

# Estimating Compensating Wage Differentials with Endogenous Job Mobility\*

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## Abstract

We demonstrate a strategy for using matched employer-employee data to correct endogenous job mobility bias when estimating compensating wage differentials. Applied to fatality rates in the census of formal-sector jobs in Brazil between 2003-2010, we show why common approaches to eliminating ability bias can greatly amplify endogenous job mobility bias. By extending the search-theoretic hedonic wage framework, we establish conditions necessary to interpret our estimates as preferences. We present empirical analyses supporting the predictions of the model and identifying conditions, demonstrating that the standard models are misspecified, and that our proposed model eliminates latent ability and endogenous mobility biases.

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# 1 Introduction

Despite substantial attention by economists, there is little consensus regarding the importance of non-wage job amenities in determining employment and wages. Since Rosen's (1974) model on the estimation of preferences for job amenities in perfectly competitive labor markets, theorists have demonstrated that allowing for search frictions causes the equilibrium wage distribution to be a biased representation of preferences. Data limitations have hindered empirical research from advancing alongside theory to consider how labor-market frictions affect the data generating processes in hedonic wage models.

Empirical work has centered on the problem of worker sorting across jobs on the basis of unobserved ability. Theory suggests that sorting on ability will negatively bias cross-sectional estimates of compensating wage differentials for disamenities (Hwang, Reed and Hubbard 1992). However, the standard panel data correction for unobserved worker ability suggests that cross-sectional estimates are instead positively biased (Brown 1980; Kniesner, Viscusi, Woock and Ziliak 2012). We argue that this apparent contradiction can be reconciled by accounting for the endogeneity of job mobility decisions. The remarkable implication, for which we find strong empirical support, is that including worker effects to alleviate ability bias also isolates variation generating endogenous mobility bias. The net effect is to increase the total bias.

In this paper, we demonstrate a strategy for using longitudinally-linked employer-employee data to estimate compensating wage differentials and identify workers' preferences for job amenities in the presence of endogenous mobility. The key intuition is that conditioning on firm identity alleviates the primary source of bias associated with job mobility. We first present a search-based theoretical framework in which endogenous mobility bias arises due to heterogeneous firm wage effects, and clarify the conditions under which hedonic wage models can identify preferences. We then estimate compensating wage differentials for occupational fatality risk using the complete census of all formal-sector jobs in Brazil between 2003-2010, show that the implications of the search model are supported by the data, and present diagnostic tests in support of the assumptions required to interpret our estimates as preferences.

Our benchmark empirical model is an adaptation of the two-way fixed effects model introduced by Abowd, Kramarz and Margolis (1999), extended to include a restricted form of job-match effects that may be arbitrarily correlated with observed time-varying worker and firm characteristics. We also show that the within-worker model commonly estimated in the literature is contaminated by endogenous mobility. If endogenous mo-

bility bias arises from workers searching across employers with different compensation policies, within-worker estimates should be biased downward relative to estimates from our benchmark model that controls for worker and employer heterogeneity. The intuition behind this is straightforward: when workers change jobs to increase utility, as when climbing the job ladder, they often receive simultaneous increases in wages and reductions in fatality rates, which introduces wrong-signed contamination. We observe exactly this pattern. Our estimate of the standard cross-sectional hedonic wage model indicates that a one-unit increase in the fatality rate per 10,000 workers is associated with an increase in wages of about 3.6%. The estimate from the worker effects model, which is prone to endogenous mobility bias, is an order of magnitude smaller at 0.4%.

When we add controls for establishment-level heterogeneity in wages, the estimated compensating wage differential exceeds the cross-sectional estimate, at 4.9%. This model relaxes several restrictive assumptions, allowing unobserved establishment heterogeneity to be correlated with both fatality rates and unobserved worker characteristics. Our findings support the qualitative implications of the search model on the nature of endogenous mobility, and suggest that the effects of endogenous mobility bias are quite large. The results also help explain the conflicting estimates of compensating differentials throughout the literature.

Hwang, Mortensen and Reed (1998) and Bonhomme and Jolivet (2009) warn that controlling for establishment heterogeneity cannot correct for bias in reduced-form hedonic wage models. However, their models do not consider variation in amenities across jobs within firms; we extend the basic structure of their theoretical search models to consider identification in this scenario. Our empirical model assumes employers pay all workers the value of their portable skills plus a premium common to all jobs in the same establishment, and also pay a compensating wage differential for fatal risk. Given these assumptions, our benchmark model estimates the hedonic pricing function. We devote considerable attention to checking the validity of our identifying assumptions and considering the robustness of our results to their violation.

Although it is common in the literature to interpret estimates of hedonic wage models as measures of worker preferences, or willingness to pay, the assumptions that underlie this interpretation are seldom laid bare. In our case, if we assume, as is also standard, that workers have common preferences over risk and wages, and that employers offer all workers in the same establishment a common increment to wages that is additively separable from any occupation wage effects, then we show that one can interpret our

estimates as measures of the willingness-to-pay for decreased fatality risk.

The consistency of our benchmark estimates with predictions from hedonic search theory is striking. However, their validity relies on the assumption that wages are separable in worker and employer heterogeneity. They also require that worker mobility across jobs with different levels of risk is exogenous after controlling for employer heterogeneity. We empirically evaluate the additive separability assumption by estimating the relative importance of job match effects, which nest any establishment-by-occupation wage premia in our model, and by testing whether the probability that a worker separates from their job depends on fatality rates after controlling for establishment-specific heterogeneity. The diagnostic analyses performed by Card, Heining and Kline (2013) are relevant in our context, and we show that in Brazil, like Germany, there is little evidence against separability or against the assumption of exogenous mobility in our preferred model.

An important aspect of our analyses is to provide evidence regarding which specifications of the hedonic wage model satisfy the exogeneity assumption. Although we clearly reject the assumption in the worker effects model, we perform several residual diagnostic analyses to test whether mobility is conditionally exogenous in our preferred benchmark model. We draw on a specification test proposed by Caetano (2015) and implemented in Caetano and Maheshri (2013). The idea behind the test is that exogeneity can be assessed by examining the empirical wage-risk profile around the corner solution at zero. Since jobs that are positively selected on unobservables are likely to be clustered around zero fatal risk, a properly-specified empirical model that accounts for these correlated unobservables will not exhibit discontinuities as the level of fatality risk approaches zero. We estimate a non-parametric hedonic wage model to implement this diagnostic strategy, and conclude that the worker effects model is misspecified, but there is no evidence against exogeneity in our benchmark model.

To further assess the impacts of any remaining endogenous mobility bias in our benchmark model, we test whether the model is robust to isolating different forms of job mobility. We find very similar estimates using mass displacement events relative to voluntary job-to-job transitions. We also show that the magnitude of endogenous mobility bias is larger in regions of Brazil with greater search frictions, and that our benchmark model eliminates this pattern of biases.

Finally, we construct an instrument for changes in occupational fatality rates using the network structure of the data. For each worker with a job-to-job transition, we calculate the average change in the fatality rate for workers who separated from jobs at

the same establishment and occupation in the two preceding years, and instrument for the realized change with this predicted change. The intuition behind this instrument is that one’s coworkers in an establishment-by-occupation cell are likely to have similar preferences for risk since they sorted into the same job, have similar characteristics, and have similar outside opportunities. However, the idiosyncratic match effects that drive a worker’s decision to change jobs are plausibly independent of the job mobility decisions of their past co-workers. The IV estimates are almost identical to the benchmark model, suggesting that any potential endogeneity between omitted match effects and fatality rates causes very little bias. We conclude by considering the implications of our analysis for other empirical settings.

## 2 Identification with Dynamic Search using Matched Data

We begin with an illustration of endogenous mobility bias that arises in hedonic wage models. Following the example, we introduce a partial equilibrium search model motivated by Hwang et al. (1998) and similar to those estimated by Dey and Flinn (2008) and Bonhomme and Jolivet (2009). We use the model to describe how conditioning on employer identity using matched employer-employee data can alleviate bias caused by search frictions, and furthermore, the empirical conditions under which this approach can be used to identify preferences in hedonic wage models.

### 2.1 Conceptual Framework

We first describe the familiar problem of bias caused by omitted worker ability, and then characterize endogenous job mobility as a closely analogous problem. Consider Figure 1 as depicting the frictionless model of hedonic wages. Worker 1 has preferences represented by the indifference curve,  $u_1$ , and chooses a wage-risk combination  $(w_1, R_1)$  to maximize utility along ‘offer curve 1’. The offer curve is the set of wage-risk bundles that are equally profitable for a representative firm.

If workers are equally productive, variation in  $(w, R)$  pairs arises because workers with different preferences choose different jobs, or because workers sort across firms with different isoprofit curves. Only in this simple case can cross-sectional variation in wage-risk pairs identify the hedonic pricing locus. In their comprehensive review of 32 studies that estimate compensating wage differentials for occupational fatality risk in the U.S., Viscusi and Aldy (2003) report that all but one relied upon this basic cross-sectional model for identification.

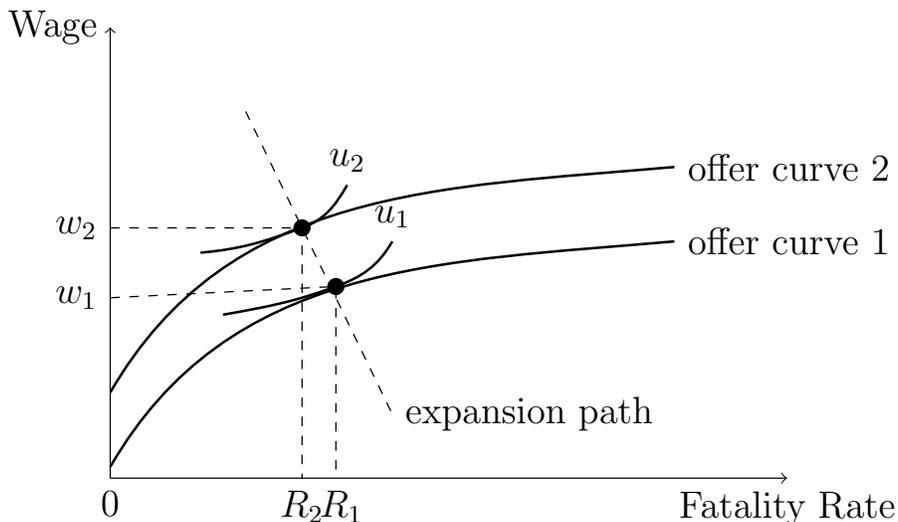


Figure 1: Equilibrium Wage-Risk Relationships

This empirical strategy is flawed if workers differ in unmeasured ability (Brown 1980; Thaler and Rosen 1976). Suppose the representative firm can operate two offer curves that generate equal profit: ‘offer curve 1’ for workers of low ability and ‘offer curve 2’ for workers of relatively high ability (Hwang et al. 1992). In the figure, low ability workers choose  $(w_1, R_1)$  on indifference curve  $u_1$  while high ability workers choose  $(w_2, R_2)$  on indifference curve  $u_2$ . In this setting, cross-sectional variation in wage-risk pairs has two sources—variation along each offer curve, due to different tastes, and variation along the expansion path, due to different abilities. Furthermore, if safety is a normal good, high ability workers trade off some of their higher earning power for reduced risk. Variation along the offer curves is needed to identify the compensating wage differential, but the observed variation in accepted wage-risk pairs is contaminated by variation along the expansion path.

This suggests cross-sectional estimates of the compensating wage differential for job disamenities are negatively biased. Hwang et al. (1992) add proxies for unobserved ability to the cross-sectional model of Thaler and Rosen (1976) and find, consistent with theory, this correction increases the estimated compensating differential for fatal occupational injury by a factor of 10 (the bias is negative for disamenities and positive for amenities). By contrast, a predominant approach, originating with Brown (1980), has been to use panel data to eliminate unobserved worker ability. Brown (1980) and Kniesner et al. (2012) find that panel data estimates of the compensating wage differential are positive, but much

smaller than comparable cross-sectional estimates. These findings contradict the intuition about the likely direction of ability bias; we argue that this apparent contradiction arises from endogenous job mobility bias.

### **Endogenous Mobility Bias**

In a first-differenced model using panel data, the compensating wage differential is identified from the relationship between changes in individual wages and changes in fatality rates, usually associated with movements from job to job. These models therefore rely on the assumption that mobility of individual workers across risk levels is exogenous—that is, changes in the wage residual are not correlated with changes in fatality rates.

We can reinterpret Figure 1 to illustrate why the exogenous mobility assumption is likely not valid, and the consequences of its violation. Suppose the two equilibria in the figure represent choices of a single worker climbing up the job ladder, from job  $(w_1, R_1)$  to job  $(w_2, R_2)$ , to increase her utility from  $u_1$  to  $u_2$ . This could occur if, for instance, it takes time for a worker to find jobs offering higher utility due to frictions in on-the-job search (Hwang et al. 1998). A similar problem occurs if workers and firms learn about ability, match quality, or comparative advantage over time, and job changes are associated with workers moving to better matches (Gibbons and Katz 1992; Gibbons, Katz, Lemieux and Parent 2005).

In either case, the movement from bundle  $(w_1, R_1)$  to  $(w_2, R_2)$  involves a simultaneous increase in the wage and a decrease in the fatality rate. If job mobility is primarily associated with changes in worker utility, then within-worker changes may isolate variation along the expansion path. As a result, panel data estimates, while correcting for unobserved ability bias, can actually make the aggregate bias worse. Bias from endogenous mobility could explain why panel data estimates of compensating wage differentials tend to be smaller than cross-sectional estimates.

We posit that endogenous mobility bias is primarily associated with on-the-job search. The search framework, outlined below, has many empirical implications. First, estimates of the compensating wage differential that correct for person-specific and employer-specific heterogeneity in wages should be larger than estimates that control only for person-specific heterogeneity. Second, the bias should be larger when the variance in establishment wage effects is larger, which increases the relative rate of return to on-the-job search. Third, the bias should be smaller in labor markets with fewer search frictions. Fourth, workers should be more likely to separate from jobs with higher levels of risk, and less likely to

separate from jobs with high employer-specific pay. We return to evaluate each of these implications as part of our empirical analysis.

## 2.2 Search Model

We introduce a simple model of on-the-job search in which wages are affected by unobserved worker and firm heterogeneity. Our goal is to characterize conditions under which the equilibrium compensating wage differential is identifiable using matched longitudinal employer-employee data linked to non-wage job amenities. We also describe conditions under which the estimated differential can be interpreted as measuring worker preferences. The key innovation is an empirical setting in which it is possible to condition on employer identity. If firms offer jobs with different levels of risk, but provide a common wage premium across all jobs, we show it is possible to identify the worker preferences in the presence of search frictions and latent employer heterogeneity. In the empirical work, we provide evidence consistent with our key assumption that establishment and occupation wage effects are additively separable.

The model is populated by jobs, indexed by  $f$ , and by workers, indexed by  $i$ . Jobs are distinguished by two characteristics: the firm that offers them,  $j$ , and the occupation,  $k$ . We use  $j(f)$  to denote the firm offering job  $f$  and  $k(f)$  to denote the occupation of  $f$ . The set of possible occupations is finite. Each firm also has a distinct type,  $n(j)$ , that affects its production technology. The set of possible types is also finite, and can be thought of heuristically as industries.

Following Hwang et al. (1998), Dey and Flinn (2008) and Bonhomme and Jolivet (2009), workers have homogeneous preferences and are either unemployed, and receive instantaneous log utility  $b$ , or they are employed and receive instantaneous log utility  $v = w - h(R)$ , where  $w$  is the log wage,  $R$  is the per-period probability of death on the job, and  $h$  is any quasiconcave function of  $R$ . The marginal willingness to accept fatal risk is therefore  $h'(R)$ . Workers differ in ability,  $\tilde{\theta}_i$ , and firms differ in productivity,  $\tilde{\psi}_{j(f)}$ . We abstract from the intensive margin labor supply problem, and assume that the number of hours worked is exogenously fixed for all workers.

A match between worker  $i$  and job  $f$  results in output given by the constant returns to scale function

$$q_{if} = \tilde{\psi}_{j(f)}^\alpha \tilde{\theta}_i^\beta. \tag{1}$$

Firms choose a wage offer,  $w_{if}$ , and an amenity level,  $R_f$ , specific to each worker and each

job. Wage offers are characterized by three components:

$$\log(w_{if}) = \theta_i + y(R_f) + \psi_j \quad (2)$$

where  $\theta_i = \beta \log(\tilde{\theta}_i)$ , so workers always recover the full value of their productive characteristics,  $y(R_f)$  is the compensating wage differential paid to the worker in exchange for accepting risk level  $R_f$ , and  $\psi_j = \xi_j \alpha \log(\tilde{\psi}_{j(f)})$  so that workers at firm  $j$  recover a fixed share  $0 \leq \xi_j \leq 1$  of the log of job surplus as a quasi-rent.<sup>1</sup> We treat the existence of these quasi-rents as a stylized labor-market primitive based on a large body of empirical evidence from labor markets worldwide, rather than as a solution to a specific wage-setting problem faced by the firm.<sup>2</sup>

With this setup, the log utility on any match is additively separable into a worker-specific and a job-specific component. Let  $v_f = \psi_j + y(R_f) - h(R_f)$  be the job-specific component. Since workers always recover the value of their individual productivity differences, worker mobility and firm profits depend only on the job-specific component  $v_f$ . This assumption is relaxed in our empirical work, which allows for arbitrary correlation between worker heterogeneity, the level of risk on the job, and the firm effect on wages.

Firms maximize expected profits on job  $f$  by solving a recruiting problem that entails choosing the reference log utility level  $v_f$  and the level of risk  $R_f$  to provide, conditional on the occupation,  $k(f)$ .

$$\max_{v_f, R_f} m(v_f) [q_{if} - w_{if} - c_{n(j),k(f)}(R_f)]$$

subject to

$$v_f = w_{if} - \theta_i - h(R_f)$$

where  $m(v_f)$  is the probability that an offer with job-specific log utility  $v_f$  is accepted. Since our focus is not on the origins of firm wage effects, we abstract from the exact nature of this recruiting problem.

Given an optimal recruiting strategy,  $v_f^*$ , the constraint can be substituted into the objective function to give:

$$\max_{R_f} m(v_f^*) [q_{if} - v_f^* - h(R_f) - \theta_i - c_{n(j),k(f)}(R_f)]$$

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<sup>1</sup>We also can, and in the empirical work do, allow for wages, and implicitly output, to vary with occupation and industry.

<sup>2</sup>See, for example, Abowd et al. (1999) and Card et al. (2013).

The first order condition with respect to  $R_f$  yields  $h'(R_f^*) = -c'_{n,k}(R_f^*)$ . This implies that any job in industry  $n$  and occupation  $k$  will provide the same level of risk, regardless of the firm that offers it.

It follows that the increment to utility from job  $f$ ,  $v_f^* = y(R_{n(j),k(f)}^*) + \psi_j - h(R_{n(j),k(f)}^*)$ , must be constant for all  $j$  and all  $k$ . This constant, however, is not separately identifiable, and is absorbed into the firm wage effect  $\psi_j$ . Therefore, within each firm  $j$

$$y(R_f^*) = h(R_f^*) \text{ for all } f. \quad (3)$$

This establishes that the compensating wage differential offered for a job with risk  $R$  is exactly equal to the disutility from tolerating risk  $R$  for all jobs within a given firm. In Appendix B.1 we extend this model to prove that this result about the observed offer distribution also holds with respect to the steady-state distribution of realized wage-risk pairs.

To be clear, our identification result does not extend to all models of job-search behavior. For example, in a search model in which wages are determined by individual Nash-bargaining with each firm, the result may not generally hold if the Nash bargaining parameters are correlated with fatality rates. However, for the broad class of wage-posting models that have been widely considered in the hedonic search literature, we have established sufficient conditions for identification of preferences using matched employer-employee data to control for firm-specific unobservables.

### 3 Data and Sample Descriptions

Our empirical analyses use matched employer-employee data from Brazil's *Relação Anual de Informações Sociais*, or Annual Social Information Survey (RAIS), from 2003-2010. These data play two roles in our analysis: as a source of information about job-level fatalities, which we use to construct fatality rates, and as a source of information about jobs and earnings.

#### 3.1 RAIS Data

RAIS is a census of all formal-sector jobs. Each year, the Brazilian Ministry of Labor and Employment (MTE) collects data on every formal sector job for the purpose of administering the *Abono Salarial* — a constitutionally mandated annual bonus equivalent to one month's earnings. The information in RAIS is provided to the MTE at the establishment

level by a company administrator.<sup>3</sup> Coverage is universal, as employers who fail to complete the survey face mandatory fines and also risk litigation from employees who have not received their *Abono Salarial*.<sup>4</sup>

For every job, the employer reports worker characteristics, establishment characteristics, and characteristics of the job. For our purposes, the most important job characteristics are the wage, whether the job ended because of a fatal injury on the job, and the worker’s occupation. The reported establishment characteristics include the plant’s industry, location, and number of employees.

That industry and occupation are reported by the employer is an advantage of RAIS relative to household surveys. Occupations are measured with error, and are not consistently coded over time in many major U.S. surveys. Inconsistent measurement of occupation can badly bias panel data models, as has been illustrated using the CPS by Moscarini and Thomsson (2007), in the PSID by Kambourov and Manovskii (2008), and in the NLSY by Speer (2016). In our employer-reported data, these measurement concerns may be substantially reduced.

### 3.2 Measuring Fatality Rates

When a job ends the employer reports the cause of separation, which determines any severance compensation to which the worker is entitled. The employer chooses from a list of 23 options, three of which cover work-related fatalities (see Appendix Table A.1). The RAIS data thus contain a census of fatal occupational injuries from which we construct measures of fatality risk.

We measure the average fatality rate for each of 11,440 two-digit industry by three-digit occupation cells as the number of fatal injuries per 100,000 full-time full-year-equivalent workers. This follows the Bureau of Labor Statistics method of reporting fatal injury rates since 2007.<sup>5</sup> See Appendix B.2 for details of this calculation. We also pool fatality data in three-year windows. For example, the measured fatality rate for an industry-occupation cell in 2005 is constructed using fatality counts and hours across all jobs in that cell from 2003, 2004, and 2005. We do so for comparability with previous literature, and to smooth

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<sup>3</sup>In smaller firms and plants, this is likely the owner or plant manager; in larger establishments there may be dedicated personnel who submit the information.

<sup>4</sup>For details on labor market formality and wage setting institutions, see Appendix B.3

<sup>5</sup>See <http://www.bls.gov/iif/oshnotice10.htm> for a description of how and why the BLS constructs hours-based fatality rates. One relative advantage of our data is that we observe both the number of months a job lasted as well as the number of contracted weekly hours. By contrast, the BLS fatality rates are scaled by average hours at work from the CPS.

out fluctuations in the annual fatality rates (Kniesner et al. 2012).

Of course, it is possible that subjective risk assessments vary systematically from the measured fatality rates. However, our disaggregation by narrow industry and occupation gives us much more variation in the fatality risk than has been available in previous studies. Since fatal accidents are rare events, one concern is that the decreased bias from this disaggregation entails a large increase in variance of estimated cell-specific fatality rates. We address this trade-off by restricting our sample to cells with at least 10,000 full-time full-year-equivalent workers. Given the documented importance of aggregation bias in estimates of compensating wage differentials (Lalive 2003; Tsai, Liu and Hammitt 2011), we prefer as disaggregate a measure as possible. We leave any remaining measurement issues for future research.

Table A.2 reports average fatality rates by major industry and major occupation. The data are broadly consistent with prior expectations about the level of fatality risk and the distribution of risk across industries and occupations. The overall fatality rate (including both men and women) is 4.9 fatalities per 100,000 full-time full-year equivalent workers. By comparison, the fatality rate in the U.S. was about 3.7 per 100,000 full-time full-year-equivalent workers over the same time period. In our data, fatality rates are highest in the Agriculture and Fishing, Mining, Construction, and Transportation industries. Among occupations, the fatality rate is highest among Production and Manufacturing I workers, and lowest among Professionals, Artists, and Scientists.

### 3.3 Analysis Sample and Variable Definitions

The unit of observation in the raw data is a job-year, where a job is defined by a person-establishment-occupation combination. We follow Abowd et al. (1999), Woodcock (2008), and Card et al. (2013) in restricting our sample to a single dominant job for every worker in every year. We define expected earnings as the product of the average monthly wage rate with the number of months the worker was employed. For each worker, their dominant job in any year is the one with the highest expected earnings.

To prepare our data, we first define a population of interest, and then construct an analysis sample that we use throughout the empirical work. The population of interest is all dominant jobs held by workers between the ages of 23 and 65. We restrict analysis to the subsample of jobs held by men with at least 30 contracted hours per week, in establishments with at least two workers. We exclude government jobs and temporary jobs. As described in Section 3.2, we consider only jobs in 2-digit industry by 3-digit

occupation cells that contain at least 10,000 full-time full-year-equivalent workers. Finally, we Winsorize the data at the 1st and 99th percentiles of the log wage distribution. After imposing these restrictions, we have a final analysis sample with about 83 million job-years.

To measure the worker's wage, we use the RAIS report of the worker's average monthly earnings. If the worker is in the job for less than 12 months during the year, the variable reported by RAIS represents one month's pay. This variable measures the monthly wage rate, which is a common institutional arrangement in Brazil. For consistency with prior research, we convert to an hourly wage rate.<sup>6</sup> We report all wages and earnings in 2003 Brazilian Reais.<sup>7</sup>

For each job, the data report the date of hire. Hence, even for the first in-sample job-year, we have an accurate measure of tenure on that job. Using tenure we impute labor market experience as the maximum of tenure in the first observed job or potential experience, whichever is largest, plus observed accumulated experience from jobs held during the years in which we have data. We also observe the worker's gender, race, and education as reported by their employer.<sup>8</sup>

Table 1 reports descriptive statistics for the male population and analysis sample. Relative to the population, observations in the analysis sample involve workers that are slightly younger, less educated, less experienced, and in riskier jobs. This is due primarily to selection on jobs with more than 10,000 full-time full-year-equivalent workers. The average monthly wage in the analysis sample is 682 Reais, and the average fatality rate is 8.29 deaths per 100,000 full-time full-year workers. Finally, 9 percent of sample observations are associated with jobs that have a measured fatality rate of zero. We discuss these zero-risk jobs in more detail in Section 4.4.

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<sup>6</sup>First we calculate a weekly wage rate as the monthly wage rate divided by 4.17. We then calculate the hourly wage rate as the weekly wage rate divided by the contracted weekly hours, which are also reported for every job.

<sup>7</sup>Conveniently, one Brazilian Real in 2003 is worth approximately 1.5 Brazilian Reais in 2010. Likewise, in 2010, one U.S. dollar was worth 1.66 Brazilian Reais. Hence, one can loosely interpret our results in 2010 dollars.

<sup>8</sup>Because individual characteristics are reported by the employer, they can change as workers move from job to job. Cornwell, Rivera and Schmutte (forthcoming) provide evidence that discrepancies in employers' reports of worker characteristics are associated with other unobserved determinants of earnings, so we leave these variables in as reported.

Table 1: Summary Statistics

	Population	Analysis Sample
Age	36.98	36.23
Race <i>branco</i> (White)	0.56	0.58
Elementary or Less	0.40	0.40
Some High School	0.09	0.10
High School	0.36	0.39
Some College	0.04	0.04
College or More	0.11	0.07
Contracted Weekly Hours	42.19	43.34
Log Hourly Wage	1.47	1.37
Total Experience (Years)	20.58	19.86
Job Tenure (Months)	58.70	44.28
Fatality Rate (per 100,000)	7.14	8.29
Zero Fatality Rate (Percent)	0.14	0.09
Number of Observations	158,254,802	83,418,032

Notes: The population includes all dominant jobs held by men between ages 23 and 65. ‘Analysis Sample’ restricts to jobs with at least 30 contracted hours per week, excluding government jobs and temporary jobs, held at establishments with at least two workers, in 2-digit industry by 3-digit occupation cells with a total of at least 10,000 full-time full-year equivalent workers, and with hourly earnings between the 1st and 99th percentiles of the Analysis Sample earnings distribution.

## 4 A Hedonic Wage Model for Matched Data

We begin by discussing the nature of the endogenous mobility problem in hedonic wage models relative to common identification strategies in the literature. We then describe our benchmark two-step orthogonal match effects model, the forms of mobility that are permissible in this model without causing endogeneity bias, estimation results, and evidence on the additive separability of wage components.

### 4.1 Model Specification and the Endogenous Mobility Problem

The general form of the hedonic wage model most frequently estimated in the literature is:

$$w_{it} = x_{it}\beta + \gamma a_{c(i,t),t} + \varepsilon_{it}, \quad (4)$$

where  $w_{it}$  is the log wage of worker  $i$  at time  $t$ ,  $x_{it}$  contains observable characteristics,  $c(i, t)$  indicates the industry-occupation cell of the job on which worker  $i$  was employed in period  $t$ , and  $a_{c(i,t),t}$  is the fatality rate associated with that job at time  $t$ . Interest centers on  $\gamma$ , which measures the relationship between wages and fatality rates. Identification

of  $\gamma$  depends on assumptions about the structure of the error term. This cross-sectional estimator imposes very strong assumptions: conditional on observed controls,  $x_{it}$ , the error term is uncorrelated with the fatality rate.

Many studies relax this assumption by using panel data on workers to estimate the following model:

$$w_{it} = x_{it}\beta + \gamma a_{c(i,t),t} + \theta_i + \nu_{it}. \quad (5)$$

The term  $\theta_i$  captures unobservable worker-specific characteristics that affect wages and may be correlated with risk. This model, together with the fixed effects assumption, purges the model of any individual-specific correlation between fatal risk and wages. However, the main source of identifying variation in this model comes from switches across jobs with different values of risk,  $a_{c(i,t),t}$ . If jobs are exogenously terminated and workers are randomly reassigned new jobs in a manner that is uncorrelated with  $a_{c(i,t),t}$ , then variation in wages and fatality rates between origin and destination jobs could provide unbiased estimates of the marginal willingness to accept fatal risk. However, workers tend to exit low-wage jobs at a higher rate, even after conditioning on unobserved ability (Abowd, McKinney and Schmutte 2015), and these workers are likely to switch to jobs at firms that pay higher wages on average and have lower fatality rates. The omission of firm-specific or match-specific heterogeneity from the model hence induces endogenous mobility bias in Equation 5.

Our baseline model accounts for these forms of endogenous mobility. We fit the model in two steps. In the first step, we estimate

$$w_{it} = x_{it}\beta + \tilde{\gamma} a_{c(i,t),t} + \Phi_{i,J(i,t)} + \epsilon_{it}. \quad (6)$$

Following Abowd et al. (1999),  $\Phi_{i,J(i,t)}$  denotes the match effect between worker  $i$  and establishment  $J$  at which worker  $i$  is employed in period  $t$ . In this first-stage model,  $\tilde{\gamma}$  is identified from the very slight within-match variation in fatality risk, which is only 3% of the total variation. These small intertemporal changes may not be sufficiently salient to trigger wage adjustments. Instead, our objective is to identify the compensating differential using variation in accepted fatality risk across jobs while correcting for the potential bias associated with the non-random decision to change jobs.

Next, we estimate

$$P_{it} = \gamma a_{c(i,t),t} + \theta_i + \Psi_{J(i,t)} + \xi_{it}. \quad (7)$$

The dependent variable is the log wage net of the effects of observable characteristics, as

estimated from Equation (6).

$$P_{it} \equiv \widehat{\gamma} a_{c(i,t),t} + \widehat{\Phi}_{i,J(i,t)} + \widehat{\epsilon}_{it} = w_{it} - x_{it} \widehat{\beta}.$$

Once again,  $\theta_i$  is a person-specific effect and  $\Psi_{J(i,t)}$  is the effect of establishment  $J$  at which worker  $i$  is employed at time  $t$ .

This model, which we call the orthogonal match effects (OME) model, is motivated by extensions of the AKM model (Abowd et al. 1999) that seek to address match quality proposed by Woodcock (2008) and Barth, Bryson, Davis and Freeman (2016). Our model allows  $x_{it}$  to be arbitrarily correlated with worker effects, establishment effects, or match effects, without imposing assumptions on their joint distribution. The second stage purges  $x_{it} \widehat{\beta}$  from the dependent variable, and regresses the remaining unexplained variation in log wages on the fatality rate, worker effects, and establishment effects. This allows fatality rates to be arbitrarily correlated with unobserved worker and establishment effects, eliminating the first major form of endogenous mobility relative to the worker effects model. Note that establishment effects also control for the average compensating differentials associated with all other establishment-level amenities besides safety, substantially mitigating this common data problem.

Of course, our model still requires several restrictions: the true match effects must be uncorrelated with worker effects, with establishment effects, and with fatality rates. The first two assumptions are less restrictive than they may seem. Since worker and establishment effects are nuisance parameters in this model rather than objects of interest, it is not problematic if some of the pure match effect loads onto these parameters, as long as doing so does not violate the third condition, that match effects are uncorrelated with fatality rates. For example, if an individual with consistently high match effects were mis-identified as a person with a high worker effect, this would not affect the estimate of  $\gamma$  under the maintained assumptions. Similarly, if all of the workers at an establishment happened to have high match effects, and these effects were wrongly attributed to the establishment effect, this too would not necessarily bias the estimate of  $\gamma$ . Whereas many empirical studies of matching impose the assumption that match effects are independently and identically distributed, this is a sufficient but not necessary condition in the above model.

The third restriction,  $E[\Phi_{i,J(i,t)} a_{c(i,t),t} | \theta_i, \Psi_{J(i,t)}] = 0$ , is potentially concerning, as it replaces the exogenous mobility assumption of the worker effects model with another, weaker, exogenous mobility assumption. It will be violated if, for instance, moves into

jobs with higher match quality are associated with movements into jobs with lower risk. Although we find a wide variety of evidence validating this assumption, as discussed in Section 4.3, we also consider an instrumental variables model that relaxes the assumption in Section 5.3.

## 4.2 Baseline Results

Table 2 compares estimates of the compensating wage differentials and implied value of a statistical life (VSL) using each of the four models from Equations 4 to 7. Column 1 includes the estimates from Equation 4, the pooled cross-sectional model. The estimate of  $\gamma$  suggests that an increase in the average fatality rate of one death per 1,000 full-time equivalent worker-years increases wages by about 36%, conditional on a cubic in experience interacted with race, job tenure, plant size, education, race, year, state, 1-digit industry and 1-digit occupation effects. Re-scaling this coefficient implies a VSL of about 2.85 million Brazilian Reais (in 2003 Reais).<sup>9</sup>

In Column 2, which includes estimates from Equation 5, the worker effects model, the compensating wage differential falls by about 89% to 0.04. This relationship between cross-sectional estimates and longitudinal within-worker estimates has been well-documented in the literature using US data, including Brown (1980), Kniesner et al. (2012), and Lavetti (2015), and we find this same pattern in the Brazilian data.

Columns 3 and 4 present estimates from each stage of the two-step estimator. Equation 6 is estimated in Column 3, and includes match effects. Not surprisingly, there is very little response to wages from the small amounts of variation that are observed within job matches over time. In fact, the estimate of -0.004 is not statistically significantly different from zero at the 0.01 level even with more than 83 million observations.

The benchmark OME specification from Equation 7 is presented in Column 4. The estimated coefficient, 0.49, is higher than the estimates from all three of the preceding models. The VSL implied by the orthogonal match effects model is 3.84 million Reais, with a 95% confidence interval of 3.81 to 3.86 million Reais.

Including worker effects decreased the estimate by 89% relative to the cross-sectional model. The benchmark OME model, which includes worker and establishment effects, more than offsets this decline, yielding an estimate larger than the cross-sectional model.

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<sup>9</sup>The VSL is calculated as:  $VSL = \frac{\partial w}{\partial a} * 1000 * 2000$ . Since wages are measured hourly while the fatality rate is measured in deaths per 1,000 full-time equivalent worker years, the derivative is scaled by 1,000 FTEs and by 2,000 hours worked per FTE. Since the dependent variable in the regressions is the log wage, the derivative is estimated by solving  $\frac{1}{w} \frac{\partial w}{\partial a} = \hat{\gamma}$  at the mean wage, so  $VSL = \bar{w} \hat{\gamma} * 2,000,000$ .

Table 2: Compensating Wage Differentials for Full-Time Prime-Age Men

	Dependent Variable: $\ln(Wage)$			
	(1) Pooled	(2) Worker Effects	(3) Match Effects	(4) Orth. Match Effects
Fatality Rate (3-Yr MA)	0.363* (0.001)	0.041* (0.001)	-0.004 (0.001)	0.490* (0.001)
Zero Fatality Rate	0.070* (0.000)	0.009* (0.000)	-0.006* (0.000)	0.027* (0.000)
Experience	0.029* (0.000)	0.099* (0.000)	0.174* (0.001)	
Experience Sq.	-0.001* (0.000)	-0.003* (0.000)	-0.002* (0.000)	
Experience Cu.	0.000* (0.000)	0.000* (0.000)	0.000* (0.000)	
Job Tenure	0.003* (0.000)	0.001* (0.000)	-0.012* (0.000)	
N	83,411,371	83,418,032	83,418,032	83,418,032
R-Sq	0.499	0.914	0.978	0.965
VSL (millions of reais)	2.85	0.32	-0.03	3.84
95% CI	[2.83, 2.86]	[0.30, 0.33]	[-0.05, -0.01]	[3.81, 3.86]

Notes: Model 1 also includes 1-digit industry effects, 1-digit occupation effects, year effects, state effects, race effects interacted with a cubic in experience, indicators for small and medium-sized establishments, and for education Model 2 includes worker effects and the same controls as Model 1 except for race and education. Model 3 includes job-match effects and the same controls as Model 2 except for industry effects and occupation effects. Model 4 includes worker effects, establishment effects, and occupation effects. The analysis sample includes dominant jobs of men between ages 23-65, with 30 or more contracted hours per week, excluding government jobs, temporary jobs, and jobs in 2-digit industry by 3-digit occupation cells that have fewer than 10,000 full-time full-year equivalent workers in the three-year moving average window used to calculate fatality rates. 'Fatality Rate' is measured in deaths per 1,000 full-time full-year equivalent workers. Log wages are Winsorized at the 1st and 99th percentiles. VSLs calculated at mean hourly wage. \* Indicates significance at the 0.01 level.

These results suggest that the bias from omitted worker and establishment effects are each very large and oppose each other in sign. The surprising implication is that cross-sectional or pooled models could potentially provide estimates with smaller net bias than worker effects models. However, we must first assess whether the OME model satisfies the key condition for identification: that job mobility is exogenous.

### 4.3 Evaluating the Importance of Endogenous Mobility

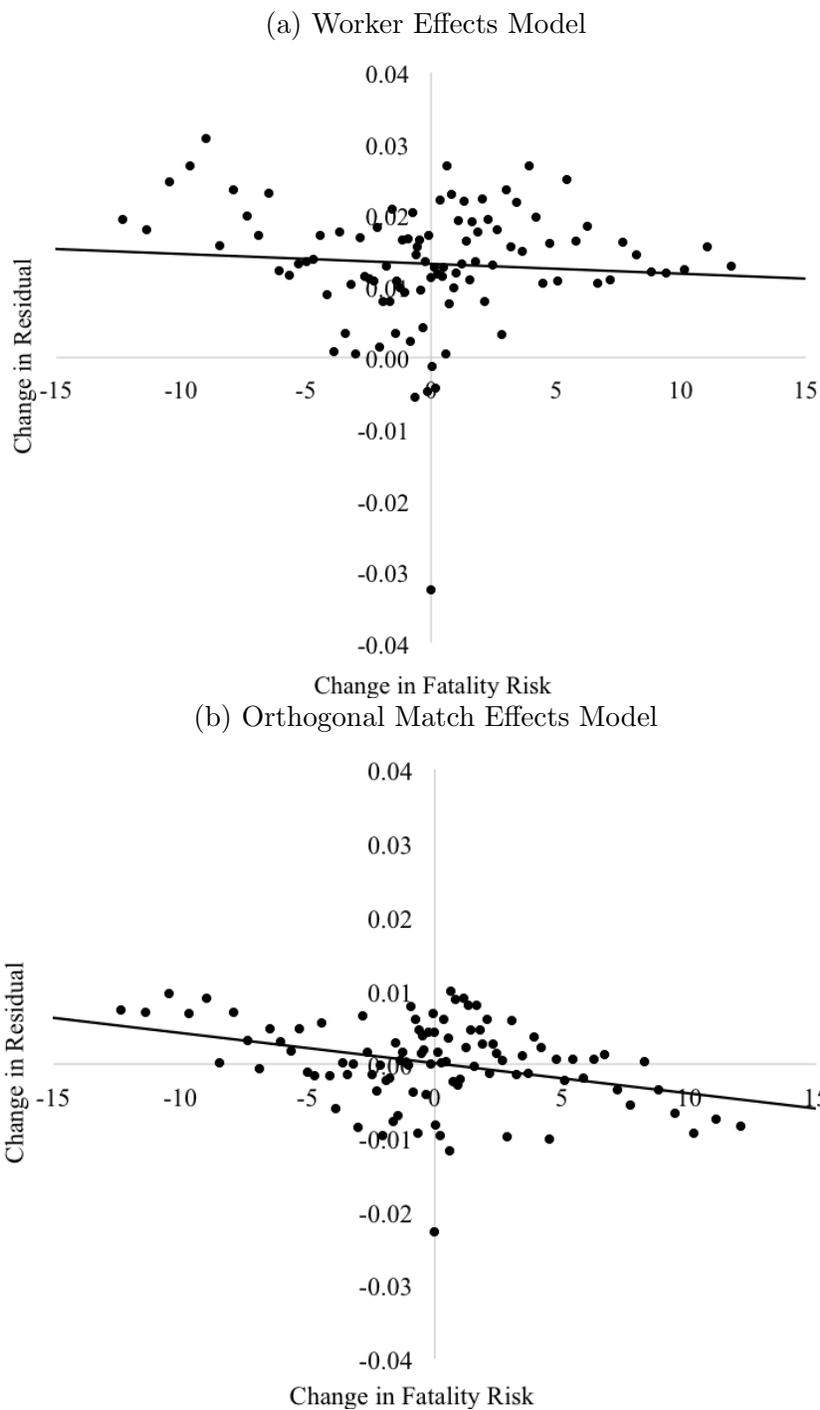
If the worker effects model in Equation (5) is properly specified, then it also follows that the expectation of the change in the residual should be zero conditional on the change in risk. Figure 2a shows this is clearly not the case. The figure displays a binned scatterplot of the average change in residuals against the change in fatality risk. The figure uses the main analysis sample restricted to observations corresponding to a job-to-job change.

First, the average change in the residual is positive. This is consistent with the ‘job ladder’ inherent in search behavior observed across many studies, including Schmutte (2015). Furthermore, Figure 2a also suggests that workers with large decreases in risk experience relatively large increases in the wage residual, which is consistent with the form of endogenous mobility suggested by the model in which job changes involve movements to jobs that are more attractive on both wage and safety dimensions. This could also indicate the presence of some self-selection associated with voluntary acceptance of an increases in risk in a direct job-to-job change, as hypothesized by Villanueva (2007).

The key question that we consider in this section is whether our preferred model that controls for plant effects and orthogonal match effects eliminates the endogenous mobility bias implied in Figure 2a. Figure 2b is the analogue of Figure 2a using residuals from the orthogonal match effects model. There are two important differences in this figure. First, the average change in residuals is substantially closer to zero. Second, over most of the domain there is not a strong systematic relationship between the change in risk and average change in residuals, although there is some evidence of a very small negative correlation. Given our large sample we technically reject the null hypothesis that mobility is exogenous. However, even for a very large change in the fatality rate of 1 deaths per 10,000 worker-years the predicted change in the residual is less than 0.01, which is about 2% of the estimated value of  $\gamma$ , suggesting limited scope for endogenous mobility bias.

Card et al. (2013) estimate a model with additively separable worker and establishment effects, and present diagnostic analyses to evaluate the separability and exogenous mobility assumptions of their specification. Following their approach, Figure A.1 reports

Figure 2: Binned Scatterplot of Average Change in Residual by Change in Fatality Risk for Job Changers



Notes: Plot of the average change in residuals for workers who change jobs year-over-year within each percentile of the distribution of change in the fatality rate. The residuals are from the worker effects and orthogonal match effects models, respectively. Fatality rates are measured in deaths per 100,000 full-time full-year equivalent workers.

Table 3: Mean Wage Change of Movers by Decile of Origin and Destination Establishment Effect, 2005–2010

		Destination Establishment Effect Decile									
		1	2	3	4	5	6	7	8	9	10
Origin Decile	1	0.001	0.129	0.241	0.332	0.417	0.504	0.596	0.716	0.880	1.199
	2	-0.126	0.001	0.079	0.156	0.230	0.308	0.390	0.490	0.624	0.903
	3	-0.241	-0.076	-0.001	0.064	0.138	0.211	0.294	0.390	0.525	0.785
	4	-0.331	-0.155	-0.064	0.000	0.064	0.133	0.209	0.307	0.432	0.696
	5	-0.415	-0.233	-0.138	-0.063	0.001	0.064	0.139	0.234	0.361	0.617
	6	-0.503	-0.306	-0.209	-0.132	-0.062	0.004	0.069	0.157	0.283	0.543
	7	-0.602	-0.390	-0.291	-0.212	-0.139	-0.067	0.001	0.080	0.198	0.451
	8	-0.717	-0.490	-0.388	-0.307	-0.236	-0.159	-0.079	0.000	0.107	0.350
	9	-0.880	-0.626	-0.518	-0.436	-0.361	-0.281	-0.197	-0.104	0.002	0.196
	10	-1.210	-0.907	-0.790	-0.700	-0.620	-0.543	-0.449	-0.353	-0.194	0.001

Notes: Table entries are mean differences between wages on the origin and destination job for workers who change jobs. Each job is classified into deciles based on the estimated establishment effect from the OME Model, Equation 7.

the mean residual within cells defined by deciles of the estimated worker and establishment effects from our benchmark model. The estimated means are all less than 0.01 in magnitude with no distinct pattern. This suggests that the separability assumption is a good approximation to the true data generating process.

Sorting into jobs based on pure match effects could cause a violation of the exogenous mobility assumption by creating correlation between  $\xi$  and  $a$ ,  $\theta$ , or  $\Psi$  in Equation 7. If match effects play an important role in job assignment, workers who move down the establishment wage effect ladder could still experience wage increases. As a result, one would expect that the wage gains associated with transitioning from one establishment to another should differ from the wage losses associated with a transition in the opposite direction.

Table 3 shows resounding evidence against this form of mobility. The table reports the average wage change associated with a move from each decile of the establishment wage effects distribution to each other decile. The first evidence against sorting on match effects is that the wage effects are remarkably symmetric—a move from the 5th decile to the 1st decile, for example, is associated with a 41.5% reduction in wages, while a move in the opposite direction increases wages by 41.7%. This close symmetry holds for every pair of deciles in the distribution. Second, job transitions within any decile of the

Table 4: Estimation Results for OME Model: RAIS 2003-2010

	Component
Std. Dev. of $(W - X\hat{\beta})$	1.425
Std. Dev. of (Worker Effect $\theta$ )	1.307
Std. Dev. of (Estab. Effect $\Psi$ )	0.388
Std. Dev. of $(\gamma a)$	0.043
Std. Dev. of (Residual)	0.267
Correlation between $(\theta, \Psi)$	0.082
Correlation between $(a, \theta)$	-0.202
Correlation between $(a, \Psi)$	-0.127
Std. Dev. of Match Effects	0.132

Notes: Variance components estimated from the orthogonal match effects model described in Equations 6 and 7. Standard deviation of match effects is estimated by the square root of the difference between the AKM mean squared error and the mean squared error from Equation 6.

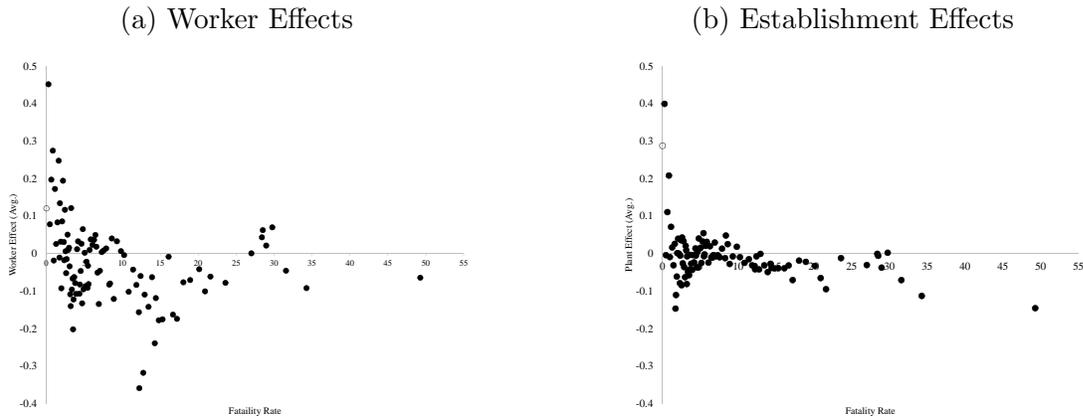
distribution (along the diagonal of the table) have roughly zero effect on wages, suggesting that there is no meaningful job mobility premium outside of the establishment wage effect. Moreover, there is very little improvement in fit between the OME model and the match effects model, suggesting a limited potential role for match effects. In the CHK analysis using West German data, the standard deviation of match effects was about 35% of the standard deviation of establishment effects; in Brazil this share is slightly smaller, 31%, as reported in Table 4.

Table 4 also shows the relative importance of each component of the OME decomposition. The standard deviations of estimated worker and establishment effects are about 92% and 27%, respectively, of the standard deviation of the dependent variable in Equation 7,  $P_{it} \equiv (w_{it} - x_{it}\hat{\beta})$ . The table also reports the correlations between each of these components and the fatality rate, -0.20 and -0.13, respectively. These estimates support the central intuition underlying both ability and endogenous mobility bias: higher paid workers tend to be employed in less risky jobs, and higher paying establishments also offer less risky jobs.

#### 4.4 Discontinuities in the Wage-Risk profile Near Zero

The correlation between fatality rates, worker effects, and establishment effects can also be seen clearly in Figure 3. Panel (a) presents a binned scatterplot, in which each point along the horizontal axis represents a percentile in the distribution of fatality rates, and

Figure 3: Binned Scatterplots of Worker and Establishment Effects versus Fatality Rates



Notes: The figures plot the average worker and establishment effects estimated from the model in Equation 6 at each percentile of the distribution of the fatality rate. Fatality rates are measured in deaths per 100,000 full-time full-year equivalent workers.

the vertical axis reports the average worker effect in a bin containing all jobs with a fatality rate between that percentile and the preceding one. Panel (b) is the equivalent figure for plant effects. Panel (a) illustrates very clearly that high-wage workers are employed on less risky jobs, consistent with the well-known idea that workers with higher lifetime earnings choose to consume lower risk.

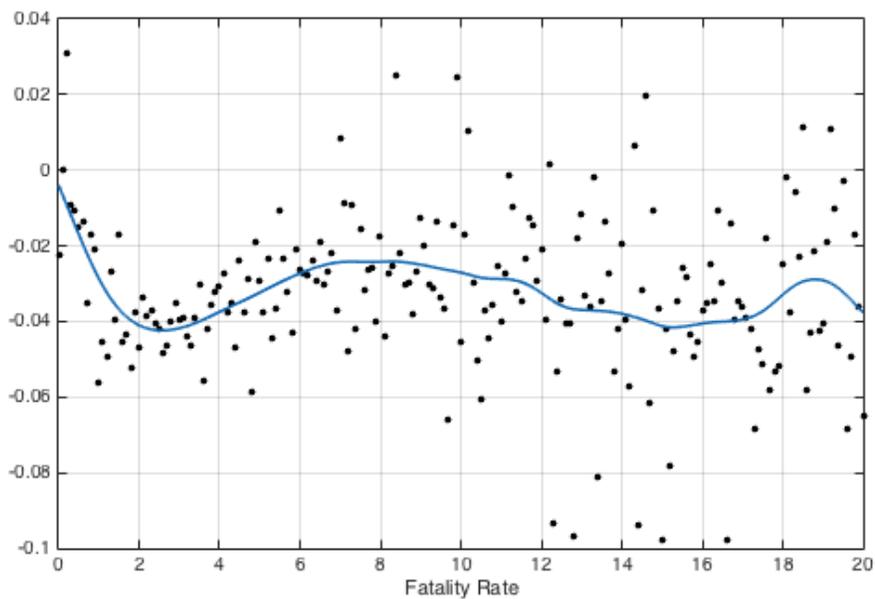
Moreover, both plots, especially Panel (b), show a very strong spike at fatality rates near zero. Intuitively, these establishments in Panel (b) have reached a corner solution in the choice of occupational safety. Although many of these establishments may prefer to reduce fatality rates further and offset the cost of this amenity with wage reductions, they are constrained from doing so. A similar pattern is present in Panel (a) for worker effects: jobs that carry the lowest level of risk have an average worker effect that is 50 percent higher than jobs with higher levels of risk.

These patterns motivate our use of a diagnostic analysis of the exogeneity of risk based on the novel test introduced by Caetano (2015). Her test exploits the intuition that the relationship between wages and fatal risk should not be discontinuous at zero. If, however, there are omitted variables correlated with both risk and wages, and these omitted variables have a mass point where risk is equal to zero, then the expectation of the wage conditional on risk will be discontinuous at zero.

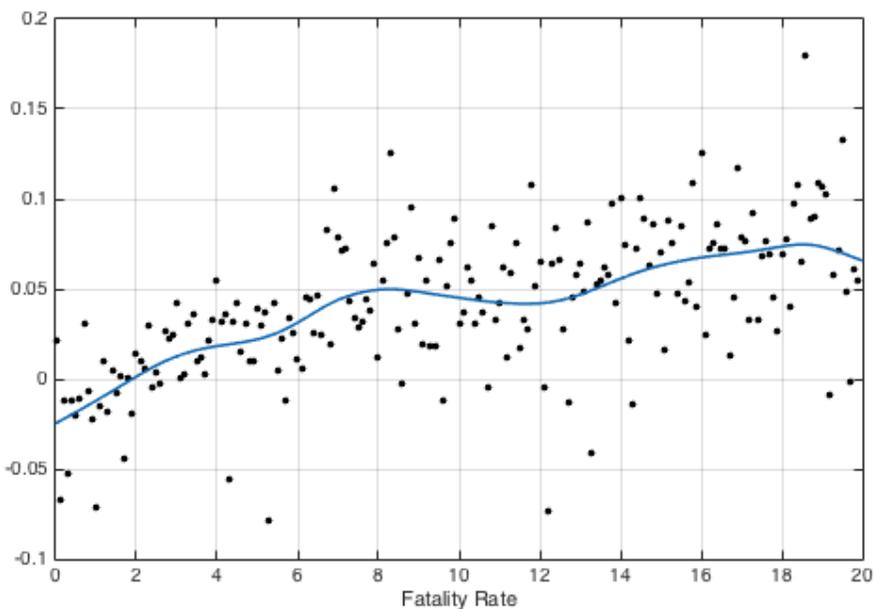
This diagnostic is particularly well-suited to our application as the theory suggests workers with especially high skill will choose jobs with minimal risk of fatal injury, and,

Figure 4: Non-parametric Estimates of the Wage-Fatality Rate Profile

(a) Worker Effects Model



(b) Orthogonal Match Effects Model



Notes: The vertical axis measures the estimated coefficients from a regression of log wages on 200 binary indicators for the fatality rate level, with each indicator representing a bin of width 0.1, and a continuous control for fatality rates above 20. Panel (a) includes the same covariates as the worker effects model, and Panel (b) is the corresponding OME model. The estimates are based on a random 5% sample of the analysis sample, for computational tractability. Fitted lines are smoothed spline functions. Fatality rates are measured in deaths per 100,000 full-time full-year equivalent workers.

when changing jobs, will move to higher paying establishments at zero risk. We observe these patterns in Figure 3. In our setting, however, the observed discontinuity in worker and establishment effects occurs fuzzily in the neighborhood of zero. This is because we construct the fatality rate by aggregating observed fatalities. In our data, some jobs may have a single fatality across a very large number of full-time equivalent jobs. These are likely jobs that individuals regard as having approximately “zero risk”, or at least the lowest possible risk, since of course there is no true zero-risk job. Conversely, even after trimming, there may be some jobs that have positive risk, but that we measure as having zero risk due to sampling. These features of our data cause bunching near a fuzzy threshold, and limit our ability to implement the Caetano test formally, although the patterns in Figure 3 are consistent with a violation of the exogeneity test.

To visualize the intuition behind this test, while allowing for sampling noise to create a fuzzy threshold near zero, we plot non-parametric estimates of the wage-risk relationship in Figure 4. The dots in the figure are estimated coefficients from versions of the worker effects model, Panel (a), and OME model, Panel (b), that include 200 binary indicators for the fatality rate, with indicators of width 0.1 partitioning the variation between 0 and 20, and a continuous control for fatality rates above 20. Panel (a) displays a very clear non-monotonicity at low fatality rates, with a pattern near zero that is distinct from the rest of the wage-risk profile. This diagnostic provides clear evidence that the worker effects model is misspecified, and is consistent with the influence of an omitted factor that is negatively correlated with risk and that is concentrated in jobs with very low risk levels.

Panel (b) shows the analogous plot for the OME specification. Relative to Panel (a), there is no visual evidence of strong non-monotonicity or discontinuity anywhere. The misspecification from the worker effects model appears to be largely remedied in the OME model according to this diagnostic, suggesting that there is limited concern about a potential violation of the exogeneity assumption in this model. The steeper wage-risk profile in the non-parametric estimates is also consistent with the corresponding linear estimates from Table 2. We return to a discussion of the apparent concavity of the non-parametric OME estimates in Section 6.

This non-parametric evidence of model misspecification is also related to the coefficients on the zero risk indicator in Table 2. The pooled specification reported in Column (1) of Table 2, for example, suggests that workers in jobs with zero risk earn a premium of 0.070 log points, relative to workers at all other levels of risk. The more direct im-

plementation of the Caetano misspecification test is equivalent to testing whether this coefficient equals zero. However, these parameter estimates do not consider the neighborhood around zero, which Figure 4 suggests are important given the sampling noise associated with very small probability events. Moreover, although one might expect that comparing the magnitudes of these coefficients across models is informative about the relative severity of any omitted variables problem, Caetano (2015) shows that this is not true. We instead rely on the visually suggestive non-parametric evidence.

#### 4.5 Interpretation of Estimates as Preferences

In Section 2, we demonstrated that it is possible to identify preferences under several assumptions. Two of these assumptions relate to the levels at which wages and fatality rates vary, and may not hold in all empirical settings. In this section we evaluate the plausibility of these conditions in the RAIS data. To be clear, these conditions affect the interpretation of the estimated parameters: if they hold our theoretical model shows that the OME parameters can be interpreted as estimates of the marginal willingness to accept fatal risk, a characterization of preferences. If not, the estimates should be interpreted as the slope of the equilibrium hedonic pricing function after removing the confounding effects of the latent wage components in Equation 7.

The first condition is that although firms may pay wage premia for potentially unobserved reasons, these wage premia vary primarily at the establishment level (conditional on worker and match effects), not at the establishment-by-occupation level. The existence of firm and establishment wage effects has been extensively documented in many labor markets, including in France by Abowd et al. (1999), in the US by Woodcock (2008) and Abowd et al. (2015), and in Germany by Card et al. (2013) and Goldschmidt and Schmieder (2015). Of course, the estimates from these papers do not necessarily imply that the identification condition holds. For example, if firms did pay establishment-by-occupation effects then firm wage effects could still be observed in the data, but they would represent employment-weighted averages of the establishment-occupation effects across the occupations and establishments within a firm.

In our analysis sample, over 97% of the total variance in log wages occurs across jobs. Of this variation, 95% can be explained by a two-way fixed effects model with worker and establishment effects alone. These facts suggest that any residual unexplained wage variation is extremely small. A decomposition of the estimated establishment effects reveals that 17% of their variation can be explained by variation within establishments

across 3-digit occupations. However, a two-way fixed effects model with worker effects and establishment-by-3-digit occupation effects explains less than 2% more of the variation in wages relative to a two-way model with only worker and establishment effects. These patterns suggests that, although there is variation in wages across occupations within establishments, the variation looks very different than a systematic wage premium, which is consistent with the additive separability assumption in our theoretical model.

The scatterplot in Figure 3 also contains evidence in support of this assumption. For fatality rates away from zero, the distribution of establishment effects is quite flat, suggesting that the choice of establishment wage effects is roughly independent of the level of risk. This pattern would be unlikely to arise if establishments paid occupation-specific wage premia, since most of the variation in fatality rates occurs across occupations.

Moreover, if any establishment-by-occupation wage effect did exist, it would be absorbed as one component of the pure match effect, and the evidence in Section 4.3 suggests that even this larger match term is still very small. We also directly estimate the standard deviation of the interaction between establishment and occupation wage effects, conditional on the additive terms, and confirm that it is smaller than the standard deviation of the match effect.

The second condition is that there is sufficient variation in fatality rates within establishments to identify the compensating wage differential in a model that includes worker and establishment effects. Of the total variation in fatality rates, only about 3% occurs within job matches. The primary source of this variation is a general downward trend in fatality rates throughout Brazil between 2003–2010. If job search is imperfect, we would not expect these decreases in fatality risk to be reflected in wage changes, particularly wage reductions, during the match. It is also possible that such small movements in fatality rates within jobs are not salient to workers. For these reasons, we do not rely upon this variation as a primary source of identification. Indeed, our estimate of the CWD using within-match variation is effectively zero, consistent with a job search model without renegotiation.

Instead our analysis leverages variation in fatality rates across jobs. Of this total across-job variation, 69% occurs across 3-digit occupations, 33% occurs across 2-digit industries, and 77% occurs across either 3-digit occupation or across 2-digit industry. Since the OME specification includes controls for one-digit occupation effects, the identifying variation in fatality rates is across 3-digit occupations conditional on establishment and 1-digit occupation effects, which is 33% of the across-match variation in fatality rates.

The combined evidence suggests that the key assumptions of the search model are not violated in the data, allowing us to interpret our OME results as estimates of worker preferences.

## 5 Corrections for Other Forms of Endogenous Mobility Bias

The results in Section 4 suggest that the primary source of endogenous mobility bias is variation in compensation practices across establishments. However, the diagnostic tests do not fully rule out other explanations. In this section, we conduct several auxiliary analyses that support our interpretation of the data generating process, and indicate that any remaining bias from endogenous mobility is at best minimal.

### 5.1 Heterogeneity in Bias from Labor Market Frictions

The process generating the observed data likely combines endogenous sources of variation in risk with other sources of variation, such as exogenous job destructions. In principle, estimates that focus on portions of data where endogenous sources of variation are less important should exhibit less bias. Our conceptual framework implies that endogenous mobility bias should be least severe in markets where search frictions are minimal. In the most competitive markets very little residual variation in wages should be explained by between-firm differences in compensation.

Following this intuition, we estimate models that allow for heterogeneity in estimates of the compensating wage differential with respect to variation in search frictions across the 136 mesoregions in Brazil. We use two different proxy measures of the extent of search frictions: (i) the within-mesoregion variance in establishment-specific wage premia, and (ii) the wage-elasticity of separation.<sup>10</sup>

Table 5 presents the results of this analysis. We expect, first, that regions with a higher variance in establishment effects should exhibit a stronger negative bias driven by the negative correlation between risk and establishment wage effects. Column (1) in the top panel of Table 5 shows that estimates from the pooled model decline from 0.505 in the regions in the lowest quintile of variance of establishment wage effects to 0.143 and

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<sup>10</sup>The latter proxy is motivated by Manning (2003), who observes that in a Burdett-Mortensen wage-posting model, the wage elasticity of separations is a sufficient statistic for the elasticity of labor supply, which is in turn a measure of market imperfection. For examples of this approach, see Hirsch, Schank and Schnabel (2010) or Webber (2015). Intuitively, if separations are driven by poaching from higher-paying competitors, a fixed unit increase in log wages will be strongly correlated with reduced probability of separation.

Table 5: Heterogeneity with Respect to Proxies for Search Frictions

	Dependent Variable: $\ln(Wage)$			
	(1) Pooled	(2) Worker Effects	(3) Match Effects	(4) Orth. Match Effects
1st Quintile Variance Estab. Effects*Fatality Rate	0.505* (0.001)	0.044* (0.002)	-0.019* (0.003)	0.449* (0.003)
2nd Quintile Variance Estab. Effects*Fatality Rate	0.452* (0.001)	0.033* (0.002)	-0.053* (0.003)	0.492* (0.003)
3rd Quintile Variance Estab. Effects*Fatality Rate	0.390* (0.001)	0.035* (0.002)	0.012* (0.003)	0.469* (0.003)
4th Quintile Variance Estab. Effects*Fatality Rate	0.144* (0.001)	0.054* (0.002)	0.049* (0.003)	0.557* (0.003)
5th Quintile Variance Estab. Effects*Fatality Rate	0.293* (0.002)	0.035* (0.003)	-0.023* (0.004)	0.485* (0.004)
N	83,411,371	83,418,032	83,418,032	83,418,032
R-Sq	0.500	0.914	0.978	0.965
1st Quintile Wage Elasticity of Job Sep.*Fatality Rate	0.029* (0.001)	-0.074* (0.002)	-0.031* (0.003)	0.348* (0.002)
2nd Quintile Wage Elasticity of Job Sep.*Fatality Rate	0.168* (0.001)	-0.032* (0.001)	0.025* (0.002)	0.346* (0.002)
3rd Quintile Wage Elasticity of Job Sep.*Fatality Rate	0.244* (0.001)	0.080* (0.001)	0.125* (0.002)	0.499* (0.002)
4th Quintile Wage Elasticity of Job Sep.*Fatality Rate	0.409* (0.001)	0.047* (0.001)	0.031* (0.003)	0.477* (0.002)
5th Quintile Wage Elasticity of Job Sep.*Fatality Rate	0.665* (0.001)	0.141* (0.002)	-0.186* (0.003)	0.370* (0.002)
N	83,411,371	83,418,032	83,418,032	83,418,032
R-Sq	0.500	0.914	0.982	0.963

Notes: Variances of establishment effects are calculated within each of 136 mesoregions. Quintiles of effects of log wages on separation rates are calculated by regressing a job separation indicator on a set of mesoregion indicators, interactions between mesoregion indicators and log wages, and all other covariates included in the respective benchmark models. We then group the estimated coefficients on the interaction terms between mesoregions and log wages into quintiles, interact the quintile indicators with the fatality rate, and re-estimate the benchmark models including the quintile effects, interactions between quintile indicators and fatality rates, and interactions between quintile indicators and the zero fatality rate indicator. The analysis sample is identical to Table 2. ‘Fatality Rate’ is measured in deaths per 1,000 full-time full-year equivalent workers. \* Indicates significance at the 0.01 level.

0.293 in the fourth and fifth quintiles, respectively. This evidence is consistent with the intuition that greater heterogeneity in establishment effects increases the expected rate of return to on-the-job search, and hence increases the bias from endogenous job mobility in the pooled model. Although column (2), the worker effects model, does not exhibit a similar pattern, the estimates are again much smaller than cross-sectional estimates in each quintile.

Our second proxy for search frictions is the wage elasticity of separation, which we estimate for each mesoregion and then group observations into quintiles.<sup>11</sup> We then interact these quintiles with the fatality rate and estimate each of the benchmark models allowing the effect of fatality rates on wages to differ across quintiles of regions with larger or smaller labor market frictions. The lowest quintile contains the mesoregions with the most negative wage elasticity of job separations, where we expect labor market frictions to be strongest.

Consistent with intuition, Column (1) in the bottom panel of Table 5 shows that the estimated compensating differential in the pooled model increases monotonically from 0.029 in the lowest quintile, where labor market frictions are expected to be strongest, to 0.665 in regions where our proxy suggests that labor market frictions are weakest. The within-worker models reported in Column (2) are also consistent with the hypothesized pattern of bias, increasing with the separation elasticity, from  $-0.074$  to  $0.141$ .

Most importantly, Column (4) from both panels of Table 5 shows that the estimated compensating differentials do not vary systematically with either proxy for market frictions when estimated with the OME model. This suggests that the OME model largely corrects the patterns of endogenous mobility bias present in the pooled and worker effects models. To be clear, while the observed pattern of heterogeneity in estimated compensating wage differentials is consistent with endogenous mobility bias arising from the job search model we advance in Section 2, we do not rule all alternative possible explanations.

## 5.2 Separation Due to Mass Displacement

If the primary form of endogenous mobility bias comes from voluntary movements up the job ladder to higher wage establishments, this bias component should differ strongly when

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<sup>11</sup>To implement this proxy, we first estimate models to predict job separation. The job separation models are interesting in their own right, and we discuss them in detail in Section 6.1. The dependent variable is a binary indicator for job separation, and the independent variables include all of the same control variables included in the respective benchmark models, a set of indicators for each mesoregion, and interaction terms between log wages and mesoregion indicators.

Table 6: Mass Displacement Estimates

	Dependent Variable: $\ln(Wage)$			
	(1) Pooled	(2) Worker Effects	(3) Match Effects	(4) Orth. Match Effects
Fatality Rate (3-Yr MA)	0.525* (0.001)	0.062* (0.001)	-0.006 (0.002)	0.653* (0.002)
Fatality Rate $\times$ Mass Displacement	0.150* (0.004)	0.048* (0.003)		0.059* (0.004)
Zero Fatality Rate	0.084* (0.000)	0.014* (0.000)	-0.004* (0.000)	0.039* (0.000)
Zero Fatality Rate $\times$ Mass Displacement	-0.008* (0.001)	0.001 (0.001)		0.014* (0.001)
Mass Displacement	-0.015* (0.000)	0.010* (0.000)		-0.052* (0.001)
N	48,795,576	48,800,263	48,800,263	48,800,263
R-Sq	0.479	0.912	0.976	0.966

Notes: The models correspond to the specifications reported in Table 2. The sample is restricted to observations within two years of a job-to-job transition. The variable “Mass Displacement” indicates that the observation is associated with a job-to-job move in which the worker separated from an establishment experiencing a mass displacement episode ‘Fatality Rate’ is measured in deaths per 1,000 full-time full-year equivalent workers. Log wages are Winsorized at the 1st and 99th percentiles. \* Indicates significance at the 0.01 level.

the model is estimated using a sample of job transitions associated with mass displacement events relative to voluntary job-to-job transitions. This strategy was initially proposed by Gibbons and Katz (1992) to eliminate bias in estimated industry wage premia that could arise if workers learn about ability or comparative advantage over time.

This approach can also help address the problem, discussed by Solon (1988), that panel estimates of compensating wage differentials may be biased due to the self-selection of job movers. A similar point is raised by Gruetter and Lalive (2009) in the context of estimating employer effects in matched employer-employee data. They argue self-selection on the basis of orthogonal match effects can lead to an attenuation of estimated employer effects. When choosing their next job, workers moving due to layoff should not be influenced by the pay or working conditions on their previous job. The mass displacement sample should have a disproportionate number of workers laid-off for reasons unrelated to their productivity. We therefore expect self-selection mechanisms that are not addressed in the benchmark model to be attenuated in the mass displacement sample.

Table 6 reports estimates of our hedonic wage model when we restrict the sample to job spells on either side of a job-to-job transition. Among these direct job-to-job transitions, we allow the compensating wage differential to differ if the job transition was initiated by a mass displacement event.<sup>12</sup> The structure of Table 6 is otherwise identical to the benchmark models in Table 2.

The key contrast is in Column (4) between the point estimate on the fatality rate ( $0.653 \pm 0.002$ ) and the point estimate on its interaction with the mass displacement indicator ( $0.059 \pm 0.004$ ). The main coefficient is slightly higher than our benchmark estimate because of the sample restriction to establishments with at least 50 FTE employees, which is used to define mass displacements. The mass displacement coefficient suggests that there is only a small difference (9%) in the compensating wage differential between these subsamples where we expect the endogenous mobility bias to be largest and smallest, respectively. The positive sign of this coefficient is consistent with the possible presence of a small remaining selection bias due to match effects, although Table 3 suggests that this is unlikely to be the case.<sup>13</sup>

Finally, note also that the estimated compensating wage differential in the worker effects model (Column 2) is nearly doubled in the mass displacement sample. This, too, is consistent with the mass displacement sample correcting for overall endogenous mobility bias from omitted establishment effects, as Gibbons and Katz (1992) originally surmised.

### 5.3 Instrumental Variables based on the Realized Mobility Network

The results of our analysis of mass displacements in Table 6 together with the residual diagnostics in Figure 2b suggest endogenous mobility bias may not be completely elimi-

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<sup>12</sup>We assemble the data for this analysis as follows. First, we define mass displacement events. Following the literature (Jacobson, LaLonde and Sullivan 1993; Abowd, McKinney and Vilhuber 2009; Couch and Placzek 2010), we restrict attention to establishments with at least fifty FTE employees, and say a mass displacement occurred if FTE employment decreased by at least thirty percent. Next, we merge the mass displacement indicator to the complete set of longitudinal work histories in the analysis data. For each worker, we take only observations that are within two years of a job-to-job transition. Out of a total sample of 48,800,263 observations associated with job-to-job transition, 2,773,298 are associated with mass displacement events. Our goal is to contrast the estimated compensating wage differential that uses variation from all job-to-job transitions with estimates restricted to mass displacement events.

<sup>13</sup>Abowd et al. (2015) find selection on match effects is stronger in high-paying firms. High-paying firms, as we have seen, offer less risky jobs on average. Hence, as Gruetter and Lalive (2009) and Abowd et al. (2015) demonstrate, wage variation from mobility across establishments will be attenuated by the offsetting match effects. Some of that wage variation will load onto risk. As a result, high-wage firms will appear to pay higher wages for their low-risk jobs, and low-wage firms will appear to pay low wages for their high-risk jobs. The net effect is to attenuate the estimated compensating wage differential toward zero in the OME model, which is consistent with the results in Table 6.

nated in our benchmark specification, though any remaining bias is likely small. Here we propose an IV estimator based on the employment histories of coworkers to address any remaining endogeneity from omitted match effects.

To develop the intuition behind our IV model, we begin with the second step of the orthogonal match effects model, Equation (7):  $P_{it} = \gamma a_{c(i,t),t} + \theta_i + \Psi_{J(i,t)} + \epsilon_{it}$ . Our concern is that the error includes a match effect plus a statistical residual  $\epsilon_{it} = \mu_{i,J(i,t)} + \varepsilon_{it}$ . In first differences, the second-stage model is:  $\Delta P_{it} = \gamma \Delta a_{c(i,t),t} + \Delta \Psi_{J(i,t)} + (\Delta \varepsilon_{it} + \Delta \mu_{i,J(i,t)})$  where  $\Delta \Psi_{J(i,t)}$  denotes the change in establishment wage effects between period  $t - 1$  and  $t$ . An unbiased estimate requires the exogenous mobility assumption  $E(\Delta a_{c(i,t),t} \Delta \mu_{i,J(i,t)} | \Delta \Psi_{J(i,t)}) = 0$ .

Our goal is to construct an instrument that is correlated with the change in accepted risk,  $\Delta a_{c(i,t),t}$ , but uncorrelated with the change in unobserved match effects,  $\Delta \mu_{i,J(i,t)}$ . We exploit the relational structure of the data to construct such an instrument as follows. First, we restrict attention to observations across pairs of years in which a worker changed dominant jobs. That is, to observations for which  $J(i, t) \neq J(i, t + 1)$ . For each such observation in the data, indexed by  $(i, t)$ , we define its ‘neighbors’, denoted  $N(i, t)$ , to be those observations,  $(i', \tau)$  for  $\tau \in \{t - 1, t - 2\}$ , satisfying (i)  $J(i', \tau) = J(i, t)$ , (ii)  $c(i', \tau) = c(i, t)$ , and (iii)  $J(i', \tau) \neq J(i', t)$ . In words, the neighbor set contains observations from workers employed at the same establishment as worker  $i$ , who had the same occupation at that establishment, and who separated from that job in the two years preceding,  $t - 1$  and  $t - 2$ .

Our proposed instrument is  $\Delta \tilde{a}_{it} = \frac{1}{|N(i,t)|} \sum_{\ell \in N(i,t)} \Delta a_{\ell}$ , the average change in risk on accepted jobs for observations in  $N(i, t)$ .<sup>14</sup> The intuition behind this instrument is that since workers in  $N(i, t)$  sorted into the same job as worker  $i$ , they are likely to have similar preferences, skills, and outside opportunities. Therefore, the characteristics of their destination jobs on separation are informative of the set of outside opportunities for  $i$ . The instrument is valid as long as the choice of risk on the destination job for workers in  $N(i, t)$  is uncorrelated with  $i$ ’s particular draw from the distribution of match effects. This assumption holds if the residual variation in  $\Delta \tilde{a}_{it}$  within plants is uncorrelated with  $\Delta \mu_{i,J(i,t)}$ , which requires that the expected change in match quality be zero within  $N(i, t)$ . The omitted match effect on accepted destination jobs reflects a predictable component, which is common across similar workers who exited the same establishment under similar circumstances, and an idiosyncratic component. The average change in risk

<sup>14</sup>Note that for observation  $\ell = (i', \tau) \in N(i, t)$ ,  $\Delta a_{\ell} = a_{c(i',\tau),\tau} - a_{c(i',\tau-1),\tau-1}$ .

within  $N(i, t)$  is correlated with the risk accepted by the focal worker,  $i$ , but independent of the idiosyncratic component of the realized match effect.

This instrument is similar in nature to instruments from other empirical matching studies, such as Akerberg and Botticini (2002), who study endogenous matching and the choice of employment contract structures. The commonality in IV strategies is to look for information about likely alternative matching choices that a worker could make, such that the difference in characteristics across jobs (in this case the difference in the fatality rate) is uncorrelated with the individual match-specific characteristics of the origin job.

### 5.3.1 Estimation Sample

We implement the IV strategy in a sample restricted to years in which workers move from one dominant job to another. For each worker, we measure the observed change in fatality rates between the origin and the destination job. We then construct instruments for each worker as the average change in fatality rates experienced by workers who departed the same origin job (establishment-occupation) in the preceding two years. The requirements for the instrument mean that the analysis is ultimately restricted to 2008–2010, with the observations that contribute to the instrument being drawn from job changes in 2006–2009. After these restrictions, the analysis sample for the IV model uses 4,599,345 workers who changed jobs between 2008–2010. We describe this sample in Table A.4. The sample is slightly younger, and slightly less-educated, but is otherwise similar to the formal workforce covered by RAIS.

### 5.3.2 IV Results

Table 7 compares the IV estimates with estimates in simple first-differences and first-differences controlling for both origin and destination establishment effects. For consistency with the earlier estimation, we fit the model in two stages. We fit the first stage of the orthogonal match effects model for the full sample, and then estimate the remaining models using the dependent variable for the second stage of the OME model. Column (1) reports a basic first-differenced estimate of the compensating wage differential of -0.025. The specification is comparable to the worker-effects model from Table 2. Column (2) adds origin and destination plant effects. The resulting estimate of 0.632 is larger, and consistent with our benchmark finding that controls for plant effects eliminate what appears to be a strong attenuation bias in models that only control for worker heterogeneity.

The instrumental variable estimates in Column (3) and (4) control for origin and

Table 7: Instrumental Variable Estimates

	(1)	(2)	(3)	(4)
	First- Differenced	Establishment Effects	IV First Stage	IV
$\Delta$ Fatality Rate (3-Yr MA)	-0.025 (0.016)	0.632* (0.010)		0.508* (0.034)
Avg. $\Delta$ Fat. Rate in $N(i.t)$			0.336* (0.001)	
N	4,599,345	4,599,345	4,599,345	4,599,345

Notes: The dependent variable is the change in log wages (net of observed time-varying characteristics) between the dominant job in the prior year and the new dominant job this year. All models control for race-specific cubics in experience and tenure through the first-stage match effects model. In addition, all models control for major occupation. Fatality rates are measured in deaths per 1,000 full-time full-year equivalent workers. Heteroskedasticity robust standard errors are shown in parentheses. \* indicates significance at the 0.01 level.

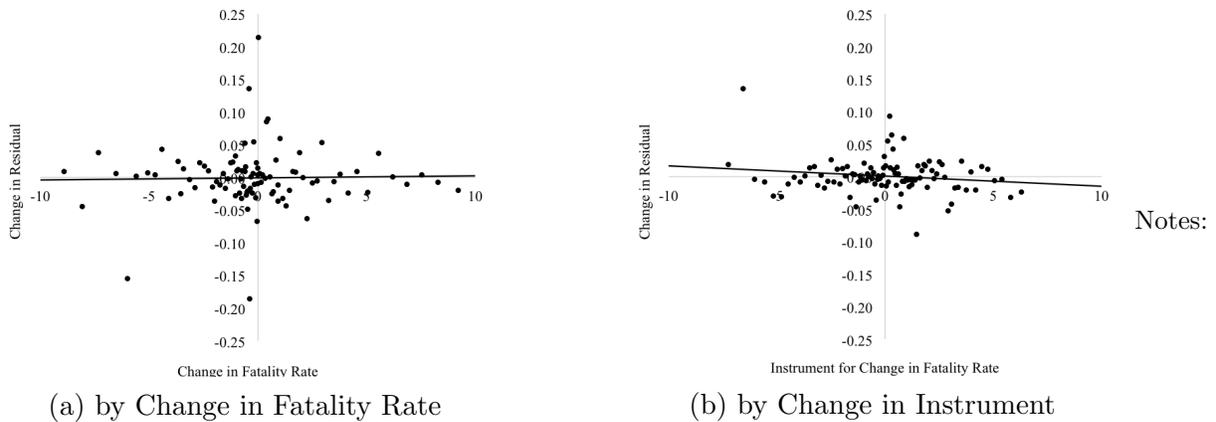
destination establishment effects, while also instrumenting for the change in fatality rates. In the first-stage model the point estimate on the instrument is 0.336 with an F-statistic of  $1.5 \times 10^5$ , indicating the instrument is strongly correlated with the change in risk.

The IV estimate of the compensating wage differential in Column (4),  $\hat{\gamma} = 0.508$  is slightly smaller than the effect estimated in the model controlling for establishment effects, and we reject the null hypothesis that the estimates are equal. The endogenous mobility bias that is corrected by the instrument relative to the establishment effects model appears to be modestly positive. However, when we use the IV sample to estimate the orthogonal match effects model, as shown in Appendix Table A.5, we estimate  $\hat{\gamma} = 0.508$ , exactly the same as the IV estimate to three decimal places. The instrumental variable results thus confirm that to the extent the exogenous mobility assumption does not hold in the orthogonal match effects model, the impact of any associated endogeneity bias is not quantitatively meaningful.

### 5.3.3 Residual Diagnostics

We conduct residual diagnostics similar to those in Section 4.3 to assess whether residuals from the IV model are uncorrelated with fatality rates. Figure 5 reports the binned scatterplot of the change in residual against the change in fatality rates and the change in the instrument. Our primary interest is on Panel (a) which shows the change in residual

Figure 5: Binned Scatterplot of Average Change in Residual from IV Model by Change in Fatality Rate



The figure displays binned scatterplots of residuals from the instrumental variables model reported in Table 7 against changes in (a) the fatality rate and (b) changes in the instrument.

against change in fatality rates and is therefore most directly comparable to Figure 2b. Whereas 2b showed a very slight negative relationship, we now can see no clear evidence of any relationship between change in risk and change in residuals. We interpret this as auxiliary evidence supporting our assertion that the stochastic assumptions of the IV model are satisfied.

## 6 Extensions and Sensitivity Analyses

The focus of our paper is on documenting and correcting for the effects of endogenous mobility bias in hedonic wage models. The literature points to other forms of model misspecification that could affect the validity, or at least the interpretation, of our results. We now consider the robustness of our results to some of the most common concerns.

### 6.1 Models of the Job Separation Probability

The frictional search model that motivates our analysis predicts workers are more likely to separate from jobs as the firm-specific component of wages decreases, and as fatality rates increase. This contrasts with the frictionless model, in which the disutility of risk is completely eliminated by wage adjustment. In this section, we verify the predictions of the job search model with respect to job separation. The probability of separation is unambiguously decreasing in both the overall wage, and also in the establishment-specific component of wages. Likewise, the probability of separation is increasing in fatality risk.

Table 8: Probability of Job Separation

	Dependent Variable: Separation			
	(1) Logistic	(2) Logistic	(3) Lin. Prob.	(4) Lin. Prob.
Fatality Rate (3-Yr MA)	0.224* (0.021)	0.291* (0.020)	0.040* (0.004)	-0.021* (0.002)
Zero Fatality Rate	-0.003 (0.006)	0.015 (0.006)	0.004* (0.001)	0.002* (0.000)
Log Wage	-0.070* (0.001)	–	–	–
Worker Effect	–	-0.455* (0.005)	-0.055* (0.001)	-0.060* (0.001)
Estab. Effect	–	-0.121* (0.009)	-0.032* (0.000)	–
Tenure	-1.093* (0.010)	-0.603* (0.013)	-0.015* (0.001)	-0.019* (0.001)
Plant Effects	N	N	N	Y
N	83,411,371	83,411,371	83,411,371	83,411,371
(Pseudo) R-Sq	0.071	0.076	0.065	0.1478

NOTE: Dependent variable is an indicator for whether the worker separates from their dominant job in the current year. In addition to those reported, the models include the same controls as the ‘Pooled’ specification in Table 2. The values in parentheses are robust standard errors clustered within plant. Fatality rates are measured in deaths per 1,000 full-time full-year equivalent workers.

\* Indicates significance at the 0.01 level.

Table 8 reports estimates from several models that use our main analysis sample to predict whether a worker separates from their job as a function of fatality risk, completed tenure, and wages. These models include the same controls as the pooled model in Column (1) of Table 2. Standard errors are clustered by establishment.

Columns (1) and (2) report results from logistic regressions. The two models are identical, except Column (1) controls directly for the log wage, and is therefore comparable to similar models estimated by Dale-Olsen (2006).<sup>15</sup> Column (2) replaces the log wage with the estimated worker and establishment effects from the OME model. In both cases, fatality risk is positively correlated with separation. The estimates in Column

<sup>15</sup>In unreported results, available by request, we estimate the exponential and gamma duration models introduced by Gronberg and Reed (1994). The results are qualitatively similar to those reported in Table 8.

(1), show workers are less likely to separate from jobs with higher log wages. Column (2) shows that workers with higher estimated worker effects are less likely to separate, consistent with mover-stayer heterogeneity. However, after conditioning on this mover-stayer heterogeneity, as well as fatality risk, workers in the same occupation with the same tenure are less likely to separate from jobs with a higher estimated establishment-specific wage premium. Note also that the strong relationship between the probability of separation, fatality risk, and wages supports our decision to control for tenure in our benchmark model.<sup>16</sup>

Column (3) reports a linear probability model with the same covariate controls as Column (2). The qualitative implications are identical, and marginal effects (unreported) are very similar when moving to the linear probability model. The key contrast is between the estimated coefficient for fatality risk ( $0.040 \pm 0.004$ ) in Column (3), which does not control for arbitrary plant-specific heterogeneity, and the estimate ( $-0.021 \pm 0.002$ ) in Column (4), which does. Clearly, any positive correlation between fatality risk and separation rates arises from between-plant variation. After controlling for plant effects, the estimated effect of fatality risk on separation rates is negative and economically negligible.

Furthermore, when we control for plant-specific heterogeneity in Column (4), the differences in fatality risk across jobs within the same plant have an economically negligible effect on the separation probability. The latter evidence supports the implication of our search model that jobs within a plant offer common utility.

## 6.2 Relaxing the Linearity Assumption

While most empirical studies of compensating wage differentials assume log wages are linear in risk, there is little evidence to support this assumption. Lavetti (2015) finds that the marginal compensating wage differential declines sharply as the level of fatality rates increases. Theoretically, indifference sets and isoprofit functions are both unlikely to be linear, so there is no reason to suspect that the set of tangency points between these functions is linear (Ekeland, Heckman and Nesheim 2004). Our non-parametric plots in Figure 4b suggest the relationship is somewhat concave. We show that our results are not sensitive to allowing a more flexible functional form.

Appendix Table A.7 presents estimates from each of the four main fixed effects speci-

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<sup>16</sup>Another approach would be to estimate the tenure and wage equation jointly, as in Bonhomme and Jolivet (2009). To do so would require strong assumptions on the nature of the joint distribution of individual and establishment heterogeneity and distract from our main objective of highlighting the bias introduced mobility across jobs with different compensation practices.

Figure 6: Marginal VSLs Implied by Cubic Models



Notes: Marginal VSLs are graphed along with 95% confidence intervals (which are very small and difficult to distinguish from the mean estimates in the graph).

fications, but allowing for a cubic in fatality rates. The same patterns of bias we detected in the linear specification of Table 2 are also evident in the linear terms. To more clearly illustrate the robustness of our main result, we plot the implied marginal VSLs in Figure 6.

The figure shows that, although the linearity hypothesis is not supported, the pooled estimates are roughly linear. However, both the worker effects and OME estimates imply sharply decreasing marginal compensating wage differentials as fatality rates increase. The implied MVSL from the OME model decreases from 9.1 million Reais at a fatality rate of 1 per 100,000 FTFY workers to 2.8 million Reais at a fatality rate of 17, which is approximately twice the mean fatality rate.

## 7 Conclusion

Our objectives have been to demonstrate the advantages of using matched employer-employee data to correct for the effects of endogenous mobility bias in estimating compensating wage differentials. Controlling for employer heterogeneity in a relatively straightforward way yields results that are strikingly consistent with the implications of basic hedonic search models. We can furthermore clearly articulate, and provide empirical

support for, the conditions under which the estimated compensating wage differential identifies workers' marginal willingness to accept fatal risk.

Our paper complements a growing body of work addressing the effects of search frictions and endogeneity bias when estimating the effects of non-wage amenities on labor market outcomes. Much of the recent work uses cross-sectional and panel data to estimate structural models of hedonic search (Bonhomme and Jolivet 2009; Dey and Flinn 2005; 2008; Villanueva 2007; Sullivan and To 2014) and Roy-style sorting (DeLeire et al. 2013). An emerging literature addresses models of compensating differentials using matched employer-employee data. In very innovative recent papers, Sorkin (2016) and Taber and Vejlin (2016) seek to explain how much variation in matching outcomes, job duration and wages can be rationalized by compensating differentials. In these analyses, unlike our paper, job amenities are not measured; the presence of amenities is inferred from variation in outcomes. Lalive (2003) and Tsai et al. (2011) estimate hedonic wage models using matched employer-employee data with observed firm-level amenities. However, the emphasis in both papers is limited to studying the effects of aggregation bias associated with measuring amenities using industry averages.

Our paper is the first to use matched employer-employee data to directly illustrate and correct for endogenous mobility bias arising from job search. In doing so, we provide a bridge between the structural, theoretical, and reduced-form literature. Specifically, this paper shows the statistical decomposition of wages originating with Abowd et al. (1999) does an extremely good job of matching the predictions of the basic hedonic search model, and in explaining the covariation between wages and job characteristics.

The analysis of hedonic wage models is fraught with challenges for applied work, and no study can resolve them all. Future work must address key measurement issues that were beyond the scope of this study. One trade-off associated with using administrative data, rather than survey data, is that we do not observe information on other job amenities. However, another advantage of our empirical model is that by controlling for establishment and occupation effects we actually reduce omitted variable bias associated with unobserved amenities, like health insurance, that are employer-specific. On the other hand, if, for example, fatal and non-fatal risk tend to be bundled together in the same way across jobs within establishments, then our model estimates the compensating differential for changes in this composite bundle. This interpretive issue is common to all studies that use observational data to study the determinants of compensation in the labor market.

There are reasons to suspect that the endogenous mobility problem we highlight is

not unique to Brazil. Our analysis is motivated in part by the contrast between cross-sectional estimates of the compensating differential for fatal injury and the much smaller estimates from U.S. panel data. This pattern is consistent with hedonic search. There is a good chance that employer and match-specific variation in wages could explain the U.S. data. Woodcock (2008) estimates that among workers in the US who experience job-to-job transitions, about 60% of their earnings growth is due to sorting into firms that pay higher average earnings to all workers for unobserved reasons.

Our results suggest that models of compensating differentials with costly search in the spirit of Hwang et al. (1998) can provide a useful guide to further empirical work. The approach we have developed is relevant for other non-wage amenities for which the literature suggests endogenous mobility bias may be present (Brown 1980; Garen 1988; Hersch 1998). There would also be considerable value in efforts to develop and estimate a structural model in the spirit of Bonhomme and Jolivet (2009) or Lavetti (2015) that can address the simultaneous determination of wages and job tenure given workers' forward-looking behavior.

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## A Additional Tables and Figures – For Web Publication Only

Table A.1: Causes of Separation Reported in RAIS

Value	Label Portuguese	Label English
0	nao desl ano	no separation this year
10	dem com jc	terminated with just cause
11	dem sem jc	terminated without just cause
12	term contr	end of contract
20	desl com jc	resigned with just cause
21	desl sem jc	resigned without just cause
30	trans c/onus	xfer with cost to firm
31	trans s/onus	xfer with cost to worker
40	mud. regime	Change of labor regime
50	reforma	military reform - paid reserves
60	falecimento	demise, death
62	falec ac trb	death - at work accident
63	falec ac tip	death - at work accident corp
64	falec d prof	death - work related illness
70	apos ts cres	retirement - length of service with contract termination
71	apos ts sres	retirement - length of service without contract termination
72	apos id cres	retirement - age with contract termination
73	apos in acid	retirement - disability from work accident
74	apos in doen	retirement - disability from work illness
75	apos compuls	retirement - mandatory
76	apos in outr	retirement - other disability
78	apos id sres	retirement - age without contract termination
79	apos esp cre	retirement - special with contract termination
80	apos esp sre	retirement - special without contract termination

Table A.2: Average Fatality Rates By Industry and Occupation

<b>Industry</b>	Average Fatality Rate	Number of Job-Years
Agriculture and Fishing	10.25	22,762,420
Mining	10.48	1,814,957
Manufacturing	5.24	76,712,576
Utilities	4.19	2,023,931
Construction	13.77	26,098,278
Trade and Repair	6.04	82,004,063
Food, Lodging, and Hospitality	4.99	15,589,304
Transportation, Storage, and Communication	14.53	20,941,098
Financial and Intermediary Services	1.01	6,947,728
Real Estate, Renting, and Services	4.59	57,447,503
Public Administration, Defense, and Public Security	0.84	72,055,976
Education	1.58	12,418,485
Health and Social Services	1.67	14,089,834
Other Social and Personal Services	3.98	15,469,519
Domestic Services	5.76	116,086
<b>Occupation</b>		
Public Administration and Management	2.63	18,035,409
Professionals, Artists, and Scientists	1.09	39,178,629
Mid-Level Technicians	2.50	40,972,375
Administrative Workers	1.87	78,792,943
Service Workers and Vendors	4.40	98,796,568
Agriculture Workers, Fishermen, Forestry Workers	9.26	25,417,204
Production and Manufacturing I	11.65	94,955,794
Production and Manufacturing II	5.28	15,947,072
Repair and Maintenance Workers	7.39	13,871,753

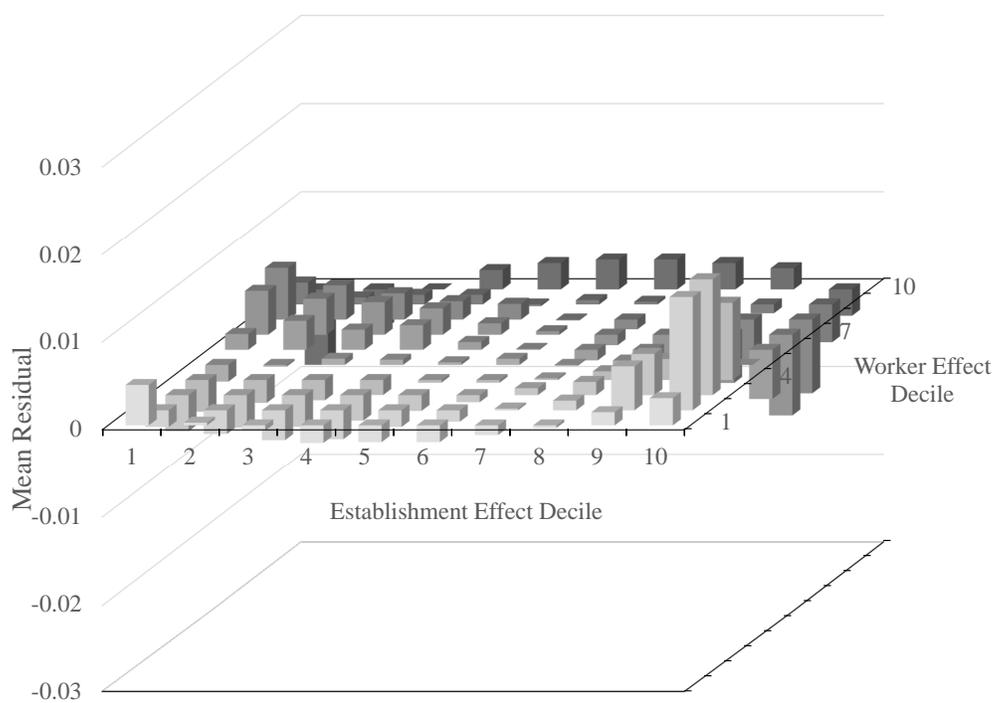
Notes: Average fatality rates are calculated as deaths per 100,000 full-time full-year-equivalent workers using the 100% Brazilian RAIS data from 2003-2010.

Table A.3: Estimated Compensating Wage Differentials for Full-Time Prime-Age Men, Excluding Industry and Occupation Effects

	Dependent Variable: $\ln(Wage)$			
	(1) Pooled	(2) Worker Effects	(3) Match Effects	(4) Orth. Match Effects
Fatality Rate (3-Yr MA)	0.574* (0.001)	0.055* (0.001)	-0.004 (0.001)	0.421* (0.001)
Zero Fatality Rate	0.184* (0.000)	0.022* (0.000)	-0.006* (0.000)	0.031* (0.000)
Experience	0.028* (0.000)	0.105* (0.000)	0.174* (0.001)	
Experience Sq.	-0.000* (0.000)	-0.003* (0.000)	-0.002* (0.000)	
Experience Cu.	0.000* (0.000)	0.000* (0.000)	0.000* (0.000)	
Job Tenure	0.003* (0.000)	0.001* (0.000)	-0.012* (0.000)	
N	83,411,371	83,418,032	83,418,032	83,418,032
R-Sq	0.437	0.912	0.978	0.965
VSL (millions of reais)	4.52	0.43	-0.03	3.31
95% CI	[4.50, 4.53]	[0.42, 0.45]	[-0.05, -0.02]	[3.30, 3.33]

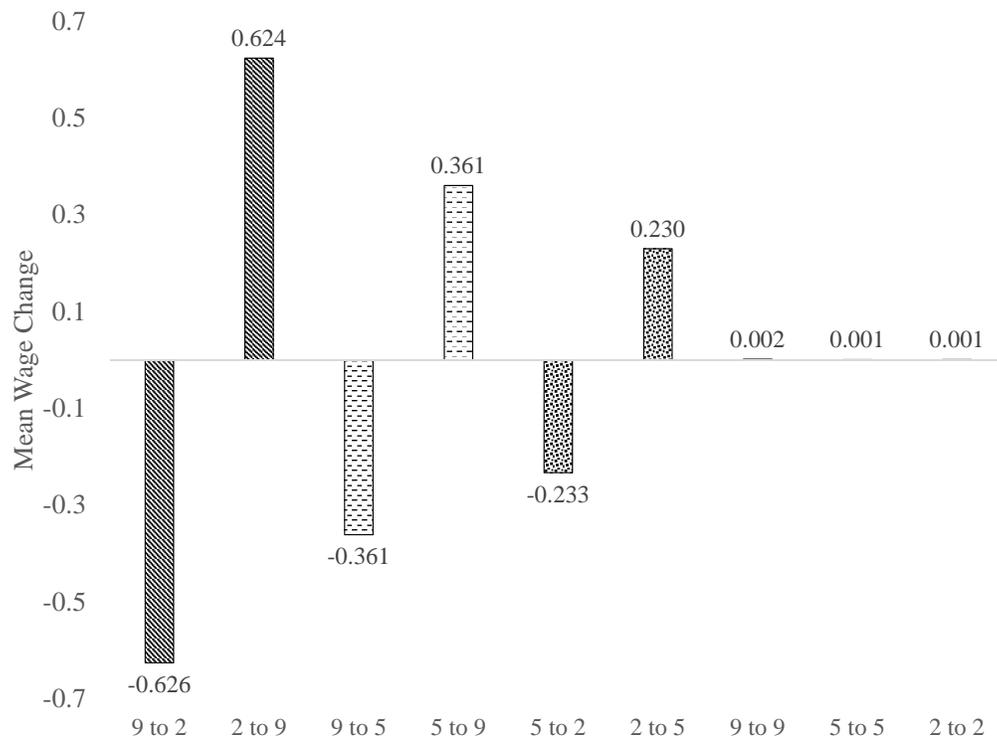
Notes: Model 1 also includes year effects, state effects, race effects, race effects interacted with each of the experience terms, indicators for small and medium-sized establishments, education indicators. Model 2 includes worker effects and the same controls as Model 1 except for race and education. Model 3 includes job-match effects and the same controls as Model 2. Model 4 includes worker effects and establishment effects. The analysis sample includes dominant jobs of men between ages 23-65, with 30 or more contracted hours per week, excluding government jobs, temporary jobs, and jobs in 2-digit industry by 3-digit occupation cells that have fewer than 10,000 full-time full-year equivalent workers in the three-year moving average window used to calculate fatality rates. ‘Fatality Rate’ is measured in deaths per 1,000 million full-time full-year equivalent workers. Log wages are winsorized at the 1st and 99th percentiles. Orthogonal Match Effects model excludes observations with singleton worker or establishment effects. VSLs calculated at mean hourly wage. \* Indicates significance at the 0.01 level.

Figure A.1: Mean Residuals by Decile of Establishment/Person Effect, 2005–2010



Notes: Figure displays the mean residual from the model estimated in Column (4) of Table 2 within cells defined by the estimated establishment effect interacted with the decile of estimated worker effect.

Figure A.2: Mean Wage Change of Movers Classified by Decile of Origin and Destination Establishment Effect, 2005–2010



Notes: Figure displays mean difference between wages on the origin and destination job for workers who change jobs. Each job is classified into deciles based on the estimated establishment effect from the model estimated in Column (4) of Table 2. The figure plots selected deciles. Table 3 reports results for all transition cells.

Table A.4: Descriptive Statistics: IV Sample

	IV Sample (1)
Race <i>branco</i> (white)	0.54
Elementary or less	0.43
Some High School	0.08
High School	0.39
Some College	0.03
College or More	0.07
Log Hourly Wage	1.46
Total Experience (Years)	19.72
Fatality Rate (per 100,000)	8.10
Zero Fatality Rate (Percent)	0.08
Number of Observations	4, 599, 345

NOTE—Means of key variables for the sample used to estimate IV models. See text for a complete description of the sample restrictions

Table A.5: Compensating Wage Differentials: IV Sample

	Dependent Variable: $\ln(Wage)$			
	(1) Pooled	(2) Worker Effects	(3) Match Effects	(4) Orth. Match Effects
Fatality Rate (3-Yr MA)	0.367* (0.004)	-0.025* (0.016)	-0.058 (0.022)	0.508* (0.026)
Zero Fatality Rate	0.069* (0.001)	0.011* (0.003)	-0.003* (0.000)	0.027* (0.006)
N	4,599,345	4,599,345	4,599,345	4,599,345
R-Sq	0.499	0.979	0.99	0.985

Notes: Estimates of benchmark specifications restricted to the IV sample. All models include a cubic in experience interacted with race, tenure, year effects, and controls for each one-digit occupation. Model 1 also includes 1-digit industry effects, state effects, race effects, indicators for small and medium-sized establishments, and education indicators. Model 2 includes worker effects and the same controls as Model 1 except for race and education. Model 3 includes job-match effects and the same controls as Model 2 except for industry effects and occupation effects. Model 4 includes worker effects, establishment effects, and occupation effects. ‘Fatality Rate’ is measured in deaths per 1,000 full-time full-year equivalent workers. Log wages are winsorized at the 1st and 99th percentiles. Orthogonal Match Effects model excludes observations with singleton worker or establishment effects. MVSLs calculated at mean hourly wage. \* Indicates significance at the 0.01 level.

Table A.6: Benchmark Models with Clustered Standard Errors

	Dependent Variable: $\ln(Wage)$			
	(1) Pooled	(2) Worker Effects	(3) Match Effects	(4) Orth. Match Effects
Fatality Rate (3-Yr MA)	0.363	0.041	-0.004	0.490
Unclustered SE	(0.001)	(0.001)	(0.001)	(0.001)
Clustered by Establishment	(0.015)	(0.004)	(0.009)	(0.007)
Clustered by Occupation*Industry	(0.151)	(0.033)	(0.029)	(0.062)
Zero Fatality Rate	0.070	0.009	-0.006	0.027
Unclustered SE	(0.000)	(0.000)	(0.000)	(0.000)
Clustered by Establishment	(0.004)	(0.001)	(0.001)	(0.001)
Clustered by Occupation*Industry	(0.021)	(0.005)	(0.010)	(0.006)
N	83,411,371	83,418,032	83,418,032	83,418,032
N Establishment Clusters	1,634,452	1,634,464	1,634,464	1,634,464
N Occupation*Industry Clusters	1,179	1,179	1,179	1,179
R-Sq	0.499	0.914	0.978	0.965

Notes: Model 1 also includes 1-digit industry effects, 1-digit occupation effects, year effects, state effects, race effects, race effects interacted with each of the experience terms, indicators for small and medium-sized establishments, education indicators. Model 2 includes worker effects and the same controls as Model 1 except for race and education. Model 3 includes job-match effects and the same controls as Model 2 except for industry effects and occupation effects. Model 4 includes worker effects, establishment effects, and occupation effects. The analysis sample includes dominant jobs of men between ages 23-65, with 30 or more contracted hours per week, excluding government jobs, temporary jobs, and jobs in 2-digit industry by 3-digit occupation cells that have fewer than 10,000 full-time full-year equivalent workers in the three-year moving average window used to calculate fatality rates. ‘Fatality Rate’ is measured in deaths per 1,000 full-time full-year equivalent workers. Log wages are winsorized at the 1st and 99th percentiles. Orthogonal Match Effects model excludes observations with singleton worker or establishment effects. Occupation\*Industry clusters use 3-digit occupation codes and 2-digit industry codes. \* Indicates significance at the 0.01 level.

Table A.7: Cubic Compensating Wage Differential Models, Full-Time Prime-Age Men

	Dependent Variable: $\ln(Wage)$			
	(1) Pooled	(2) Worker Effects	(3) Match Effects	(4) Orth. Match Effects
Fatality Rate (3-Yr MA)	0.333* (0.003)	0.212* (0.003)	-0.043* (0.004)	1.052* (0.004)
Fatality Rate Squared	1.135* (0.013)	-0.528* (0.012)	0.140* (0.015)	-2.229* (0.017)
Fatality Rate Cubed	-2.395* (0.014)	0.259* (0.012)	-0.087* (0.015)	1.832* (0.017)
Zero Fatality Rate	0.072* (0.000)	0.015* (0.000)	-0.007* (0.000)	0.047* (0.000)
Experience	0.029* (0.000)	0.100* (0.000)	0.174* (0.001)	
Job Tenure	0.003* (0.000)	0.001* (0.000)	-0.012* (0.000)	
N	83,411,371	83,418,032	83,418,032	83,418,032
R-Sq	0.499	0.914	0.978	0.965

Notes: All models and sample selection criteria are identical to those in Table 2, except for the quadratic and cubic fatality rate terms. ‘Fatality Rate’ is measured in deaths per 1,000 full-time full-year equivalent workers. \* Indicates significance at the .01 level.

## B Appendix – For Web Publication Only

### B.1 Identification in Steady-State

Prior studies of hedonic wages in the presence of dynamic search have empirical settings in which longitudinally-linked employer-employee data were not available. These studies emphasize that the empirical literature on compensating wage differentials, by ignoring search, may have produced biased estimates of the marginal willingness to pay for job amenities. Dey and Flinn (2008) make the identification condition explicitly: “...the difference in cross-sectional mean wages is a consistent estimator of the willingness to pay if and only if”

$$\int v f(v|R=0)dv = \int v f(v|R=1)dv$$

where  $v$  is log utility as above, and  $R$  is a discrete job amenity (although the logic holds for continuous amenities as well.)<sup>17</sup> The key departure in our model is that we condition on firm identity. In this case, when firms have common compensation policies but the technology for producing safety is job-specific, Equation 3 shows that the job offer function, if it were observed, would identify preferences.

It remains to show that the identification result holds in the steady-state distribution of realized wage-risk pairs, rather than just in the unobserved offer distribution. To extend the model to the steady-state, suppose that jobs end exogenously at rate  $\delta$  and workers sample new offers at rate  $\lambda$  from a joint distribution over job-level wage increments (net of worker heterogeneity), risk, and firm identity,  $F(w_f, R, j)$ . We relate this to a univariate offer distribution over utility increments,  $F_v(\psi_j)$ . We again abstract from worker heterogeneity, which is nevertheless controlled for empirically in a manner that allows for arbitrary correlation with employer heterogeneity and risk.

In steady-state, flows into unemployment balance flows out, so that steady-state unemployment is  $U = \frac{\delta}{\delta+\lambda} = \frac{1}{1+\kappa}$  where  $\kappa = \frac{\lambda}{\delta}$  measures the extent of market frictions. As is standard, we define the steady-state distribution of accepted offers,  $G_v(v)$  implicitly by:

$$[\delta + \lambda \bar{F}_v(v)](1 - U)G_v(v) = \lambda U F(v) f(j). \quad (8)$$

We use the notation  $\bar{F}_v(v) = 1 - F_v(v)$  and derive

$$G(v) = \frac{F(v) f(j)}{1 + \kappa \bar{F}_v(v)}, \quad (9)$$

with corresponding density

$$g(v) = \frac{f(j)[f(v)(1 + \kappa \bar{F}_v(v)) + \kappa F(v) f(v)]}{[1 + \kappa \bar{F}_v(v)]^2}. \quad (10)$$

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<sup>17</sup>Hwang et al. (1998) similarly argue that utility differentials affecting wages are correlated with risk, and further, that using fixed effects to control for firm heterogeneity will not solve this identification problem if amenities and utility vary at the same level.

By means of convolution, our definition of  $g_v(v)$  entails a definition of  $g_v(v|j)$ , the (degenerate) steady-state distribution of realized offers conditional on firm identity.

Our objective is to define the steady state distribution of utility conditional on risk and firm identity, which, intuitively, is also degenerate. The derivation follows from an application of Bayes' rule. The probability of being on a job with risk level  $r$  given utility  $v$  and working for employer  $j$  is

$$p(R = r|v, j) = \frac{f(v + h(r), r|j)}{\int_x f(v + h(r), x|j)dx} \quad (11)$$

where  $f$  is the joint distribution of firm-level wage and risk offers, conditional on employer type. Similarly, the marginal distribution  $p(R = r|j) = \int p(R = r|v, j)g(v|j)dv$ , which measures the distribution of employment within firm  $j$  across jobs offering different levels of risk, must also be degenerate. Specifically,  $p(R = r|j) = g(v|j)$  if  $p(R = r|v, j) = 1$  and equals zero otherwise. Finally, the conditional distribution of utility offers within firm  $j$  given the risk level is:

$$g(v|r, j) = \frac{p(R = r|v, j)g(v|j)}{p(R = r|j)}. \quad (12)$$

Clearly, the density in (12) equals 1 whenever  $p(R = r|v, j) = 1$  and equals zero otherwise. As a result, the identifying condition from Dey and Flinn (2008) is satisfied in this model, and the difference in mean wages between jobs with different levels of risk is an unbiased estimator of workers' preferences for safety after conditioning on the firm identity.

To close the argument, consider the conditional steady-state distributions of wages directly. The difference in mean firm-specific wage offers in steady state across jobs with different risk levels  $R_1$  and  $R_2$ , conditional on firm effects is:

$$\begin{aligned} E_{SS}(w_1|R = R_1, j) &- E_{SS}(w_2|R = R_2, j) \\ &= \int (v_j - h(R_1))g(v|R = R_1, i, j)dv - \int (v_j - h(R_2))g(v|R = R_1, i, j)dv \\ &= \int v_j g(v|R = R_1, i, j)dv - \int v_j g(v|R = R_2, i, j)dv - h(R_1) + h(R_2) \end{aligned}$$

Plugging in the utility differential gives:

$$\begin{aligned} &= \int [y(R_1) + \psi_j - h(R_1)] g(v|R = R_1, j)dv \\ &\quad - \int [y(R_2) + \psi_j - h(R_2)] g(v|R = R_2, i, j)dv - h(R_1) + h(R_2) \end{aligned}$$

By (3),

$$= \int g(v|R = R_1, j)dv - \int g(v|R = R_2, j)dv - h(R_1) + h(R_2)$$

Therefore, since the conditional density in (12) is degenerate,

$$E_{SS}(w_1|R = R_1, j) - E_{SS}(w_2|R = R_2, j) = h(R_2) - h(R_1) \quad (13)$$

This result shows that the steady-state change in wages across jobs with different fatality rates is equal to the difference in the willingness to accept fatal risk, demonstrating the possibility of identifying preferences in the presence of certain forms of job search frictions.

## B.2 Details of Fatality Rate Calculations

Within a cell,  $c$ , we construct the fatality rate  $a_c$  as

$$a_c = \frac{F_c}{(H_c/2,000)} \times (100,000). \quad (14)$$

The numerator,  $F_c$  is the number of fatal injuries in cell  $c$ . The denominator is the number of full-time full-year-equivalent jobs, assuming a baseline 40 hour work week and a 50 week work year.  $H_c$  is the total number of contracted hours worked over the year.<sup>18</sup> For each job,  $j$ , in the cell  $c$ , we count the number of hours worked as  $H_i = (MonthsWorked/12) * 50 * (Hours/Week)$ .  $H_c$  is the sum of  $H_i$  over all  $i$  in cell  $c$ . Finally, we inflate the count by 100,000 for consistency with the BLS measure. In some models we re-scale the fatality rate to deaths per 1,000 workers for ease of presentation of results.

## B.3 Brazil's Labor Market Institutions

### B.3.1 Formal Employment

In Brazil a worker is formally employed if he or she has a registered identification number with one of two social security programs: the *Programa de Integração Social* (PIS), or Social Integration Program, or the *Programa de Formação do Patrimônio do Servidor Público* (PASEP), or Civil Servants Equity Formation Program, depending on whether the worker is employed in the private sector or the public sector. PIS/PASEP numbers are consistent across workers and follow a worker for life. For firms, formal employment means that the employer contributes the *Abono Salarial* along with other social security payments to a bank account administered by either *Caixa Econômica Federal* if registered with PIS, or *Banco do Brasil* for PASEP workers. Formal employers must also have employment contracts for all employees. The most common contract type is the *Consolidação das Leis de Trabalho* (CLT), or Labor Law Consolidation. Other contract types include internships, independent contractors, directorships and government contractors. The Brazilian government defines formal employment with these criteria, and this definition is consistent with definitions used by researchers when studying other Latin American

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<sup>18</sup>Changes in the definition of full-year work will only affect the scale of our fatality rates. We chose a definition close to the BLS definition, although in Brazil full-year work may be closer to 48 weeks.

economies (Gasparini and Tornarolli 2009). Formal employment grew steadily in Brazil during our sample period, from nearly 42 million jobs in 2003 to over 65 million jobs in 2010. Unemployment decreased from eleven percent to five percent, and real wages grew over the period as well. Our sample therefore covers a period of growth and tightening labor-market conditions.

### B.3.2 Wage Regulations

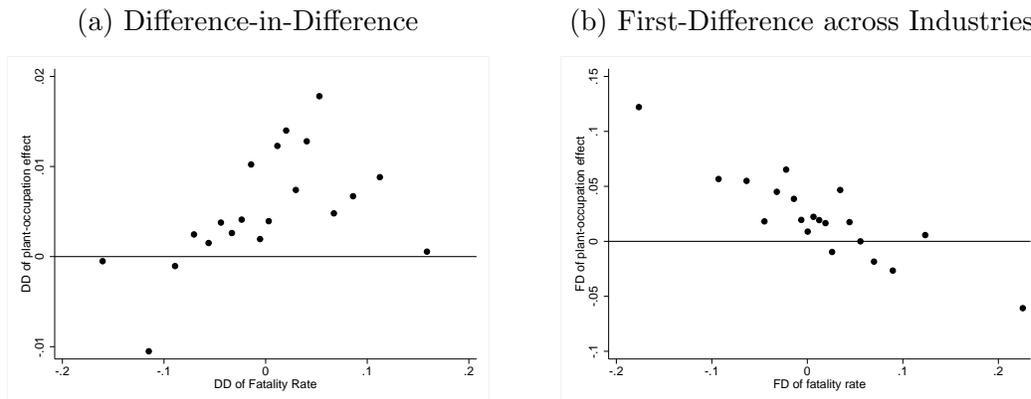
The formal sector of Brazil's labor market is governed by several overlapping institutions, some understanding of which is relevant to the interpretation of our results. Our data record the total monetary compensation that the employer is contracted to pay the worker. The data do not report non-monetary compensation, including employer-provided health and life insurance. As in the U.S., in Brazil, life and health insurance are frequently provided by one's employer. The value of such insurance is another amenity whose provision may be associated with that of occupational safety and earnings. We note that this shortcoming of the data is common to almost the entire literature. Nevertheless, any structural interpretation of our results depends on standard assumptions that unobserved workplace amenities are conditionally uncorrelated with observed amenities.

Additionally, in Brazil, wages are tied to safety formally through health and safety regulations known as *Norma Regulamentadora de Seguranca e Saude no Trabalho* (NR). The NRs stipulate a schedule of wage premia to be paid in association with work activities deemed to be unpleasant or dangerous. If these wage setting institutions were strong, we would still expect to find evidence of compensating wage differentials, but their presence would complicate our interpretation of the estimates as measuring individual preferences. A complete accounting of this complex institutional environment would require richer data on the NRs and enforcement activity. However, a couple of factors suggest these institutions have a small effect on our data. First, the statutory premia are generally 10-20 percent of the Federal minimum wage, which is quite low in absolute terms, so likely to be non-binding. Second, and relatedly, compliance with NRs are not a focus of the enforcement activities of the labor ministry, as they have very little influence on health and safety outcomes. We therefore proceed under the assumption that these institutions do not substantially alter the behavior of workers and firms.

In Brazil the NRs are norms elaborated and enforced by the MTE. They seek to promote health and safety in the workplace in compliance with constitutional (art. 7, XXII) and statutory (CLT arts. 60, 189, 200) obligations, as well as with international agreements and standards. The NRs affect all employers of labor in the formal sector, both public and private. The NRs stipulate a schedule of wage premia to be paid in association with work activities deemed to be unpleasant or dangerous.

In practice, each establishment is required to produce, in consultation with health and safety specialists, a document classifying the degree of exposure to harm for all jobs (occupations) within the establishment (known in most sectors as a PPRA). According to the regulations set forth in the NRs and CLT, the resulting premium for the specific plant-occupation pair is set as a percentage between zero and forty percent of the Federal

Figure B.3: Identifying variation: The relationship between wages and fatality risk



NOTE: Figure B.3a shows a binned scatterplot of the average difference-in-difference of establishment-specific occupation effects described in (17) against the difference-in-difference of fatality risk in (16). Figure B.3b shows a binned scatterplot of the average within-industry difference across occupations of establishment-specific occupation effects against the within-industry difference across occupations in fatality risk in (16).

minimum wage. The employer can reduce the wage premium in two ways: first, by investing in collective risk mitigation mechanisms, which reduce risk exposure for the all workers, and second by investing in individual protection mechanisms, which reduce risk exposure for a specific worker.

#### B.4 Identifying Variation in Risk and Wages Across Jobs

Figure B.3a presents a more transparent illustration of the variation identifying the relationship between fatality risk and wages. We fit the following model, following Abowd et al. (1999), to our preferred analysis sample:

$$w_{it} = x_{it}\beta + \theta_i + \psi_{J(i,t),k(i,t)} + \epsilon_{it}. \quad (15)$$

The dependent variable is the natural logarithm of the monthly wage. The regressor of interest is the fatality risk in industry-occupation cell  $c(i, t)$  in period  $t$ , denoted  $ac(i, t), t$ . This model allows for a worker-specific effect,  $\theta_i$  and for an effect that is specific to the combined establishment ( $J(i, t)$ ) and occupation ( $k(i, t)$ ).

In the preceding model, any variation in compensation associated with variation in fatality risk across different jobs loads onto the establishment-occupation effect,  $\psi_{J(i,t),k(i,t)}$ . In our data, fatality risk is measured in industry-occupation cells, and we identify the compensating wage differential from movements of workers across industry-occupation cells with differing levels of risk.

Using the estimated establishment-occupation effects,  $\hat{\psi}_{J(i,t),k(i,t)}$ , we construct an industry-occupation level dataset whose entries,  $(\bar{a}_{k,n}, \bar{\psi}_{k,n})$ , are the average risk and aver-

age establishment-occupation effect for a given occupationindustry pair where  $k$  indexes occupations and  $n$  indexes industries.

Figure B.3a presents a binned scatterplot of all pairwise differences of average fatality risk:

$$(\bar{a}_{k,n} - \bar{a}_{k',n}) - (\bar{a}_{k,n'} - \bar{a}_{k',n'}) \quad (16)$$

against the corresponding difference-in-differences for industry average plant-occupation wage effects:

$$\left(\bar{\psi}_{k,n} - \bar{\psi}_{k',n}\right) - \left(\bar{\psi}_{k,n'} - \bar{\psi}_{k',n'}\right). \quad (17)$$

The quantity in (16) is the excess change in fatality risk associated with moving to a job in occupation  $k$  from a job in occupation  $k'$  when that job is offered in industry  $n$  rather than industry  $n'$ . The quantity in (17) measures the change in job-specific compensation associated with moving from a job in occupation  $k$  from a job in occupation  $k'$  when that job is offered in industry  $n$  rather than industry  $n'$ . The latter measure is free of individual characteristics by construction, and is also purged of establishment-specific components of compensation by comparing jobs offered in the same establishment, but different occupations.

The measure in (17) captures expected differences in pay across jobs, net of individual characteristics, occupation and establishment heterogeneity. The figure shows that the residual variation in compensation is strongly associated with the residual variation in risk across these jobs.

It is instructive to compare Figure B.3a with its first-differenced counterpart. Figure B.3b is a binned scatterplot of difference in average plant-occupation effect across occupations  $\left(\bar{\psi}_{k,n} - \bar{\psi}_{k',n}\right)$  for each industry against the difference in risk across occupations. In contrast to the difference-in-difference plot, Figure B.3b shows that, within industries, jobs in occupations with higher average wages are also less risky. This highlights the central identification problem in our study – jobs that offer more desirable working conditions also offer better wages, as predicted by Hwang et al. (1998). Failure to correct for job-specific variation in total compensation will lead to an upward bias, and possibly sign reversal, in the estimated compensating wage differential.