The Estimation of Compensating Wage Differentials: Lessons from the *Deadliest Catch*[*]

Kurt Lavetti[†]
Ohio State University

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Abstract

I use longitudinal survey data from commercial fishing deckhands in the Alaskan Bering Sea to provide new insights on empirical methods commonly used to estimate compensating wage differentials and the value of statistical life (VSL). The unique setting exploits intertemporal variation in fatality rates and wages within worker-vessel pairs caused by a combination of weather patterns and policy changes, allowing identification of parameters and biases that it has only been possible to speculate about in more general settings. I show that estimation strategies common in the literature produce biased estimates in this setting, and decompose the bias components due to latent worker, establishment, and job-match heterogeneity. The estimates also remove the confounding effects of endogenous job mobility and dynamic labor market search, narrowing a conceptual gap between search-based hedonic wage theory and its empirical applications. I find that workers' marginal aversion to fatal risk falls as risk levels rise, which suggests complementarities in the benefits of public safety policies.

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[†]Ohio State University, Department of Economics. Correspondence: lavetti.1@osu.edu

1 Introduction

A substantial body of theoretical and empirical literature suggests that labor markets reward workers for accepting fatal risk. However, the collective empirical evidence on the size of compensating differentials for fatal risk is extremely imprecise. The wide variation in estimates across studies is likely a consequence of two types of endogeneity biases: (1) omitted variable biases caused by unobserved differences across jobs that are correlated with observed characteristics of workers and/or firms in a hedonic wage model.[1] And (2), bias caused by the endogeneity of variation in wage-risk pairs, which can be exacerbated in panel studies that identify compensating differentials from within-worker job switches in an effort to reduce bias of the first type.[2] For example, if workers searching for jobs in frictional labor markets tend to move when they receive offers that provide higher utility, then job changes may be associated with both higher wages and better non-wage amenities.[3]

In this paper I use a unique new panel dataset from the commercial fishing industry to estimate a model of compensating wage differentials for occupational safety that addresses both of these key estimation problems. The model allows for arbitrary unobserved worker heterogeneity, establishment heterogeneity, match-specific heterogeneity, and non-random assignment of workers to establishments that may be correlated with each of these latent components, relaxing a broad array of modeling assumptions. Avoiding the problems caused by labor market frictions and the assortative matching of workers and firms narrows what Hwang, Mortensen, and Reed (1998) describe as a "substantial gap between conventional hedonic wage theory and the real world data to which it is applied." The estimates provide new information about how workers make decisions when facing the risk of injury or death, and how individuals' marginal decisions are affected as the magnitudes of risks become very large. Estimates of the marginal willingness to accept fatal risk are critical to cost-benefit analyses for a wide range of public safety and health policies, and influence tens of billions of dollars of federal spending in the US each year.

I chose this empirical setting to exploit several unique institutional features of the labor market that aid identification. Conceptually, a nearly ideal experiment for estimating the compensating wage differential for occupational safety would have the following setup. Take a sample of worker-firm pairs, and in each time period exogenously change the technology used by the firm to provide safety, shifting the isoprofit function of the firm in wage-safety space. After the technology shock, the same worker-firm pair renegotiates a spot employment contract for that period. The Bering Sea commercial fisheries that I study have features very similar to such an experiment. These fisheries experience large, but predictable, changes in occupational hazards over time. This variation occurs both within years, for example the fatality rate in the fisheries studied is about five times higher in winter months than in summer months, and

[1] See, for example, Card, Heining, and Kline (2013), Krueger and Summers (1988), Murphy and Topel (1987, 1990), Gibbons and Katz (1992), Abowd, Kramarz, and Margolis (1999), and Taber and Vejlin (2016).

[2] See Gibbons and Katz (1992) for empirical evidence suggesting that endogenous job mobility biases estimates of compensating differentials.

[3] See Bonhomme and Jolivet (2009) and Dey and Flinn (2008) for discussions of identification of compensating wage differentials under frictional search.

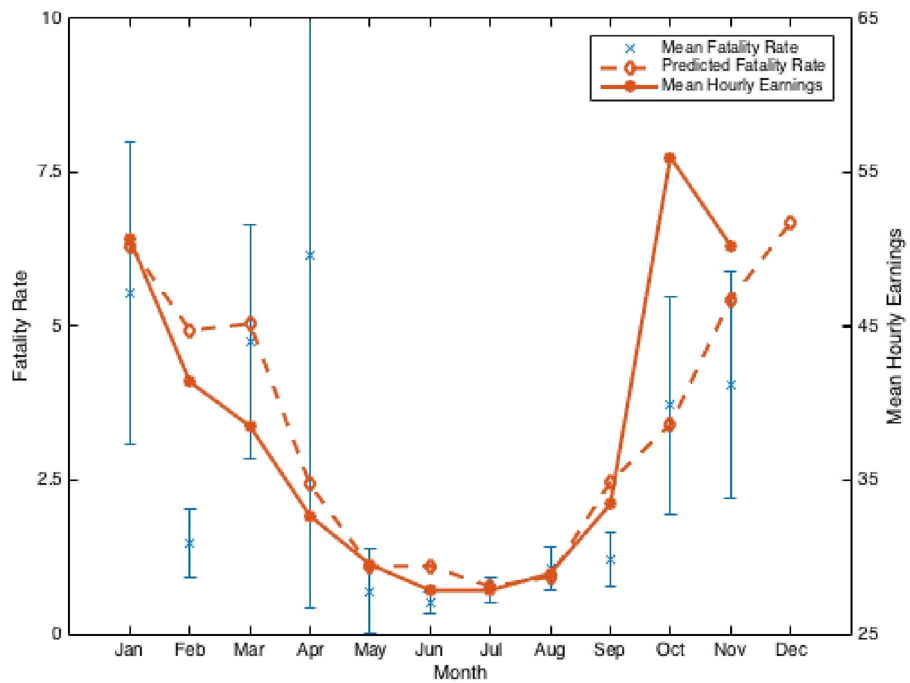
across years, as intermediate-term La Niña weather patterns tend to increase the fatality rate, and several policy changes caused improvements to safety. This variation improves upon what Ashenfelter and Greenstone (2004) describe as the major limitation in estimating the VSL using compensating wage differentials, that "most methods for assessing the VSL from labor market data are not based on choices made in the face of exogenous safety risks." A second important feature is that labor contracts are very short-term spot contracts, typically lasting a few weeks to a few months. The combination of these features causes fatality rates and hourly earnings to vary substantially and with high frequency, even for workers who remain employed by the same vessel. The seasonal variation in average fatality rates and average wages is shown in Figure 1.

The goal of this paper is to use a specific labor market with very appealing features to learn 1) how the marginal value of statistical life changes when the same set of workers is exposed to high risks, and 2) to evaluate empirical methodologies commonly used to estimate compensating wage differentials and the value of statistical life, a classic topic in labor economics that has long been considered notoriously difficult to solve (Bonhomme and Jolivet, 2009), by taking advantage of the unique institutional features of this setting. The results of this study are not intended to inform safety policies generally, or to contribute a numerical estimate of the value of statistical life (VSL) to the literature. I present a case study of a single industry that directly resolves some of the major identification challenges in the literature, with the hope that this study can guide the methodological decisions of future researchers, and help understand and precisely target the key remaining estimation challenges. This in turn may help influence a wide range of public health and safety policies that are based on the VSL, and other important parameters derived from compensating wage differentials.

Since administrative data are not available at the individual job level, I conducted a survey of commercial fishing deckhands who worked in the Alaskan Bering Sea and Aleutian Islands (BSAI) fisheries between 2003 and 2009. The survey was designed to capture information about variation in wages and fatality rates within worker-vessel pairs over time. In the sample I observe on average 9 job-spells per worker and 6.5 job-spells per worker-vessel pair, a long panel by labor-market standards. Estimating a fixed effects specification with worker-vessel match effects allows for the possibility that workers have unobserved differences in productivity for which they are compensated, that firms differ in productivity and pay quasi-rents to workers, that labor markets may not be perfectly competitive, and that each unique worker-firm pair may have unobserved productive complementarities, some of which are paid to the worker.

Although similar models are technically identifiable in general labor market settings where matched employer-employee data can be linked to data on job amenities, the features of the labor market for fishing deckhands are unique in that they cause an unusually large amount of variation in fatality rates and safety within worker-vessel pairs. In contrast, Lavetti and Schmutte (2016) estimate a comparable model using the census of jobs in Brazil, but find that less than 3% of the total variance in fatality rates occurs within jobs. As a consequence of the small amount of variation, workers do not appear to renegotiate wages in response to within-job variation, perhaps due to a lack of salience or to wage stickiness. In the current setting the

Figure 1: Monthly Average Fatality Rate and Hourly Earnings



Notes: Average fatality rates are measured in deaths per 1,000 full-time equivalent worker-years based on data from 1995-2009. Vertical bars are two standard deviations. 'Predicted Fatality Rate' is the weather-based expectation of the fatality rate from Model 2 in Appendix Table A5. Mean hourly earnings are weighted by the length of job spells. The mean and standard deviation of fatality rates in April are skewed by one month in which 15 fatalities occurred. Fatality rates and hourly earnings for December are not shown because monthly average FTE employment is about 87-99% lower in December, so there are very few data points.

majority of the variance in fatality rates occurs within jobs over time, and the magnitude of the within-job variation is as large as 100 times the average manufacturing fatality rate level in the US, offering a unique opportunity to learn about decision-making in high-risk situations. At the same time, these risk levels are not unusual relative to many other health-related decisions. For example, the mortality rate attributable to smoking is 3.84 per 1,000 person-years,[4] and the rate among military infantry is over 6,[5] both of which are substantially higher than the average risk in Bering Sea fisheries.

Despite the unique empirical setting, there are several methodological lessons that are broadly relevant to estimating compensating wage differentials. First, by replicating each of the identification strategies that can be implemented with standard data sources, along with more robust models, the estimates provide a decomposition of the directions and magnitudes of the bias components associated with each source of unobserved wage heterogeneity, which

[4]Source: CDC. State-Specific Smoking-Attributable Mortality and Years of Potential Life Lost — United States, 2000–2004. MMWR 2009;58:29-33.

[5]See Greenstone et al. 2014.

have not previously been estimated. Consistent with results based on data from the PSID,[6] I find substantial positive bias in cross-sectional specifications relative to within-worker estimates. Although these results may have led to the assumption that the remaining bias components due to latent firm and job-match heterogeneity are also positive, I find the opposite. The estimated covariance between unobserved firm heterogeneity and risk is negative, while the covariance between unobserved worker heterogeneity and risk is positive, so the bias components partially oppose each other in cross-sectional models. This correlation between latent firm wage heterogeneity and job amenities has been a central focus in many recent advances in improving the estimation of compensating wage differentials, including Sorkin (2016), Taber and Vejlin (2016), and Lavetti and Schmutte (2016). Still, the worker effects model offers a substantial improvement relative to cross-sectional specifications, as it removes about 84% of the bias in the cross-sectional model relative to the match effects model at the mean fatality rate.

Using pooled cross-sectional variation across jobs, I estimate the marginal value of statistical life (MVSL) to be $12.6 million at a fatality rate of 1 death per 1,000 full-time equivalent worker-years (FTEs). After controlling for unobserved worker, firm and match heterogeneity, and removing the effects of endogenous job assignment, the estimated MVSL decreases by 48% to $6.6 million. This suggests that endogeneity biases from these latent heterogeneity components are a substantial estimation concern.

Second, because of the large amount of variation in risk and wages, it is possible to more precisely estimate how the MVSL changes as the same workers face very different levels of risk. The vast majority of studies assume a linear relationship between risk and earnings. This linearity assumption is described by Ekeland, Heckman, and Nesheim (2002) as 'arbitrary and misleading',[7] and Ekeland, Heckman and Nesheim (2004) show that the assumption is a cause of major identification problems in hedonic models. Estimates from this setting suggest that the MVSL depends strongly on the level of risk, falling by 78%, from $6.0 million to $1.3 million, when the risk of death rises from 1.2 to 4.1 (25th percentile to 75th percentile) deaths per 1,000 FTE worker-years. Since the estimates are identified by within-job variation, this finding is not a result of sorting on heterogeneity in aversion to fatal risk, but instead implies that workers' aversion to marginal increases in risk falls as the level of risk rises, consistent with patterns of risk-taking behaviors in other high-risk settings (Gertler, Shah, and Bertozzi, 2005).

Although this finding comes from a unique sample of individuals, if it were to hold more generally, a question for future research, this would have substantial implications for the efficiency of spending on a broad range of safety and public health policies. In general, efficient allocation of public safety spending equates the marginal cost of reducing fatalities to the marginal willingness to pay. These findings suggest that willingness to pay decreases as baseline mortality risk levels increase, which implies complementarities in the benefits of improving safety. That is, implementing a policy that reduces mortality rates *increases*, rather than decreases, the benefits

[6] Such as Kniesner, Viscusi, and Ziliak (2010)

[7] This assumption is implicit in nearly all research on the value of statistical life (VSL) that describe 'The VSL' as a fixed value.

of subsequently reducing other competing mortality risks. This may seem to be a surprising finding, but it is conceptually consistent with what Pratt and Zeckhauser (1996) call the 'high-payment' effect, a theoretical effect suggesting that higher concentrations of mortality risk could increase the marginal utility of income and reduce willingness to pay for risk reductions. If this pattern holds generally in the population, it would suggest that it may be possible to improve the efficiency of safety policies by evaluating portfolios of policies collectively and considering the marginal benefits of safety relative to baseline risk levels.[8]

The paper proceeds as follows: Section 2 provides an overview of the identification strategy. Section 3 provides details on the empirical setting and institutional features of the industry that are relevant to the model and estimation, and describes the survey and other data sources. Section 4 presents the empirical models and results. Section 5 concludes.

2 Identification in Hedonic Wage Models

This section provides a brief theoretical and illustrative overview of the two key identification problems that I address in the paper, omitted variable bias due to unobserved differences across jobs, and endogenous job-mobility. The key challenge is that these two sources of bias can be interdependent—attempts to reduce the first form of bias can strongly exacerbate the second form, leading to a net increase in bias. I describe the empirical strategy used in this paper, and explain how it addresses each of these two problems.

2.1 Theoretical Framework

Rosen (1974) describes the first fundamental identification problem for hedonic wage models, which Ekeland, Heckman, and Nesheim (2004) also expound upon. The basic problem is that workers have heterogeneous preferences $U(c, z, x, \epsilon, A)$, where c is consumption, z is a non-wage job amenity (or disamenity), x and ϵ are observable and unobservable characteristics of the worker, respectively, and A are preference parameters that are common across workers. Similarly, firms have profit functions determined by heterogeneous technology $\Gamma(z, y, \eta, B)$, where y and η are observable and unobservable characteristics of the firm, respectively, and B are technology parameters that are common across firms.

The equilibrium hedonic price function is determined by the tangencies of the indifference curves of workers and isoprofit functions of firms, along with the assortative matching processes through which workers are assigned to firms. In a perfectly competitive model with frictionless

[8] Of course, policies that consider only efficiency could have potentially inequitable implications. For example, if willingness to pay for safety is increasing in income, a pure efficiency argument would imply spending more public resources per life saved in high income areas. Although the White House's Office of Management and Budget requires federal agencies to use estimates of the VSL in calculating the benefits of significant safety policies (See Federal Register Vol. 67, No. 60, March 28, 2002, p. 15044 and Executive Order 12866,) federal policies tend to use uniform VSLs rather than adjusting for demographics, where efficiency conditional on this equitable restriction occurs when the marginal cost of safety equals the average marginal willingness to pay. However, the choice of VSLs differs substantially across policies, providing scope for policies to consider the average characteristics of the affected populations, including the baseline mortality rate of the population.

matching the slope of this equilibrium pricing function is determined by a weighted average of the curvature of firms' isoprofit functions and the curvature of workers' indifference curves, where the weights depend on the relative variances of the distributions of unobserved heterogeneity parameters ϵ and η.[9] Even in this perfectly competitive model it is not trivial, though it is possible, to identify the preference and technology functions separately. The main source of difficulty is that the distributions of heterogeneity parameters are unknown.

However, Rosen (1974) and Ekeland, Heckman, and Nesheim (2004) describe several special cases in which preferences and/or technology are directly identified by exogenous supply or demand shift variables. The first case is one in which firms have identical technology, causing firm-specific heterogeneity η to drop out of the equilibrium hedonic price function. Heterogeneity in worker preferences over z identifies the firm offer function. If there are changes over time that affect firms' profit functions, then the cross-sectional variation in U_z can be used to estimate the offer function, while the intertemporal variation in Γ_z sweeps out information about workers' preferences, identifying demand functions. The second special case is where workers are identical, so ϵ drops out of U, but firms differ. In this case simple cross-sectional observations identify workers' preferences U_z as long as there is variation in Γ_z across firms.

Although these two special cases have been the cornerstones of the empirical hedonic wage literature for decades, the assumptions of perfect competition and frictionless search that they require have caused concern about the unbiasedness of estimates of workers' preferences among researchers and policymakers alike. As Hwang, Mortensen and Reed (1998) show, when job search matters then firm and match effects are likely to differ across jobs, and even the special cases discussed by Rosen (1974) have additional identification challenges. However, the empirical labor economics literature has not fully addressed this problem of endogenous job mobility in the estimation of compensating wage differentials.

Consider the labor market depicted in Figure 2 in which a worker is initially employed at a job in which her fixed skills are not used as productively as possible. I_1 depicts the isoprofit function of the firm at which she is employed, U_1 is her indifference curve over wage and fatality rate pairs (where higher fatality rates are bad), and their tangency point is (W_1, R_1). Over time the worker learns about her abilities and comparative advantages, samples posted wage-safety offers from other firms, and discovers that she is better suited to work in job two. Job two lies on a higher offer curve, I_2; that is, for any given level of the fatality rate, firm two is willing to pay her a higher wage than firm one. The difference between offer curve one and offer curve two, for example, is consistent with the existence of high wage firms (firm 2) and low wage firms (firm 1), a well-established empirical fact of labor markets (Abowd, Kramarz, and Margolis, 1999).

Switching from job one to job two not only increases wages, but if safety is a normal good then as wages rise due to changes in firm or match effects, workers optimally sort into safer jobs. This leads the tangency point (W_2, R_2) to move to the left of (W_1, R_1). If one were to assume perfect competition and apply Rosen's second case, the standard within-worker estimate of her preferences would be described by the least-squares equation that contains both tangency

[9] See Ekeland, Heckman, and Nesheim (2004).

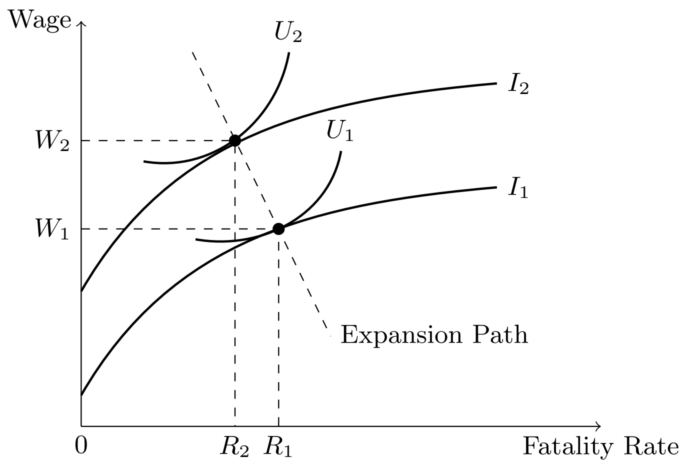


Figure 2: Wage-Risk Variation with Search Frictions

points, labeled the 'expansion path'. Of course, this estimate wrongly suggests that the worker likes fatal risk, when in fact her indifference curves show that the opposite is true. The reason for this bias is that instead of a single classical offer curve function, the presence of search frictions in this example causes realized job matches to be observed in a field of heterogeneous isoprofit functions over which the worker is gradually searching for the job that yields the highest utility. A switch across jobs could be associated with a change in wages that is caused by something other than a change in fatality rates, such as unobserved firm wage premia.

This is one specific motivating example, but any heterogeneity in firm-specific and job match-specific wage premia can cause the same type of identification problem. Differences in firm-level productivity that cause firms to pay quasi-rents to workers can cause separation between offer curves across firms. Similarly, any complementarities between the skills of a particular worker and technology of a particular firm that give rise to match-specific wage premia can also cause the same type of identification problem.

Does this kind of problematic wage variation exist empirically? The empirical literature has extensively documented firm and match-specific wage effects with the exact properties depicted in Figure 2. Sorensen and Vejlin (2013) estimate that unobserved worker heterogeneity explains about 37.7% of the total variation in wages in Denmark, while firm effects explain 14.3% and match effects explain 10.9%. Woodcock (2011) uses US data from the LEHD and estimates that the rate of earnings growth among workers who experience job-to-job transitions is about three times larger than that of job stayers, and among switchers about 60% of the differential earnings growth is due to sorting into higher paying firms, while an additional 29% is due to sorting into jobs with larger pure match effects. As a consequence, when identifying variation in a hedonic wage model is limited to within-worker job switches, the relative importance of sorting on the determination of wages increases substantially. Abowd, Kramarz and Margolis (1999) show that both unobserved worker and firm characteristics explain substantial shares of the residual variation in wages in France, and Card, Heining and Kline (2013) show similar patterns in Germany. These studies all suggest the existence of ”good” jobs and ”bad” jobs, empirically demonstrating the importance of job search and unobserved heterogeneity in the determination of wages.

Figure 2 is also useful for considering which objects are actually of interest to researchers and policymakers. The common association with the term ‘compensating wage differential’ is with the equilibrium market (implicit) price of a job amenity like safety. In Figure 2 this is the slope of the equilibrium wage function (labeled ‘Expansion Path’), which is negative in the depicted sample with two observations. More generally, the equilibrium wage function in a setting with many workers and firms is defined primarily by the differences across workers' indifferences curves and across firms' isoprofit functions, and the potentially frictional matching process that links workers to firms. It contains almost no discernible information about either the preferences of workers or the isoprofit functions of firms. For example, previous empirical studies using cross-sectional data have shown that this equilibrium envelope function appears concave, with a negative and significant coefficient on the quadratic fatality rate term, which could be driven entirely by sorting across workers into jobs.[10]

However, in many policy settings compensating wage differentials are treated as though they represent preferences of workers. For example, in the evaluation of federal safety policies, policymakers very frequently use the VSL to infer the benefits of safety policies. In many cases these VSL estimates are derived from normalizations of compensating wage differentials, and so the object of policy interest is actually the preferences of workers for safety, and not the equilibrium price function. For this reason, it is conceptually important to eliminate wage heterogeneity caused by firm and match effects, and to account for the impacts of search frictions in the estimation of compensating wage differentials.

2.2 Empirical Framework and Omitted Variable Bias

The approach that I take to solving these two identification problems is to make use of the unusually large amount of within-match variation in wage-risk pairs in this labor market. The basic empirical specifications that I estimate are variations of the following fixed effects model:

$$\ln(w_{ijt}) = x_{it}\beta + f(a_{jt};\gamma) + \theta_i + \Psi_{J(i,t)} + \Phi_{i,J(i,t)} + \epsilon_{ijt} \tag{2}$$

where $\ln(w_{ijt})$ is the log earnings of worker $i = 1, \ldots, N$ who is employed at establishment j at time t, x_{it} is a vector containing time-varying characteristics of worker i, a_{jt} are time-varying non-wage amenities of the job at which worker i is employed at time t, and f is a function of job amenities with a vector of parameters γ.

Following the wage decomposition model proposed by Abowd, Kramarz, and Margolis (AKM, 1999), θ_i is the pure person effect, $\Psi_{J(i,t)}$ is the pure effect of establishment J at which worker i is employed at time t, $\Phi_{i,J(i,t)}$ is the pure worker-establishment match effect. In practice, since the values of θ_i and $\Psi_{J(i,t)}$ are not of interest, the main specification that I estimate only

[10] See Arnould and Nichols (1983), Olson (1981), Dorsey and Walzer (1983).

includes $\Phi_{i,J(i,t)}$, which absorbs both the worker and establishment effects. However, I also estimate models with just $\Psi_{J(i,t)}$, just $\Phi_{i,J(i,t)}$, and with both of these terms in order to diagnose separate bias components under alternative models that are feasible to estimate in more general labor-market settings.

The fixed match effects model most directly relaxes the identifying assumptions of Rosen's second case. As long as preferences are static, so that there is no within-worker variation in preferences across jobs, the first condition of case two holds. The inclusion of firm and match effects controls for vertical shifts in the offer curve across jobs, removing the confounding effects of search and matching depicted in Figure 2. After conditioning away the vertical shifts, the set of offer curves collapses to a function with the same properties as in the perfectly competitive model. As a result, information about preferences is identifiable using intertemporal variation across jobs even if the assumption of frictionless matching is relaxed to allow for firm and match-specific wage effects. To be clear, this model does not eliminate biases under every possible form of labor-market imperfection. For example, if wages are determined by bilateral Nash bargaining rather than wage posting, the identification conditions would require that the Nash bargaining parameters do not vary within match in a way that is correlated with safety. Identification also requires the assumption that Γ_z is a common parameter, which is to say that the production function for amenities is the same in each firm. This type of assumption is used throughout the literature since data are not generally available to estimate firm-specific risk functions over events with such small probabilities, especially in small firms as in the current setting. [11]

Under the same set of assumptions, Rosen's first case can also be applied. If preferences are static then ϵ drops out of U in the within-worker model. If one can condition on differences in isoprofit functions across jobs, then there is no residual job-specific heterogeneity in wages, and workers' preferences are identified as they would be in a frictionless labor market. The assumptions of the fixed effects model require that these differences in isoprofit functions can only occur in the wage dimension, not in the risk dimension. This requires that the production function of amenities is the same in each firm, as in case two.

Lavetti and Schmutte (2016) build upon the search models of Hwang, Mortensen and Reed (1998), Sullivan and To (2014), Dey and Flinn (2008), and Bonhomme and Jolivet (2009) to show the general conditions under which preferences of workers can be identified using matched longitudinal employer-employee data. The key intuition of their analysis is that if firms have wage effects that are common across jobs at the firm, as depicted by the level shift in offer functions in Figure 2, then variation in fatality rates within the firm can be used to identify the preferences of workers even in the presence of search frictions. In the present study the identifying assumptions are even weaker than in Lavetti and Schmutte (2016), since there is no requirement that firms offer common wage premia across workers. For example, if worker and firms negotiate bilaterally and workers differ in bargaining power, creating match-specific latent

[11] This may not be unrealistic in the current setting, since fatal accidents are often direct acts of nature. Roughly half of the total fatality rate can be attributed to entire vessels capsizing, often due to an unusually large 'rogue,' or unpredictable, wave.

wage effects, this will not bias the estimates since they only rely on within-match variation.

Although the evidence is limited, the latent wage components in Equation 2 have been shown to have substantial effects on estimated compensating wage differentials. A small number of studies have used panel data from workers who switch jobs in order to control for worker effects θ_i. All of these studies find that including worker effects significantly decreases the estimated compensating differential. Brown (1980) uses data from young male job-switchers in the NLSY and finds within-worker estimates that are statistically indistinguishable from zero, while Kniesner et al (2012) find that the VSL implied by the compensating wage differential decreases from about \$29 million to about \$6 million when worker heterogeneity is removed using PSID data. Tsai, Liu and Hammitt (2010) find similar patterns in Taiwanese data using a two-way fixed effects model.

3 Empirical Setting and Data

The target population that I focus on in the survey includes male commercial fishing deckhands between the ages of 22 and 55 who worked on fishing vessels that conducted business in the town of Dutch Harbor, Alaska, between 2003 and 2009. Dutch Harbor has been the largest seafood processing port in the US for over twenty years, and has a large, active labor market for fishing deckhands. Most deckhands who work in Bering Sea fisheries reside throughout the pacific northwest, and travel to the fisheries each season. Only 20.7% of the population of commercial deckhands lives in Alaska, and 64.5% lived in either California, Washington, or Oregon.

3.1 Sources of Variation in Safety and Earnings

The key empirical features of this labor market are the large variations in fatality rates over time, and the variation in hourly earnings. There are multiple dimensions that contribute to the variations in fatality rates. The first is seasonal variation within years, which is driven in part by weather patterns. The majority of deaths occur due to hypothermia and/or drowning, and this source of risk depends on the size of waves and the temperature of the water.Appendix Figures A3 and A4 show that waves more than double in average height between summer and winter months, and the water temperature drops to nearly zero degrees Celsius. Correspondingly, there is a five-fold increase in the average fatality rate from the summer to the winter.

Second, there are also across-year intermediate-term weather patterns, primarily La Niña and El Niño patterns, that affect ocean currents and are correlated with fatality rates. Conditional on a given calendar month, these patterns contribute to across-year variation in safety that is somewhat predictable.

Finally, there were also reductions in fatality rates over time caused by policy changes. The first major change occurred in 2005, when some open-access fisheries were converted to a quota-based tradeable permits system. This reduced the incentive to race against other vessels, since each vessel had protected property rights, allowing vessels to take greater safety precautions. Second, in 2006 an important Coast Guard dockside safety protocol was revised following a high-profile accident that led to five fatalities.[12] Lincoln et al. (2013) show that these and other related safety policies caused a gradual but substantial improvement in occupational safety throughout Alaska. Consistent with this pattern, in my data a regression of the fatality rate on an annual linear time trend gives a coefficient of -0.21 [SE 0.05] deaths per 1,000 worker-years.

These policy changes cause variation outside of weather-based channels, reducing potential concern about collinearity between weather and safety, each of which may have direct effects on earnings. To quantify the relative contributions of each source of variation, I regress the fatality rate on a set of month indicators, and find that seasonality alone explains only about 8.4% of the total unconditional variation in fatality rates. Replacing month effects with year effects indicates that the across-year variation explains about 12% of the total variation. The relative importance of the third component, attributed to policy variation, is assessed by regressing the fatality rate on a linear time trend with breaks at the two major policy changes. This model explains 7.5% of the total unconditional variation, suggesting that each of the three sources contributes meaningfully to the identifying variation in safety.

Although it may seem strange to choose to fish during the winter, the timing of fishing seasons regulated to minimize interference with the procreation and sustainability of each species, so seasons for different species are spread throughout the year. This makes much of the variation in safety beyond the control of either workers or firms.

Variation in fatal risk levels is also accompanied by substantial hourly earnings premia, as shown in Figure 1. An institutional feature of this setting is that all deckhands are compensated by revenue-sharing rather than fixed salaries. In the survey data I observe substantial variation in both hourly earnings and in the contracted revenue-share rate across seasons even for deckhands that remain employed by the same vessel throughout the year. Although firms do not insure deckhands against shocks to their hourly earnings, the amount of financial risk deckhands face is attenuated by the use of forward contracts in output markets. Vessels contract with processors before the start of a season to deliver a specified amount of catch for a fixed price by a predetermined date, making the total revenue of the vessel and the worker reasonably predictable.The fisheries studied are not unique for having spot labor markets. Beaudry and DiNardo (1991) find insignificant and wrong-sided evidence of implicit contracts in the forestry and fishing industry using CPS data, in contrast to most other industries.

3.2 Survey Data

The main source of labor-market data come from a survey that I conducted of deckhands working in Bering Sea fisheries. The survey was conducted in several rounds, including mailing

[12] In 1998 the Coast Guard implemented a mandatory Dockside Stability and Safety Compliance Check Examination in BSAI fisheries. The examination reviewed the engineering designs of each vessel to assure that they were sufficiently stable given the fishing gear on deck and had proper lifesaving equipment on board before they were permitted to begin fishing. In January 2005 the F/V Big Valley skipped the mandatory inspection and left port with twice as many crab pots on deck as the stability requirement would have allowed, and subsequently sank killing five of the six crew members. This event contributed to a 2006 review and revision of the examination policy to improve its effectiveness.

components and a field survey in Dutch Harbor, Alaska. The survey includes questions on demographics, income, wealth, subjective risk perceptions, and recall-based panel questions on employment histories for all jobs, fishing and other. Regarding fishing-related employment, detailed questions were asked about each trip taken, including the month(s) during which the trip occurred, the species fished, the number of days spent fishing, the vessel name, all aspects of the labor contract, the average number of hours worked per day, and total earnings.

The survey data include 133 respondents who worked a total of N=1,351 fishing job spells. Of these respondents, 80 were in the direct survey group completed in October 2009, at the beginning of the Red King Crab season, and the response rate for this survey was 62.2%. This sample represents about 20 percent of the relevant population of deckhands who worked in this fishery in 2009. The remaining 53 respondents were from two separate rounds of mailing interviews. The first round was sent to individuals who purchased commercial fishing licenses in Dutch Harbor at any point between 2002-2008, and a second mailed directly to all fishing vessels that registered for the 2010 Opilio Crab season. The response rates for these two mail-based components were 4.2%[13] and 16%, respectively.[14] Although there may be differences in responses across survey waves, in fixed effects specifications the worker or match effects absorb any such differences.

Although the response rate is fairly low, the entire target population of the survey is already non-representative of US labor markets generally, so a low response rate does not add to the non-representativeness of the sample. My objective in this study is not to inform any specific policy, but to provide insight into common methodological challenges that can be uniquely addressed in this setting. Nonetheless, I show in Appendix Section 1.4 that there is no significant difference in responses across survey waves.

Table A2 reports demographic summary statistics of the sample. The median deckhand in the sample is 37 years old, white, and has less than a college graduate education. Slightly more than half of workers are married, and about the same percentage have children. Similar to the population of deckhands, the sample is of diverse geographic origin, with only 16% coming from the state of Alaska, and about 64% from California, Washington, and Oregon.[15]

Table 2 and Appendix Table A1 report summary statistics on earnings and work experience. On average, each respondent worked 10.2 fishing job-spells during the survey window, and about 6.5 job-spells per vessel. About 74% of workers in the sample had at least one job outside of fishing during the survey window. Hourly earnings are calculated by dividing earnings per job-spell by hours worked, and the term 'wage' is used for brevity. The mean hourly wage from outside jobs held by this subsample was \$13.61, which was substantially lower than the mean fishing hourly wage of \$54.16.[16] The mean annual earned income from all sources in the sample

[13]The sampling frame for this survey was derived from an Alaska Department of Fish and Game (ADFG) database of commercial fishing licenses sold, which provided the name, mailing address, and location of license purchase for every commercial fishermen in the state since 1988. However, the mailing addresses in the database were several years old, and many deckhands had moved and the surveys were not forwarded.

[14]A copy of the survey instrument is available at http://www.kurtlavetti.com/research.

[15]There are no deckhands in the sample that were not US residents at the time of survey.

[16]This is a weighted average, weighted by job-spell duration measured in days.

Table 1: Summary Statistics: Survey Demographics

Age	36.99
White	85.71%
Less than HS Educ.	9.02%
High School Diploma	47.37%
Some College	39.10%
Ever Married	47.37%
AK Resident	15.91%
CA Resident	5.30%
OR Resident	7.58%
WA Resident	50.76%
Other US Resident	19.69%
N Respondents	133
N Worker-Vessel Pairs	183
N Job-Spells	1195

Table 2: Summary Statistics: Earnings

	Mean	S.E.
Percent of Respondents with Non-fishing Job	74.44%	
Conditional Non-fishing Hourly Wage	$13.61	[$7.74]
Mean Fishing Job Spells per Respondent	10.15	
Fishing Hourly Earnings (unweighted)	$35.26	[$28.04]
Fishing Hourly Earnings (weighted)	$54.16	[$61.65]
Income per Fishing Job Spell	$34,980	[$28,627]
Hours Worked per Fishing Job Spell	955	[817]
Individual Annual Income, All Sources	$91,263	[$58,379]

Note: Mean fishing hourly earnings are weighted by the length of job spells.

was $91,263.

Since the majority of the sample was collected during a field survey at one point in time, the quantitative results are not necessarily generalizable to the entire population of Bering Sea deckhands, much less to labor markets more broadly. There are, however, some limited data available about the population of deckhands from an administrative state commercial fishing license database, which I use to test the representativeness of the sample. These data include state of residency and the number of years in which each person purchased a commercial fishing license in Alaska. Using a chi-share test I fail to reject the hypothesis that the survey sample is of different geographic origin (p-value 0.53) or has a different distribution of potential experience (p-value 0.24).

3.3 Fatality Data and Non-Fatal Injury Data

Data on fatalities come from the Alaska Occupational Injury Surveillance System (AOISS), which is an administrative database of every individual work-related traumatic injury and fatality in the state of Alaska. The AOISS is maintained by the National Institute for Occupational Safety and Health, and collects information on each injury and fatality from US Coast Guard reports, Alaska State Trooper reports, medical examiner documents, and death certificates. The NIOSH provided a subset of this database containing information on every commercial fishing fatality from 1990-2007. The data include the longitude and latitude of each accident, the circumstances of the accident, the date of the accident, vessel characteristics, the type of fishery, and the number of fatalities. Fatality rates are calculated in each month-by-year by combining AOISS fatality data with data from the NIOSH estimating the number of full-time equivalent workers in each Alaskan fishery in each year, and data from the Alaska State Department of Labor on the number of deckhands working in each fishery in each month of the year. The NIOSH data are used to adjust the monthly Department of Labor counts of workers to estimate the number of full-time equivalent workers.

The AOISS has a few advantages over datasets that have been used in the past to study occupational fatalities. First, the data contain individual fatal accidents from a single well-defined labor-market in which workers have the same occupation. Studies that focus on more general labor markets often use data from the Bureau of Labor Statistics' Census of Fatal Occupational Injuries, where fatal accidents are linked to industry codes or occupation codes that may have some measurement error. Knowing that a fatal accident occurred on a commercial fishing vessel at a specific longitude and latitude, along with a description of the accident, there is less concern about potential mismatching in calculating fatality rates in this single-industry setting. The AOISS microdata also permit the estimation of seasonal changes in fatality rates by allowing matching of contemporaneous weather conditions to the date and specific location of fatal accidents.

I also use administrative data on non-fatal injuries from the Alaska Trauma Registry (ATR) to test the sensitivity of estimates to the inclusion of non-fatal injury rates, including measures that account for injury severity, the duration of hospital stays, and the medical costs associated with each injury. The ATR data include all work-related injuries in the state that led to acute care, and are sufficiently detailed to identify which injuries were related to commercial fishing in the regions of interest. These data are unusually rich in providing multiple measures of the severity of injuries. Most previous studies that include non-fatal injuries are only able to control for average injury rates, leaving variation in injury severity as a form of measurement error.

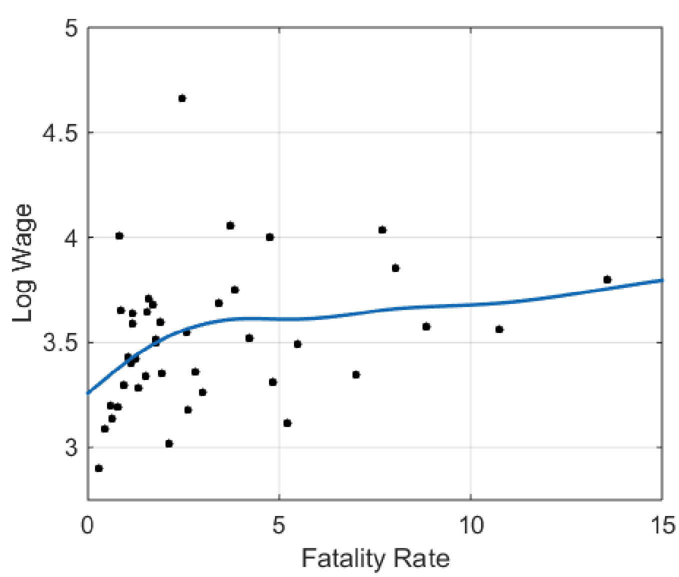
4 Empirical Estimation and Results

I begin by describing the empirical specification of Equation 2, followed by the main estimation results. The results replicate the pooled cross-sectional and worker effects specifications that are common in the literature, along with a firm effects model and the main match effects model. I then discuss a set of residual diagnostic plots that show that endogeneity bias comes primarily from latent worker and firm effects, and suggest that there is very little evidence of non-random job selection based on pure match effects. I describe how the estimated marginal aversion to fatal risk changes with the level of risk, and provide a variety of robustness checks related to potential measurement error in fatality rates, non-random selection of worker into seasons of the year, and the measurement of non-fatal injury severity.

4.1 Basic Model and Results

Figure 3 shows a binned scatterplot of the unconditional relationship between log wages and fatality rates in the data. The fitted semi-parametric function suggests that log wages are increasing in the fatality rate, as expected, and that the slope of the wage profile decreases as the fatality rate increases.

Figure 3: Binned Scatterplot of Log Wage vs. Fatality Rate



Notes: Fatality rates measured in deaths per 1,000 full-time equivalent worker-years.

To capture these features of the data, the empirical specification of Equation (2) that I estimate is the following fixed effects model:

$$\begin{aligned}\ln(w_{ijt}) \;=\; & R_t\gamma_1 + R_t^2\gamma_2 + R_t^3\gamma_3 + R_t^4\gamma_4 + RZero_t\gamma_5 + InjRate_t\gamma_6 + MedCost_t\gamma_7 \\ & + MeanWave_t\gamma_8 + VarWave_t\gamma_9 + AirTemp_t\gamma_{10} + x_{it}\beta + \Phi_{i,J(i,t)} + \epsilon_{ijt}\end{aligned} \tag{4}$$

The nonwage amenity component $f(a_{jt};\gamma)$ includes a quartic polynomial in the monthly fatality rate, R, a binary indicator $RZero_t$ that equals one when R equals zero, two measures of non-fatal injury rates, and three weather variables that are correlated with risk but potentially have their own compensating wage differentials. The quartic specification was chosen by testing the incremental explanatory power of each polynomial term using an orthogonalized polynomial test. The orthogonalized quartic term is statistically significant, but the quintic term is not. The purpose of $RZero_t$ is to control for the fact that in months with zero fatalities, workers' ex ante expectations were very likely not zero, and γ_5 captures the effect of this measurement error. I also use several other approaches to assess potential measurement error in the fatality rate, which I discuss in Section 4.4.

The first non-fatal injury variable $InjRate$ is the monthly number of traumatic non-fatal injuries per 1,000 FTE worker-years, and the second variable, $MedCost$, is the monthly average hospital costs associated with non-fatal injuries measured in $100,000s per 1,000 FTE worker-years. Costs include payments from all sources, and are calculated using allowed reimbursement rates for insured patients. Both of these variables are constructed using individual events data from the administrative records on commercial fishing accidents in the Alaska Trauma Registry.

The weather variables include $MeanWave$, the mean wave height, $VarWave$, the within-month variance of hourly average wave heights, and $AirTemp$, the average air temperature.[17] x_{it} includes industry-specific fishing experience, experience squared, and year effects. Year effects control for annual variation in wages associated, for example, with the policy changes in BSAI fisheries or changes in fish prices, which affect all workers. $\Phi_{i,J(i,t)}$ are job-match effects, which are modeled as fixed, and absorb the latent worker and establishment characteristics θ_i and $\Psi_{J(i,t)}$. The fixed effects model allows all three components of latent heterogeneity to be correlated with the fatality rate and weather conditions, and allows for the possibility of non-random sorting of workers and vessels.

The inclusion of weather variables in the model is important, because weather may affect the desirability of a job, creating a direct effect on wages. For this reason, weather conditions cannot be used as instrumental variables, as doing so would violate the exclusion restriction. As Appendix Table A5 shows, the best-fitting weather-based model explains only 35% of the variation in fatality rates, and policy-based variation assists in separately identifying the effects of safety and weather conditions on earnings. This approach to estimating compensating differentials for multiple job amenities is not uncommon—Bonhomme and Jolivet (2009) estimate compensating differentials for five different job characteristics that are also correlated with each other. By including match effects, all static unobserved aspects of the job are controlled for, making it easier to narrow attention to amenities that change within jobs over time. Conditional exogeneity in this model requires that any other aspects of the job that change over time and are correlated with earnings must be included in the model.

To show the relative impact of each of the latent heterogeneity components, I first estimate a pooled cross-sectional version of Equation (4) that also includes time-invariant observable characteristics: education, marital status, and race. The results from this model are shown in column (1) of Table 3. All four of the quartic risk components are statistically significant, and

[17] Historical weather data come from NOAA weather buoy station #46035, located in the Bering Sea, which provides hourly weather data dating back to 1985.

an F-test rejects the null hypothesis that wages are a linear function of fatality with a p-value of 0.016.

Table 4 shows the MVSLs implied by the parameter estimates in column (1) of Table 3 at the 25th, 50th, and 75th percentiles of the distribution of fatality rates and at the mean fatality rate. MVSLs are calculated as:

$$MVSL(R) = \frac{\partial w(R)}{\partial R} * 1,000 * 2,000$$

Since $w(R)$ is an hourly earnings measure, while R is measured as the number of deaths per 1,000 full-time full-year equivalent workers, the derivative must be scaled by 1,000 workers and by 2,000 hours per full-time equivalent worker-year. The second step is to estimate $\frac{\partial w(R)}{\partial R}$. Since Equation 4 is a log wage model, taking the derivative with respect to R gives:

$$\frac{1}{w}\frac{\partial w(R)}{\partial R} = \gamma_1 + 2R\gamma_2 + 3R^2\gamma_3 + 4R^3\gamma_4$$

Using $\widehat{\gamma}$ and the mean wage to estimate $\widehat{\frac{\partial w(R)}{\partial R}}$, and then rescaling this derivative gives $\widehat{MVSL}$. Standard errors on the estimated MVSL are calculated using the delta method.

The pooled estimates imply an MVSL of $12.6 million [SE = 2.8] at a fatality rate of 1. Neither the average non-fatal injury rate nor the average non-fatal medical spending has a significant effect on wages. Mean wave heights and air temperatures also have no significant effect, but higher variance in wave heights has a significant positive effect on earnings.

Column (2) adds fixed worker effects and drops the time-invariant characteristics from the pooled model. This model is analogous to the longitudinal hedonic wage models that have been estimated by Brown (1980) and Kniesner et al. (2012). The fixed effects specification accounts for unobserved heterogeneity across workers that could otherwise cause omitted variable bias. Relative to the pooled model, the estimated coefficients on the fatality rate terms all shrink towards zero, and only the first-order component is significant at the 5% level. The MVSL falls by more than half, to $5.8 million [SE = 2.1] at a fatality rate of 1. This is consistent with evidence from Abowd, Kramarz, and Margolis (1999) and Kniesner et al. (2012) that unobserved heterogeneity across workers explains a substantial share of the residual variation in wages, and this residual variation is correlated with job amenity choices, as predicted by theory. The bias from unobserved worker heterogeneity has the same direction and approximate magnitude as estimates from Kniesner et al (2012) using the PSID. Wave height is also significant in this model, and suggests a large compensating wage differential of about 28% per meter of average wave height.

Including establishment (vessel) effects instead of worker effects identifies the compensating differential using variation across workers during a given season, and over time across seasons for the same vessel, controlling for time-invariant unobserved differences in vessel productivity. Column (3) reports estimates from this model. All four of the quartic components are statistically significant at the 5% level, and the MVSL is higher than the worker effects estimate,

Table 3: Fixed Effects Models, Nonlinear in Fatality Rate

	(1)	(2)	(3)	(4)
	Dependent Variable: ln($Wage$)			
Fatality Rate	0.313***	0.134**	0.216***	0.153***
	[0.069]	[0.053]	[0.053]	[0.049]
Fatality Rate Sq.	–0.062***	–0.023*	–0.041***	–0.026**
	[0.014]	[0.012]	[0.011]	[0.011]
Fatality Rate Cu.	0.005***	0.002*	0.003***	0.002**
	[0.001]	[0.001]	[0.001]	[0.001]
Fatality Rate 4th	–0.000***	–0.000*	–0.000**	–0.000**
	[0.000]	[0.000]	[0.000]	[0.000]
Non-Fatal Injury Rate	–0.008	–0.004	–0.007	–0.004
	[0.008]	[0.006]	[0.007]	[0.006]
Injury Medical Costs	0.051	0.031	0.059**	0.040
	[0.032]	[0.025]	[0.029]	[0.026]
Mean Wave Height	0.106	0.282***	0.222**	0.284***
	[0.093]	[0.063]	[0.084]	[0.071]
Variance of Wave Height	0.109**	0.012	0.093*	0.041
	[0.052]	[0.045]	[0.049]	[0.041]
Air Temperature	–0.015	0.001	0.009	0.012
	[0.014]	[0.011]	[0.012]	[0.011]
Worker Effects	N	Y	N	Y
Vessel Effects	N	N	Y	Y
Match Effects	N	N	N	Y
N	1,195	1,195	1,195	1,195
N Clusters	128	128	183	183
R-Sq	0.318	0.733	0.690	0.791

Notes: Fatality rate is measured as the number of fatalities per 1,000 FTE worker-years. Variance of Wave Height is the within-month variance of hourly wave height measurements. 'Non-Fatal Injury Rate' is measured as the number of traumatic injuries per 1,000 FTE worker-years. 'Injury Medical Costs' is the average total cost of hospital care for non-fatal injuries, measured in $100,000s based on reimbursement rates, per 1,000 FTE worker-years. Model 1 also includes education, race, and marital status. All models include year effects, experience, experience squared, and a zero fatalities indicator, and are weighted by the length of job spells. Standard errors are clustered at the worker level in Models 1-2, and at the match level in Models 3-4. *** Significant at the .01 level, ** significant at the 0.05 level, * significant at the 0.10 level.

at $8.9 million [SE = 2.1] at a fatality rate of 1. Non-fatal injury medical costs also become significant in this model. The coefficient suggests that a $100,000 increase in the total cost of injuries per 1,000 FTE worker-years is associated with 5.9% higher wages.

Table 4: MVSL Functions Implied by Fixed Effects Estimates

	(1) Pooled	(2) Worker Effects	(3) Vessel Effects	(4) Match Effects
	MVSL ($ Millions) by Fatality Rate			
$R = 1.2$ (25th Pctl)	$11.29	$5.29	$8.01	$6.03
	[2.51]	[1.83]	[1.86]	[1.73]
$R = 1.9$ (50th Pctl)	$7.57	$3.92	$5.55	$4.42
	[1.71]	[1.19]	[1.25]	[1.15]
$R = 3.1$ (Mean)	$3.25	$2.31	$2.70	$2.49
	[1.04]	[0.69]	[0.78]	[0.69]
$R = 4.1$ (75th Pctl)	$0.67	$1.34	$0.99	$1.30
	[0.91]	[0.65]	[0.68]	[0.61]

Notes: All estimates correspond to the respective Models (1)-(4) in Table 3. Fatality rates are measured as the number of fatalities per 1,000 FTE worker-years. MVSL estimates are measured in millions of 2009 dollars. Standard errors are calculated using the delta method.

Finally, column (4) presents estimates of Equation 4, the main specification. The model includes match effects, which absorb both worker and establishment effects, and allow for non-random assignment of workers to firms and mobility between jobs to be correlated with the components of unobserved heterogeneity. The ability to estimate this model, which requires substantial variation in safety within jobs, is a primary unique feature of the empirical setting. Lavetti and Schmutte (2016) show that in more general labor-market settings a similar match effects model produces biased estimates of compensating wage differentials, largely because only 3% of the variance in fatality rates occurs within jobs in the markets they study. The results show that all four of the estimated fatality rate polynomial terms are again significant, and together they imply an MVSL of $6.6 million [SE = 1.9] at a fatality rate of 1. Average wave height also remains significant.

The correlation between unobserved worker, establishment, and match effects and the explanatory variables inflates the MVSL estimate in the pooled model upward by 91% of the match effects estimate. However, the components of the change in bias are partially offsetting. Relative to the match effects model, the within-worker model yields estimates that are about 12% too low. The reason for this is evident in Table 5, which shows the correlations between each of the components of a decomposition of log wages. The decomposition is estimated using a two-way fixed effects model with worker and firm effects, similar to Abowd, Kramarz, and Margolis's (1999) Equation 2.2. Match effects are not included in the decomposition, because they are not separately identified in a fixed effects specification without imposing orthogonality assumptions between match effects and worker and establishment effects. The correlation

Table 5: Correlations between Components of the Log Wage Rate

	Mean	Std. Dev.	Correlation					
			Log Wage	$X\beta$	θ	Ψ	ϵ	$A\gamma$
Log Wage	3.43	0.67	1					
Time-Varying Characteristics	−0.09	0.11	−0.211	1				
Worker Effect	−0.31	0.69	0.497	−0.204	1			
Firm Effect	−0.35	0.59	0.215	−0.114	−0.595	1		
Residual	0.00	0.31	0.457	0.000	−0.000	0.000	1	
Non-Wage Job Amenities	0.71	0.25	0.335	−0.158	0.075	−0.101	0.000	1

Notes: Correlation parameters are between components from a decomposition of log wages into observable time-varying characteristics ($X\beta$), unobservable worker heterogeneity (θ), unobservable firm heterogeneity (Ψ), and non-wage job amenities (including a fourth order polynomial in the fatality rate, non-fatal trauma rate, injury medical costs, mean wave height, variance of wave height, and air temperature) using a two-way fixed effects model. The column headers use symbols from Equation 2 while row headers provide short descriptions. Correlations, means, and standard deviations are all weighted by the length of job spells, as in the regression models.

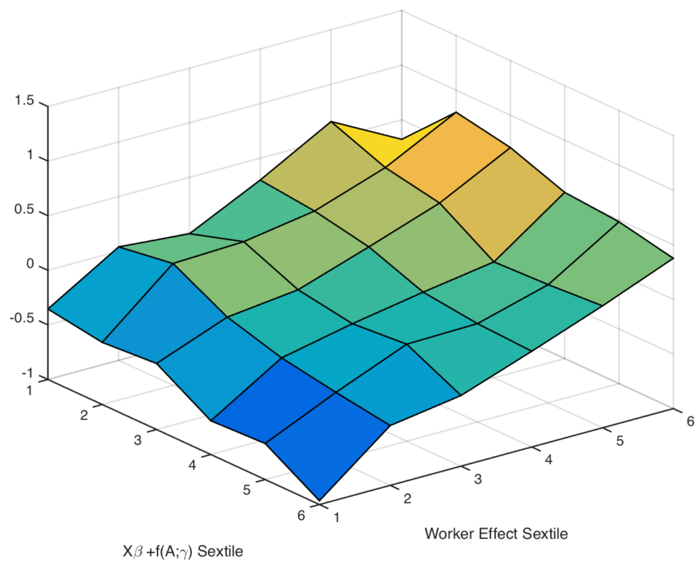
matrix of the log wage components shows that wages attributable to unobserved worker effects are positively correlated with the non-wage job amenity component, which includes risk, with a correlation coefficient of 0.08. However, the component of wages attributable to unobserved establishment heterogeneity is negatively correlated with both the worker and non-wage amenity components, with correlations of -0.60 and -0.10 respectively. The corresponding elements of the covariance matrix are reported in Appendix Table A3. The signs of the covariance terms indicate the directions of the bias components from models that omit each of the terms. The omission of worker effects generates a positive bias component, while the omission of establishment effects generates a negative one. This is consistent with estimates from the pooled model being higher than estimates from the within-worker model, and MVSL estimates from both the establishment and match effects models being higher than the worker effects estimates.

4.2 Residual Diagnostics

To visualize why the pooled model estimates from column (1) of Table 3 are biased, Figure 4 graphs the average residual by sextile of the distribution of worker effects and the distribution of variation in wages explained by observables, $(X\beta + f(A;\gamma))$. The average residual is largest among workers with high latent worker effects, since the pooled model omits worker effects. There is also a clear pattern in the correlation between latent worker effects and the explained variation in wages, which causes endogeneity bias. For example, among low-wage workers the average residual is negative for every sextile of the distribution of explained wage variation, while among high wage workers the average residual is always positive.

To see why including worker effects does not fix this endogeneity problem, Figure 5 graphs the average residual from column (2) of Table 3, the worker effects model, by sextile of the distribution of firm effects and the explained variation in wages, $(X\beta + f(A;\gamma) + \theta_i)$. A similar residual pattern that is indicative of endogeneity still holds in this figure. The average residual

Figure 4: Average Pooled Residual by Sextile of Worker Effect and $X\beta + f(A;\gamma)$



Notes: Plot depicts average residuals from Model (1) in Table 3 by sextile of the worker effects distribution estimated by a two-way AKM fixed effects model and by sextile of the predicted wage from Model (1).

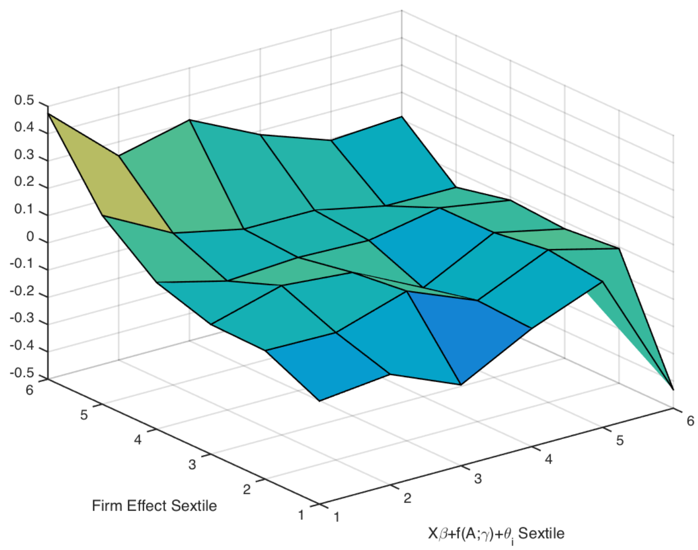
increases in the firm effect sextile, and residuals among low wage firms are substantially lower than the residuals at high wage firms for every sextile of the distribution of explained variation in wages.

This pattern in the residuals is consistent with the form of job mobility depicted in Figure 2, in which unobserved firm heterogeneity is correlated with amenities. The problem of endogenous job mobility is studied in a more general setting by Abowd et al. (2012), who devise a test for endogenous mobility, which they define as a systematic relationship between pure match effects and wage rates following subsequent job transitions. Abowd and Schmutte (2012) implement this test and strongly reject the null hypothesis of exogenous job mobility, finding that workers with more negative pure match effects are more likely to switch jobs, and job transitions tend to increase wages. All of this evidence suggests a large potential for omitted variable biases in models that exclude firm and match heterogeneity, and a likely correlation between variation in amenities and variation in wage components.

The final residual diagnostic graphs the average residual from a two-way AKM fixed effects model by sextile of the worker effect and firm effect distributions. The figure is shown in the Appendix (Figure A1) because it looks indistinguishable from a flat plane. Since the residuals from this model include the pure match effect term from Equation 4, the diagnostic plot provides evidence of any patterns in the correlation between match effects and worker or firm effects. The average residuals are extremely small in every cell, suggesting that the assignment of jobs based on match quality is uncorrelated with the latent ability of workers or with firm wage effects. As a result, the parameter estimates from the AKM two-way fixed effects model are nearly identical to estimates from the match effects model in column (4) of Table 3.[18] This finding is consistent

[18] Although the AKM model is more common in the labor economics literature, I report the match effects

Figure 5: Average Within-Worker Residual by Sextile of Firm Effect and $X\beta + f(A;\gamma) + \theta_i$



Notes: Plot depicts average residuals from Model (2) in Table 3 by sextile of the firm effects distribution estimated by a two-way AKM fixed effects model and by sextile of the predicted wage from Model (2).

with patterns in German wage data documented by Card, Heining, and Kline (2013).

4.3 The Marginal Value of Statistical Life

Several previous studies using cross-sectional data have estimated compensating wage differentials for fatal risk and included a quadratic term.[19] In general, these studies find that the coefficient on the quadratic term is negative and statistically significant. What we can conclude from such a finding in a cross-sectional setting is that people have different preferences for safety, and the people who are least averse to occupational hazards sort into riskier jobs, consistent with hedonic wage theory.

In contrast, consider the interpretation of a similar finding from a model in which match effects are also included. Since match effects capture all of the static unobserved differences across worker-firm pairs, a negative quadratic term can no longer be interpreted as evidence of sorting based on aversion to fatal risk. If preferences are static, all of this variation is absorbed by the match effects, along with the impacts of search frictions or firm heterogeneity. Under the assumptions described in Section 2.1, the seasonal shifts in offer functions identify the marginal willingness of workers to accept fatal risk, giving the estimated MVSLs an interpretation as preferences for marginal changes in risk. Although the two model specifications may appear similar, the implications of the estimates are very different when match effects are included;

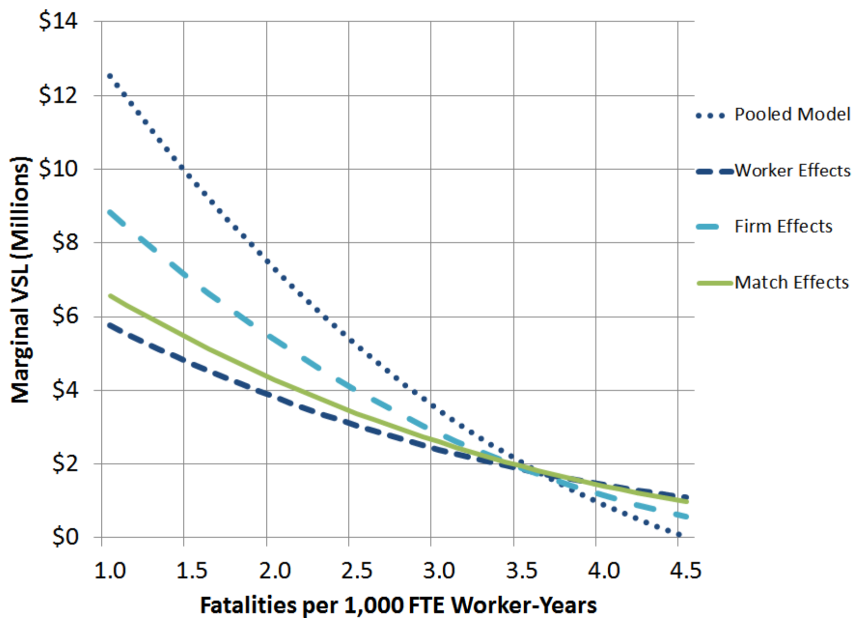
specification because it produces virtually identical results, requires weaker assumptions, is easier to conceptualize than a two-way fixed effects model.

[19]See Arnould and Nichols (1983), Olson (1981), Dorsey and Walzer (1983), for example.

Hwang et al. (1998) make a similar point about the inclusion of firm effects.

The estimates from each of the models in Table 3 show how severe the restrictions imposed by a linear hedonic wage model can be. The predicted MVSL falls sharply with the level of the fatality rate, whereas the linear model would force the VSL to be constant, independent of the level of risk. In the match effects model, for example, the MVSL falls from \$6.0 million [SE = 1.7] at the 25th percentile of the fatality rate to \$1.3 million [SE = 0.6] at the 75th percentile. Similar patterns in the MVSL function are implied by every model in Table 3.

Figure 6: Marginal VSL vs. Fatality Rate



Notes: MVSLs are based on estimates from Table 3.

To demonstrate how the linearity assumption common in the literature can bias estimates of the MVSL, Table 6 presents a similar set of models that assume linearity in the effect of fatality rates on log wages. In every specification the log-linear restriction causes substantial bias relative to the more flexible main specifications. In the match effects model the estimated MVSL is about 59% lower in the log-linear specification at the median risk level, and about 27% lower at the mean risk level.

Since the conclusion of declining marginal aversion to fatal risk depends strongly on the coefficients of the higher order polynomial terms, and since polynomial regressions often have substantial correlation between terms, I also assess the sensitivity of the model by estimating a version of Equation (4) in which the quartic fatality rate function is orthogonalized. The orthogonalized quartic polynomial is identical to the non-orthogonalized function, except that it imposes zero correlation between each of the fatality rate terms. The parameter estimates from this model are order-dependent; as each higher order term is added, the coefficients on previously included terms do not vary. This provides a conveniently direct way of testing for the marginal impact of each term in the polynomial conditional on the lower-order terms using

Table 6: Fixed Effects Models, Linear in Fatality Rate

	(1)	(2)	(3)	(4)
	Dependent Variable: ln($Wage$)			
Fatality Rate	0.036***	0.031***	0.035***	0.029***
	[0.011]	[0.008]	[0.008]	[0.007]
Non-Fatal Injury Rate	–0.004	–0.003	–0.005	–0.003**
	[0.008]	[0.006]	[0.007]	[0.006]
Injury Medical Costs	0.037	0.026	0.049*	0.036
	[0.031]	[0.024]	[0.028]	[0.024]
Mean Wave Height	0.163	0.303***	0.254***	0.307***
	[0.093]	[0.061]	[0.066]	[0.074]
Variance of Wave Height	0.047	–0.011	0.054	0.012
	[0.050]	[0.041]	[0.043]	[0.040]
Air Temperature	–0.016	0.001	0.008	0.011
	[0.014]	[0.010]	[0.011]	[0.011]
Worker Effects	N	Y	N	Y
Vessel Effects	N	N	Y	Y
Match Effects	N	N	N	Y
N Obs.	1,195	1,195	1,195	1,195
N Clusters	128	128	183	183
R-Sq	0.307	0.732	0.685	0.790
VSL ($ Millions)	$2.22	$1.92	$2.15	$1.81
	[0.68]	[0.47]	[0.50]	[0.43]

Notes: Fatality rates are measured as the number of fatalities per 1,000 FTE worker-years. Variance of Wave Height is the within-month variance of hourly wave height measurements. 'Non-Fatal Injury Rate' is measured as the number of traumatic injuries per 1,000 FTE worker-years. 'Injury Medical Costs' is the average total cost of hospital care for non-fatal injuries, measured in $100,000s based on reimbursement rates, per 1,000 FTE worker-years. Model 1 also includes education, race, and marital status. All models include year effects, experience, experience squared, and a zero fatalities indicator, and are weighted by the length of job spells. Standard errors are clustered at the worker level in Models 1-2, and at the match level in Models 3-4. VSL estimates are measured in millions of 2009 dollars, with standard errors calculated using the delta method. *** Significant at the .01 level, ** significant at the 0.05 level, * significant at the 0.10 level

the t-statistic for each term. Table 7 includes estimates for the same four models as Table 3, but with orthogonalized polynomial terms. Column (4), for example, shows that each of the polynomial terms is statistically significant even after conditioning on lower-order terms. All of the MVSL estimates are similar to those shown in Table 3, since orthogonalization does not affect the levels or derivatives of the original function.

The implication of this finding that MVSLs are declining functions with respect to fatality rates is that when workers face high baseline levels of risk, they become less averse to marginal

Table 7: Fixed Effects Models, Orthogonalized Fatality Rate

	(1)	(2)	(3)	(4)
	Dependent Variable: ln($Wage$)			
Orthogonalized Fatality Rate	0.159***	0.110***	0.134***	0.111***
	[0.036]	[0.025]	[0.028]	[0.024]
Orthogonalized Fatality Rate Sq.	–0.064**	–0.024	–0.039**	–0.031**
	[0.026]	[0.015]	[0.017]	[0.015]
Orthogonalized Fatality Rate Cu.	0.063**	0.025	0.043**	0.034**
	[0.024]	[0.017]	[0.015]	[0.015]
Orthogonalized Fatality Rate 4th	–0.084***	–0.030*	–0.056***	–0.033**
	[0.018]	[0.017]	[0.016]	[0.015]
Non-Fatal Injury Rate	–0.008	–0.004	–0.007	–0.004
	[0.008]	[0.006]	[0.007]	[0.006]
Injury Medical Costs	0.051	0.031	0.059**	0.040
	[0.032]	[0.025]	[0.029]	[0.026]
Mean Wave Height	0.106	0.282***	0.222**	0.284***
	[0.093]	[0.063]	[0.084]	[0.071]
Variance of Wave Height	0.109**	0.012	0.093*	0.041
	[0.052]	[0.045]	[0.049]	[0.041]
Air Temperature	–0.015	0.001	0.009	0.012
	[0.014]	[0.011]	[0.012]	[0.011]
Worker Effects	N	Y	N	Y
Firm Effects	N	N	Y	Y
Match Effects	N	N	N	Y
N	1,195	1,195	1,195	1,195
N Clusters	128	128	183	183
R-Sq	0.317	0.733	0.688	0.791

Notes: Fatality rate polynomial terms are orthogonalized using the Christoffel-Darboux formula, and are measured as the number of fatalities per 1,000 FTE worker-years. Estimates are marginal effects of the fatality rate conditional on observing at least one fatality. Variance of Wave Height is the within-month variance of hourly wave height measurements. Model 1 also includes education, race, and marital status. All models include year effects, experience, experience squared, and a zero fatalities indicator, and are weighted by the length of job spells. Standard errors are clustered at the worker level in Models 1-2, and at the match level in Models 3-4. *** Significant at the .01 level, ** significant at the 0.05 level, * significant at the 0.10 level increases in risk. Note that this conclusion does not violate the convexity of preferences. An example of convex preferences consistent with this finding would be if indifference curves in Figure 2 become flatter at high levels of risk for a given wage. This finding has important implications for the efficient allocation of public resources devoted to safety, suggesting that improvements in safety are complementary rather than substitutable. Although the estimates come from a narrow sample, they offer evidence on a previously undocumented finding and expand the scope of questions that future research from more general labor market settings can build upon.

4.4 Measurement Error in Fatality Rates

One potential concern with estimating Equation (4) in this empirical setting is that the population of interest is quite small and fatalities are low-probability events, so the average monthly fatality rate has a large variance. For example, observed monthly fatality rates are often zero. The fatality rate that is relevant for estimating compensating wage differentials is the fatality rate that workers *expect* at the time they accept a job. In large industries with many workers there are typically enough fatalities for the mean fatality rate to be a good approximation to workers' expectations, but this assumption may not hold in the current setting. If workers expect a fatality rate that differs from the realized rate, the key variable of interest suffers from measurement error.[20]

I use two approaches to assessing the potential bias from this type of measurement error. The first approach is to use fatality rate data from multiple years, which may reduce noise and be a reasonable representation of how workers form expectations. The second approach is to directly model workers' expectations about fatality rates using weather data.[21] The advantages of this approach are that it relies upon easily observable information that workers may actually use to form expectations, and it also allows expectations to depend on prospective beliefs about intermediate-term weather patterns. For example, if El Niño weather patterns affect fatality rates and are predictable far in advance, this approach may more accurately capture expectations to the extent that they differ from retrospective average fatality rates.

4.4.1 Alternative Fatality Rate Measures

To test the sensitivity of the estimates to potential measurement error in the fatality rate, I re-estimate each of the specifications from Table 6 using both the contemporaneous and lagged fatality rates from the same calendar month in the prior year. Table 8 presents estimates from these sensitivity analyses.

The results show that when a quartic polynomial in lagged fatality rates is included in the model, none of the lagged terms is statistically significant conditional on contemporaneous

[20]Dickstein and Morales (2013) call this type of error "expectational error," and show that it creates attenuation bias very similar to measurement error.

[21]Note that weather is *not* used as an instrumental variable. The observed fatality rate is not endogenous, nor is weather excludable from the wage equation.

Table 8: Sensitivity to Measurement of Fatality Rates

	(1)	(2)	(3)	(4)
	Dependent Variable: ln($Wage$)			
Fatality Rate	0.338***	0.136**	0.211***	0.154***
	[0.078]	[0.057]	[0.055]	[0.053]
Fatality Rate Sq.	-0.069***	-0.025*	-0.042***	-0.028**
	[0.016]	[0.013]	[0.012]	[0.012]
Fatality Rate Cu.	0.005***	0.002**	0.003***	0.002**
	[0.001]	[0.001]	[0.001]	[0.001]
Fatality Rate 4th	-0.000***	-0.000**	-0.000***	-0.000**
	[0.000]	[0.000]	[0.000]	[0.000]
Lagged Fatality Rate	0.040	0.044	-0.039	-0.038
	[0.065]	[0.046]	[0.042]	[0.043]
Lagged Fatality Rate Sq.	-0.007	0.009	0.005	0.006
	[0.016]	[0.012]	[0.011]	[0.011]
Lagged Fatality Rate Cu.	0.001	-0.000	0.001	0.000
	[0.001]	[0.001]	[0.003]	[0.001]
Lagged Fatality Rate 4th	-0.000	0.000	-0.000	-0.000
	[0.000]	[0.000]	[0.000]	[0.000]
Worker Effects	N	Y	N	Y
Firm Effects	N	N	Y	Y
Match Effects	N	N	N	Y
N Obs.	1195	1195	1195	1195
N Clusters	128	128	183	183
R^2	0.340	0.748	0.708	0.802

Notes: Fatality rates are measured as the number of fatalities per 1,000 FTE worker-years. Lagged fatality rate refers to the fatality rate in the same calendar month of the prior year. Model 1 also includes education, race, and marital status. All models include year effects, experience, experience squared, non-fatal injury rate, injury medical costs, zero fatalities indicators, mean wave height, variance of wave height, and air temperature, and are weighted by the length of job spells. Standard errors of coefficients, in brackets, are clustered at the worker level in Models 1-2, and at the match level in Models 3-4. *** Significant at the .01 level, ** significant at the 0.05 level, * significant at the 0.10 level.

fatality rates. Moreover, all of the contemporaneous fatality rate terms remain statistically significant, and the magnitude of each coefficient is very similar to the baseline estimate. This suggests that, although the measures may be noisy, contemporaneous fatality rates contain variation that is absent from retrospective measures, and affects wages.

Although the majority of the literature on compensating wage differentials uses contemporaneous fatality rates (Viscusi and Aldy, 2003), the papers that assess sensitivity to measurement of fatality rates use similar specifications with either lagged fatality rates or moving average rates. Kniesner et al. (2012) argue that using single-year fatality rates is preferable in panel settings because it better captures temporal variation. Although Table 8 shows that the estimates are not very sensitive to this choice, I follow Kniesner et al. (2012) is using the single-year estimates as the benchmark model.

4.4.2 A Weather-Based Model of Expected Fatality Rates

The second approach that I use to assess measurement error in expected fatality rates is to model workers' expectations using weather variables. Of course, the only weather conditions that are relevant to workers' decisions are those that are known at the time the labor supply decision is made, which is almost always in advance of any precise weather forecasts.[22]

There are two main channels through which weather causes fatalities. The first is that large waves either cause one or more deckhands to fall overboard or cause an entire vessel to capsize, and the second is that, conditional on a fall overboard, the temperature of the water affects the amount of time available for a potential rescue before hypothermia occurs.[23] I use hourly weather data from 1985 to 2011 obtained from an NOAA weather buoy located in the Bering Sea to estimate a zero-inflate negative binomial model of the number of fatalities per month as a function of average wave height, average water temperature, and the monthly variance of hourly wave heights. Since water temperature has a direct mechanism for affecting fatality rates, while air temperature is likely to be more salient for discomfort, I use water temperature to predict fatality rates but air temperature in the wage equation. Additional details about this model, including parameter estimates, are reported in Appendix Section 1.2. The full model has a pseudo-R^2 of 0.35, consistent with the evidence that much of the variation in safety is driven by factors other than weather, such as policy variation.

To test if the weather-based predicted fatality rates affect the estimated coefficients, I re-estimate the same baseline models from Table 3 including both the realized fatality rate and the predicted fatality rate. The results, displayed in Table 9, suggest that using weather to model expectations does not significantly affect the estimates. The coefficients on fatality rate terms all remain very close to the baseline estimates, and all of the baseline coefficients that were significant at the 0.05 level remain significant. Moreover, zero of the sixteen predicted fatality rate terms is statistically significant. Most importantly, the key patterns in the estimates suggesting declining MVSLs as a function of risk and the directions of the bias components, all remain the same.

The two separate approaches to addressing potential concerns about measurement error suggest that, although the fatality rates are measured in a small population with noise, this variation does not appear to substantially alter either the qualitative patterns in the results that underlie the key conclusions of the analysis, or the quantitative levels of the parameter

[22] Deckhands typically arrive to help prepare a vessel about 2 weeks prior to departure, and the contract terms are generally set in advance of arrival since transportation to the vessel is often expensive.

[23] See Lincoln and Conway (1999).

Table 9: Fixed Effects Models, Predicted Fatality Rate

	(1)	(2)	(3)	(4)
	Dependent Variable: ln($Wage$)			
Fatality Rate	0.266***	0.116**	0.205***	0.140***
	[0.073]	[0.055]	[0.054]	[0.050]
Fatality Rate Sq.	-0.051***	-0.019	-0.039***	-0.024**
	[0.015]	[0.012]	[0.011]	[0.011]
Fatality Rate Cu.	0.004***	0.001	0.003***	0.002**
	[0.001]	[0.001]	[0.001]	[0.001]
Fatality Rate 4th	-0.000***	-0.000	-0.000***	-0.000**
	[0.000]	[0.000]	[0.000]	[0.000]
Predicted Fatality Rate	0.237	-0.002	-0.021	0.097
	[0.158]	[0.131]	[0.155]	[0.120]
Predicted Fatality Rate Sq.	-0.036	0.010	0.007	-0.018
	[0.039]	[0.031]	[0.037]	[0.029]
Predicted Fatality Rate Cu.	0.002	-0.002	-0.001	0.001
	[0.004]	[0.003]	[0.003]	[0.003]
Predicted Fatality Rate 4th	-0.000	0.000	0.000	-0.000
	[0.000]	[0.000]	[0.000]	[0.000]
Worker Effects	N	Y	N	Y
Firm Effects	N	N	Y	Y
Match Effects	N	N	N	Y
N Obs.	1195	1195	1195	1195
N Clusters	128	128	183	183
R^2	0.329	0.735	0.693	0.794

Notes: 'Predicted Fatality Rate' terms are predicted fatality rates from Model 2 in Table A5, and are measured as the number of fatalities per 1,000 FTE worker-years. Model 1 also includes education, race, and marital status. All models include year effects, experience, experience squared, non-fatal injury rate, injury medical costs, a zero fatalities indicator, mean wave height, variance of wave height, and air temperature, and are weighted by the length of job spells. Standard errors of coefficients, in brackets, are clustered at the worker level in Models 1-2, and at the match level in Models 3-4. *** Significant at the .01 level, ** significant at the 0.05 level, * significant at the 0.10 level.

estimates.

4.5 Robustness Checks

4.5.1 Seasonal Variation in Productivity

A potential interpretation concern could arise if firms are differentially productive in different seasons of the year. For example, suppose a vessel captain is more skilled at choosing fishing locations for a particular species that can only be harvested in the summer. In this case the compensation of workers on the vessel could be higher in the summer without being due to any changes in the fatality rate. To test whether this potential concern affects results, I re-estimate each of the main specifications from Table 3 including quarter effects and interactions between the worker, vessel, and match effects with species effects. Estimates from these models allow each worker, vessel, and job to have independent effects on wages across fishing trips for different species and in different quarters of the year. These estimates eliminate differences in average productivity during different seasons of the year and differences in the average productivity in a particular job-match that differs across species being harvested.

Estimates from these models are presented in Table 10. Despite the quarter and species interaction effects absorbing a large amount of the identifying variation, the qualitative conclusions from these models are the same as those from the baseline estimates. The squared, cubic, or quartic fatality rate term is significant in every specification, and the estimates still imply a declining MVSL function. The directions of the bias components all remain the same, and the worker effects estimates are still quite similar to the match effects estimates. These models suggest that the results and conclusions from the analyses are robust to potential variation in productivity across seasons of the year, and to workers, vessels, or jobs having differential productivity when different types of species are harvested.

4.5.2 Sensitivity of Estimates to Measurement of Non-Fatal Injury Severity

An empirical challenge associated with measuring occupational safety is that, unlike fatal injuries, non-fatal injuries vary in severity. Both the injury rate and the conditional severity of injuries could potentially affect wages. In most settings there are not sufficient data to measure variation in injury severities, so studies that control for non-fatal injuries tend to use average injury rates. The Alaska Trauma Registry is a substantial improvement in data quality in this regard, as it includes a variety of rich measures of injury severities. In addition to using the injury microdata to construct monthly average non-fatal trauma rates, I construct three distinct measures of injury severity. The first is a monthly average Injury Severity Score (ISS), which is based on a medically-established anatomical scoring system for grading trauma severity.[24] Second, I measure the monthly average number of days of inpatient hospitalization associated with injuries in the relevant commercial fisheries. Third, I use data from injury medical records on the costs of hospitalizations associated with each injury in the ATR, and construct a monthly average medical cost measure.[25]

Table 11 presents estimates from match effects models that include each of the three measures of injury severity. Since the Injury Severity Score, the number of days of hospitalization, and the cost of hospitalization are highly correlated with each other, each model in the table includes the

[24] The ISS is a modified measure of the Abbreviated Injury Score that takes into account the severity of multiple simultaneous injuries to different regions of the body.

[25] Hospital costs are based on up to three hospitalization spells per injury, and data on the source of payments is available for up to two of those spells.

Table 10: Fixed Effects Models, Including Quarter and Species Effects

	(1)	(2)	(3)	(4)
	Dependent Variable: ln($Wage$)			
	Pooled with Quarter and Species Effects	Worker-by--Species and Quarter Effects	Vessel-by--Species and Quarter Effects	Match-by--Species and Quarter Effects
Fatality Rate	0.270***	0.100**	0.162***	0.107**
	[0.057]	[0.046]	[0.052]	[0.048]
Fatality Rate Sq.	–0.057***	–0.019*	–0.035***	–0.021**
	[0.012]	[0.010]	[0.011]	[0.011]
Fatality Rate Cu.	0.004***	0.001*	0.003***	0.002*
	[0.001]	[0.001]	[0.001]	[0.001]
Fatality Rate 4th	–0.000***	–0.000*	–0.000***	–0.000*
	[0.000]	[0.000]	[0.000]	[0.000]
Non-Fatal Injury Rate	–0.006	0.003	–0.001	0.002
	[0.008]	[0.005]	[0.006]	[0.005]
Injury Medical Costs	0.051	-0.003	0.042	0.007
	[0.029]	[0.024]	[0.027]	[0.024]
Mean Wave Height	0.039	0.117**	0.065	0.124**
	[0.112]	[0.054]	[0.066]	[0.050]
Variance of Wave Height	0.067	–0.039	0.019	–0.021
	[0.052]	[0.040]	[0.051]	[0.042]
Air Temperature	0.011	0.059***	0.049	0.053***
	[0.027]	[0.020]	[0.020]	[0.020]
N	1,189	1,195	1,195	1,195
N Clusters	128	128	183	183
R-Sq	0.387	0.857	0.777	0.874

Notes: Fatality rate parameters are marginal effects of the fatality rate conditional on observing at least one fatality, and are measured as the number of fatalities per 1,000 FTE worker-years. See notes to Table 3 for definitions. Column 1 also includes education, race, and marital status. All models include quarter-of-year effects, experience, and experience squared, and are weighted by the length of job spells. Species are grouped into similar types as cod, salmon, crab, or other. Standard errors are clustered at the worker level in Models 1-2, and at the match level in Models 3-4. *** Significant at the .01 level, ** significant at the 0.05 level, * significant at the 0.10 level.

average injury rate, along with one of these three measures. The results suggest that non-fatal injury rates have no significant effect on earnings, and that the estimated coefficients on the fatality rate terms are not very sensitive to how the severity of non-fatal injuries is measured.

Table 11: Sensitivity of Estimates to Measurement of Non-Fatal Injury Severity

	(1)	(2)	(3)
	Dependent Variable: $\ln(Wage)$		
Fatality Rate	0.143***	0.148***	0.153***
	[0.048]	[0.048]	[0.050]
Fatality Rate Sq.	-0.023**	-0.025**	-0.026**
	[0.011]	[0.011]	[0.011]
Fatality Rate Cu.	0.002*	0.002**	0.002**
	[0.001]	[0.001]	[0.001]
Fatality Rate 4th	-0.000*	-0.000**	-0.000**
	[0.000]	[0.000]	[0.000]
Non-Fatal Injury Rate	0.013	-0.001	-0.004
	[0.008]	[0.001]	[0.006]
Injury Severity Score	-0.004*		
	[0.002]		
Injury Hospital Stays (Days)		0.001	
		[0.002]	
Injury Medical Costs			0.040
			[0.026]
Match Effects	Y	Y	Y
N Obs.	1195	1195	1195
N Clusters	183	183	183
R^2	0.792	0.791	0.791

Notes: All models are fixed effects specifications and include job match effects, year effects experience, and experience squared, mean wave height, variance of wave height, and air temperature (as in Model 4 from Table 3) and are weighted by the length of job spells. Fatality Rate terms are measured as the number of fatalities per 1,000 FTE worker-years. 'Non-Fatal Injury Rate' is measured as the number of traumatic injuries per 1,000 FTE worker-years; the 'Injury Severity Score' is a medically-established anatomical scoring system for grading trauma severity, including multiple simultaneous injuries, on a 1 to 75 scale, and is measured here as the expected sum of ISS scores per 1,000 FTE worker-years; 'Injury Hospital Stay (Days)' is the average number of days of inpatient hospitalization due to non-fatal injuries, per 1,000 FTE worker-years; 'Injury Medical Costs' is the average total cost of hospital care for non-fatal injuries, measured in $100,000s based on reimbursement rates, per 1,000 FTE worker-years. Standard errors are clustered at the match level. *** Significant at the .01 level, ** significant at the 0.05 level, * significant at the 0.10 level.

4.5.3 Seasonal Variation of 'The Marginal Worker'

According to hedonic theory, the equilibrium compensating wage differential is determined by the preferences of the marginal worker in the relevant labor market. Given that there are more deckhands employed in summer months, another potential concern for estimation is that the marginal worker in the summer may have different preferences for fatal risk than the marginal worker in the winter. Theoretically, one would expect the least risk averse individuals to work in the winter, and the marginal worker in the summer to be more averse. If this were the case, the compensating differential could increase in the summer. This would attenuate the average VSL estimate, but potentially increase the rate of change of the MVSL with respect to risk levels. The reason for this is because, as with all studies of compensating differentials in the literature, estimates are based only on data from accepted job offers, which provide information about upper bounds on preferences for safety.

Table 12: Alternative Specifications and Subsamples

		R	R^2	R^3	R^4	N
1.	Baseline Specification	0.153*** [0.050]	-0.026** [0.011]	0.002** [0.001]	-0.000** [0.000]	1195
2.	Subsample Work during Summer	0.138** [0.053]	-0.024** [0.012]	0.002* [0.001]	-0.000* [0.000]	946
3.	Subsample Work during Winter	0.162*** [0.050]	-0.028** [0.011]	0.002** [0.001]	-0.000** [0.000]	1171
4.	Subsample Work during Summer and Winter	0.148*** [0.054]	-0.026** [0.012]	0.002* [0.002]	-0.000* [0.000]	926
5.	Not Weighted by Spell Length	0.320*** [0.055]	-0.053*** [0.012]	0.003*** [0.001]	-0.000*** [0.000]	1195
6.	Unweighted Random Match Effects	0.350*** [0.050]	-0.059*** [0.011]	0.004*** [0.001]	-0.000 [0.000]	1195

Notes: Model 1 is the baseline match effects model from column (4) of Table 3. Model 2 includes only the subsample of workers who worked at least one season between May and September. Model 3 includes only the subsample of workers who worked at least one season between November and March. Model 4 includes only the subsample of workers who worked at least one season between May and September and at least one season between November and March. Model 5 does not weight observations by the length of job spells. Model 6 is an unweighted random effects specification comparable to Model 6. All models include match effects, year effects, experience, experience squared, non-fatal injury rate, injury medical costs, zero fatality rate indicator, mean wave height, variance of wave height, and mean air temperature. Standard errors, in brackets, are clustered at the match level. *** Significant at the .01 level, ** significant at the 0.05 level, * significant at the 0.10 level.

Although the estimates from Table 10 suggest that results hold even when limiting identification to variation within quarters and within worker-by-vessel-by species, I also test whether estimates differ across subsamples of workers who worked during the winter, those who worked during the summer, and those who worked in both the summer and the winter. Table 12 presents these robustness estimates. Row 1 is the baseline match effects model. Row 2 estimates the same model for the subset of workers who worked during the summer, and the estimates are very similar to the overall estimates. Row 3 shows estimates for the subsample who worked during the winter, and row 4 contains workers who worked in all seasons of the year. In each case there is no statistically significant difference in any of the coefficients from the baseline model, suggesting that there are not significant compositional changes in the types of workers employed across seasons. This result makes sense intuitively given that workers are being drawn from a very large geographic area, including most of the pacific northwest, so there is not a relative scarcity of people who are willing to accept risk.

Table 12 also presents several additional sensitivity analyses, including a specification that removes the effect of weighting observations by the length of job spells, and estimates from a random effects model. The estimates are qualitatively similar to the baseline estimates, although each of the three specifications implies a more steeply declining MVSL function than the baseline estimates. The random effects estimates are very similar to the fixed effects estimates, and satisfy a Wu-Hausman test. Despite this, I use the more conservative fixed effects model throughout the analyses.

4.5.4 Survey-Based Measurement Error

Although there may be concerns about using recall-based survey data, there is a unique reason to be optimistic about the recall data from this survey. Deckhands were very involved in the design, implementation, and review of the major policy change through which the fisheries were transitioned from open-access to rights-based in 2005. Workers were very aware of the effects of the policy on the length of fishing seasons and on their earnings, and appeared easily able to make comparisons between the years immediately before the policy change, 2003-2004, and those immediately after.

In addition, Schnier, Horrace, and Felthoven (2010) also study the tradeoff between earnings and risk in the Bering Sea crab fisheries, using administrative data. They analyze the decisions of vessel captains to leave port as a function of contemporaneous weather conditions to infer the VSL, which they estimate to be \$4.0 to \$4.8 million. Their estimates reflect a blend of the captain's own VSL and the captain's altruistic value of deckhands' lives. Whereas the aim of my paper is to use this unique empirical setting to guide improvements to the estimation of hedonic wage models generally, Schnier, Horrace, and Felthoven (2010) circumvent the complications of labor-market estimation and use weather variation to estimate a behavioral-based VSL. However, since they use different data from administrative sources, their estimates provide a useful benchmark for corroborating my survey data. When I replicate a model similar to the specification that they use, the estimated MVSL using the survey data is about \$4.13 million to \$5.04 million at comparable average risk levels, suggesting that my survey data are consistent with administrative records.

5 Conclusions

Using panel survey data from the unique empirical setting of commercial fishing deckhands in the Alaskan Bering Sea, I document several new facts related to how workers make decisions in the presence of changing occupational safety hazards. The first set of findings relate to assessing and improving methodologies that are frequently used to estimate the value of statistical life, which factors into cost-benefit analyses of a wide array of pubic health and safety policies. The results suggest that attempts to reduce omitted variable bias using within-worker variation caused by job switches can lead to overcorrection, resulting in estimates that are below those from more robust models. By decomposing the sources of potential bias, I find that the reason for this overcorrection is due to the fact that latent worker heterogeneity is positively correlated with fatality rates, while latent firm heterogeneity is negatively correlated with fatality rates. Despite this, the worker-effects model offers a substantial estimation improvement that may be valuable in situations where matched employee-employer data are not available. In the current setting, I estimate that the worker effects model eliminates about 84% of the bias in VSL estimates from the cross-sectional model relative to the benchmark match effects model.

The second set of findings take advantage of the large variations in risks of injury or death over time for the same worker to assess how marginal aversion to physical hazards changes as risk levels change. I document new information suggesting that the benefits of safety improvements across multiple competing risks are complementary. This result has substantial implications for understanding how the concentration of fatal risk affects the efficiency of regulatory policies differently than aggregate levels of risk, a question that was introduced by Pratt and Zeckhauser (1996). The theoretical ambiguity relates to two potential opposing effects, the 'dead-anyway' effect and the 'high-payment' effect. The former suggests that marginal aversion to fatal risk should increase as the level or risk rises, while the latter suggests that as the level of risk increases the total expenditure on risk reduction increases, increasing the marginal utility of wealth and causing an ambiguous relationship between risk levels and willingness to pay for risk reductions. Whereas Evans and Smith (2008) found that the dead-anyway effect dominates when studying changes in the VSL following health shocks in HRS data, the estimates from this setting suggest the opposite—that the 'high-payment' effect dominates the 'dead-anyway' effect in this empirical setting. A dominant high-payment effect is also consistent with the illustrative example from Kahneman and Tversky (1979, which they attribute to Zeckhauser) that most people, if forced to play Russian roulette, would pay more money to reduce the probability of death from 1/6 to zero than they would pay to reduce it from 4/6 to 3/6.

Knowing whether preferences have this shape has considerable implications for public policy, since it would suggest that the marginal benefit of reducing small baseline risks is higher per unit of risk than the marginal benefit of a policy that focuses on safety improvements in high-risk settings. In particular, the implication is that public safety policies are complementary with each other, which implies that efficient policy design choices should be made at the portfolio level to account for spillover benefits of safety improvements across public policies. One implication of this pattern of preferences for healthcare policy, relative to the implicit assumption in the literature that marginal preferences are independent of risk levels, is an increase in the value of preventative care relative to ex post treatment of life-threatening conditions, for example.

Of course, there are many caveats to keep in mind regarding these results. The substantial improvements to identification in the chosen empirical setting come at the cost of representing only a very narrow subset of labor markets generally. For that reason, the quantitative estimates of compensating differentials and their implied MVSLs are not intended to be used for evaluating public safety policies. However, the results provide new insights on the nature of endogeneity biases present under various methods used to estimate the value of statistical life, and suggest new patterns about the marginal aversion to physical risk. My hope is that these findings can help clarify the importance of several previously unidentified sources of estimation bias, so that future research may address these challenges in more representative settings.

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1 Online Appendix

1.1 Additional Supporting Tables

Table A1: Summary Statistics: Work Experience

		S.E.
Years F.T. Work Experience		
Mean	19.39	[10.10]
10th Percentile	7	
50th Percentile	20	
90th Percentile	34	
Years Fishing Experience		
Mean	14.47	[9.37]
10th Percentile	3	
50th Percentile	14	
90th Percentile	27	

Table A2: Summary Statistics: Survey Demographics

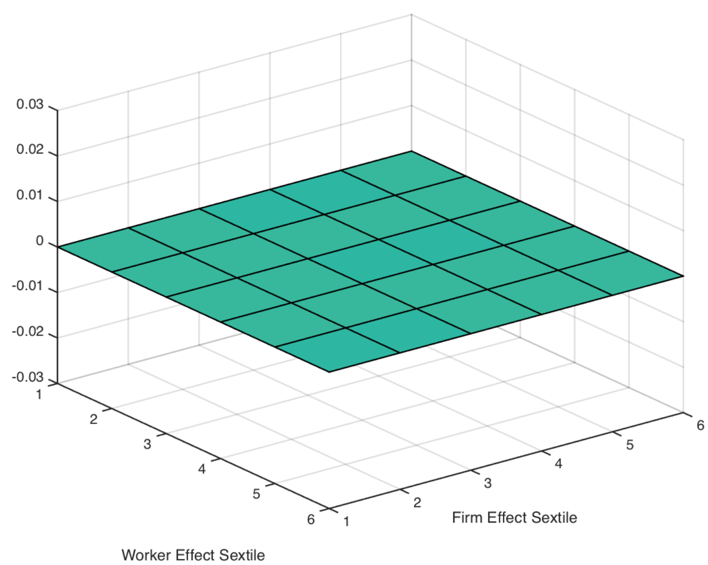
Age	36.99
Race	
White	85.71%
Black	1.50%
Non-White Hispanic	4.51%
Asian	3.76%
Other	4.51%
Education	
Less than High School Diploma	9.02%
High School Diploma/Equivalent	47.37%
Some College	39.10%
College Degree or More	12.78%
Ever Married	47.37%
At Least One Child	47.37%
Has Health Insurance	46.21%
Has Life Insurance	34.85%
State of Residency	
AK	15.91%
CA	5.30%
OR	7.58%
WA	50.76%
Other US	19.69%
N Respondents	133
N Worker-Vessel Pairs	183
N Job-Spells	1195

Table A3: Covariance Matrix of Components of the Log Wage Rate

			Covariances					
	Mean	Std. Dev.	Log Wage	$X\beta$	θ	Ψ	ϵ	$A\gamma$
Log Wage	3.43	0.67	0.450					
Time-Varying Characteristics	−0.09	0.11	−0.016	0.013				
Worker Effect	−0.31	0.69	0.230	−0.016	0.476			
Firm Effect	−0.35	0.59	0.086	−0.008	−0.244	0.353		
Residual	0.00	0.31	0.094	0.000	−0.000	0.000	0.094	
Non-Wage Job Amenities	0.71	0.25	0.056	−0.004	0.013	−0.015	0.000	0.062

Notes: Covariance terms are among components from a decomposition of log wages into observable time-varying characteristics ($X\beta$), unobservable worker heterogeneity (θ), unobservable firm heterogeneity (Ψ), and non-wage job amenities (including fatality rate variables and weather variables) using a two-way fixed effects model. The column headers use symbols from Equation 2 while row headers provide short descriptions. Covariances, means, and standard deviations are all weighted by the length of job spells, as in the regression models.

Figure A1: Average Two-Way Fixed Effects Residual by Sextile of Worker and Firm Effect



Notes: Plot depicts average residuals from an AKM two-way fixed effects model by sextile of the worker and firm effects effects distributions.

1.2 The Relationship between Weather and Fatality Rates

I estimate a count model of the number of fatalities per 1,000 FTE worker-years conditional on weather variables. The empirical distribution of fatality rates is characterized by overdispersion and excess zeros, so a zero-inflated negative binomial model provides a better fit than alternative count models tested. The negative binomial model has density:

$$h[y|\mu,\alpha] = \frac{\Gamma(\alpha^{-1}+y)}{\Gamma(\alpha^{-1})\Gamma(y+1)}\left(\frac{\alpha^{-1}}{\alpha^{-1}+\mu}\right)^{\alpha^{-1}}\left(\frac{\mu}{\mu+\alpha^{-1}}\right)^{y}$$

where $E[y|\mu,\alpha] = \mu_i = \exp(\mathbf{x_t'}\beta)$ and $V[y|\mu,\alpha] = \mu(1+\alpha\mu)$.

The weather variables $\mathbf{x_t}$ include average significant wave height,[26] average water temperature, and the monthly variance of hourly wave height measurements.[27] The variance term is included to explain the frequency of extreme weather events within each month, during which fatal accidents may be more likely.

The zero-inflation mixture model has density:

$$g(y) = \begin{cases} f(0) + (1-f(0))h(0) & \text{if } y = 0 \\ (1-f(0))h(y) & \text{if } y \geq 1. \end{cases} \tag{4}$$

where $f(\cdot)$ is a logit model with the same explanatory weather variables $\mathbf{x_t}$ used in $h(\cdot)$.

Table A5 presents estimates of the effects of weather on the fatality rate. The first model includes only the key weather variables. As expected, larger waves and more variation in wave heights have significant positive effects on the fatality rate, while lower water temperature has a slightly positive, though insignificant, effect. The second model, which is used as the main model throughout the remaining analyses, includes a quadratic spline function of each of the three weather conditions, with knots at the 25th, 50th, and 75th percentiles of the respective distributions. This model provides a significant improvement in fit relative to the first model (the p-value of the LR tests is less than 10^{-3}). The full mixture model has a pseudo-R^2 of 0.35, suggesting that weather patterns provide useful information for predicting fatality rates. The predicted fatality rates from this model are shown in Appendix Figures A3 and A4, along with wave heights and temperatures.

[26] Average significant wave height is defined as the average wave height, from trough to crest, of the one-third largest waves.

[27] Other weather variables were tested, but had negligible additional explanatory power and are very highly correlated with the included weather variables.

Table A5: First-Stage Estimate of Expected Fatality Rate
Zero-Adjusted Negative Binomial Models

	(1)		(2)	
Dependent Variable: Fatalities per 1,000 FTE Worker-Years				
Negative Binomial Parameters				
$Avg. Wave\, Height$	0.480	[0.188]	-2.084	[3.620]
$Avg. Wave\, Height^2$			0.836	[1.146]
$I(> P25) * (Avg. Wave\, Height - P25)^2$			-1.440	[1.831]
$I(> P50) * (Avg. Wave\, Height - P50)^2$			1.039	[1.723]
$I(> P75) * (Avg. Wave\, Height - P75)^2$			-2.918	[2.371]
$Var. Wave\, Height$	0.431	[0.167]	2.237	[1.700]
$Var. Wave\, Height^2$			-1.752	[1.633]
$I(> P25) * (Var. Wave\, Height - P25)^2$			3.018	[2.381]
$I(> P50) * (Var. Wave\, Height - P50)^2$			-1.782	[1.315]
$I(> P75) * (Var. Wave\, Height - P75)^2$			0.173	[0.661]
$Avg. Water\, Temp.$	-0.055	[0.037]	-6.053	[2.275]
$Avg. Water\, Temp.^2$			1.502	[0.549]
$I(> P25) * (Avg. Water\, Temp. - P25)^2$			-2.028	[0.755]
$I(> P50) * (Avg. Water\, Temp. - P50)^2$			0.553	[0.307]
$I(> P75) * (Avg. Water\, Temp. - P75)^2$			0.076	[0.150]
Constant	4.967		11.829	
Logit Parameters				
$Avg. Wave\, Height$	-0.073	[0.277]	12.077	[9.196]
$Avg. Wave\, Height^2$			-3.970	[2.845]
$I(> P25) * (Avg. Wave\, Height - P25)^2$			6.972	[4.268]
$I(> P50) * (Avg. Wave\, Height - P50)^2$			-7.273	[3.492]
$I(> P75) * (Avg. Wave\, Height - P75)^2$			8.321	[3.904]
$Var. Wave\, Height$	-0.065	[0.064]	6.612	[5.471]
$Var. Wave\, Height^2$			-4.0450	[4.716]
$I(> P25) * (Var. Wave\, Height - P25)^2$			3.096	[6.333]
$I(> P50) * (Var. Wave\, Height - P50)^2$			2.569	[3.145]
$I(> P75) * (Var. Wave\, Height - P75)^2$			-3.330	[1.931]
$Avg. Water\, Temp.$	0.139	[0.241]	-10.163	[5.228]
$Avg. Water\, Temp.^2$			2.563	[5.228]
$I(> P25) * (Avg. Water\, Temp. - P25)^2$			-3.662	[1.546]
$I(> P50) * (Avg. Water\, Temp. - P50)^2$			1.322	[0.505]
$I(> P75) * (Avg. Water\, Temp. - P75)^2$			-0.282	[0.282]
Constant			-0.996	
N Obs.	180		180	
Pseudo-R^2 Conditional Model	0.484		0.831	
Pseudo-R^2 Full Mixture Model	0.166		0.352	
Log Likelihood	-617.85		-584.91	

Notes: Wave height is the 'significant wave height', which is the average of the highest one-third of all of the wave, measured in meters. Averages and variances are calculated based on hourly measurements within a month. $P25$, $P50$, and $P75$ are the 25th, 50th, and 75th percentiles of the respective distributions of weather conditions.

1.2.1 Alternative Model Specification Using Weather-Based Partition

An alternative potential specification is to partition the variation in weather into two components, one that affects fatal risk and a residual component that is orthogonal to risk. Consider the basic model:

$$w_{ijt} = x_{it}\beta + \widehat{r}_t(W_t)\gamma_1 + W_t\gamma_2 + \Phi_{i,J(i,t)} + \epsilon_{ijt} \tag{1}$$

where the period t fatality rate, $r_t(W_t)$, is a function of weather conditions W_t, and W_t can also affect the reservation wage of a worker directly by creating uncomfortable work conditions. Although γ_1 and γ_2 are identified using OLS as long as they are not perfectly collinear, which they are not, an alternative specification is to force any variation in weather that is correlated with fatal risk to enter the model through γ_1, while identifying γ_2 using only residual variation in weather.

To do this, define $P_W = W(W'W)^{-1}W'$ as the matrix that projects onto the space spanned by the columns of W, and define $N_W = I - P_W$ as the matrix that projects onto its null space. Similarly, define $P_{\widehat{r}}$ and $N_{\widehat{r}}$. Since

$$\widehat{r}_t(W_t) + W_t = P_W\widehat{r}_t + N_W\widehat{r}_t + P_{\widehat{r}_t}W_t + N_{\widehat{r}_t}W_t$$

Model (1) can be expressed as:

$$w_{ijt} = x_{it}\beta + (P_W\widehat{r}_t + P_{\widehat{r}_t}W_t + N_W\widehat{r}_t)\bar{\gamma}_1 + N_{\widehat{r}_t}W_t\bar{\gamma}_2 + \theta_i + \Psi_{J(i,t)} + \Phi_{i,J(i,t)} + \epsilon_{ijt} \tag{2}$$

where $\bar{\gamma}_1$ is now the coefficient on total fatality risk. That is, rather than using the first-stage weather model to estimate the fatality rate for the second-stage, the actual ex post fatality rate is included in the second stage, and the first-stage model is used to estimate the partition of the variation in fatality rates into a component explained by weather conditions and a residual component. Model (2) can be relaxed further by allowing the coefficient on $P_W\widehat{r}_t$ to differ from that on $N_W\widehat{r}_t$. Model (3) is the linear version of this specification.

$$w_{ijt} = x_{it}\beta + (P_W\widehat{r}_t + P_{\widehat{r}_t}W_t)\breve{\gamma}_1 + N_W\widehat{r}_t\gamma_3 + N_{\widehat{r}_t}W_t\bar{\gamma}_2 + \theta_i + \Psi_{J(i,t)} + \Phi_{i,J(i,t)} + \epsilon_{ijt} \tag{3}$$

Estimates from this model are shown in Table A6, and estimates from a comparable cubic model are shown in Table A7. The implied MVSLs and patterns of estimation bias are similar in these specifications as in the main results, as shown in Figure A2.

Table A6: Linear Fixed Effects Models

	(1)	(2)	(3)	(4)
	Dependent Variable: $\ln(Wage)$			
Fatality Rate	0.059	0.063	0.064	0.060
S.E. (Clustered)	[0.011] ***	[0.009] ***	[0.012] ***	[0.011] ***
S.E. (with M-T Adj.)	[0.031] **	[0.029] **	[0.028] **	[0.029] **
Avg. Wave Height Residual	0.301	0.421	0.445	0.438
S.E. (Clustered)	[0.088] ***	[0.075] ***	[0.096] ***	[0.086] ***
S.E. (with M-T Adj.)	[0.190]	[0.178] **	[0.175] **	[0.178] **
Var. Wave Height Residual	0.053	-0.012	0.074	0.036
S.E. (Clustered)	[0.055]	[0.046]	[0.050]	[0.045]
S.E. (with M-T Adj.)	[0.205]	[0.196]	[0.180]	[0.189]
Avg. Water Temp. Residual	0.000	0.023	0.039	0.036
S.E. (Clustered)	[0.020]	[0.017]	[0.018] **	[0.018] **
S.E. (with M-T Adj.)	[0.036]	[0.033]	[0.031]	[0.032]
Fatality Rate Residual	0.001	0.004	-0.000	0.005
S.E. (Clustered)	[0.007]	[0.005]	[0.005]	[0.005]
S.E. (with M-T Adj.)	[0.011]	[0.009]	[0.009]	[0.009]
Worker Effects	N	Y	N	Y
Firm Effects	N	N	Y	Y
Match Effects	N	N	N	Y
N Obs.	1195	1195	1195	1195
N Clusters	128	128	183	183
R^2	0.275	0.719	0.660	0.781

Notes: *** Significant at the .01 level, ** significant at the .05 level. Column 1 includes experience, experience squared, education, race, and marital status. All models are weighted by the length of job spells. All models include year effects. 'Fatality Rate' measured as the number of fatalities per 1,000 FTE worker-years. Weather-related variables are the residuals from regressing the respective weather conditions on the polynomial in predicted fatality rates. 'Fatality Rate Residual' is the residual from regressing the observed actual fatality rate on the fatality rate predicted by weather conditions. First standard errors are clustered at the worker level in Columns 1 and 2 and at the worker-firm level in Columns 3 and 4. Second standard errors are clustered and adjusted for generated regressors using Murphy-Topel (1985).

Table A7: Nonlinear Fixed Effects Models

	(5)	(6)	(7)	(8)
	Dependent Variable: $\ln(Wage)$			
Fatality Rate	0.342	0.253	0.286	0.283
S.E. (Clustered)	[0.054] ***	[0.050] ***	[0.048] ***	[0.041] ***
S.E. (with M-T Adj.)	[0.117] ***	[0.062] ***	[0.069] ***	[0.065] ***
Fatality Rate Sq.	-0.042	-0.025	-0.031	-0.030
S.E. (Clustered)	[0.009] ***	[0.007] ***	[0.007] ***	[0.005] ***
S.E. (with M-T Adj.)	[0.026]	[0.011] **	[0.013] **	[0.012] **
Fatality Rate Cu.	0.002	0.001	0.001	0.001
S.E. (Clustered)	[0.000] ***	[0.000] ***	[0.000] ***	[0.000] ***
S.E. (with M-T Adj.)	[0.002]	[0.001]	[0.001]	[0.001]
Avg. Wave Height Residual	0.257	0.411	0.450	0.421
S.E. (Clustered)	[0.092] ***	[0.077] ***	[0.098] ***	[0.082] ***
S.E. (with M-T Adj.)	[0.104] **	[0.078] ***	[0.103] ***	[0.086] ***
Var. Wave Height Residual	-0.025	-0.039	0.042	0.003
S.E. (Clustered)	[0.059]	[0.049]	[0.050]	[0.050]
S.E. (with M-T Adj.)	[0.070]	[0.053]	[0.069]	[0.067]
Avg. Water Temp. Residual	0.001	0.023	0.038	0.037
S.E. (Clustered)	[0.021]	[0.017]	[0.019] **	[0.019] **
S.E. (with M-T Adj.)	[0.022]	[0.017]	[0.020]	[0.019]
Fatality Rate Residual	0.003	0.005	0.001	0.006
S.E. (Clustered)	[0.008]	[0.006]	[0.006]	[0.006]
S.E. (with M-T Adj.)	[0.008]	[0.006]	[0.006]	[0.006]
Worker Effects	N	Y	N	Y
Firm Effects	N	N	Y	Y
Match Effects	N	N	N	Y
N Obs.	1195	1195	1195	1195
N Clusters	128	128	183	183
R^2	0.279	0.720	0.660	0.782
p-value of F-test for Nonlinearity (M-T Adj.)	0.098	0.022	0.016	0.013

Notes: *** Significant at the .01 level, ** significant at the .05 level. Column 5 includes experience, experience squared, education, race, and marital status. All models are weighted by the length of job spells. All models include year effects. 'Fatality Rate' measured as the number of fatalities per 1,000 FTE worker-years. Weather-related variables are the residuals from regressing the respective weather conditions on the polynomial in predicted fatality rates. 'Fatality Rate Residual' is the residual from regressing the observed actual fatality rate on the fatality rate predicted by weather conditions. First standard errors are clustered at the worker level in Columns 5 and 6 and at the worker-firm level in Columns 7 and 8. Second standard errors are clustered and adjusted for generated regressors using Murphy-Topel (1985).

Figure A2: Marginal VSL vs. Fatality Rate Implied by Estimates from Table A7

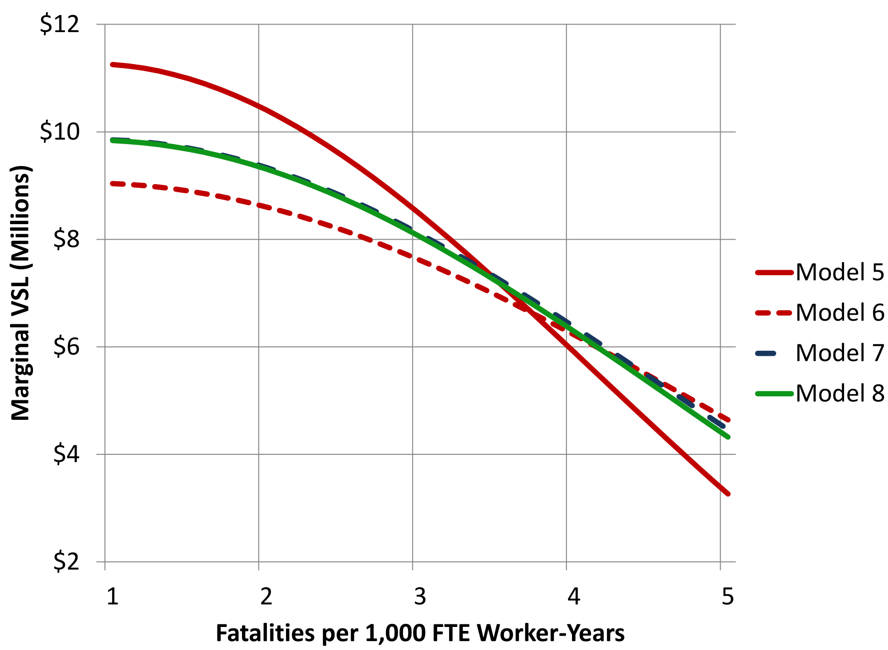
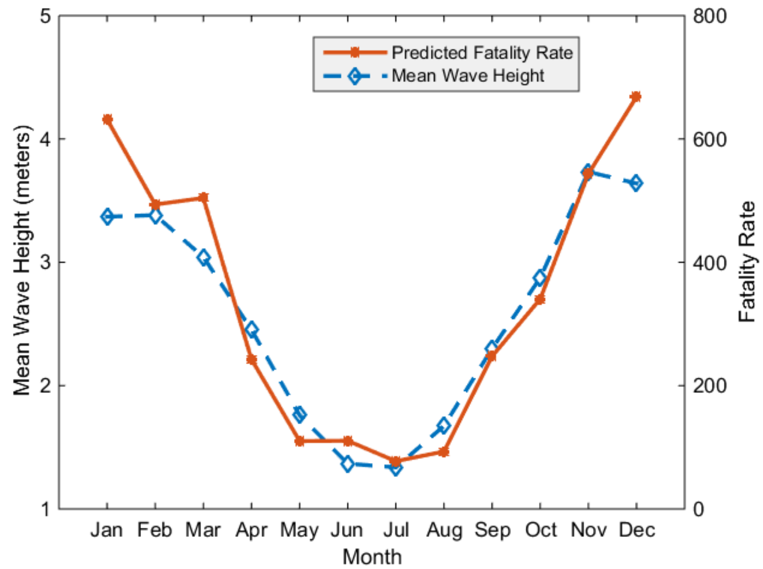
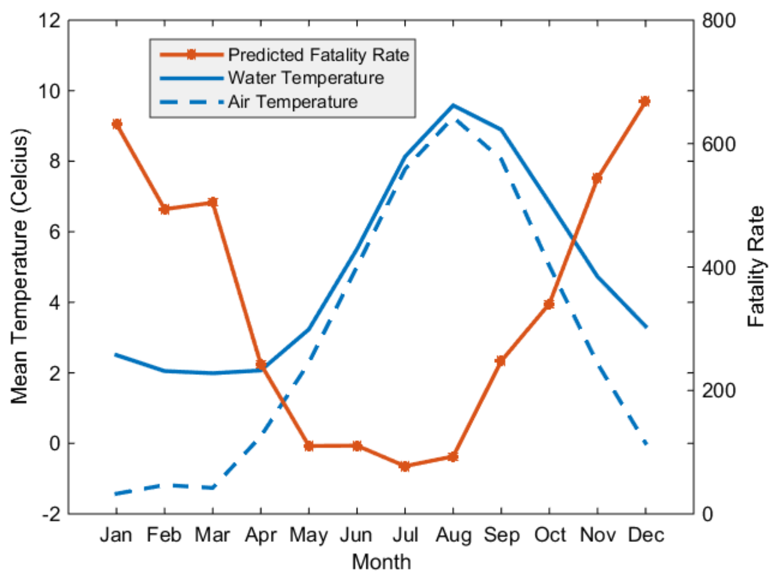


Figure A3: Fatality Rate vs. Wave Height



Notes: Calculations are based on from AOISS Fatality Data, NIOSH FTE Workers Data, and NOAA Weather Data. Fatality rates are measured in deaths per 100,000 full-time equivalent worker-years.

Figure A4: Fatality Rate vs. Water and Air Temperature



Notes: Calculations are based on from AOISS Fatality Data, NIOSH FTE Workers Data, and NOAA Weather Data. Fatality rates are measured in deaths per 100,000 full-time equivalent worker-years.

1.3 Alternative Standard Error Estimates

The following table presents alternative estimates of the standard errors for the main reduced-form models. The first two alternative estimates cluster standard errors at the worker and firm levels, respectively. There are 128 worker clusters and 123 firm clusters. The final three estimates cluster by time, the first by month-year pairs, the second by month, and third by year. Since job spells frequently last several months, each 'month' cluster is a unique combination of months observed in the data, of which there are 62. There are 283 month-year clusters.

Table A7: Alternative Standard Error Estimates

	(1)	(2)	(3)	(4)
	Dependent Variable: ln($Wage$)			
Fatality Rate	0.3131	0.1336	0.2161	0.1532
S.E. (Clustered: Worker)	[0.0694]***	[0.0533]**	[0.0519]***	[0.0490]***
S.E. (Clustered: Firm)	[0.0683]***	[0.0492]***	[0.0526]***	[0.0496]***
S.E. (Clustered: Month-Year)	[0.1022]***	[0.0680]**	[0.0831]***	[0.0594]***
S.E. (Clustered: Month)	[0.0989]***	[0.0585]**	[0.0778]***	[0.0561]***
Fatality Rate Sq.	-0.0620	-0.0228	-0.0411	-0.0263
S.E. (Clustered: Worker)	[0.0143]***	[0.0118]*	[0.0111]***	[0.0106]**
S.E. (Clustered: Firm)	[0.0141]***	[0.0109]**	[0.0114]***	[0.0108]**
S.E. (Clustered: Month-Year)	[0.0224]***	[0.0154]	[0.0188]**	[0.0138]*
S.E. (Clustered: Month)	[0.0235]**	[0.0147]	[0.0196]**	[0.0141]*
Fatality Rate Cu.	0.0047	0.0017	0.0031	0.0019
S.E. (Clustered: Worker)	[0.0010]***	[0.0009]*	[0.0009]***	[0.0008]**
S.E. (Clustered: Firm)	[0.0010]***	[0.0008]**	[0.0009]***	[0.0008]**
S.E. (Clustered: Month-Year)	[0.0017]***	[0.0012]	[0.0014]**	[0.0011]*
S.E. (Clustered: Month)	[0.0018]**	[0.0012]	[0.0016]*	[0.0011]
Fatality Rate 4th	-0.0001	-0.0000	-0.0001	-0.0000
S.E. (Clustered: Worker)	[0.0000]***	[0.0000]*	[0.0000]***	[0.0000]**
S.E. (Clustered: Firm)	[0.0000]***	[0.0000]*	[0.0000]***	[0.0000]**
S.E. (Clustered: Month-Year)	[0.0000]***	[0.0000]	[0.0000]**	[0.0000]*
S.E. (Clustered: Month)	[0.0000]**	[0.0000]	[0.0000]*	[0.0000]
F-Statistic p-values of Fatality Rate Terms				
S.E. Clustered: Worker	0.0000	0.0014	0.0000	0.0005
S.E. Clustered: Firm	0.0000	0.0006	0.0001	0.0005
S.E. Clustered: Month-Year	0.0000	0.0035	0.0000	0.0003
S.E. Clustered: Month	0.0002	0.0008	0.0000	0.0003
Worker Effects	N	Y	N	Y
Firm Effects	N	N	Y	Y
Worker-Firm Effects	N	N	N	Y
N Obs.	1195	1195	1195	1195
R^2	0.318	0.733	0.690	0.791

Fatality rate parameters are marginal effects of the fatality rate conditional on observing at least one fatality, and are measured as the number of fatalities per 1,000 FTE worker-years. All models also include the mean wave height, variance of wave height, mean air temperature, non-fatal injury rate, injury medical costs, year effects, experience, and experience squared. Pooled estimates in column 1 also include education, race, and marital status. All models are weighted by the length of job spells. *** Significant at the .01 level, ** significant at the 0.05 level, * significant at the 0.10 level.

1.4 Survey Response Rates

As described in the paper, the main source of labor-market data come from a survey of deckhands working in Bering Sea fisheries, which was conducted in several rounds, including mailing components and a field survey conducted in Dutch Harbor. The survey data include 133 respondents who worked a total of N=1,351 fishing job spells. Of these respondents, 80 were in the direct survey group completed in October 2009, at the beginning of the Red King Crab season, and the response rate for this survey was 62.2%. The remaining 53 respondents were from two separate rounds of mailing interviews, which had average response rates of 4.2% and 16%.

The low response rates from the mailing portion of the survey raise two important questions. First, to what extent might the low response rates affect estimates? Second, does it matter if the estimates are affected by response rates?

With respect to the first question, several referees have suggesting include a control variable for the round in which the survey was completed. Since the main specifications are fixed effects models, in both the within worker and within match models the survey round indicator is perfectly collinear with the person effect or match effect, so this is not possible. In this sense, the person effects and match effects control for any variation in sample composition between the different rounds of the survey. If we were interested in estimating the values of the person and match effects, this could potentially be concerning. However, since person and match effects are essentially nuisance parameters in the model, the fact that they also control for potential differences in survey rounds is not problematic.

In the cross sectional model it is of course possible to assess whether the low response rate introduces bias into the models. Appendix Table A8 presents estimates of the cross-sectional model with controls for survey format. The first comparison includes a dummy variable for in-person surveys and an interaction between the in-person survey indicator and the fatality rate. The results suggest that there is no statistically significant difference in log wages between the respondents to the mail-based and in-person surveys, and there is no significant difference in the estimated compensating wage differential. With such a limited sample there is not enough power to compare estimates between two different quartic functions, so the full set of interaction terms in the nonlinear model is not informative. Columns 3 and 4 of the table show, however, that including a control variable for the survey format has no meaningful or significant effect on the estimated compensating wage differentials, and there is no significant difference in average log wages between the two groups of respondents.

A second related question is: does the response rate matter? In most settings response rates can be very important for assessing the representativeness of a sample. In the current study, the sample is already highly selected from one specific geographic location and industry. That is, the entire population of interest is already not representative for any policy-relevant questions that are discussed in the paper. Even a representative sample of a non-representative population would still not be representative. The more important question seems to me to be: "what can we learn from this specific case study that can help improve our understanding of a classic problem in labor economics, the estimation of compensating wage differentials?", rather than "are the estimates produced representative of parameters for one specific fishery in Alaska?"

Table A8: Effects of Survey Format on Estimates

	(1)	(2)	(3)	(4)
	Dependent Variable: ln($Wage$)			
Fatality Rate	0.036***	0.039**	0.313***	0.314***
	[0.011]	[0.020]	[0.069]	[0.069]
In-Person Survey		0.021		0.014
		[0.108]		[0.097]
In-Person Survey*Fatality Rate		–0.005		
		[0.016]		
Fatality Rate Sq.			–0.062***	–0.062***
			[0.014]	[0.014]
Fatality Rate Cu.			0.005***	0.005***
			[0.001]	[0.001]
Fatality Rate 4th			–0.000***	–0.000*
			[0.000]	[0.000]
N	1,195	1,195	1,195	1,195
N Clusters	128	128	128	128
R-Sq	0.307	0.307	0.318	0.318

Notes: Models are identical to column 1 of Table 3, except that the second column includes an indicator for respondents who were surveyed in-person. *** Significant at the .01 level, ** significant at the 0.05 level, * significant at the 0.10 level.