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Working Paper No. 1505
October 2015

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The Effect of Income on Mortality – New Evidence for the Absence of a Causal Link[*]

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October 2015

Abstract

We analyze the effect of income on mortality in Austria using administrative social security data. To tackle potential endogeneity concerns arising in this context, we estimate time-invariant firm-specific wage components and use them as instruments for actual wages. While we do find quantitatively small yet statistically significant effects in our naïve least squares estimations, IV regressions reveal a robust zero-effect of income on ten-year death rates for prime-age workers, both in terms of coefficient magnitude and statistical significance. These results are robust to a number of different sample specifications and both linear and non-linear estimation methods.

JEL Classification: J14, J31, I10.
Keywords: Income, health, mortality, wage decomposition.

[*]We thank René Böheim, Ana Rute Cardoso, Wolfgang Frimmel, Martin Halla, Jörg Heining, Øystein Kravdal, Gerald Pruckner, Bernhard Schmidpeter, Tom Schober and seminar participants in Mannheim, Linz, and at the WUWAETRIX[3] in Vienna for valuable comments. We also thank Franz Eder and Mario Schnalzenberger who provided help with the data. *Email addresses:* alexander.ahammer@jku.at, Thomas.Horvath@wifo.ac.at, rudolf.winterebmer@jku.at. Financial support from the Christian Doppler Laboratory on Aging, Health, and the Labor Market is gratefully acknowledged.

1 Introduction

The positive correlation between income and health or longevity is a well-documented empirical fact.[1] Whether this correlation also reflects a causal relationship is indeed another question: both reverse causality and unobserved confounding variables may pose problems in empirical analyses. The former arises when bad health affects the choice of occupation, reduces work effort or labor force participation, and thus results in lower wages (Currie and Madrian, 1999). Omitted variable bias, on the other hand, may be caused by unobservable factors such as genetics, parental income, social background or heterogeneity in individual time discount factors that influence both income and health (Frijters *et al.*, 2005).

In this paper, we study the causal effect of labor income on mortality using Austrian social security data. In order to tackle potential endogeneity concerns, we use firm-specific wage components as instruments for current labor income of workers employed in these firms.[2] Firm rents are estimated from a wage decomposition proposed by Abowd *et al.* (1999), where annual labor income is decomposed into time-varying productivity components as well as time-invariant worker fixed-effects and firm fixed-effects. Similar decompositions have recently been used to explain the German wage structure (Card *et al.*, 2013b) or rent-sharing and hold-up problems in Italy (Card *et al.*, 2014). We take particular care to test whether the necessary exogenous mobility conditions are met in our data.

Theoretically, the Grossman model of health production (Grossman, 1972a,b) predicts that higher wage rates lead to increasing investments into health-related goods. Pathways how higher income might trigger better health and thus lower mortality include, for instance, access to the health care system (Schoen *et al.*, 2010), better knowledge about treatments (Kenkel, 1991) and, in particular, stronger adherence to therapies (Goldman and Smith, 2002), less involvement in risky behaviors (Adler *et al.*, 1994) or status-related stress, as has been shown in the Whitehall studies (Marmot, 2002). On the other hand, when higher income comes at the expense of increased work-pace or psychological stress, it could also lead to *higher* mortality (Adler *et al.*, 1994; Kivimäki *et al.*, 2002). Moreover, alcohol and illegal drug consumption

[1] Some of the more prominent papers examining the correlation between income and mortality include Kitagawa and Hauser (1973), Duleep (1986), or Deaton and Paxson (1998).

[2] See Shea (2000) or Pischke (2011) for earlier applications of such an idea.

have been shown to be pro-cyclical with respect to income streams as well (Dobkin and Puller, 2007).

Other empirical studies striving to investigate causality in the relationship between income and mortality include, for example, Lindahl (2005) who uses lottery prizes as an instrument for labor income among Swedish lottery players, or Schnalzenberger (2011) who analyzes income shifts stemming from disability pension reforms in Austria. Both papers do not find a significant effect of income on mortality. Based on quasi-experimental evidence from the U.S. social security notch, Snyder and Evans (2006) even report an inverse effect as higher income leads to higher mortality in their empirical framework – a finding which is confirmed by Evans and Moore (2011).

Studying the income-health gradient in Austria is particularly interesting because of universal health care access: Almost all Austrians are insured and have access to the same medical system, which is generally free of charge and involves only very minor co-payments. Income-health gradients, therefore, cannot stem from differential access to health care, but rather from one of the other reasons discussed above. Another contribution of our paper is the use of a novel instrumentation strategy – to our knowledge, we are the first to use estimated firm fixed-effects as instruments for actual wages. As these firm rents are shifting all sampled individuals' wages by a varying extent, we can interpret our results as a weighted average treatment effect rather than a local average treatment effect as typically asserted in the instrumental variables literature.

2 Data

We use matched employer-employee data from the Austrian Social Security Database (ASSD)[3] linked with administrative tax files and death register records. The ASSD contains detailed information on all private sector workers covered by the Austrian social security system, comprising – amongst others – demographics, occupational details, and employment histories. Since these administrative data are used to calculate income taxes and social security benefits, employment and earnings are measured very precisely. For our analysis we draw a cross-section

[3] A detailed description of the data can be found in Zweimüller *et al.* (2009).

of all workers above age 40 employed on April 1, 2002.[4] After dropping observations with missing values on income, we are left with a sample of 661,801 men and 514,518 women.

Detailed summary statistics are provided in Table 2. Our outcome is a binary variable equal to unity if the person died within 10 years after the date of the cross-section (i.e., until April 1, 2012). The main explanatory variable is log annual gross income received in 2002 according to tax files. As an indicator for general health, we use total days of *extended* sickness leave between 1992 and 2002 (we only observe sickness leaves that last at least six weeks unless they are caused by work accidents or occupational diseases). In our regressions we additionally control for firm size, tenure, experience, unemployment spells occurring between 1992–2002, the number of different jobs, and full sets of occupational class, education, industry sector, age, and country-of-birth dummies. Moreover, we use mean days of sickness leave per co-worker per year between 1992–2002 and mean days of sickness leave following work accidents or occupational diseases per co-worker per year between 2000–2002 as measures of workplace security. In order to ensure an adequate sample size, we mean-impute missing values on firm size and experience. Individuals with missing occupational class, education, industry sector, or country-of-birth are flagged and controlled for using binary indicators in our regressions.[5]

In Figure 1 we illustrate the relationship between income and mortality in our raw data graphically. Men's ten-year death probabilities decrease monotonically at a slightly diminishing rate over the whole income distribution. In the bottom decile, death rates are more than twice as high as in the top decile. For women, the data also suggest a negative relationship between income and mortality, although death rates vary much less across the income distribution. Women in the tenth decile show slightly higher mortality rates compared to those in the ninth – this pattern, however, disappears once we control for age and health.

[4]The reason why we focus on workers above age 40 is that (1) death rates are even more right-skewed for younger than for older workers, and (2) looking only at older workers may result in non-random sample attrition due to sick workers going into invalidity pension. Note, however, that our main conclusions are not affected by choosing different age thresholds; results for workers above age 30 and workers above age 50 are available upon request.

[5]We decided to keep 324,887 observations that have missing values on either education or occupational class in the sample and control for them using missing indicator dummies. Note that correlations between those dummies and both our main explanatory variable (income) and our outcome (death indicator) are close to zero anyways: $\mathrm{Corr}[\mathbf{1}\{\textit{education}\text{ missing}\}, \mathbf{1}\{\textit{dead}\}] = 0.0302$, $\mathrm{Corr}[\mathbf{1}\{\textit{class}\text{ missing}\}, \mathbf{1}\{\textit{dead}\}] = 0.0171$, $\mathrm{Corr}[\mathbf{1}\{\textit{education}\text{ missing}\}, \textit{income}] = 0.0509$, $\mathrm{Corr}[\mathbf{1}\{\textit{class}\text{ missing}\}, \textit{income}] = -0.0637$.

3 Methods

Consider the empirical model

$$\mathrm{P}[dead_i] = \alpha + \beta w_i + \mathbf{\Gamma}'\mathbf{x}_i + \varepsilon_i, \qquad i = 1, \ldots, N; \tag{1}$$

where the binary outcome $dead_i = \mathbf{1}\{i \text{ died until 2012}\}$ is explained by a constant α, the natural logarithm of annual gross income w_i in 2002, a vector $\mathbf{x}_i$ of additional covariates including person and firm characteristics as well as health and workplace security proxies, and an error term ε_i. Because both omitted variable bias as well as reverse causality could result in income being correlated with the error term ε_i, we employ an instrumental variables approach where time-invariant firm-specific wage components ("firm rents") are used as instruments for wages (see Section 3.2 for details). Hence, our first-stage equation reads

$$w_i = \gamma + \delta\Lambda_j + \mathbf{\Pi}'\mathbf{x}_i + \xi_i, \qquad i = 1, \ldots, N; \quad j \in 1, \ldots, J; \tag{2}$$

where γ is again a constant, Λ_j is the firm fixed-effect of i's dominant firm j in 2002,[6] and ξ_i is an *i.i.d.* error term with mean zero and constant variance.

The intuition behind our approach is clear; individuals being matched to "better" firms (i.e., firms that pay higher rents) will receive higher wages and vice versa. This relationship is graphically depicted in Figure 5, where each point represents the predicted log total income in a given decile of the firm fixed-effect distribution when age and education are held constant. While incomes increase relatively strongly between the first and second firm fixed-effect decile, we observe an almost linear relationship afterwards. In the highest decile, men (women) are estimated to earn about € 17,717 (€ 11,709) more per year than in the bottom decile.

We estimate the set of structural parameters (α,β,$\mathbf{\Gamma}$) by two-stage least squares (2SLS) separately for men and women. Under weak regularity conditions outlined in Angrist and Imbens (1995), our coefficient of interest $\hat{\beta}$ can be interpreted as a weighted average of unit causal responses due to a 100 percent increase in income, where weights are determined by how com-

[6] We define a dominant firm as the firm where i received her highest (annual) income in 2002. Of course, j is not a unique subscript to each individual.

pliers are distributed over the support of w_i. As already outlined in Section 1, the sign and magnitude of $\hat{\beta}$ are *a priori* undetermined.

In order to check robustness of our linear model specification, we additionally employ a two-step control function probit estimator proposed by Heckman (1978).[7] Consider the latent variable model

$$dead_i^* = \beta_p w_i + \mathbf{\Gamma}_p \mathbf{x}_i + v_i \tag{3}$$

$$w_i = \delta_p \Lambda_j + \mathbf{\Pi}_p \mathbf{x}_i + u_i \tag{4}$$

$$dead_i = \mathbf{1}\{dead_i^* \geq 0\}, \qquad i = 1, \ldots, N; \quad j \in 1, \ldots, J; \tag{5}$$

where Λ_j is again the firm fixed-effect, $\mathbf{x}_i$ is a vector of exogenous covariates, and $(u_i, v_i) \sim \text{Normal}(0, \sigma)$. We further assume (u_i, v_i) to be independent of $(w_i, \mathbf{x}_i)$.

First we run an OLS regression of w_i on Λ_j and $\mathbf{x}_i$ to obtain residuals $\hat{u}_i$. In the second step, we run a probit of $dead_i$ on w_i, $\mathbf{x}_i$, and $\hat{u}_i$, which allows us to consistently estimate population parameters scaled by the factor $1/\sqrt{1 - \text{Corr}[u_i, v_i]^2}$. Let $\hat{\theta}_i$ be the second-stage probit coefficient corresponding to the residual $\hat{u}_i$, then dividing each parameter by the scalar

$$\kappa_i = \sqrt{\hat{\theta}_i^2 \hat{u}_i^2 + 1} \tag{6}$$

before calculating marginal effects gives us average partial effects for the true population effects (Wooldridge, 2002). Note that consistently estimating the control function probit requires the first-stage equation (4) to be correctly specified, in particular u_i has to be homoskedastic.

3.1 Instrument Validity

Credibility of our instrument requires conditional independence of Λ_j with respect to ε_i. In particular, we assume that firm rents affect mortality only indirectly through their effect on earnings. Under endogenous job mobility, however, this assumption may be violated in case there are certain unobserved variables determining both the job matching procedure as well

[7] For a detailed discussion on advantages and disadvantages of 2SLS and control function estimators for models with dichotomous outcome variables and continuous endogenous regressors, see, e.g., Lewbel *et al.* (2012).

as individuals' mortality risks. Another violation of the conditional independence assumption would be if "good" firms were either characterized by better workplace security and healthier conditions in general, or paid compensating wage differentials for risky jobs.

As we have extensive information on past health outcomes, work accidents, and occupational diseases for all workers in our sampled firms, we are highly confident that we can account properly for compensating wage differentials and prevailing heterogeneities in terms of workplace security across firms. To do this, we first include control variables for the individual number of sickness days during the last ten years, as well as the number of sickness days following work accidents or occupational diseases during the last three years before the cross-section. Beyond these individual health records, we additionally include measures for the average amount of sick leaves, and for the prevalence of occupational diseases and work accidents for all workers within a firm. These variables can proxy for work and safety conditions on the workplace and serve as valuable controls for compensating wage differentials.

Finally, arguing that compensating wage differentials will mostly affect wages of blue collar workers rather than those of white collar workers, we also estimate our main regressions for these two groups separately within the course of our sensitivity analyses in Section 4.1. Moreover, since our tests in Section 3.3 provide strong evidence against endogeneity of job mobility in our sample, we believe that the conditional independence assumption is likely to hold, thereby entailing validity of our instrument.

3.2 Deriving the Instrumental Variable

As outlined above, we use firm-fixed wage components as instrumental variables for actual wages paid by a firm. Estimation of these firm fixed-effects is based on a decomposition method proposed by Abowd *et al.* (1999, AKM henceforth) which, given a multilevel panel structure of the underlying data, allows wages to be decomposed into observable time-varying productivity characteristics as well as time-invariant worker-fixed and firm-fixed components. The latter can be interpreted as firm *rents* – or in more technical terms, as average deviations in wages paid by firms to their employees, irrespective of the employees' individual productivity levels (these rents could reflect, for instance, efficiency wages or strategic wage posting behavior of firms).[8] Under the exogenous mobility assumption, which we discuss in detail below, firm fixed-effects serve as proper instrumental variables satisfying the conditional independence assumption.

Formally, consider the two-way additive fixed-effects model

$$w_{it} = \boldsymbol{\beta}\mathbf{x}'_{it} + \theta_i + \psi_j + r_{it}, \tag{7}$$

where w_{it} is the natural logarithm of annual wages of individual $i = 1,\ldots,N$ at time $t = 1,\ldots,T_i$, $\mathbf{x}_{it}$ is a vector of time-varying worker-specific productivity characteristics (including a quadratic in tenure and experience),[9] θ_i is the individual worker fixed-effect, ψ_j is the firm fixed-effect of i's dominant firm $j \in 1,\ldots,J$ in year t, and r_{it} is an *i.i.d.* error term with $\mathrm{E}[r_{it}|\mathbf{x}_{it},\theta_i,\psi_j,t] = 0$ and $\mathrm{Var}[r_{it}] < \infty$.

Following Card *et al.* (2013b), we assume the residual r_{it} to be a linear combination of a random match component η_{ijt}, a unit root component m_{it}, and a stochastic mean-zero error υ_{it}. That is,

$$r_{it} = \eta_{ijt} + m_{it} + \upsilon_{it}, \tag{8}$$

where we additionally impose $\mathrm{E}[\eta_{ijt}] = 0$, meaning that wage premia arising from a "good" match between workers and firms are idiosyncratic.

Identification of the AKM model requires that workers' mobility between firms is exogenous conditional on our observables $\mathbf{x}_{it}$, the worker fixed-effect θ_i, and the firm fixed-effect ψ_j. We therefore assume that mobility of "good" workers to "good" firms is not driven by any factors other than those accounted for in (7). This assumption would be violated if, e.g., workers selected themselves into jobs based on the match-specific error component η_{ijt}. However, even when we generalize the AKM model by allowing for a match-specific component in the wage setting process, i.e.,

$$w_{it} = \boldsymbol{\beta}\mathbf{x}'_{it} + \theta_i + \psi_j + \phi_{ij} + r_{it}, \tag{9}$$

where ϕ_{ij} is a worker-firm match-effect (this model has been proposed by Woodcock, 2007), our

[8] The person fixed-effect, on the other hand, can be interpreted as an indicator of workers' individual unobserved time-invariant productivity, in particular ability or diligence.

[9] Additionally, we control for a full set of time dummies.

estimated firm fixed-effects are remarkably similar to those obtained from model (7) (in fact, the correlation between the estimated fixed-effects $\hat{\psi}_j$ is 0.9897). Whether we condition on the match-effect or not, therefore, does not affect our results at all. However, there could still be other factors leading to endogenous mobility which are not accounted for by the variables in (9), in Section 3.3 we thus provide various suggestive tests of the exogenous mobility assumption that have recently been proposed in the empirical literature.

In order to recover an estimate for ψ_j, we construct a panel of all Austrian full-time workers who were employed at some point of time between 2002 and 2012. This gives us a sample of 4,623,881 workers in 374,062 distinct firms over 11 periods, which amounts to a total of 31,223,561 observations.[10]

Writing (7) in matrix notation, we have

$$\mathbf{w} = \boldsymbol{\beta}\mathbf{X} + \boldsymbol{\theta}\mathbf{D} + \boldsymbol{\psi}\mathbf{F} + \mathbf{r}, \tag{10}$$

where $\mathbf{w}$ is a stacked $N^* \times 1$ vector of annual log wages sorted by worker and time (with $N^* = \sum_i T_i$ being the total number of observations), $\mathbf{D}$ is a $N^* \times N$ design matrix of person-specific effects and $\mathbf{F}$ is a $N^* \times J$ design matrix of firm-specific effects. AKM show that equation (10) has a least squares solution that solves the following system of normal equations:

$$\begin{bmatrix} \mathbf{X}'\mathbf{X} & \mathbf{X}'\mathbf{D} & \mathbf{X}'\mathbf{F} \\ \mathbf{D}'\mathbf{X} & \mathbf{D}'\mathbf{D} & \mathbf{D}'\mathbf{F} \\ \mathbf{F}'\mathbf{X} & \mathbf{F}'\mathbf{D} & \mathbf{F}'\mathbf{F} \end{bmatrix} \begin{bmatrix} \boldsymbol{\beta} \\ \boldsymbol{\theta} \\ \boldsymbol{\psi} \end{bmatrix} = \begin{bmatrix} \mathbf{X}'\mathbf{w} \\ \mathbf{D}'\mathbf{w} \\ \mathbf{F}'\mathbf{w} \end{bmatrix}, \tag{11}$$

or, adopting a more compact notation similar to the one used in Card *et al.* (2013b),

$$\mathbf{Z}'\mathbf{Z}\boldsymbol{\zeta} = \mathbf{Z}'\mathbf{w}, \tag{12}$$

where $\mathbf{Z} \equiv [X, D, F]$ and $\boldsymbol{\zeta} \equiv [\boldsymbol{\beta}', \boldsymbol{\theta}', \boldsymbol{\psi}']'$.

For a unique solution, the cross-product matrix $\mathbf{Z}'\mathbf{Z}$ must have full rank. Due to its high

[10]Note that this sample is different from the one we use for our main regressions – however, it obviously nests the 2002 cross-section we draw.

column dimension – which makes the matrix computationally infeasible to invert – we have to use an iterative conjugate gradient method discussed at length in Abowd *et al.* (2002) in order to obtain a solution. Worker and firm fixed-effects are only identified within sets of connected firms, that is, firms that are linked (directly or indirectly) by worker mobility. Our largest connected set has 31,223,561 observations, while the second largest only has 16. Therefore, we restrict our sample to the largest connected set.

We proceed by normalizing the estimated firm fixed-effects $\hat{\psi}_j$ around their average values within each industry sector. Let S_j be the two-digit NACE industry sector of firm j, and let $K_j = \{k \in S_j : k \neq j\}$ be the set of all firms in S_j other than j. Our instrument is then defined as

$$\Lambda_j = \hat{\psi}_j - \frac{1}{|K_j|} \sum\nolimits_{k \in K_j} \hat{\psi}_k, \tag{13}$$

where $|K_j|$ is the number of firms in K_j. Summary statistics for the sector-standardized firm fixed-effect Λ_j can be found in Table 2.

3.3 Testing the Exogenous Mobility Assumption

Our identification strategy relies crucially on the exogenous mobility assumption being satisfied, which requires that workers – conditional on observables and time-invariant worker and firm fixed-effects – are matched randomly to firms.[11] Note that this assumption does *not* require us to neglect sorting in our model. However, we do have to assume that mobility decisions that actually lead to sorting are based on either our observables or our fixed-effects.

Although there is little evidence of sorting in our data anyway (in fact the correlation between worker and firm fixed-effects is very close to zero: $\mathrm{Corr}(\theta_i, \psi_j) = 0.0154$),[12] we proceed by providing various suggestive tests on the exogenous mobility assumption that are largely based on Card *et al.* (2013b) (CHK, henceforth), Flabbi *et al.* (2014) (FMMS), and Card *et al.* (2013a) (CCK). Similar to these papers that use German (CHK), Italian (FMMS), and Portuguese (CCK)

[11] This is a frequent assumption in job search theory (see, e.g., Mortensen, 2005, for a prominent example).

[12] Some researchers argue that the correlation between worker and firm fixed-effects could in fact be biased if the additivity assumption implied by the model in (7) was violated, e.g., in case of non-linearities in the wage setting process. If this was the case, the true extent of sorting would be shrouded (Eeckhout and Kircher, 2011; Lise *et al.*, 2013; Lopes de Melo, 2009). Since we are not particularly interested in the actual degree of sorting (as long as it is the result of a conditionally exogenous matching process), we stick to this simple measure for now.

data, we find that job mobility is likely to be exogenous in our sample of Austrian workers.

As CHK point out, systematic trends in wage profiles prior to job changes could, for instance, be a major indicator for endogenous matching in the labor market. If productivity is revealed only gradually over time, then good workers employed in bad firms will experience wage increases already at their current employer, and will be more likely to move to better firms in subsequent periods. The same holds true for bad workers in good firms; under endogenous mobility they will experience wage decreases and will be more likely to move on to worse firms afterwards. In the absence of endogenous mobility, we would see flat profiles before and after job moves, but strong wage increases (decreases) for workers moving to a higher (lower) firm wage quantile.

This is exactly what we observe in our data. For presentational reasons, we assign each job-mover to one of sixteen cells representing the firm fixed-effect quartile of her origin and destination firm. Figure 2 shows wage profiles of workers who moved between the first and fourth firm fixed-effect quartile at some point in time. Similar to CHK, FMMS, and CCK, we do not observe any systematic trends in wages prior to or after job transitions. In fact, wages across quartile cells are considerably stable before moves, and they monotonically increase with each quartile a worker moves up the firm ladder. Effects of moving down the ladder are more or less symmetrical. In Figure 3, we additionally show wage profiles of workers who move to firms within their fixed-effect quartile. Again, we do not observe any systematic patterns in wages as each line is roughly horizontal two years before the job transition.

Note that, judging from Figure 2, initial wage levels prior to job moves seem to be systematically lower for workers who move down the firm ladder and vice versa, which could be a sign of endogenous mobility as well. However, if we subtract wage components that job mobility can be conditioned on in our framework, i.e., time-varying observables and worker as well as firm fixed-effects, we find that these differences are quantitatively negligible and not systematic anymore.

In Table 1, we show such mean residual log wages obtained from our AKM regression for workers who moved to new firms at some point in time between 2004 and 2012.[13] In each

[13]We exclude workers who moved in 2002 and 2003 because we cannot calculate two lags of residual wages for them.

panel, we compare mean residual wages of movers moving up or down the firm fixed-effect distribution with residual wages of movers who stay within their fixed-effect quartile (indicated by the gray lines). As mentioned before, under the presence of endogenous mobility, we would suspect workers moving up the ladder to earn more already at their initial employer compared to others who stay within the same quartile or move down the firm ladder (and vice versa).

However, the pattern we see in Table 1 does not support this hypothesis: Calculating differences in mean residual log wages with respect to the base group (i.e., again, workers who stay within the same quartile of the firm fixed-effect distribution as their origin firms), we see that only 11 out of 24 differences have the predicted sign (i.e., a negative difference for downward movers and a positive difference for upward movers), while 13 show a wrong sign indicating that upward movers earn less before their move and vice versa. Two years prior to a move, only one difference has the expected sign. All in all, this pattern seems completely arbitrary and wages do not seem to follow any systematic trends before job transitions.

Moreover, endogenous mobility would imply asymmetric wage gains (losses) for workers moving up (down) the firm fixed-effect distribution.[14] If a worker is actively inspiring a new job based on unobserved characteristics, she will achieve a reasonably high wage gain due to the perfect match at her new employer. Instead, exogenous upward movers would only get an average reward. The opposite applies to downward movers: workers *actively* looking for a good match (or those stuck in a bad match in the first place) would lose less compared to exogenous downward movers. In Figure 4 we plot wage changes for all upward and downward movers between firm fixed-effect deciles, where each point represents a decile pair movers are transitioning in-between.[15] Match-specific wage effects would result in points lying below the diagonal. In our case, all points are in fact very close to the diagonal – if at all, they lie above it. Card *et al.* (2013b) also point out that symmetry of wage gains and losses is a necessary condition for the additivity assumption imposed by the AKM model to hold true. Consider two firms k and j with $\psi_k > \psi_j$. If wages are properly characterized by equation (7), the wage gain for moving from firm j to firm k is $\psi_j - \psi_k$, and the wage gain for moving from firm k to j is

[14] Thanks to Ana Rute Cardoso for pointing this out.

[15] For instance, the point "1-10" comprises all job movers who either move from decile 1 to decile 10, or from decile 10 to decile 1. Mean log wage changes of the former group (i.e., upward movers) are depicted on the horizontal axis, whereas wage changes for the latter group (downward movers) are depicted on the vertical axis.

$\psi_k - \psi_j$. That is, wage changes are symmetric for moving between firms at different levels of the firm fixed-effect distribution.

Judging from our test results, we therefore conclude that the exogenous mobility assumption is likely to hold in our sample of Austrian workers. As wage bargaining is largely centralized in Austria,[16] and wages have been found to be much more rigid compared to other OECD countries (especially for older workers, see, e.g., Hofer and Weber, 1996), freedom in the wage bargaining process is substantially reduced – making it reasonable to assume random matching that is not reflected in wages. Moreover, asymmetric information in the labor market also supports our assumption of exogeneity of job mobility. On the firm side, it is plausible to assume that screening in the hiring process is based primarily on observable worker characteristics. Note that even if workers were hired according to their personal fixed-effects – which include, amongst other things, their time-invariant ability and work morale – the exogenous mobility assumption we impose would still be met.

4 Results

Main results are given in Table 3. We report both OLS and 2SLS estimations of our linear probability model (LPM) specification separately for men and women, with ten-year death probability being the outcome variable throughout.[17] Even after controlling for education, personal job characteristics, past health outcomes, workplace security proxies, as well as industry, age and country-of-birth fixed effects, we observe statistically significant negative correlations between income and mortality in our naïve OLS regressions. For men, a 100 percent increase in income corresponds to a decrease in ten-year death probability by 1.39 percentage points, which is around 28 percent of the sample mean of 4.95%. As expected, the effect is smaller for women at -0.38 percentage points.

[16] One explanation why CHK find an upward trend in assortative matching for their sample of German workers is because firms increasingly took the opportunity to opt out of collective bargaining agreements between 1985 and 2009 (this is the main explanation for rising wage inequality in Germany according to Dustmann *et al.*, 2014). In Austria, such a possibility to opt out of collective bargaining is not possible. In some years, however, firms had the option to trade wage increases for other worker benefits, but this was rarely used.

[17] We decided to report analytical standard errors instead of doubly bootstrapped ones. The reason is twofold: First, bootstrapping the AKM regression is computationally extremely tedious, and second, analytical standard errors are smaller, thus providing a more conservative estimate of the population standard deviation. Since our results generally show zero-effects, this choice seems reasonable.

These relatively small coefficients become even smaller and statistically insignificant once we account for endogeneity bias utilizing our instrumental variables framework. First-stage coefficients are positive and highly significant for both genders, indicating that a one standard deviation increase in the firm fixed-effect raises income of men (women) by about 19.9% (14.1%). Using 2SLS, the income effect for males diminishes tremendously from -0.0139 to -0.000002. For females, the coefficient is now even positive, but close to zero as well. Due to the strong power of our instrument (the first-stage F-value is always above 140) and the large sample size, these estimates carry relatively low standard errors. Both effects are economically and statistically indistinguishable from zero.

Coefficients of the control variables largely adhere to *a priori* expectations. Days of extended sick leave and days of sick leave following work accidents or occupational diseases are positively related to mortality risk, both for men and women to a similar degree. The same applies to health and workplace security proxies of co-workers. While all four indicators consistently have positive coefficients, only total days of extended sick leave are also statistically significant for both genders. For men, the average amount of sick leaves taken by co-workers seems to have a significant impact on their own mortality risk as well.

We also find that longer unemployment spells result in higher death rates, whereas job tenure seems to lower mortality. Blue collar workers are more likely to die within the sample period than white collar workers, and education decreases mortality risk monotonically with each further degree obtained. Work experience has different signs for males and females, revealing a negative effect on mortality for the former and a positive for the latter. Note also that our instrumental variables estimation leaves coefficients of the control variables practically unchanged, which suggests that our instrument is in fact uncorrelated with all observable characteristics affecting mortality in our sample of workers.

4.1 Robustness

Our results thus far raise the question as to why income effects actually diminish to such a large extent once endogeneity is controlled for. It seems that reverse causality and unobservable health-promoting characteristics correlated with both income and mortality are main drivers of the correlation between these two variables. Simple reverse causality may occur if bad health, i.e., high mortality risk, reduces income opportunities. Omitted variables bias, on the other hand, can easily rationalize a negative correlation between mortality and income when these omitted variables (in particular genetic predisposition, effort, motivation, perseverance or health-promoting behaviors) are both positively correlated with income and negatively with mortality. In such a case, the OLS coefficient on income will be biased downwards.

Why is the correlation between income and mortality much higher for males than for females, although causal parameters are zero for both? One explanation may be that reverse causality is more pronounced among men. This is rather unlikely though, because studies examining the causal impact of health on income tend to find effects that are actually stronger for women than for men (e.g., Case *et al.*, 2005; Halla and Zweimüller, 2011). The second explanation is simply that omitted variable bias is larger for men. In fact, the impact of our control variables in Table 3 is almost uniformly stronger for men as compared to women. Given that the aforementioned omitted variables are likely correlated with our observed ones, we might suspect that their impact is also stronger for men.

Finally, our income indicator could simply be measured incorrectly, yielding coefficients that are biased towards zero. In fact, it annual labor market income could be a bad indicator for health-enhancing income, either because (1) it disregards actual working hours, (2) it is simply unrepresentative for income over a longer period of time, or (3) couples tend to share incomes (in particular when it comes to health-related investments).[18] We address all these issues by using different sample restrictions within the course of this section. Finally, we use a different empirical specification for our main model as well in order to test whether results hold when we relax the linearity assumption implied by the LPMs we estimate.

In Table 4, Panel [a], we restrict the sample to employees working in a full-time position only. Columns (1) and (4) show again the results of the 2SLS regressions in Table 3 for the full sample. As expected, the sample size remains relatively stable for males, whereas the number of observations for females drops by a third. While the coefficient for males becomes slightly positive, it turns negative and increases somewhat in magnitude for women – both, however,

[18]Classical measurement error in the income variable would, however, lead to an attenuation bias of an OLS estimate.

remain statistically insignificant at any conventional level.

Current wages may also not necessarily be representative for workers who change their jobs very often. Also, the instrument may be weaker in explaining current wages if the worker just started her job. In Table 4, Panel [b], we therefore restrict the sample to employees who work at least five years in their current job. This reduces the sample size considerably, but leaves results unchanged compared to those obtained for the full sample: the coefficient on income is now positive for both genders, but statistically indistinguishable from zero.

In a similar vein, we introduce long-term income as another, potentially better indicator for average earnings.[19] For this purpose we use average income over the last eight years for each individual as our main explanatory variable (instead of income in 2002 only). We try two different specifications of our instrument. First, we use the firm fixed-effect in 2002 as before. Second, we define the instrumental variable as an average firm fixed-effect over all firms i has worked in during this period. Let $j(i,t)$ be the firm i is employed in at time t and let $|J_i|$ be the total number of i's employers during the entire period $T = [1994, 2002]$. Then, for every i we have

$$\bar{\Lambda}_{j,t\in[1994,2002]} = \frac{1}{|J_i| \cdot T} \sum\nolimits_{j(i)\in J_i} \sum\nolimits_{t\in T} \Lambda_{j(i,t)}. \tag{14}$$

Using these long-term income measures, again, we do not find any significant causal effects on mortality. Both OLS and 2SLS estimates are very similar to those obtained for the full sample, although our second instrument yields somewhat higher coefficients which is most likely due to its comparably weak first-stage. Independent of the choice of the instrument, however, estimated causal effects are in fact zero.

Own labor market earnings may also be a bad indicator for overall disposable income in case couples share their incomes. We therefore construct a subsample of individuals for whom we know from different sources that they were married on April 1, 2002.[20] We observe 38% men and 27% women who are married – note, however, that this subsample might be selected

[19] Sullivan and von Wachter (2009a) stress this long-term view. Another reason why we consider average income observed over a longer period of time is that it might also be a better indicator for individuals' wealth. See, e.g., Attanasio and Emmerson (2003) or Michaud and van Soest (2008) who explore the wealth-mortality gradient.

[20] We use the marriage register, social security data, and tax files to spot married individuals. None of these sources, however, provides complete information about marriage status for our sample (especially for older cohorts).

on unobservables; results should therefore be interpreted with caution.

Individuals in our married sample are on average slightly older, earn more, are more likely to be white collar workers, and are better educated compared to our full sample.[21] Regression results are given in Table 6; for both men and women, the first two columns show our baseline regressions for the married sample, in the second two columns we additionally control for spousal income. While OLS coefficients change only slightly compared to those obtained for the full sample, 2SLS estimates are positive for men and negative for females, but remain statistically insignificant. Controlling for spousal income leaves the coefficients of own income virtually unchanged. However, while own income is still insignificant, we do observe that men whose wives earn more are somewhat more likely to die during the ten-year period: *ceteris paribus*, a 100 percent increase in spousal income increases death probability by roughly 0.07 percentage points. Note, however, that this effect does not have a causal interpretation on its own. For women, the effect of the husband's income is exactly zero and insignificant.

In Section 3 we also raise the point that compensating wage differentials might be a threat to the validity of our instrument. We therefore stratify the sample into white collar and blue collar workers, arguing that compensating wage differentials for risky jobs should mainly be paid for blue collar jobs. In Table 7, we see again a zero effect both for white collar as well as for blue collar workers.

Finally, we test whether our results are robust to non-linear estimators as well. In Table 8 we apply a two-step control function probit estimator as outlined in Section 3. Results are fairly robust insofar as simple probit marginal effects are close to point estimates obtained from the LPM in Table 3. Marginal income effects estimated by the control function probit are now slightly positive for men, but still insignificantly different from zero. To wrap up our empirical analysis, we can conclude that labor income is very likely to have no effect on mortality among prime-age Austrian workers.

[21] Full summary statistics for the subsample of married individuals are available upon request.

5 Conclusions

In this paper we use a novel instrumental variables strategy to study the causal effect of income on mortality in Austria. Utilizing a multilevel panel where workers are matched to firms, we estimate time-invariant firm-specific wage components (rents), which we use to instrument for actual wages in a 2002 cross-section of workers. Identification relies crucially on the exogenous mobility assumption being met in the data, which we test extensively. While we do find statistically significant negative income effects on mortality in our naïve least squares estimations, these effects turn zero in our IV regressions, both in terms of coefficient magnitude and statistical significance. Introducing other measures which ought to better reflect long-term income leaves our conclusions unchanged, as does the consideration of spousal incomes or the use of non-linear estimation methods.

We interpret our findings as evidence that reverse causality as well as unobservable confounders affecting both income and mortality explain a large part of the correlation between those two factors. Why is there no causal relationship between income and mortality in Austria? The universal health care system is likely to absorb some potential mediating effects that have been shown to fill the link between income and health, in particular accessibility and affordability of medication or surgeries. Moreover, health and mortality may not be influenced by the availability of financial resources as such, but rather by education and certain behaviors. Note also that our research design allows us to only look at the working population – income effects for, e.g., people that are unemployed or out of the labor force might differ substantially from those found in our analysis and shall therefore be tackled in future research (e.g., along the lines of Sullivan and von Wachter, 2009b).

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Appendix

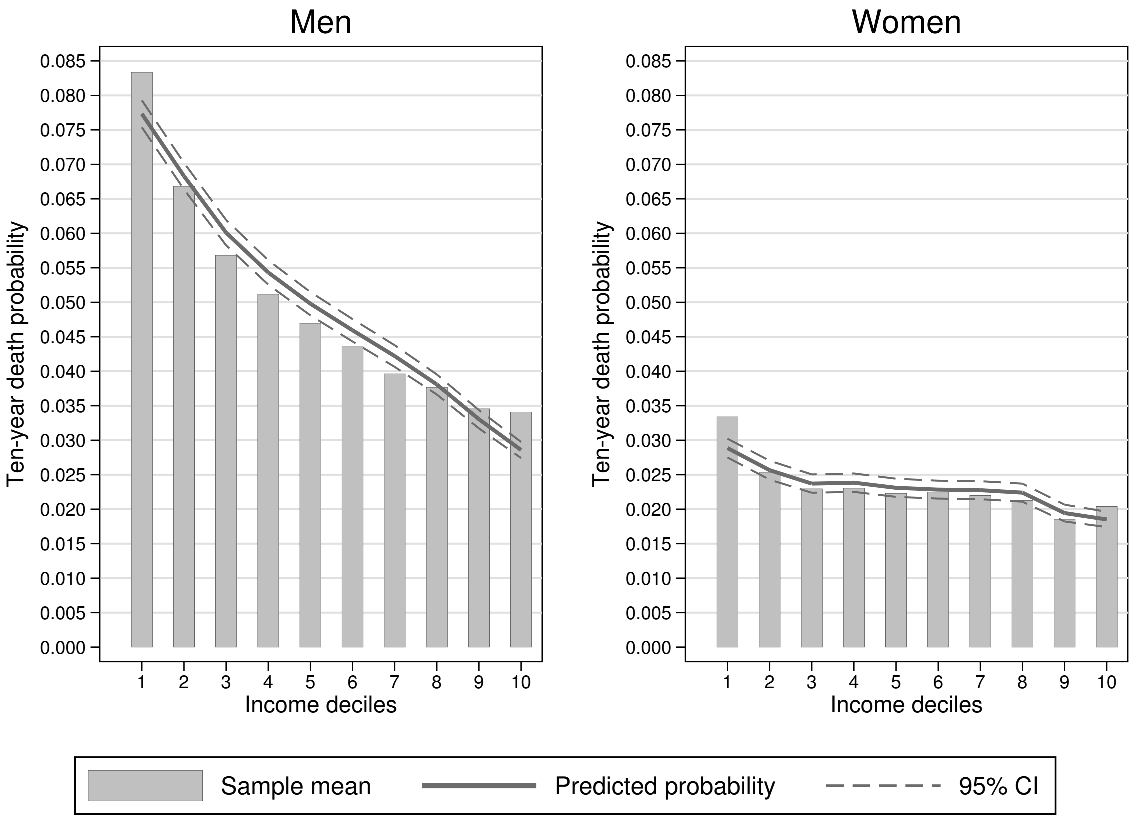

FIGURE 1 — Ten-year death probabilities against ten deciles of the income distribution. Bars represent raw sample means of ten-year death probability, lines are predicted death probabilities, regression-adjusted for age and extended sickness leaves. The 95% confidence intervals depicted as dashed lines correspond to the latter.

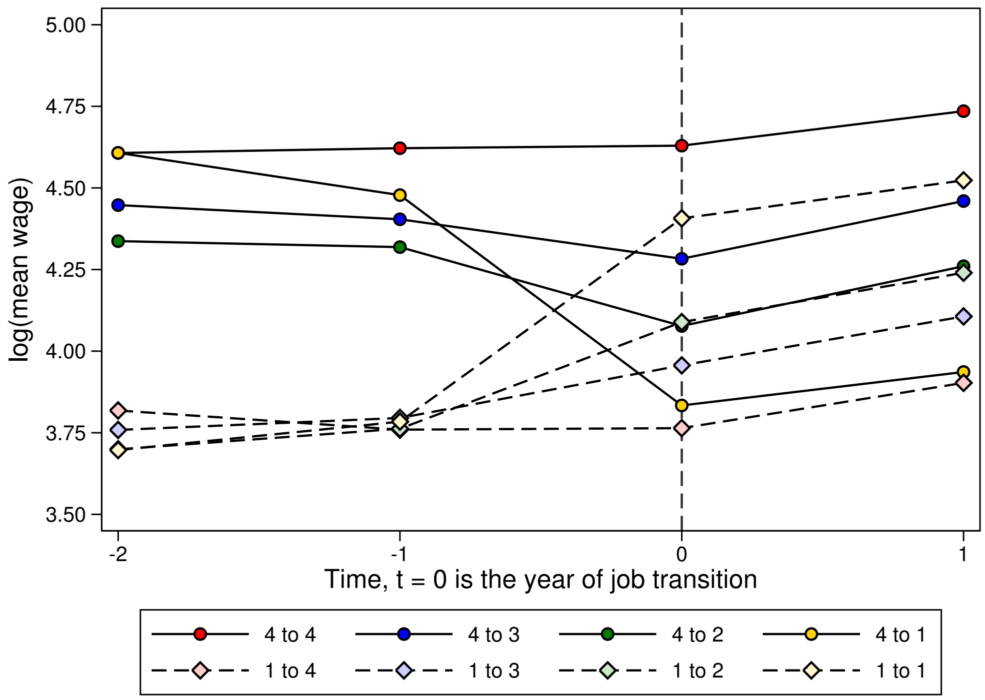

Figure 2 — Wage profiles of job movers between the first and the fourth quartile of the firm fixed-effect distribution.

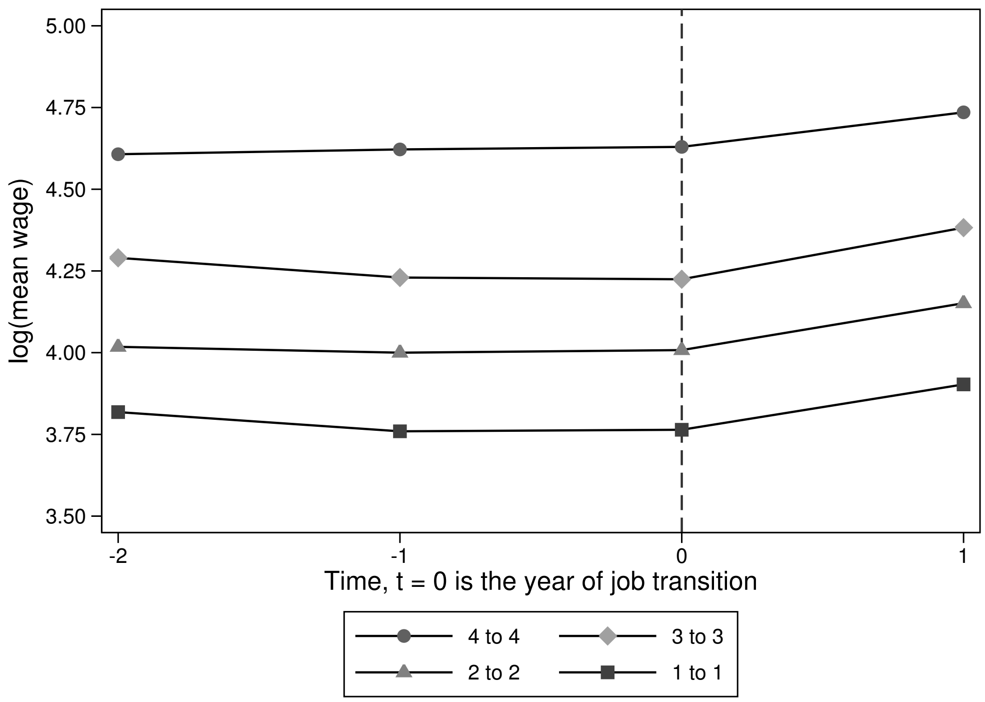

Figure 3 — Wage profiles of job movers moving between firms within the same quartile of the firm fixed-effect distribution.

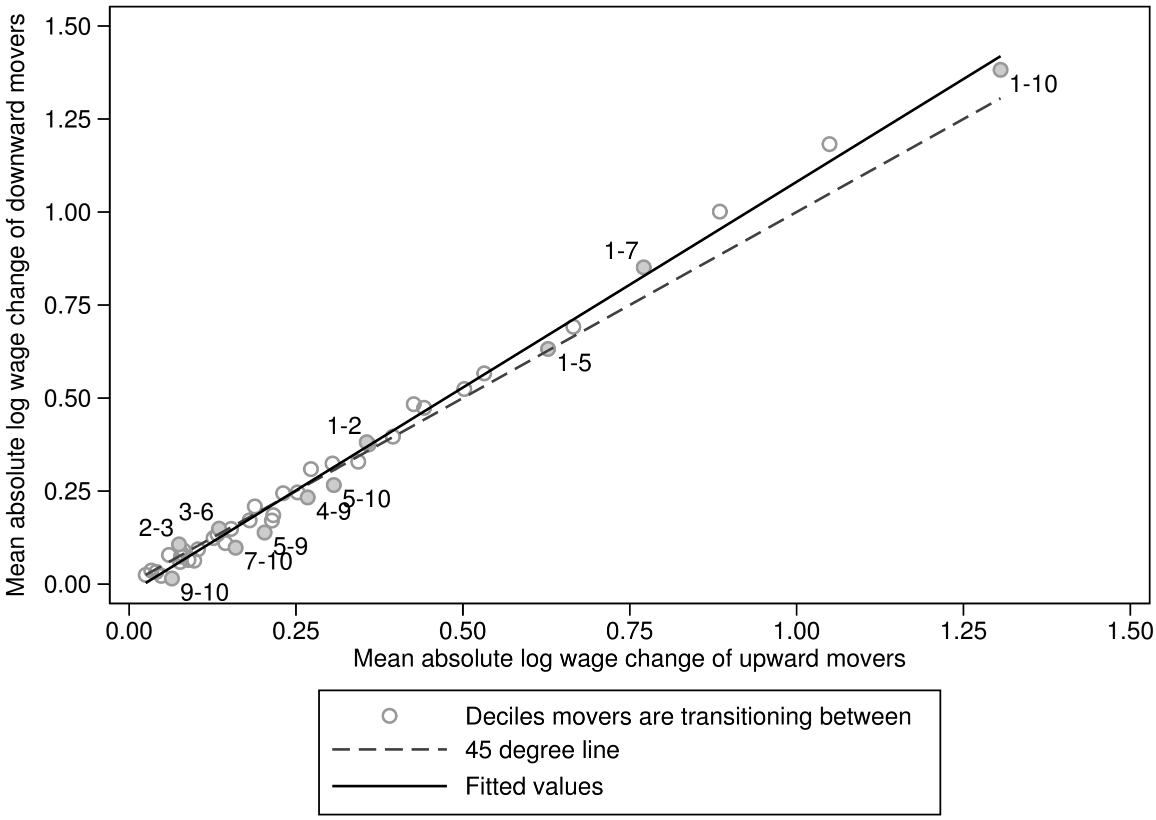

FIGURE 4 — Corresponding mean absolute log wage gains and losses of workers moving from one decile of the firm fixed-effect distribution to another. Each point represents two deciles movers are transitioning in-between, for instance "1-10" comprises all workers who move either from decile 1 to decile 10 or vice versa (for presentational reasons we only label some randomly selected decile pair points). The mean wage gain of upward movers is depicted on the horizontal axis, whereas the mean wage loss of downward movers is depicted on the vertical axis.

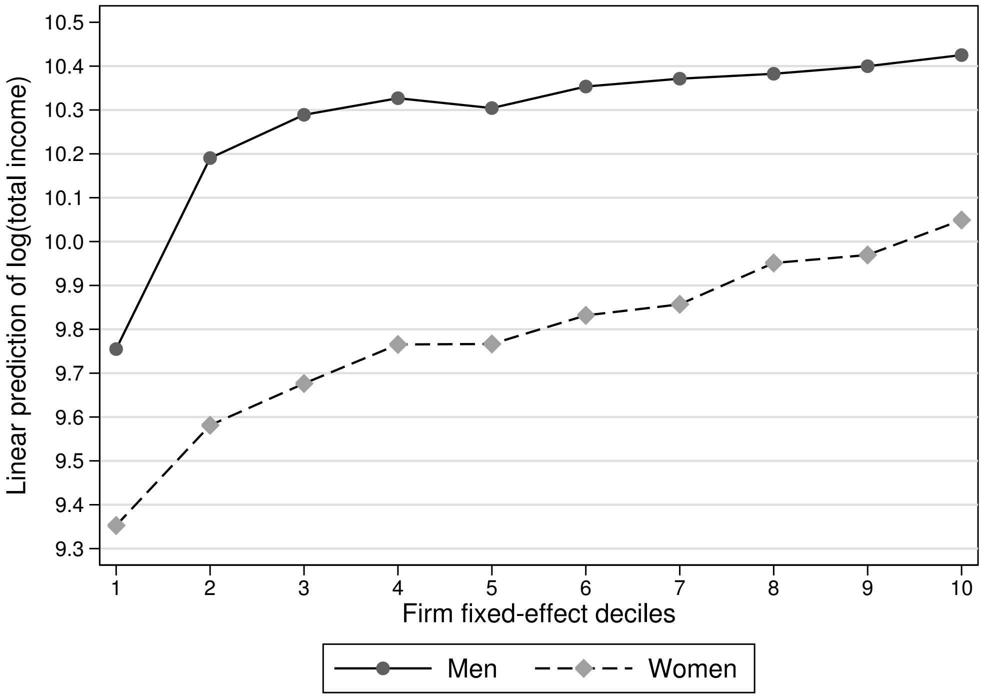

FIGURE 5 — Predicted log annual incomes in 2002, regression-adjusted for age and education, for ten deciles of the firm fixed-effect and both genders.

TABLE 1 — Residual log wages estimated from an AKM regression two years prior to job transitions.

		Residual log wages					
		2 years prior to move			1 year prior to move		
Quartile[†]	# of movers	Mean	Std. dev.	Difference[‡]	Mean	Std. dev.	Difference[‡]
Panel [a]: Origin quartile 1							
1 to 1	137,477	-0.0309	0.30	0.000	-0.0040	0.34	0.000
1 to 2	86,905	-0.0501	0.31	-0.019	0.0086	0.37	0.013
1 to 3	56,406	-0.0641	0.34	-0.033	0.0042	0.42	0.008
1 to 4	39,374	-0.0972	0.39	-0.066	-0.0426	0.53	-0.039
Panel [b]: Origin quartile 2							
2 to 1	78,340	-0.0031	0.27	0.012	-0.0208	0.32	-0.023
2 to 2	114,568	-0.0156	0.23	0.000	0.0019	0.27	0.000
2 to 3	107,427	-0.0197	0.24	-0.004	0.0140	0.29	0.012
2 to 4	52,359	-0.0176	0.28	-0.002	0.0154	0.34	0.014
Panel [c]: Origin quartile 3							
3 to 1	58,297	0.0207	0.31	0.022	-0.0162	0.33	-0.016
3 to 2	85,523	-0.0012	0.23	0.000	-0.0118	0.27	-0.011
3 to 3	126,856	-0.0009	0.23	0.000	-0.0005	0.26	0.000
3 to 4	117,055	-0.0006	0.23	0.000	0.0105	0.28	0.011
Panel [d]: Origin quartile 4							
4 to 1	70,877	0.0384	0.29	0.043	0.0088	0.33	0.008
4 to 2	50,700	0.0076	0.23	0.012	-0.0185	0.28	-0.019
4 to 3	102,397	0.0002	0.20	0.005	-0.0114	0.25	-0.012
4 to 4	311,797	-0.0046	0.24	0.000	0.0005	0.24	0.000

The sample consists of all individuals who moved to a new job between 2004–2012, these are the workers for whom we can estimate two lags of their residual log wages.

[†] Quartiles of the firm fixed-effect distribution of origin and destination firms.

[‡] In each panel, the difference in mean residual log wages is calculated with respect to the stayers (highlighted in gray), i.e., workers who move to firms within their origin firm's fixed-effect quartile.

Table 2 — Descriptive statistics.

	Men (N = 661,801)				Women (N = 514,518)			
	Mean	Std. dev.	Min.	Max.	Mean	Std. dev.	Min.	Max.
Age in years	48.04	5.76	40.00	65.00	47.14	5.16	40.00	65.00
Ten-year death probability ($dead_i$)	0.05	0.22			0.02	0.15		
Income measures								
log(total annual income 2002)	10.31	0.75	0.00	17.97	9.76	0.82	0.00	13.58
log(mean annual income between 1994–2002)	10.19	0.62	1.06	15.67	9.58	0.77	1.98	13.23
Instrumental variables								
Standardized firm fixed-effect (Λ_j)	0.24	0.29	-12.67	4.86	0.19	0.33	-12.67	13.26
Mean standardized firm fixed-effects between 1994–2002	1.47	0.80	-12.67	13.64	1.45	0.73	-23.67	14.28
Health and workplace security proxies								
Total days of extended sickness leave per year (1992–2002)	0.01	0.05	0.00	2.78	0.01	0.04	0.00	2.71
Mean days of sick leave of co-workers per year (1992–2002)	0.01	0.02	0.00	2.62	0.01	0.02	0.00	1.65
Total days of sick leave following work accidents or occ. diseases per year (2000–2002)	0.01	0.05	0.00	2.00	0.00	0.03	0.00	2.00
Mean days of sick leave following work accidents or occ. diseases of co-workers per year (2000–2002)	0.00	0.01	0.00	1.56	0.00	0.01	0.00	1.49
Other personal and firm characteristics								
Firm size	3.16	8.91	0.00	43.58	2.47	5.89	0.00	43.58
Tenure in years	8.21	7.74	0.00	30.25	7.15	6.88	0.00	30.25
Experience in years	22.54	7.33	0.00	30.25	18.44	7.44	0.00	30.25
Total unemployment spells in years between 1992–2002	4.10	4.52	0.00	10.00	4.73	4.53	0.00	10.00
Number of different jobs	1.01	0.10	1.00	4.00	1.02	0.16	1.00	5.00
Employed in a full-time position	0.93	0.25			0.69	0.46		
Known to be married	0.37	0.48			0.27	0.44		
Occupational class								
Blue collar worker	0.43	0.49			0.34	0.47		
White collar worker *(reference group)*	0.39	0.49			0.54	0.50		
Civil servant	0.18	0.38			0.12	0.32		
Missing	0.00	0.03			0.00	0.03		
Education								
No compulsory school	0.01	0.08			0.01	0.10		
Compulsory school	0.12	0.32			0.19	0.39		
Apprenticeship training *(reference group)*	0.37	0.48			0.23	0.42		
Middle school	0.06	0.23			0.12	0.33		
High school	0.08	0.27			0.06	0.24		
University	0.11	0.31			0.10	0.29		
Missing	0.26	0.44			0.30	0.46		

Table 3 — Linear regressions of ten-year death probability.

$P[dead_i]$	Men OLS	Men 2SLS	Women OLS	Women 2SLS
Income				
log(total income 2002)	-0.0139	$-1.83 \cdot 10^{-6}$	-0.0038	0.0002
	(0.001)***	(0.002)	(0.000)***	(0.002)
Health and workplace security proxies				
Total days of extended sick leave per year (1992–2002)	0.1048	0.1096	0.1119	0.1127
	(0.027)***	(0.028)***	(0.022)***	(0.022)***
Mean days of sick leave of co-workers per year (1992–2002)	0.0627	0.0664	0.0181	0.0197
	(0.021)***	(0.022)***	(0.012)	(0.012)
Total days of sick leave following work accidents or occupational diseases per year (2000–2002)	0.0051	0.0076	0.0026	0.0021
	(0.006)	(0.006)	(0.009)	(0.009)
Mean days of sick leave following work accidents or occupational diseases of co-workers per year (2000–2002)	0.0386	0.0231	0.0180	0.0134
	(0.032)	(0.032)	(0.024)	(0.024)
Other personal and firm characteristics				
Firm size · 1/1000	0.0003	0.0003	0.0002	0.0001
	(0.000)***	(0.000)***	(0.000)**	(0.000)
Tenure	-0.0004	-0.0006	-0.0002	-0.0003
	(0.000)***	(0.000)***	(0.000)***	(0.000)***
Experience	0.0000	-0.0002	0.0005	0.0004
	(0.000)	(0.000)***	(0.000)***	(0.000)***
Total unemployment spell in years (1992–2002)	0.0007	0.0010	0.0002	0.0002
	(0.000)***	(0.000)***	(0.000)***	(0.000)***
Number of different jobs	-0.0095	-0.0075	-0.0051	-0.0046
	(0.002)***	(0.002)***	(0.001)***	(0.001)***
Occupational class *(baseline group: white collar workers)*				
Blue collar worker	0.0114	0.0165	0.0031	0.0050
	(0.001)***	(0.001)***	(0.001)***	(0.001)***
Civil servant	-0.0001	-0.0004	0.0028	0.0014
	(0.002)	(0.002)	(0.001)***	(0.001)
Education *(baseline group: apprenticeship training)*				
No compulsory school	0.0086	0.0087	0.0024	0.0019
	(0.004)**	(0.004)**	(0.002)	(0.002)
Compulsory school	0.0052	0.0054	0.0023	0.0021
	(0.001)***	(0.001)***	(0.001)***	(0.001)***
Middle school	-0.0028	-0.0036	-0.0010	-0.0012
	(0.001)***	(0.001)***	(0.001)	(0.001)*
High school	-0.0027	-0.0066	-0.0012	-0.0020
	(0.001)***	(0.001)***	(0.001)	(0.001)**
University	-0.0083	-0.0153	0.0005	-0.0012
	(0.001)***	(0.001)***	(0.001)	(0.001)
Other covariates†	Yes	Yes	Yes	Yes
First-stage coefficient		0.6871		0.4257
		(0.029)***		(0.035)***
First-stage F-statistic		552.1		148.3
N	661,801	661,801	514,518	514,518
Mean of $dead_i$	0.0495	0.0495	0.0232	0.0232

Standard errors given in parentheses are robust and clustered on the firm level, stars indicate significance levels: * $p < 0.1$ ** $p < 0.05$, *** $p < 0.01$. The sample consists of all private sector workers above age 40 employed on April 1, 2002 in Austria. All estimations also include a constant and missing indicator dummies for education and occupational class which are not reported.

† Contains full sets of industry sector, age, and country of birth dummies.

TABLE 4 — Linear regressions of ten-year death probability with sample restrictions.

	Men			Women		
$P[dead_i]$	Baseline 2SLS	OLS	2SLS	Baseline 2SLS	OLS	2SLS
Panel [a] — only full-time employees						
log(total income 2002)	-0.0000	-0.0143	0.0005	0.0002	-0.0046	-0.0013
	(0.002)	(0.001)***	(0.002)	(0.002)	(0.001)***	(0.002)
Other covariates†	Yes	Yes	Yes	Yes	Yes	Yes
First-stage coefficient	0.6871		0.6643	0.4257		0.4960
	(0.029)***		(0.032)***	(0.035)***		(0.052)***
First-stage F-statistic	552.1		441.3	148.3		91.4
N	661,801	617,950	617,950	514,518	355,187	355,187
Mean of $dead_i$	0.0495	0.0485	0.0485	0.0232	0.0237	0.0237
Panel [b] — only employees with tenure ≥ 5						
log(total income 2002)	-0.0000	-0.0116	0.0059	0.0002	-0.0028	0.0039
	(0.002)	(0.001)***	(0.004)	(0.002)	(0.001)***	(0.003)
Other covariates†	Yes	Yes	Yes	Yes	Yes	Yes
First-stage coefficient	0.6871		0.4550	0.4257		0.3123
	(0.029)***		(0.036)***	(0.035)***		(0.057)***
First-stage F-statistic	553.1		157.7	148.3		29.7
N	661,801	355,966	355,966	514,518	258,769	258,769
Mean of $dead_i$	0.0495	0.0441	0.0441	0.0232	0.0220	0.0220

Standard errors given in parentheses are robust and clustered on the firm level, stars indicate significance levels: * $p < 0.1$ ** $p < 0.05$, *** $p < 0.01$. The baseline sample consists of all private sector workers above age 40 employed on April 1, 2002 in Austria. All estimations also include a constant and missing indicator dummies for education and occupational class which are not reported.

† Contains health and workplace security proxies, other personal and firm characteristics (both as specified in Table 3), occupational class, education, and full sets of industry sector, age, and country of birth dummies.

TABLE 5 — Linear regressions of ten-year death probability with average income between 1994–2002 as the explanatory variable, IV1 $\equiv \Lambda_{j,t=2002}$ (firm fixed-effect in 2002), IV2 $\equiv \bar{\Lambda}_{j,t\in[1994,2002]}$ (average of fixed-effects of all firms i has worked in between 1994–2002).

	Men				Women			
$P[dead_i]$	Baseline 2SLS	OLS	IV1 2SLS	IV2 2SLS	Baseline 2SLS	OLS	IV1 2SLS	IV2 2SLS
Income								
log(average income 1994–2002)		-0.0177	-0.0000	-0.0119		-0.0011	0.0002	-0.0143
		(0.001)***	(0.003)	(0.027)		(0.000)***	(0.002)	(0.020)
log(total income 2002)	-0.0000				0.0002			
	(0.002)				(0.002)			
Health and workplace security proxies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other personal and firm characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupational class & education	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other covariates†	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First-stage coefficient	0.6871		0.4676	0.0152	0.4257		0.3234	0.0166
	(0.029)***		(0.023)***	(0.006)**	(0.035)***		(0.027)***	(0.007)**
First-stage F-statistic	552.1		416.5	6.4	148.3		142.1	5.2
Kleibergen-Paap rk F statistic	11,406.7		10,222.7	340.2	4,923.6		4,491.4	191.4
N	661,801	661,801	661,801	661,801	514,518	514,518	514,518	514,518
Mean of $dead_i$	0.0495	0.0495	0.0495	0.0495	0.0232	0.0232	0.0232	0.0232

Standard errors given in parentheses are robust and clustered on the firm level, stars indicate significance levels: * $p < 0.1$ ** $p < 0.05$, *** $p < 0.01$. The baseline sample consists of all private sector workers above age 40 employed on April 1, 2002 in Austria. All estimations also include a constant and missing indicator dummies for education and occupational class which are not reported.

† Contains full sets of industry sector, age, and country of birth dummies.

TABLE 6 — Linear regressions of ten-year death probability for the subsample of married individuals.

	Men				Women			
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
log(total income 2002)	-0.0118	0.0018	-0.0116	0.0018	-0.0026	-0.0026	-0.0026	-0.0026
	(0.001)***	(0.003)	(0.001)***	(0.003)	(0.001)***	(0.003)	(0.001)***	(0.003)
log(1 + total annual income of spouse 2002)			0.0007	0.0007			-0.0000	-0.0000
			(0.000)***	(0.000)***			(0.000)	(0.000)
Health and workplace security proxies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other personal and firm characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupational class & education	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other covariates†	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First-stage coefficient		0.6175		0.6173		0.4180		0.4201
		(0.030)***		(0.030)***		(0.038)***		(0.038)***
First-stage F-statistic		421.4		418.6		121.5		120.3
N	247,934	247,934	247,934	247,934	139,274	139,274	139,274	139,274
Mean of $dead_i$	0.0488	0.0488	0.0488	0.0488	0.0229	0.0229	0.0229	0.0229

Standard errors given in parentheses are robust and clustered on the firm level, stars indicate significance levels: * $p < 0.1$ ** $p < 0.05$, *** $p < 0.01$. The sample consists of all private sector workers above age 40 employed on April 1, 2002 in Austria of whom we know they are married at that point of time. All estimations also include a constant and missing indicator dummies for education and occupational class which are not reported.

† Contains full sets of industry sector, age, and country of birth dummies.

Table 7 — Linear regressions of ten-year death probability for white and blue collar workers separately.

	White collar workers				Blue collar workers			
	Men		Women		Men		Women	
$P[dead_i]$	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Income								
log(total income 2002)	-0.0112	0.0004	-0.0040	0.0017	-0.0204	-0.0022	-0.0041	-0.0031
	(0.001)***	(0.002)	(0.001)***	(0.002)	(0.001)***	(0.003)	(0.001)***	(0.003)
Health and workplace security proxies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other personal and firm characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupational class & education	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other covariates†	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First-stage coefficient		0.7399		0.4655		0.6989		0.4456
		(0.047)***		(0.021)***		(0.018)***		(0.021)***
First-stage F-statistic		252.7		501.5		1,504.8		430.1
N	259,432	259,432	279,550	279,550	284,057	284,057	175,262	175,262
Mean of $dead_i$	0.0419	0.0419	0.0210	0.0210	0.0594	0.0594	0.0275	0.0275

Standard errors given in parentheses are robust and clustered on the firm level, stars indicate significance levels: * $p < 0.1$ ** $p < 0.05$, *** $p < 0.01$. The sample consists of all private sector workers above age 40 employed on April 1, 2002 in Austria with non-missing occupational class. All estimations also include a constant and a missing indicator dummy for education and occupational class which are not reported.

† Contains full sets of industry sector, age, and country of birth dummies.

TABLE 8 — Control function probit estimations.

	Men		Women	
$P[dead_i]$	Probit	CF Probit	Probit	CF Probit
log(total income 2002)	-0.0100	0.0010	-0.0030	0.0004
	(0.000)***	(0.002)	(0.000)***	(0.002)
Health and workplace security proxies	Yes	Yes	Yes	Yes
Other personal and firm characteristics	Yes	Yes	Yes	Yes
Occupational class & education	Yes	Yes	Yes	Yes
Other covariates†	Yes	Yes	Yes	Yes
N	661,695	661,695	514,415	514,415
Log-likelihood	-123,688.8	-123,658.0	-54,717.2	-54,714.7
Mean of $dead_i$	0.0495	0.0495	0.0232	0.0232

Reported are marginal effects at the mean, firm-level clustered standard errors calculated by the delta method are given in parentheses, stars indicate significance levels: * $p < 0.1$ ** $p < 0.05$, *** $p < 0.01$. Coefficients have been divided by the scalar κ_i before calculating marginal effects (see Section 3 for details). The sample consists of all private sector workers above age 40 employed on April 1, 2002 in Austria.

† Contains full sets of industry sector, age, and country of birth dummies.