.0005$, $w_1 = 20,000$) for worker 1 and ($p_2 = 0.001$, $w_2 = 22,000$) for worker 2 with p representing risk and w indicating annual wage. These figures imply a slope of the expansion path of ($\frac{22{,}000-20{,}000}{0.001-0.0005}$ =) 4,000,000 and also that worker 1 (and also worker 2) would accept an increase in his fatality risk by $\frac{1}{100{,}000}$ (which we assume to be marginal) if and only if the employer would increase his wage by($\frac{1}{100{,}000} \times 4,000,000 =$) 40 €. Provided point ($p_1$, w_1) not only represents one individual but 100,000 workers, the latter would demand an aggregate amount of 4 million € in order to accept one almost certain additional case of death out of their group. Thus, the VSL and the slope of the expansion path are identical.

hedonic wage regression can be written as

$$w_i = \alpha p_i + \mathbf{x}_i \boldsymbol{\beta} + u_i, \tag{1}$$

where w_i is the wage of worker i, p_i stands for the workers occupational fatality risk, $\mathbf{x}_i$ represents a $1 \times K$ vector of personal characteristics of the worker as well as characteristics of his job, and u_i an error term. Among the coefficients of the equation, which are depicted in Greek letters, α is the one of central interest because it represents the slope of the wage-risk expansion path and, thus, the VSL. What is usually estimated in empirical VSL studies using cross sectional data, however, is not equation (1) but rather a modification which can be written as

$$\ln w_{ij} = \alpha^{CS} p_j + \mathbf{x}_{ij} \boldsymbol{\beta} + u_{ij}. \tag{2}$$

Apart from the purely technical fact that the dependent variable enters the equation as a natural logarithm in order to correct for outliers in the wage data, an index j representing the occupation and/or industry of a worker has been added to all variables. The fatality risk no longer has an index i. This accounts for the fact that worker specific risk information is usually not available to the researcher. Instead, mean risks by occupation and/or industry from official statistics are used as proxies. In order to obtain unbiased estimates for α^{CS} from equation (2) the approximation of the job risk perceived by the individual through aggregate data has to be accurate and p_j must be uncorrelated with u_{ij}. Provided the first condition can be fulfilled by using sufficiently detailed occupational/industry risk data, the validity of the second condition has been questioned by the theoretical papers of Hwang et al. (1992) and Shogren and Stamland (2002).[4]

Hwang et al. (1992) argue that unobserved differences in individual productivity, e.g. intelligence, motivation or ability to cope with pressure, may create a severe downward bias of estimations of the wage-risk tradeoff. If productivity differences are introduced into the model of compensating wage differentials, those workers with an above average productivity - among those workers sharing the same risk preference - would realize higher wages for a given risk level or lower risk for the same wage. Thus, productivity differences create a dispersion of wage-risk choices of workers away from the theoretical wage-risk expansion path. Provided this dispersion is strong relative to the desired wage-risk variation along the expansion path and a significant part of the productivity differences can not be observed by the researcher, coefficients of the risk variable in a hedonic wage regression will be biased against zero or even carry an unexpected negative sign.

[4]Apart from unobserved productivity the condition of uncorrelatedness between fatal risk measure and error term can also be violated if other unpleasant job characteristics like dirt, noise, heat or shift work are omitted as regressors. If these disamenities are positively correlated with the fatality risk and also compensated by the employer their omission would result in an upward bias of the effect of the fatality rate. Unfortunately, we can not control for these factors in the empirical analysis because they are not observed in our data set. We did run estimations in which besides the fatality rate the non fatal accident risk was included as an explanatory variable. Whilst in cross-sectional estimations we often obtained unexpected negative significant effects for the non-fatality risk with partly substantial increases of the coefficients of the fatality rate the effects in the panel and job changer models were small and mostly insignificant (results available on request). From this finding we conclude that the effect of the fatality risk is relatively robust to omissions of other occupational disamenities - at least as far as our preferred models are concerned.

In contrast to general productivity, Garen (1988) and Shogren and Stamland (2002) make *risk-specific* productivity a subject of discussion. Risk-specific productivity is a feature of workers which - for example as a result of cool-headedness or good physical agility - enables them to be more productive in hazardous professions than "normal" workers, whilst playing no role in nonhazardous jobs. Therefore, workers who are highly skilled in handling risk select themselves into more risky occupations in which they have a comparative wage advantage. If risk-specific productivity is not observed by the researcher, the positive correlation between risk-specific productivity and the fatality-risk measure on the one hand, and risk-specific productivity and wages on the other, will lead to an upward bias of the coefficient of the fatality-risk measure in a hedonic wage regression.

Usually, neither all facets of risk-specific productivity nor of productivity in general are observed in labor market data sets. Therefore, conventional cross-sectional studies have only produced unbiased VSL-estimates if the differently signed biases described above have exactly cancelled each other out. As this is unlikely to be the case, more refined estimation techniques and more informative data sets are needed. Shogren and Stamland (2006), for instance, propose a GMM approach on cross-sectional data which, however, is costly because the required data is not available an, thus, has to be especially collected. A less costly alternative is to exploit the panel structure of existing data sets.

As is well known, the fixed-effects and first-difference estimator allow to control at least for that part of unobserved heterogeneity which is constant over time. The random-effects model has the advantage that regressors may be time-invariant or weakly time-variant. Since our main regressor - the fatality rate - exhibits sufficient variation over time, this feature of the random-effects model is not decisive for the model choice.[5] It is rather the fact that the random-effects model requires the absence of correlation between the unobserved effect (which is part of the error term in the random-effects model) and the explanatory variables. Since this condition is obviously not met for the fatality rate the random-effects model is not suitable for our empirical analysis.

Deciding between the fixed-effects and the first-difference model for $T > 2$ (for $T = 2$ the models yield identical results) is not an easy task (see the pertinent discussion in Wooldridge, 2002). In the context of the present study, there are three reasons why we prefer the first-difference model. First, if the wage and risk variables are integrated of order one, first-differencing the data will render the series stationary and avoid the spurious regression problem. Second, if dynamic misspecification of the static fixed-effects model requires the inclusion of a lagged dependent variable as a regressor, first-differencing the data is necessary in any case (in combination with a suitable instrumentation of the lagged dependent variable) in order to obtain unbiased parameter estimates. Third, the first-difference model is technically equivalent to the

[5]Actually, the only time-constant variable in our empirical analysis is nationality.

job changer model presented below and, thus, more suitable for comparisons of VSL-estimates.[6] The first-difference model can be written as

$$\Delta \text{ln} w_{ijt} = \alpha^{FD} \Delta p_{jt} + \Delta \mathbf{x}_{ijt} \boldsymbol{\beta} + \boldsymbol{\lambda}_t + \Delta u_{ijt}, \tag{3}$$

where t is an index for the calendar year and Δ is the first-difference operator implying, for instance, that $\Delta \text{ln} w_{ijt} = \text{ln} w_{ijt} - \text{ln} w_{i,j-1,t-1}$, with $j-1$ representing the occupation of worker i in period $t-1$. The $1 \times T$ vector of time dummies $\boldsymbol{\lambda}_t$ is not differenced since first-differencing would have the implausible implication that all time effects are completely undone in the following year.

Controlling for individual heterogeneity by performing a first-difference (or within) transformation of the data comes at the cost of eliminating cross-sectional variation of variables and identification of parameters relying heavily on within-variation (i.e., variation of variables across time). Within-variation of the occupational fatality rate may be due to changes in risk on the job (i.e., in a given occupation) or changes in risk resulting from job changes. It is the former type of variation which may complicate our empirical task of accurately estimating the wage-risk-tradeoff. On the one hand, it is not plausible to assume that (presumably very small) year to year on-the-job changes in fatality risk - especially if they are not noticed by the workers or due to random rather than actual variations - will be reflected in

[6]In spite of our preference for the first-difference model, we also present the results from fixed-effects estimation.

systematic wage variations corresponding with the theory of compensating wage differentials. On the other hand the general tendency towards rising real wages (due to productivity gains) and falling fatality risks (due to continuous improvements in industrial safety) could create a spurious negative relationship between risk and wage if third variables in a regression do not fully absorb these trends.[7] If relevant, both sources of within-variation of risk on the job will bias estimates of the wage risk tradeoff downwards.

Another shortcoming of the first-difference (and fixed-effects) estimator is that only time constant individual heterogeneity can be controlled for. This may become a problem if the panel is rather long (as in our study, where $T = 11$) since only few unobserved characteristics of the person can be perceived to be really time constant - one example being intelligence. Others however, like motivation or ability to cope with risk may well change over time. As a consequence, we propose a model making lower demands on the time constance of individual heterogeneity and circumventing the problem of "unwanted" within variation of risk on the job. This model, which can be written as

$$\Delta \ln w_{ijt} = \alpha^{JC} \Delta p_{jt} + \mathbf{z}_{ijt}\boldsymbol{\gamma} + \boldsymbol{\lambda}_t + \Delta u_{ijt}, \quad j \neq (j-1), \tag{4}$$

differs from the first-difference model with respect to only considering ob-

[7] In our data set the mean real gross wage per day (in prices of 2004) of male blue collar workers increased from 78.00 € in 1985 to 83.70 € in 1995, whilst the mean occupational fatality rate decreased from 10 to 8 deathly work accidents per 100,000 fulltime man years over the same period.

servations for which $j \neq (j-1)$, i.e., the focus is on observations from consecutive years / cutoff dates $t-1$ and t for which different occupations are reported for worker i. Moreover, $\Delta\mathbf{x}$ is replaced with a more informative vector of control variables $\mathbf{z}$.[8]

Exclusively considering the more promising wage-risk variation across jobs and controlling for unobserved heterogeneity, the job changer model combines the advantages from the cross-sectional and first-difference model. Actually, the former model is an even more promising approach to handle unobserved heterogeneity than the latter, since time constance of unobserved effects is only required for those years in which a job change takes place.[9] Furthermore, since repeated observations for individual workers are the exception in the job changer model, it is likely to be less affected by problems of dynamic misspecification than conventional panel data models.[10]

From the preceding discussion we can capture a working hypothesis concerning the size of the risk coefficients from different estimators. Since potential downward biases of the risk effect from first-difference estimation are not relevant for the job changer model, we expect $\alpha^{JC} > \alpha^{FD}$. Whether estimates considering individual heterogeneity will be smaller ($\alpha^{CS} > \alpha^{JC} > \alpha^{FD}$) or

[8]Whilst, for instance, $\Delta\mathbf{x}$ would contain a composite indicator for the change of marital status (taking on the value "1" in the case of marriage, "-1" in the case of divorce and "0" otherwise) $\mathbf{z}$ contains separate indicators for marriage and divorce and, therefore, allows for more flexibility of the model.

[9]Even if a worker has changed his job several times in the sample period individual heterogeneity may change over time as long as it is constant for the respective pairs of cutoff dates on the basis of which the differences in equation 4 are calculated.

[10]In our sample 18,000 workers perform 23,000 job changes. In contrast to this the conventional panel estimates are based on more than 500,000 observations from 88,000 workers (see Tables 2 and 6).

larger ($\alpha^{JC} > \alpha^{FD} > \alpha^{CS}$) than conventional cross-sectional estimates, however, remains an open question which can only be determined empirically.

3. Data and the Fatality Risk Measure

A labor-market data set, on the basis of which the VSL is to be examined, must contain reliable wage information and as many variables as possible which have a potential influence on wages. Apart from variables like age, gender and education, that are usually contained in labor-market datasets, further factors influencing wages - which have been discussed in the preceding section - are, for instance, intelligence, motivation and coolheadedness. Standard labor market data generally have no information on the latter indicators. In order to be able to take account of the time constant components of these unobserved variables, *panel data* is helpful. The most important explanatory variable in a VSL study is the individual's fatality risk in the workplace. As such a variable is usually also not included in a labor-market data set, the latter must at least feature suitable *interfaces* by means of which aggregated risk data from other sources may be incorporated. Where it is the case that indicators of occupation (and/or industrial sector) can be found both in the labor-market and in the risk data set and where these indicators possess compatible characteristic values in both data sets, the individual labor market data and the aggregated risk data can be merged. In this section we first present our labor market data followed by an introduction of the occupational risk data. Finally, we describe how both data sources are combined and the fatality risk measure is calculated.

3.1 Labor Market Data

Our labor market data set is the IABS - the Employment Subsample of the Institute for Employment Research (IAB)[11], which is available for researchers as a scientific use file. The IABS is a 1% random sample of German employees subject to compulsory social security contributions. It covers the period 1975–1995 including 560,000 individuals in total and approx. 200,000 individuals per year.[12] Not included in the IABS are the self-employed, civil servants, judges, professional soldiers, military and community-service conscripts, the marginally employed, full-time students and unpaid family workers, because none of these groups is subject to compulsory social security contributions. According to the data description by Bender and Haas (2002, pp. 8), in 1995 employment statistics on the base of which the IABS is randomly selected covers approx. 79% of the working population in West-

[11] The Institute for Employment Research (Institut für Arbeitsmarkt und Berufsforschung [IAB]) is the research institute of the German Federal Employment Service (Bundesagentur für Arbeit).

[12] There also exists a more current version of the IABS covering the period 1975–2001 (IABS regional sample). Apart from additional years, this sample does also contain regional information on the county level (the only regional information contained in the 1975–1995 sample is a dummy variable distinguishing East and West Germany.) This additional information comes at the cost of significantly reducing the number of characteristic values of some variables in order to avoid identification of anonymized individuals. The most crucial impact of this security policy for the present analysis is the reduction of characteristic values of the occupation variable from 335 (1975–1995 sample) to 130 (1975–2001 regional sample). Since our aim is to achieve the best possible approximation of individual occupational risk with mean risks by occupation, we prefer more detailed occupational to more detailed regional information.

Germany, and approx. 86% of the working population in East-Germany. For all employees covered by the employment statistics, the following personal and professional characteristics are available: gender, year of birth, marital status, number of children, citizenship, exact start and end date of an occupation, schooling and job training, professional status (including details on full and part-time occupation), gross wage, industry of the employer and total number of employees of the employer. Finally, a variable indicating the occupation of a worker is included. This variable is coded according to the 3-digit classification of occupations of the Federal Statistical Office from 1975, which normally has 334 characteristic values. In order to avoid identification of individuals some occupations are summarized in the IABS reducing the number of characteristic values to 275.

Apart from its large sample size and panel structure, another important feature making the IABS suitable for investigating compensating wage differentials is its high quality of information concerning dates of commencement and cessation of employment and, most importantly, gross wages. This is a result of the IABS being a *process-produced* data set, i.e. a data set based on employers' declarations within the framework of compulsory social-insurance registration.[13] However, not all variables included in the IABS are of high

[13]The German social security registration procedure is organized as follows: Employers are obliged to report information about their employees - above all exact dates of commencement and cessation of employment and gross wages but also supplementary information (see above) - on a regular basis to the three main branches of the social security system so that entitlements to sickness, pension, unemployment and other social benefits can be determined. At first, the information is sent to health insurance companies, who forward it to the pension insurance who, finally, forward the data to the unemployment insurance. Since in contrast to health and pension insurance, unemployment insurance

quality. Erroneous information is most likely to appear in the case of variables having no direct relevance to social security contributions (e.g. marital status, number of children, education) and which are reported for statistical purposes only by company staff who do not always take due care. Since there is no sign of selectivity in the errors of these variables they should at most cause a bias of the corresponding coefficients against zero in hedonic wage regressions.[14]

Panel attrition, which is a problem in labor market data sets based on surveys, does also affect the IABS but in a different way. The IABS does not lose those individuals who are simply unwilling to participate in interviews but rather those who leave jobs which are subject to compulsory social security contributions (without changing to registered unemployment) or who lose their eligibility for unemployment benefits (without being reemployed in a job subject to compulsory social security contributions). Examples of panel attrition in the IABS are people becoming self-employed or civil servants or who fall out of the labor force. Besides being restricted to the workforce subject to compulsory social-insurance contributions, further disadvantages of the IABS are to be found in the censoring of wage figures at the assessment

is centralized in one organization, the Federal Employment Agency holds records on all individual employment spells in Germany as far as they concern times of legal occupation subject to social insurance contribution. These records in combination with information about times of registered unemployment (also centrally maintained by the Federal Employment Agency) form the basis of the IABS.

[14]Number of children, however, is of such a bad quality that Bender, Hilzendegen, Rohwer, and Rudolph (1996) in their data description recommend its exclusion from empirical studies.

ceiling.[15] Overall, approximately 10% of the wage figures in the IABS are censored. This percentage becomes substantially lower if, as in the following analysis, the focus is on male blue-collar workers, for whom only 2% of the wage figures are censored.

The individual level data of the IABS consist of employment (and unemployment) spells. Spells are defined by the beginning and the end of a person's employment with a specific employer/firm, or by beginning or end of an period of unemployment. For some workers (the sum of) these spells may cover the whole sample period (1975–1995), for others only few and short spells may be observed. In the empirical analysis weuse an (unbalanced) yearly panel data set for the period 1985–1995 with the last day of the year (December 31) being the cut-off date.[16] Thus, the processed data set contains a maximum of one observation per year for each individual and a maximum of 11 observations for each individual across the sample period 1985–1995.

[15]The assessment ceiling defines a wage level up to which contributions to social security have to be paid proportional to the actual wage. The assessment ceiling is subject to annual adjustment. For wages exceeding the actual assessment ceiling in a given year the quantity of social security contributions equals the social security contribution rate times the assessment ceiling. Since pension insurance has the highest threshold levels among all social security insurances, the wage figures in the IABS are censored at that threshold level which was 33,132 euros in 1985, 47,857 euros in 1995 and 62,400 euros in 2005 (in prices of the respective year) (Wikipedia, 2006).

[16]We have to restrict the analysis to the period 1985–1995 because of the limited availability of risk data (see next subsection) .

3.2 Occupational Accident Data

As the IABS contains no data on the individual risk of suffering injury to life and limb in the course of working activities, information about occupational accidents has to be obtained from a different source. To this end, the statutory regulations on social insurance can be again availed of. Whilst the IABS is a result of the registration procedure for health, pension, and unemployment insurance, representative data on occupational accidents may be gleaned from the remaining branch of social insurance - statutory accident insurance (SAI). All accidents at the workplace which lead to an employee's death or inability to work for more than three days must be reported by the employer to the risk-bearing SAI corporation. A decisive feature of this reporting process is that in addition to the personal information about the accident victim and the circumstances of the accident the occupation of the victim is also recorded, in fact - with the exception of the agricultural sector - according to the same coding scheme as in the IABS.

There exist 80 German SAI corporations which are organized in three umbrella organizations covering the industrial, public and agricultural sector, respectively. Since the umbrella organizations collect the micro data of all their associated SAI corporations, they were the suitable addresses for our data request. The industrial as well as the public sector SAI umbrella organization could provide annual data for fatal and non-fatal work accidents aggregated with respect to the 3-digit occupational code covering the period

1985–1995.[17]

In keeping with the other social-insurance branches, the SAI does not cover the entire workforce. Once again, excluded are civil servants, judges and soldiers, whose risk of accident at work is safeguarded directly by their employer, which is the (federal) state. The self-employed outside the agricultural sector are equally exempt from statutory insurance. They may insure themselves in the SAI on a voluntary basis. According to the umbrella organization of the industrial SAI corporations, 1.43 mill. self-employed persons were insured in its domain as of 31.12.2002 (HVBG, 2003), corresponding to a share of approx. 40% of all self-employed in Germany. On account of this partial registration of the self-employed, the SAI covers a greater section of the labor force than other branches of social insurance and, thus, the IABS.

3.3 Fatality Risk Measure and Estimation Sample

In order to obtain an occupational risk measure for the subsequent empirical analysis, it is necessary to put absolute occupational accident frequencies (from the SAI) in relation to figures indicating the quantity of employment in the respective professions. We, therefore, define our fatality risk measure

[17] Data was also made available by the agricultural SAI umbrella organization. This data, however, could not be used in the empirical analysis because the internal coding scheme of the agricultural SAI corporations is not compatible with the classification of occupations of the Federal Statistical Office. As a consequence, we had to exclude all observations from individuals in agricultural occupations from our analysis.

as follows:

$$p_{jt} = \frac{Number\ of\ Fatal\ Work\ Accidents_{jt}}{Number\ of\ Fulltime\ Equivalent\ Workers_{jt}}. \tag{5}$$

The information needed to assess the denominator of the equation can be calculated using the original (i.e. unprocessed) version of the IABS because it contains the exact duration of employment spells (number of days) of workers in a specific occupation and year as well as information on part-time work. Aggregating working days adjusted for daily working hours by occupation yields the occupation specific working quantity according to the IABS which - since the IABS is a 1% random sample of the entire German working population - must simply be multiplied by 100 and divided by 365 in order to obtain the number of fulltime equivalent workers.

Table 1 depicts the correspondingly calculated fatality risks with respect to the 10 most dangerous occupations in Germany - excluding occupations in the agricultural sector and occupations (mainly) exercised by workers not subject to compulsory social security benefits (e.g. policemen, firefighters, judges, attorneys of state, teachers, entrepreneurs). Allmost all of these occupations are construction, navigation or mining occupations. In the subsequent section we use this indicator as an explanatory variable in hedonic wage regressions in order to determine the VSL.

Table 2 presents the summary statistics for our estimation sample including all variables entering the estimations with the exception of 21 industry dummies. Like many other VSL studies we restrict our sample to male blue collar workers. We do this for for two reasons. First, for this subgroup the actual relevance of compensating wage differentials is most obvious.[18] Second, concentrating on male blue collar workers has the advantage that the problem of wage censoring in the IABS can be neglected, as only two percent of the blue collar workers earn wages figures high enough to be censored. Although restricted to male blue-collar workers, the estimation sample is still very large, including more than half a million observations from 88,000 workers.

4. Empirical Results

In this section we provide the estimation results of the hedonic wage regressions from equations 2 to 4 for the coefficients of the fatality risk measure ($\hat{\alpha}$) together with the implied VSL. Since hedonic wage models usually follow a semi-logarithmic specification the VSL can only be evaluated at specific points of the wage distribution. We follow the convention of reporting the VSL at the mean wage. Moreover, assessing the VSL from $\hat{\alpha}$ the dimensions of the fatality risk measure (fatality risk per 1,000 workers in our case) and of the wage variable (*daily* gross wage in our case) have to be taken into

[18]Work related health risks for white collar workers - such as stress related heart attacks of managers or radiation induced cancer of pilots and flight attendants - may also be significant but are usually much more difficult to measure than workplace accidents.

account. This results in the following formula for the VSL:

$$\overline{VSL} = \frac{\partial w}{\partial p} = \hat{\alpha} \times \bar{w} \times 365 \times 1,000. \quad (6)$$

In addition to estimations with the contemporary fatality rate according to equation 5, we also perform estimations with an alternative risk measure (5-year-fatality rate) which has the advantage of being more robust to normal fluctuations or outliers in the incidence of fatal occupational accidents and may, thus, be a more precise approximation of the actual danger at the workplace.[19] The 5-year fatality rate is an uncentered moving average calculated as $p_t^5 = \frac{1}{5}\sum_{t-4}^{t} p_t$. Since we need the actual through the four year lagged value of p_t in order to construct p_t^5, the estimation period is reduced to 1989–1995.

Table 3 shows a summary of conventional VSL estimates based on cross-sectional and pooled estimation. Cross-sectional estimates with the contemporary (annual) fatality rate turn out to have a wide range (3.3–8 million euros) and a median of 4.4 million euros which, not surprisingly, is very close to the VSL from the pooled estimation for the whole sample (4.5 million euros). VSL estimates based on the alternative risk measure exhibit less variation (4.5–7.6 million euros) and a higher median (6.4 million euros). This result suggests that the contemporary fatality rate is a less precise approximation of the actual (individual) risk at the workplace than a measure

[19]Since deathly workplace accidents are rare events small changes in absolute numbers (especially in occupations with relatively few workers) may cause substantial fluctuations in the fatality rate.

combining and smoothing fatality rates from various years.[20]

The fixed-effects and first-difference panel estimates controlling for unobserved worker heterogeneity are reported in Table 4. In these models the cross-sectional differences in the levels of the variables are canceled out and the estimation of the coefficients relies mainly on within worker variation. In this case the influence of measurement errors in the risk variable becomes much larger compared with cross-sectional estimates. The VSL derived from estimates with the contemporary risk measure (ranging from 0.2 to 1.5 million euros) are much smaller than the estimates using the 5-year fatality rate (ranging from 1.6 to 2.8 million euros).

The fact that each point estimate reported in Table 4 (regardless of the choice of risk measure) is substantially smaller than the corresponding result from pooled estimation in Table 3 is a first hint that ignoring unobserved worker heterogeneity leads to overestimates of the VSL. However, most of the results displayed in Table 4 should be interpreted with caution. Fixed-effects estimates, for instance, might be biased if variables are non-stationary.[21] A further source of inconsistency - first discussed by Nickell (1981) - arises if a lagged dependent variable is added as a regressor, as in the dynamic fixed-effects model. The Nickel bias might be tackled by instrumental variable estimation techniques proposed by Anderson and Hsiao (1997) and Arellano

[20]It is interesting to note that the median of the VSL estimates with the 5-year fatality rate is not far away from the median (of $7 million) from 30 US cross-sectional labor market based VSL studies surveyed by Viscusi and Aldy (2003).

[21]Indeed, there exists evidence that wages in Germany follow an I(1) process (Breitung & Meyer, 1994).

and Bond (1991). However, none of our Anderson-Hsiao or Arellano-Bond estimations passed the test of over-identifying restrictions implying that the (necessary) instrumentation of the lagged dependent variable is invalid.[22]

As a consequence the static first-difference estimator might be the best choice since, due to first-differencing of the data, I(1) series are turned into I(0) series. The quality of the estimation results might nevertheless be questionable if the model is subject to dynamic misspecification. If - as in our case - tests of autocorrelation of the residual yield significant results, the inclusion of a lagged dependent variable as an additional regressor might be in order. This, however, confronts us with the dynamic panel estimators mentioned above and their specific problems.

Apart from the problem to determine the best alternative among a variety of panel estimators, there might be a more general problem of investigating compensating wage differentials with panel data. This problem can best be demonstrated by means of Table 5. In the upper panel of the table we present results from panel estimations exclusively based on workers who did not change their job in the IABS. As can be inferred from the small negative and mostly insignificant coefficients, the within variation of wage and risk on the job is not systematic and obviously biases the VSL downwards. Evidence for this presumption can be directly gathered from the comparison of the results from Table 4 with the results from the lower panel of Table 5. The latter stem

[22] The dynamic first-difference estimates reported in Tables 4 and 5 were obtained using the Arellano-Bond technic.

from a sample restricted to workers who have experienced at least one job change. Restricting the sample in this way removes a substantial fraction of noisy within variation and substantially increases the VSL-estimates across all specifications. The noisy within variation is due to variation in risk which results of technological progress but also of measurement error. On the other side real wages vary across time (there is a rise in real wages over time). These variations are not based on the changes in the risk variable. It is remarkable that the increase in the VSL is stronger for the estimates based on the contemporary risk measure compared with the estimates based the 5-year fatality rate. Obviously, the bias of measurement error and this independent variation of risk and wages is higher using the contemporary risk.

Although the estimates presented in the lower panel of Table 5 are exclusively based on those individuals who changed their job at least once there remains a larger number of observations which stems from years in which the workers remained in the same job. The wage/risk variation contained in these (pairs of) observations may still be a source of bias. Consequently, we go a step further and restrict our analysis to pairs of observations surrounding a job change of a certain worker. On the basis of these observations we create first differences of the variables (see equation 4) analogous to the first-differences panel models. There remain 24,296 observations (differences) of almost 18,000 workers in the new estimation sample. Selected summary statistics are presented in Table 6. These job changes are associated with a mean real pay increase of almost 3 euros. 60% of the job changes are performed by workers aged 20 to 35, while only 42% of the whole sample (see Table 2) are in this age group.

In addition to estimating the model of job changes with the lagged fatality rate and suitable representations of the other time-variant variables, we also add the levels of some variables like age, nationality and work experience (which are either time-invariant or have time-invariant differences) to the set of regressors. We also add time dummies and an indicator if there was a period of unemployment between two observations surrounding a job change. Our decision to include these additional variables is based on the fact that job changes have very different reasons which we do not directly observe in our data set. For instance, it is straightforward to assume that advancements in jobs are involved with wage increases while job changes because of dismissals do not result in significant wage increases or even in wage decreases. Another reasonable assumption may be that job changes of younger workers are more often aimed at salary increase whilst changes of older workers might more often be driven by the threat of unemployment or early retirement programs.

The estimated coefficients and the corresponding VSL of the regressions exclusively based on job changes are presented in Table 7. The point estimates of the VSL equal 2.8 million euros for the contemporary and 3.5 million euros for the 5-year fatality rate. These results have three implications. First, removing the remaining within variation which is not based on job changes and which is potentially noisy does further increase the VSL confirming our hypothesis from Section 2 that $\alpha^{JC} > \alpha^{FD}$. Second, we provide further evidence that it is rather an overestimation than an underestimation of the VSL which results if unobserved individual heterogeneity is not controlled for (i.e., $\alpha^{CS} > \alpha^{JC} > \alpha^{FD}$). Finally, the downward bias of the VSL due to measurement error in the risk variable is larger in specifications relying more heavily on within variation which is not caused by job changes.

5. Conclusion

Controlling for unobserved heterogeneity in hedonic wage studies of the value of a statistical life (VSL) has not played a role for a long time. Only very recently two panel studies (Spengler, 2004 and Kniesner et al., 2005) were successful in controlling for time constant components of unobserved heterogeneity using random effects, fixed-effects and first-difference estimators. The implied VSL from these studies turns out to be at least 50% lower than conventional cross-sectional estimates. This result might be explained by the dominance of an upward bias of the VSL due to neglecting risk related skills / productivity (see the theoretical work of Shogren & Stamland, 2002) relative to a downward bias inflicted by unobserved productivity in general (see the theoretical work of Hwang et al., 1992).

Performing first-difference estimations exclusively on the basis of pairs of observations surrounding a job change of a certain worker, this paper presents an alternative approach to control for unobserved heterogeneity, which we assess to be favorable to standard panel estimators for at least three reasons - especially if the time dimension of the data set is relatively long (e.g. if $T = 11$, as in the present study). First, identification of compensating wage differentials does not hinge on (obviously) noisy within worker variation of wage and risk on the job. Second, in order to be fully controlled for, this individual productivity is only required to be constant for the year of a job change. Third, issues of dynamic misspecification of the econometric model are largely irrelevant.

With respect to our preferred (5-year) fatality risk measure, we find point estimates for the VSL of 3.5 million euros from the job-changer specification, 1.9 million euros from the static first-differences panel model and an median of 6.4 million euros from conventional cross sectional estimations. Thus focusing on job changes yields substantially higher VSL estimates than the most closely paralleling panel model. We attribute this result to the omission of within variation, i.e. variation of wage and risk which is unspecific with respect to the model of compensating wage differentials and which cannot be controlled for with third variables. Moreover, the estimates based on job changers/changes are more robust with respect to the choice of the risk measure and, thus, less sensitive to measurement error in the focal explanatory variable.

Although we corroborate the findings of Spengler (2004) and Kniesner et al. (2005) that controlling for individual heterogeneity yields lower VSL-estimates compared with the conventional cross-sectional approach, we find that the reduction is at most 50% rather than at least 50% because estimates exclusively based on job changes yield higher VSL than conventional panel estimates. Our results can be used to perform (more reliable) cost-benefit-analysis of public projects aimed to reduce fatality risks, e.g., in the domains of health, environmental or traffic policy.

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Table 1: Annual occupational fatality risk per 1.000 fulltime-equivalent-man-years of the 10 most dangerous occupations, 1985-1995

Rank	Occupation (3-digit)	Mean	Std. Dev.	Min.	Max.
1	Scaffolders	0.798	0.364	0.186	1.56
2	Inland waters navigator / Sundry waterways occupations	0.714	0.249	0.348	1.10
3	Deckhands	0.681	0.428	0.135	1.37
4	Nautical navigators	0.513	0.334	0.155	1.07
5	Roofers, slaters	0.418	0.128	0.179	0.610
6	Miners	0.361	0.132	0.146	0.653
7	Machine, electrical and shot colliers	0.331	0.254	0.000	0.945
8	Air traffic occupations	0.290	0.225	0.000	0.732
9	Blasters / Sundry civil engineering occupations	0.277	0.069	0.125	0.404
10	Excavator drivers	0.267	0.097	0.110	0.406

Table 2: Selected Summary Statistics

	Mean	Standard Deviation
Gross daily wage (in euro from 2004)	81.5	20.4
Log(gross daily wage)	4.37	0.280
Married	0.579	0.494
German	0.875	0.332
Age groups		
15–20	0.016	0.124
20–25	0.123	0.328
25–30	0.159	0.366
30–35	0.137	0.344
35–40	0.116	0.320
40–45	0.106	0.308
45–50	0.115	0.319
50–55	0.122	0.328
55–60	0.085	0.279
60–70	0.021	0.142
With any vocational qualification	0.726	0.446
Professional position		
Non-skilled worker	0.371	0.483
Skilled worker	0.576	0.494
Master craftsman/Foreman	0.053	0.224
Job Tenure (in years)	11.4	5.21
Job Tenure squared	156	116
Job Tenure potentially censored	0.483	0.500
Work experience (in years)	7.49	5.85
Work experience squared	90.4	108
Work experience potentially censored	0.211	0.408
Unemployment preceding in the current year	0.062	0.241
Number of employees		
≤ 9	0.115	0.319
10–19	0.086	0.281
20–49	0.123	0.329
50–99	0.097	0.297
100–499	0.239	0.426
500–999	0.090	0.286
≥ 1.000	0.249	0.432
Fatality rates per 1.000 fulltime-man-years		
Annual fatality rate	0.083	0.096
5-year fatality rate	0.082	0.087
Number of observations	553,862	
Observations with censored wage	0.021	
Number of workers	88,115	

Table 3: Summary of values of a statistical life from cross-sectional and pooled estimations

	Annual fatality rate (1985-1995)	5–Year fatality rate (1989-1995)
Cross-sectional estimations		
Range	3.32 – 8.00	4.50 – 7.64
Mean	4.84	6.26
Median	4.43	6.37
Pooled estimation	4.50	6.07

Notes: The table displays VSL-estimates in million euros. All underlying coefficients of fatality rates are significant at the 1%–level.

Table 4: Values of a statistical life from various panel estimations

	Annual fatality rate (1985-1995)		5-Year fatality rate (1989-1995)	
	$\hat{\alpha}$	VSL	$\hat{\alpha}$	VSL
Static Fixed-Effects Estimates	0.050	1.49	0.092	2.77
	(0.003)		(0.007)	
Dynamic Fixed-Effects Estimates				
Short-Run Effect	0.020	0.62	0.060	1.86
	(0.003)		(0.007)	
Long-Run Effect	0.032	0.97	0.082	2.54
	[0.000]		[0.000]	
Static First-Difference Estimates	0.007	0.23	0.060	1.87
	(0.003)		(0.015)	
Dynamic First-Difference Estimates				
Short-Run Effect	0.006	0.17	0.051	1.61
	(0.003)		(0.009)	
Long-Run Effect	0.009	0.28	0.083	2.63
	[0.102]		[0.000]	

Notes: Standard errors are recorded in parentheses and p-values of the null-hypothesis that the long-run effect is zero are recorded in square brackets. VSL is the implied value of a statistical life (in 1 million euros) calculated as $\hat{\alpha} \times$ *mean annual income.*

Table 5: Values of a statistical life from various panel estimations

	Annual fatality rate (1985-1995)		5-Year fatality rate (1989-1995)	
	$\hat{\alpha}$	VSL	$\hat{\alpha}$	VSL
Employees *without* job change				
Static Fixed-Effects Estimates	-.0004	-.01	-.0077	-.24
	(.0043)		(.0144)	
Dynamic Fixed-Effects Estimates				
Short-Run Effect	-.0100	-.31	-.0084	-.26
	(.0038)		(.0128)	
Long-Run Effect	-.4851	-.49	-.0115	-.36
	[.0078]		[.5130]	
Static First-Difference Estimates	-.0084	-.26	-.0136	-.43
	(.0030)		(.0175)	
Dynamic First-Difference Estimates				
Short-Run Effect	-.0062	-.20	-.0052	-.17
	(.0041)		(.0201)	
Long-Run Effect	-.0097	-.31	-.0086	-.27
	[.1331]		[0.794]	
Employees *with* job change				
Static Fixed-Effects Estimates	.092	2.64	.113	3.35
	(.006)		(.009)	
Dynamic Fixed-Effects Estimates				
Short-Run Effect	.048	1.42	.076	2.29
	(.005)		(.009)	
Long-Run Effect	.074	2.21	.103	3.11
	[.000]		[0.00]	
Static First-Difference Estimates	.026	.78	.073	2.22
	(.006)		(.017)	
Dynamic First-Difference Estimates				
Short-Run Effect	.020	.60	.062	1.90
	(.006)		(.012)	
Long-Run Effect	.032	.98	.100	3.10
	[0.001]		[.000]	

Notes: Standard errors are recorded in parentheses and p-values of the null-hypothesis that the long-run effect is zero are recorded in square brackets. VSL is the implied value of a statistical life (in 1 million euros) calculated as $\hat{\alpha} \times$ *mean annual income.*

Table 6: Selected Summary Statistics for Job Changers

	Mean	Standard Deviation
Difference gross daily wage (in euro from 2004)	2.93	14.5
Difference Log(gross daily wage)	0.045	0.233
Change in marital status		
Marriage	0.083	0.276
Divorce	0.042	0.201
Acquirement of vocational training	0.093	0.290
Advancement to ...		
... Skilled worker	0.125	0.331
... Master craftsman	0.023	0.149
Relagation to ...		
... Unskilled worker	0.146	0.353
... Skilled Worker	0.005	0.071
Characteristics of person (no changes)		
German	0.852	0.356
Age groups		
15–20	0.014	0.118
20–25	0.195	0.396
25–30	0.237	0.425
30–35	0.164	0.370
35–40	0.111	0.314
40–45	0.084	0.278
45–50	0.080	0.271
50–55	0.070	0.255
55–60	0.039	0.193
60–70	0.007	0.084
Work experience (in years)	9.89	5.06
Work experience squared	123	106
Work experience potentially censored	0.327	0.469
Unemployment preceding new job	0.174	0.379
Change in fatality rates per 1.000 fulltime-man-years		
Annual fatality rate	-0.003	0.126
5-year fatality rate	0.000	0.110
Number of observations	24,296	
Observations with censored wage	0.014	
Number of workers	17,916	

Table 7: Estimates of the wage-fatal risk tradeoff for job changers

	Annual fatality rate (1985-1995)		5-Year fatality rate (1989-1995)	
	$\hat{\alpha}$	VSL	$\hat{\alpha}$	VSL
All job changers	0.099	2.79	0.121	3.46
	(0.012)		(0.019)	

Notes: Robust standard errors in parentheses. VSL is the implied value of a statistical life (in 1 million euros) calculated as $\hat{\alpha} \times$ *mean annual income* .