Essays in Health and Labor Economics

by

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Abstract

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This dissertation examines how occupational injuries vary with the business cycle, the relationship between healthcare staffing levels and patient outcomes, and whether workers are compensated for changes in occupational risk.

In the first chapter I examine how fatal and non-fatal occupational injury rates vary over the business cycle. Past research on the relationship between workplace safety and the business cycle has found only non-fatal accidents to be pro-cyclical. The failure of previous research to convincingly identify a pro-cyclical fatality relationship has led researchers to focus on a claims reporting moral hazard explanation of the pro-cyclicality of non-fatal injuries. Using state and firm level workplace safety data and local area unemployment rates, I find that workplace safety is pro-cyclical in *both* fatal and non-fatal injury rates, contrary to previous research. The occupational fatality rate elasticity (-0.15) is larger in magnitude than its non-fatality counterpart (-0.10).

The next chapter explores how a change in nurse staffing levels for intensive care patients improved patient outcomes in Arizona. Using data from the Healthcare Cost and Utilization Project's (HCUP) State Inpatient Databases (SID) for Arizona, I evaluate the impact of Arizona's October 1, 2002 mandate that no more than three intensive care patients be assigned to one nurse on nurse-sensitive patient health outcomes: mortality, length of stay, pressure ulcers, hospital-acquired pneumonia, urinary tract infections, and sepsis. I contribute to the literature regarding nurse staffing levels' impact on patient outcomes in the following ways: 1) the exploitation of the exogenous variation imposed by Arizona's regulation provides credible causal estimates of the impact of increased (marginal) nurse staffing levels on patient outcomes—mitigating the potential impact of omitted variables bias inherent in cross-sectional analysis, 2) it provides a sample size large enough to observe changes in low probability events such as hospital mortality, and 3) this is the first analysis of Arizona's intensive care services regulation. A difference-in-differences empirical analysis between

intensive and non-intensive care patients finds no evidence that intensive care patient outcomes improved after the regulation was imposed.

In the final chapter I examine the compensating wage differential for occupational risk in the mining industry. I create a balanced panel of county-year mining labor market observations (including occupational injury rates) from Quarterly Census of Employment and Wages (QCEW) and Mine Safety and Health Administration (MSHA) administrative data. The MSHA's mandate to inspect mining operations at least twice a year provides an objective measure of occupational risk through citations and their associated monetary penalties. I estimate the reduced form impact of changing occupational risk on hourly real wages. Employing fixed effect models to mitigate the impact of omitted variables bias, I find that increases in once-lagged citations increase current wages, providing evidence of compensating wages in the mining industry for increases in occupational risk. I estimate a $106,528 compensating wage differential for a non-fatal occupational injury using instrumental variable analysis.

To Diana, Soren and Daniel

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Preface

When I began writing this dissertation, my intent was to study the Economics of Health with a particular focus on the role that institutional structures play. That narrow focus broadened as I encountered past research on the relationship between the business cycle and health (particularly the work by Ruhm). This led me to study how occupational injuries (both fatal and non-fatal) vary over the business cycle. With the encouragement of my committee I expanded my analysis of occupational injuries to examine compensating wage differentials. The unifying theme of the three chapters of this dissertation is its focus on economic and structural determinants of health.

Chapter 1

Unemployment and Workplace Safety: New Evidence on the Pro-cyclicality of U.S. Workplace Injuries

1.1 Introduction

The literature on the business cycle pattern of workplace injuries dates back to the Great Depression (Kossoris, 1938). The rate of workplace accidents rises and falls with output. While subsequent research has confirmed the initial findings, determining causality between workplace accidents and economic booms in the presence of confounding economic incentives remains elusive. The pro-cyclical pattern of workplace injuries appears to have occurred again with the Great Recession of 2008-2009. Work-related fatalities dropped from 5,214 in 2008 to 4,340 in 2009 and the number of injuries requiring time away from work dropped by 9 percent over the same period (BLS 2010a, 2010b). A number of factors may account for the observed relationship: 1) claims reporting moral hazard, 2) a workforce composition effect, 3) behavioral changes by workers (or firms) that lead to increased workplace safety—risk bearing moral hazard, 4) production effort, or 5) a higher level of safety technology per worker in the production process.

Determining whether there is a pro-cyclical fatal workplace injury rate is important in understanding factors driving the pro-cyclical relationship of the non-fatal workplace injury rate. While workplace risk could also drive non-fatal injuries, another potentially important determinant could be claims reporting moral hazard, defined as a change in the propensity to file a claim for a change in the (expected) benefit level holding risk level constant.[1] In other words, the unemployment rate might influence the degree to which workers file claims rather than actual workplace safety. If workplace illness and injury rates decline when unemployment is high but fatality rates remain unchanged, it is likely that the change in workplace safety is best characterized as a change in reporting (the first cause listed in the previous paragraph) rather than an actual change in workplace safety (which would be observed with the final four causes listed). The failure of previous research to convincingly identify a pro-cyclical fatality relationship has led researchers to focus on a claims reporting moral hazard explanation of the pro-cyclicality of non-fatal injuries.

I contribute to the literature on pro-cyclical workplace accidents by exploiting variation in unemployment and injury rates within states from 1992-2009 to examine the degree to which these different channels contribute to the observed business cycle pattern in workplace safety.

1.2 Possible Channels of Causation

Economic booms increase employee turnover as new labor force participants enter the labor market to meet the increased demand. The subsequent workplace environment and safety reflects workers new to their jobs (a composition change in the workforce). To the degree that workplace safety is a function of accumulated human capital (industry, occupation and/or firm specific), workers in new jobs will experience more accidents. As the workers acclimate to their environment and acquire the requisite skill they reduce their own propensity toward accidents and, collaterally, that of their co-workers as well. In economic downturns, employers may selectively discharge those workers with characteristics that are correlated with workplace risk (i.e., lacking job specific human capital).

Unemployment might increase the cost of a workers' compensation claim if employers—on the margin—lay off (past) claimants illegally. The poor labor market reduces the probability of immediate job acquisition thereby increasing the potential cost of a layoff if a workers' compensation claim is filed. On the other hand, workers who anticipate a potential layoff may be more inclined to file a workers' compensation claim preemptively when unemployment is high. This results in a theoretically ambiguous prediction about the unemployment rate's impact on workers' compensation claims with respect to claims reporting moral hazard over the business cycle.

[1] See Butler and Worrall (1991) for a discussion of claims reporting and risk bearing moral hazard. Bolduc et al. (2002) refer to them respectively as "ex-post" and "ex-ante" moral hazard.

An alternative scenario also producing cyclical variation in workplace injuries operate through risk bearing moral hazard (a change in workers' risk bearing activities), or a safety-inducing effect, rather than a claims reporting moral hazard effect. If employers lay off workers with injuries when unemployment rises, this increases the costs of unemployment to workers, inducing them to take greater work precaution in a labor market where finding replacement employment is difficult.

In addition to the incentives workers have to take more care during business downturns, Ruhm (2000) offers reasons why workplace risk increases in business upturns. He finds that mortality is pro-cyclical in the United States and postulates that one channel linking unemployment and mortality is "Health as an Input into Production" (621). The cumulative effect of working long hours during economic upswings means less time for exercise and health promoting activities, thus reducing workers' health. High peak production activity may impact workplace safety both on the extensive (working more hours) and intensive (working harder per hour) margins. Movement along either margin could plausibly impact the ability of the worker to maintain a safe environment for himself and his co-worker, adding to pro-cyclical occupational risk.

1.3 Previous Research

Kossoris (1938) reports that non-fatal industrial injuries decreased during the first part of the Great Depression; the hours-based rate of non-fatal injuries in 29 manufacturing industries tracked closely to the *level of employment* between 1929 and 1935. He proposes a number of explanations for this pro-cyclicality: a composition effect, an intensive margin effect, firms adopting more advanced technology in the production process, or a reduced claims reporting moral hazard. Kossoris provides supporting evidence for both the first explanation (workers in the petroleum industry during the period with less than one year of job tenure constituted 15.1 percent of employees but 33.0 of accidents) and the last (the percentage of accidents that were short, i.e. less than eight days in length, fell in percentage terms). Kossoris estimates an 8% decrease in accidents reported in 1932 for a 25% decrease in employment level. This estimate and the 1932 change in the employment level suggest a non-fatal accident elasticity relative to the employment level of 0.32.

Smith (1973) uses 22 years (1948-1969) of national manufacturing injury and unemployment rate data to calculate the coefficient of the change in the unemployment level on the change in the level of "disabling" hours-based workplace injuries (injuries per million hours worked). Smith finds that a one percentage point increase in the unemployment rate *lowers* the hours-based injury rate by 0.46 (by way of comparison the rate was 20 in 1943 and 14.8 in 1969).

Viscusi (1986) analyzes the impact of Occupational Safety and Health Administration (OSHA) inspections on workplace safety using industry and year fixed effects for 220 industries from 1973-1983. He also includes the percent change in the industry's *employment level* as a control. Looking at the log odds ratio of non-fatal workplace accidents (total and those with lost work days) he reports the coefficient for the percentage change of employment was 0.0071 for the total case rate and the rate of cases involving lost workdays (significant at the one percent level). Viscusi notes that accidents are pro-cyclical but that result was secondary to his analysis of the impact of OSHA inspections on safety rates. The non-fatal lost total case rate and workday accident elasticity relative to the employment level calculated from Viscusi's finding is 0.71.

Ruhm (2000) analyzes total mortality rates, age-specific mortality rates, and the mortality rates for ten categories of cause of death by state between 1972 and 1991. Ruhm finds that total mortality is negatively associated with the state unemployment rate and the existence of an age gradient, with the unemployment rate having a greater impact on the total mortality rate of those aged 20-44 (primary working age) than those over age 45. When Ruhm examines mortality rates by cause of death he finds that the unemployment rate is negatively related (and significant) to eight of the ten causes (suicide and cancer are the two exceptions). Ruhm does not directly identify work-related fatalities. However, Ruhm finds that "Vehicle accidents" and "Other accidents" (which both likely include a subset of accidents that are related to employment) are negatively related to the state unemployment rate. In Ruhm's specification controlling for time trends (the most similar to the estimation strategy I take), Ruhm finds a -0.22 and -0.09 elasticity to the state unemployment rate respectively. I later decompose the "Other accidents" utilized by Ruhm to identify a subset of work-related fatalities over a similar time period.

Schmid (2009) uses annual manufacturing industry workplace safety rates from 1926-2007 and an augmented set of data for all private industry from 1972-2007 waves of the Survey of Occupational Injury and Illnesses (SOII) to estimate a structural time series model of how job creation and destruction (generated from the Business Employment Dynamics (BED)) impact the non-fatal workplace accident rate (accident cases per 100 workers). For nine industries, from 1993-2007, newly created jobs (as opposed to workers moving between jobs) lead to an increased non-fatal accident rate which is consistent to the degree that new labor participants (or workers new to their jobs) are more likely to have a workplace accident. Schmid uses first differenced log transformations to estimate that a one percent increase in the rate of job creation leads to a 0.37 percentage increase in growth rate of non-fatal injuries and that a one percent increase in the rate of job destruction leads to 0.27 percentage. The difference in relative magnitudes is evidence consistent with workers new to jobs being an important contributor to the pro-cyclicality of non-fatal workplace accidents.

Kohstall and Süssmuth (2010) use German time-series data from 1886-2009 to calculate long and short term elasticities of fatal and non-fatal workplace accidents to GDP and include workplace accident prevention expenditures by the German Social Accident Insurance as a control. Kohstall and Süssmuth confirm a pro-cyclical elasticity relative to non-fatal workplace accidents. They find that fatal workplace accidents are also pro-cyclical, once asymmetry around deviations of long term GDP in the short-run elasticity is accounted for. In their preferred specification, the short-run elasticity of non-fatal (fatal) accidents in a boom is 0.24 (0.10). The short-run elasticity of non-fatal (fatal) accidents in a recession is -0.78 (-1.89) respectively. All coefficients are significant at the 5% level. Kohstall and Süssmuth's finding of a pro-cyclical fatality rate with an elasticity that appears to be larger in absolute magnitude is consistent with the results that I describe below.

Svensson (2010) replicates Ruhm's (2000) analysis for 21 Swedish regions from 1976-2005, analyzing age and gender-specific mortality rates for separate causes. Similar to Ruhm, there is a catch-all category that includes occupational injuries "other accidents". Svensson finds that a one percent increase in the unemployment rate leads to a 13.9 percent decrease in "other accidents" (significant at the one percent level). Using the reported mean unemployment rate, it is possible to calculate an elasticity of "other accidents" to the unemployment rate of -0.61. Although Svensson's results are suggestive that there might be a pro-cyclicality to occupational injuries (consistent with Ruhm (2000)), but Svensson never enumerates what constitutes "other accidents".[2]

The paper most similar in motivation and empirical strategy to this research is Boone and van Ours (2006, BvO hereafter). They present a model where workers maximize their discounted present value utility with respect to their workplace safety effort given an exogenous probability of suffering an accident. In their model, workers who report accidents are more likely to be subsequently fired in the presence of increased unemployment and consequently engage in less claims reporting moral hazard and report fewer accidents. Using data from 16 OECD countries (*N*=314) BvO employ country fixed effects, country trend lines, and year fixed effects and find that the *worker*-based fatality rate does not vary with unemployment but that non-fatal accidents per worker do. The elasticity of non-fatal (fatal) work-place accidents to unemployment is -0.32 (-0.03) and is (not) significant at the one percent level. They also find no

[2] The accidents studied (the complement to help identify what "other accidents" might entail) are heart disease, suicides, homicides, traffic accidents, influenza, cancer, alcohol, and falling accidents (621). After describing all other categories of fatalities Svensson writes "finally 'other accidents', including many types of job-related fatalities" (618).

significant impact of either the log transformation of hours worked or being located in a highly unionized country on either measure of workplace safety. However, the unemployment rate elasticity is smaller in absolute magnitude for high unemployment benefit countries rather than low benefit countries, consistent with a claims reporting moral hazard explanation that workers incur greater costs with low unemployment benefits and are therefore more likely to change their reporting behavior.

The BvO study is limited by the inter- and intra-country differences in reporting and their sample size. The accident data varies by source, whether accidents that occurred while traveling to work were included, whether a claim was filed or the worker received compensation, the minimum number of work days lost (1 to 4) necessary to constitute an accident, the years of analysis, and changes in reporting within countries across time. Data from 12 of the 16 OECD countries were generated from insurance claims which (by definition) are more likely to consist of claims reporting moral hazard cases than a workplace-based measure of the accident rate.

1.4 Analytical Framework

The influence of the unemployment rate on workplace effort has been modeled as a "discipline" mechanism by economists including Marx (1867)[3] and Shapiro and Stiglitz (1984), and Kimball (1994). In these models, the increased cost of job displacement imposed by a tight labor market drives workers to avoid shirking and increases their workplace productivity. Models of workplace safety and its relation to worker or firm effort include Viscusi (1979), Krueger (1990), and Butler and Gardner (2010). The number of papers that incorporate *both* unemployment and workplace effort as determinants of workplace safety is more limited. Recent papers include BvO and Sasaki (2010).[4]

The following is a modified version of Krueger's (1990) model with two periods that illustrates how risk bearing moral hazard might be changed by the unemployment rate. At the beginning of Period 1, a representative worker is able to devote effort, e, to lowering the probability, $P(e)$, of a workplace accident in Period 1 (I assume that accidents do not affect Period 2 worker productivity and that accidents cannot occur in Period 2) where $P' < 0$ and $P'' > 0$. Due to the

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[3] From Marx's Capital "The industrial reserve army, during the periods of stagnation and average prosperity, weighs down the active labour-army; during the periods of over-production and paroxysm, it *holds its pretensions in check*. Relative surplus population is therefore the pivot upon which the law of demand and supply of labour works. It confines the field of action of this law within the limits absolutely convenient to the activity of exploitation and to the domination of capital" (emphasis added, 598).

[4] Sasaki (2010) models workplace safety effort from the firm's perspective.

random nature of workplace safety environments, $0 < P(e) < 1$. Workplace effort is normalized between zero and one, representing the fraction of discretionary workplace time devoted during the time at work (annually) to safety. Workers discount at the real interest rate, r, and their period wage is equal to the real value of their marginal revenue product, W. The non-injured workers two period income stream in present value terms is equal to:

$$W_h = W + \frac{W}{(1+r)} \tag{1.1}$$

Injured workers receive a fraction of their wage ($\alpha < 1$ is the replacement rate) in Period 1 and are laid off by the firm at the end of Period 1, to reenter the labor market. This is similar to the BvO model where workers who report accidents are more likely to be fired than those who do not. Reporting a workplace accident might signal a worker is either engaged in claims reporting moral hazard or is intrinsically more accident prone and therefore more likely to generate future cost to the firm. The worker knows the unemployment rate, μ, which is common to both periods. The worker with an injury in Period 1 is able to find a job and receive a Period 2 wage with 1- μ probability. Therefore injured workers two period income stream in expected present value terms is:

$$W_i = \propto W + (1 - \mu)\frac{W}{(1+r)} \tag{1.2}$$

The state-dependent utility functions for the worker are concave and twice-differentiable $U(\cdot)' > 0, U(\cdot)'' < 0, V(\cdot)' > 0,$ and $V(\cdot)'' < 0$, where U represents the utility of being in the working state, and V the injured worker's state. Workplace safety effort generates negative linear utility at marginal rate δ that the workers experience regardless of their final state. It is assumed that $U(W_h) > V(W_i)$ and that the utility of unemployment is zero (i.e., the utility generated by W_h and W_i is normalized relative to the utility provided by unemployment benefits).

The timing of the model is as follows: 1) currently employed workers must choose their workplace safety effort level prior to production, 2) workers are exposed to the possibility of an injury or accident during production at the beginning of Period 1 and receive W_h over the two work periods if healthy and W_i, on average, over the two periods if injured in Period 1, 3) at the end of Period 1, injured workers are laid off by the firm (healthy workers are not) and must reenter the labor market, and 4) no workplace safety effort is allocated in Period 2. Individual workers maximize the expected utility of the value of their state-dependent income with respect to their workplace safety effort level, e:

$$G(e) = \Big(\big(1 - P(e)\big)U(W_h) + P(e)V(W_i)\Big) - \delta e \tag{1.3}$$

It is possible to show by the Implicit Function Theorem (suppressing e in the notation) that:

$$\frac{de}{d\mu} = \frac{\left(\frac{W}{1+r}\right) P'V'}{P''(V(W_i) - U(W_h))} > 0 \tag{1.4}$$

and

$$\frac{de}{d\alpha} = \frac{-WP'V'}{P''(V(W_i) - U(W_h))} < 0 \tag{1.5}$$

Hence, in this steady state stylized model of risk bearing moral hazard, as μ increases (unemployment rises), workplace safety effort e increases and the likelihood of fatal and nonfatal injuries, $P(e)$, falls.

1.5 Data and Descriptive Statistics

The Local Area Unemployment Statistics (LAUS) data from the Bureau of Labor and Statistics (BLS) reports unemployment rates at the state, MSA and county levels.[5] The state unemployment rates are calculated from the monthly Current Population Survey (CPS). In order to calculate the unemployment rate, the BLS estimates the number of employed workers (“Workers” in Table 1.1). This number is the denominator for the employment-based fatality rate. Unemployment rates are available at the state (county) level from 1976 (1990) to the present. Figure 1.1 shows the distribution of the state unemployment rates from 1992-2009; the mean state unemployment rate is 5.2 percent from 1992-2009 with a right-skewed distribution. The highest periods of unemployment occur in 1992, 2003 and 2009.

Monthly CPS micro data from 1992-2009 were employed to create a demographic profile of the “average” worker in that state-year using CPS person weights,[6] including average hours worked within state and year. I evaluate the

[5] For a description of the CPS LAUS unemployment rates see the BLS Handbook of Methods Chapter 4 (see BLS (2011)).

[6] The profile of the “average” worker is similar to that found in the U.S. Department of Labor, 2001 Report on the American Workforce which roughly represents the midpoint of the period analyzed. Educational attainment distribution is comparable (p. 140-141), as is the midpoint bin

model with respect to *both* employment- and hours- based fatality measures. The hours based state-year fatality measure uses the CPS micro data hours estimate. In Table 1.1, the term "CPS Observations" refers to the person-month observations per state-year used to calculate the additional hours worked and demographic characteristics. The mean number of CPS state-year observations is 14,465. The lowest CPS Observation is 5,786. In advocating for the use of hours-based fatality rate, Ruser (1998) states that "the CPS provides highly credible hours worked estimates that can be used in place of employment in the denominator of fatality rates" (156). Table 1.1 provides descriptive characteristics in Column 1 and a comparison between subsets of state-years that correspond to the workplace safety data described below (the final column labeled "OLS" is described later).

The Census of Fatal Occupational Injuries (CFOI) has identified decedents who were engaged in legal employment activity at the time of their death since 1992 in the 50 states and the District of Columbia. It is intended to be a complete enumeration of all work-related fatalities (CFOI counts fatalities occurring at establishments as well as in transit. The fatality count is generated by states consulting multiple sources including death certificates and workers' compensation claims. The CFOI work related fatality identification standard is that two or more documents must identify the fatality as being employment related. Fatalities with only one employment-related identifying document are reviewed and given a final determination jointly by the BLS and the participating organization in the state of occurrence.[7]

Based in part on Ruser's (1998) recommendation to use hours-based fatality rates, the CFOI began reporting hours-based fatality measures nationally in 2007 (see Northwood (2010)). The two measures (i.e., employment- and hours-based rates) have a correlation of 0.99 in 222 occupations examined by Ruser (1998), but the importance of an hours-based measure will likely be magnified when the unemployment rate is changing because the hours worked will also likely be negatively correlated with unemployment (verified in this analysis and reported later), creating a smaller window of exposure for workplace accidents or fatalities. The mean hours worked by state-year observations is 39.2 (Table 1.1).

The employment-based fatality rate is equal to $\frac{N}{W} \cdot 100{,}000$ where N is the number of fatalities annually and W is the number of employed workers. The hours-based fatality measure is $\frac{N}{W \cdot H} \cdot 2{,}000{,}000$ where H is the mean hours worked generated from the CPS (from Northwood (2010)). The scaling term

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weighted white mean age (p. 32), 53.7 percent of the labor force was male (p. 37), the fraction white was 75.1 (p. 29), 35.6 hours per week (p. 122), 83.05 percent were full time (p. 132).

[7] See BLS Handbook Chapter 9.

(200,000,000) is equal to the time spent at work for 100,000 workers under the assumption that they work 40 hours per week and 50 weeks per year. Thus the hours-based fatality measure is the number of fatalities per 100,000 full-time equivalent workers working for one year. I maintain the BLS hours-based unit of analysis identification assumption that a representative worker works 50 weeks a year at 40 hours per week when calculating hours-based fatality rates.

The CPS does not ask employment questions for individuals under age 16 or in the military. The denominator of a worker or hours based fatality rate generated from the CPS will fail to capture such employment. From the publicly available CFOI data it is not possible to consistently identify fatalities suffered by military personnel and those under age 16 *by state*. Military and under age 16 fatalities represent 1.9 percent of CFOI fatalities nationally from 1992-2009. I am unable to separate these fatalities at the state-year level in the publicly available data but given the small number of deaths that they represent it is unlikely that their inclusion biases the results significantly.

The SOII samples the OSHA employer workplace injury and illness records of U.S. firms[8] to calculate the incident rate of *new* illness or injury within a calendar year. Employers eligible for the SOII survey include all firms except agricultural firms with less than 11 workers, the self-employed, private household and government workers (BLS Handbook Chapter 9). By construction, the SOII definition of an occupational injury or illness is tied to OSHA's definition. Under OSHA regulations, employers must record injuries or illnesses where the worker experiences "loss of consciousness, days away from work, restricted work activity or job transfer, or, medical treatment beyond first aid" or where the worker has a medically diagnosed work-related illness or injury.[9] SOII participation by the employer is mandatory (see Wiatrowski (2004)).

The SOII reports an hours-based Total Case Rate (TCR) and Days-Away-from-work, Restriction, or Transfer cases (DART). The TCR is the total count of injuries or illness that met the OSHA definition in that calendar year. DART is the count of accidents that led to time missed from work and is a subset of TCR. The rates are reported per 100 equivalent worker years. The formula is

$\frac{I}{W \cdot H} \cdot 200{,}000$ where I is the annual number of injuries and illnesses recorded in the OSHA logs, W is workers and H is hours worked (also reported to the BLS in the SOII). The 200,000 scalar is the equivalent time of 100 workers working 40 hours per week for 50 weeks per year. State level rates are publicly available from 1996 to 2009 for participating states. I omit states that participated sporadically which leaves 38 states.[10] The mean TCR is 5.7 (as reported in Table 1.1).

[8] BLS Handbook Chapter 9 indicates that 230,000 firms were surveyed in 2009.
[9] See BLS Handbook Chapter 9 for additional reporting requirements for specific injuries.
[10] Massachusetts (2003, 2009) and Rhode Island (2008, 2009) account for four total missing years.

OSHA changed their record keeping requirements effective January 1, 2002. SOII survey respondents report their workplace injuries and illnesses for a given year at the beginning of the following calendar year. Friedman and Forst (2007) argue that the 2002 record-keeping change could impact the 2001 SOII data. Using SOII data from 1992-2003 on lost workdays, they find evidence that the change in the record keeping requirement constituted a trend break which overstated reductions in subsequent days away from work rates. BLS Handbook Chapter 9 indicates that the change in the OSHA record keeping requirement "may limit the comparability of SOII estimates". Wiatrowski (2004) compares SOII rates and levels of workplace safety before and after the record keeping requirements change and provides some guidelines for reasonable comparisons over time. Wiatrowski argues that any discontinuous trend break in workplace safety measures should preclude comparisons across time.

The TCR rate (as opposed to the lost days of work studied by Friedman and Forst (2007)) appears to meet the criteria allowing for comparison. In the upper right panel of Figure 1.2, the mean state TCR level steadily decreases over time. To further control for the possibility of a trend break in the TCR, in the multivariate analysis I include state fixed effects, year fixed effects, and individual state-specific trend lines for *both* the pre and post-OSHA record keeping requirement change.

There is some evidence that the SOII undercounts workplace injuries, typically when compared to administrative workers' compensation claims at a state level, although the degree of undercounting is disputed.[11] Ruser (2008, 2010) summarizes and responds to the criticism of the SOII. He points out that the SOII's standard measure of workplace safety across industries and states is one of its greatest strengths; allowing for inter- and intra-state comparisons as in this analysis. Recently researchers have begun to study what determines the propensity for a workers' compensation claim to appear in the SOII data. Boden, Nestoriak, and Pierce (2010) compared SOII survey responses to workers' compensation claims in Wisconsin and find that the SOII is less likely to capture injuries that are cumulative in nature (such as carpal tunnel) rather than acute injuries. Injuries that occur within a calendar year but are only later diagnosed by a physician may fall outside of the view of the survey or may not be reported if employers lose track of the event. Through employee interviews about SOII reporting practice and compliance, Phipps and Moore (2010) identify as a possible source of bias the underreporting of accidents involving temporary workers and late arriving claims related to physician diagnosis. Claims reporting moral hazard is likely to be more present in slowly developing injuries, among temporary workers, or physician related claims relative to acute injury episodes.

[11] See Rosenman et al. (2006); Leigh, Marcin, and Miller (2004); Boden and Ozonoff (2008); Oleinick and Zaidman (2010); Boden and Ozonoff (2010); and Dong et al. (2011).

Therefore the SOII data may understate non-fatal workplace elasticity. Table 1.1 compares the SOII state-years to the non-SOII state-years that are reported in the CFOI for comparison. The SOII state-years have a lower fatality rate, a lower unemployment rate and more workers. I replicated the multivariate analysis of fatalities, restricted to the SOII years of availability, and found qualitatively similar results.

The primary data for this analysis is a balanced panel of the 50 states and the District of Columbia over 18 years providing 918 total observations. The data include local area unemployment rates, fatal and non-fatal occupational injury rates, and a demographic profile of the "average" worker generated from employed monthly CPS respondents.

1.6 Empirical Approach and Results

As suggested by the upper left panel of Figure 1.2, the mean hours-based fatality rate is 5.4 for 918 state-year observations. Both injury and fatality rates decline over time. To account for the downward secular trend, I include individual state-specific trend controls. The lower left panel shows the distribution of hours-based fatality rates,[12] and total case rate (in the lower right panel), both with right-skewed distributions. The right-ward skewness of accidents suggests a log transformation model of workplace safety (*ws*) is appropriate:

$$\ln(ws_{it}) = \beta_1 \ln(u_{it}) + \beta_{2t} y_t + \beta_{3i}\tau + f_i + \varepsilon_{it} \tag{1.6}$$

where u_{it} is state *i*'s unemployment rate at time *t*, y_t are calendar year fixed effects, f_i are state fixed effects, τ is a year trend line, and β_{3i} are state-specific trend lines. The fixed effects estimation strategy leverages within-state variations in the unemployment rate while controlling for state workers' compensation laws, state level industry composition, and local labor markets to the degree that they are time invariant. Although there is a downward secular trend in occupational injuries (Figure 1.2), the individual state trend lines allow for variation in the degree to which each individual state follows the overall national secular trend.

The natural log transformation follows Ruhm (2000) and BvO and provides the direct estimation of the elasticity of safety with respect to unemployment represented by the parameter β_1.[13] Reported standard errors are

[12] Six extreme outliers from Alaska, Montana and Wyoming are omitted to preserve the scale of the graph.

[13] A Box Cox transformation test on the hours-based fatality rate and the unemployment rate indicates the null hypotheses that $\lambda = 1$ or $\lambda = -1$ are rejected at the 0.001 probability level. The

generated by clustering at the state level to allow for correlation within the errors over time. Alternative estimations using the square root of the BLS estimate of state-year employment and with Huber-White sandwich variance estimates provided qualitatively similar results.

Panel A of Table 1.2 contains the elasticity of workplace (hours-based and worker-based) fatality rates to the unemployment rate. The estimated workplace fatality elasticities with respect to the unemployment rate are statistically significant at the five percent level. The estimated safety elasticity indicates a 1.6 to 1.8 percent decrease in fatalities for a 10 percent (not percentage point) increase in the unemployment rate. Using the midpoint of those elasticity estimates (-0.17), this suggests that the drop in the hours-based fatality rate from 2008 to 2009 (3.7 to 3.3) can be attributed to a change in the business cycle.[14] Given that 254,771 million hours were worked in 2009 and the 0.4 reduction in *the hours-based rate*, 510 hours-adjusted workplace fatalities were averted in 2009 because of the Great Recession which accounts for an estimated 58.4 percent of the 874 fatality drop between 2008 and 2009 (5,214 to 4,340 fatalities).

Panel B of Table 1.2 presents the estimation of Equation 1 with TCR (non-fatal accident rate) as the dependent variable. The workplace injury and illness elasticity is -0.103 (Column 1), significant at the one percent level. Due to the change in OSHA reporting standards in 2002 for the SOII data on injuries in the lower panel, I replicate the analysis with both a pre *and* post trend line for each state.[15] The elasticity estimate is -0.097 (Column 2) and remains significant at the one percent level. I proceed using a pre and post 2001 trend line with state and year fixed effects for TCR.

There is a significant workplace fatality elasticity regardless of whether the adjustment is based on an hours of work adjustment or employment adjusted that is *larger* in magnitude[16] than the injury and illness workplace. This larger

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null hypothesis that $\lambda = 0$ has a p-value of 0.111, suggesting the log transformations fit the data reasonably well. Gerdes (2010) points out that log transformation of a ratio provides a symmetric measure of relative change while an untransformed ratio does not.

[14] Information comes from BLS News Releases (accessed April 22, 2011): http://www.bls.gov/iif/oshcfoi1.htm#rates. The percentage increase (measured as a fraction) in unemployment nationally from 2008 to 2009 (from 5.8 in 2008 to 9.3 in 2009) was 0.603. Using the estimated elasticity, (1-(0.603*0.17))*(3.7) = 3.32. The national drop in TCR from 3.9 in 2008 to 3.6 in 2009 is also consistent with the SOII elasticities [(1-(0.603*0.1))*(3.9) = 3.66]. See http://www.bls.gov/news.release/osh.nr0.htm (accessed May 10, 2011).

[15] Friedman and Forst (2007) argue that 2001 is relevant reporting year of change because firms report 2001 injuries in 2002. The BLS indicates 2002 as the relevant year. I estimated with both 2001 and 2002 as the post period identification. The results are qualitatively similar. The estimations using 2001 as the delineation between pre and post results are presented here.

[16] Brooker, Frank and Tarasuk (1997) find that acute injury and low back pain claims are pro-cyclical and that their elasticities are similar in magnitude -0.25 and -0.20 respectively. Consistent

magnitude could be the result of differences in claims reporting moral hazard (the propensity to report a claim for any given level of safety) and risk bearing moral hazard (change in real safety behavior). Workers' compensation benefits replace a higher fraction of the pre-injury wage (nominally, two thirds) than unemployment insurance (which nominal replacement rate is 50 percent of pre-injury wage) and are also received tax free, while unemployment insurance benefits may be taxed.[17] Those employees expecting to be laid off have a period of work to qualify for workers' compensation payments before they leave the firm, and so engage in claims reporting moral hazard behavior that actually increases the likelihood of being laid off. Moreover, some workers' compensation claims, such as low back pain and strains, are both extremely difficult to monitor and potentially open-ended in terms of claim duration. So while fatal injuries more likely reflect real safety behavior, injuries in general include both the risk bearing moral hazard response and claims reporting propensities, which work in opposite directions with respect to the effect of unemployment.[18]

As Kossoris (1938) noted, the composition of the workforce and hours worked are likely to be correlated with the unemployment rate. If more experienced (safer) workers are less likely to be laid off when unemployment is high then on average workplace safety should increase. There is also evidence that the amount of hours worked (extensive effort margin) matters to workplace safety.[19]

Using the monthly CPS micro data I generate a state-year average for hours worked, full time status (35 hours of work or more), employee educational attainment, age, race, gender, and work type. To test for whether there is either a worker composition, or extensive effort effect, I regress the mean characteristics of the workforce (*wc*) for the state-year on the state-year unemployment rate (including fixed effects, etc.):

$$wc_{it} = \beta_1 u_{it} + \beta_{2t} y_t + \beta_{3i} \tau + f_i + \varepsilon_{it} \tag{1.7}$$

The results in the last column of Table 1.1, with their reported standard errors clustered at the state level, indicate considerable correlation between the number

with my results the more objective workplace safety elasticity (acute injury) is larger in magnitude.

[17] See Butler and Gardner (2010) for a description of the difference between workers' compensation and unemployment benefits.

[18] Following Firebaugh and Gibbs (1985) and Kronmal (1993) I used fatality counts as the dependent variable on the number of unemployed and employed workers. The elasticity of fatality counts with respect to the number of unemployed workers was -0.27, statistically significant at the one percent level.

[19] See Johansson, (2004) and Vesgo et al., (2007) for recent examples.

("Labor Force" in Table 1.1) and characteristics of employed workers and the unemployment rate. The labor force diminishes as the unemployment rises. As expected, the fraction of full time workers and hours of work fall as the unemployment rate increases. Educated workers appear to be laid off with relatively lower frequency as unemployment increases. The data further suggests that females compromise a larger fraction of the workforce when unemployment is high. This could be a labor supply offset by spouses of displaced male workers. Race and union status are uncorrelated with the unemployment rate during this period. The agricultural, mining, and construction industries have historically high fatality rates and there are fewer workers engaged in these industries during economic downturns.

Given the evidence that the unemployment rate affects the workforce composition and extensive effort I add a vector of control variables (X_{it}) to Equation 1.6.[20]

$$\ln(ws_{it}) = \beta_1 \ln(u_{it}) + \beta_2 X_{it} + \beta_{3t} y_t + \beta_{4i} \tau + f_i + \varepsilon_{it} \tag{1.8}$$

The results for workplace fatalities and non-fatal workplace injury estimates are in Table 1.3 (the primary findings of this paper). To test whether effort at the extensive margin has a workplace safety relationship I include hours and hours squared in the estimation for *worker-based* fatality rates. Even with the inclusion of the extra controls for workforce demographics, I find that higher unemployment rates are associated with improved workplace safety; the fatality elasticity is again larger than the total injury elasticity. Indeed, with demographic controls, the fatality elasticity is about 1.5 to 2.5 times greater than the total illness and injury elasticity, with both elasticities generally statistically significant at the one percent level. Hours of work and hours of work squared are also significant (at the five percent level) for the log transformation of the worker based fatality rate, which suggests that workplace fatalities tend to decrease as hours increase, up to 40 hours per work, and increase thereafter. This is at least consistent with the idea that workweek length is determined endogenously in part for safety considerations.

Conditional on the inclusion of the unemployment rate and hours of work, the demographic variables are jointly statistically insignificant for fatalities, with both expected and unexpected sign patterns. The fraction male is significant (at the ten percent level) and positive in the worker-based fatality estimates but not

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[20] Controls include age, fraction with 12 years of education, fraction with 13-15 years of education, and fraction with 16 or more years of education, the fraction male, white and unionized along with the percentage of the employed working in the agriculture, mining, and construction industries.

for non-fatal accidents. This could be self-selection by males into occupations with different risk levels or a manifestation of higher male risk preferences. The fraction unionized is significant at the 10 percent level but only in the total injury regression (Column 4). The reversal of sign on the union variables, between the fatality regressions and the total regressions, may again reflect a difference in the composition of responses. But unions may facilitate claims reporting moral hazard by facilitating claims filing for non-fatal injuries, reversing the sign of the union effect in for non-fatal injuries (Hirsch, Macpherson, and DuMond, 1997). The insignificance of the demographic characteristics may be attributable to attenuation bias associated with the use of averages. Much of the variation is likely also absorbed in the state fixed effects, year fixed effects, and state-specific trend lines.

As a check on the robustness of the non-fatal injury pro-cyclicality result, I employ data from individual firms and add individual firm fixed effects, rather than aggregated data. The OSHA Data Initiative (ODI) is a non-random selection of U.S. firms by size and industry. OSHA generates the same set of workplace injury and illness rates for firms as does the SOII. There is a monitoring element to the ODI data with OSHA following high accident rate firms over time. The ODI data includes the employer name and address down to the street level.[21] It is possible to create a panel data set of firms with their non-fatal occupational injury rates. Of firms with multiple observations, the mean number is 3.9 firm level observations. Using the street address, I am able to match the ODI data to the BLS LAUS county level unemployment rate. Because the ODI collects OSHA logs like the SOII, there is a break in comparability in 2001. Part of the 2002 OSHA reporting change is that it is no longer possible to identify whether days away from work were caused by injury or illness. The pre-2002 measure of workplace safety in the ODI data is Lost Workday Illness and Injury (LWDII) rate. The post-2002 measures are TCR and DART (as described for SOII). The TCR is higher in the ODI data than the SOII data (8.8 vs. 5.7). Figure 1.3 presents the distributions of the county unemployment rate, LWDII, DART, and TCR. LWDII, DART, and TCR have right-skewed distribution but there is a large mass of data points at zero for these individual firms, dictating the use of a linear (versus a log transformation of the dependent variable) specification of the injury/unemployment relationship. The fixed effect injury regressions employed to examine the effect of the local unemployment rate on firm safety takes the following form:

[21] There are two establishment name fields. I conduct the analysis identifying on the primary and then the combination of the primary and secondary names. The qualitative results are unchanged. The primary establishment name only results are presented here.

$$ws_{it} = \beta_1 u_{it} + \beta_{2t} y_t + \beta_{3j} d_j + f_i + \varepsilon_{it} \qquad (1.9)$$

where ws_{it} is the workplace safety of firm *i* at time *t*. Controls include year fixed effects, y_t, firm fixed effects, f_i, and the county unemployment rate, u_{it}. Because of the monitoring element of the ODI data, I also include fixed effects for the number of years that the employer has been in the ODI to allow their workplace safety effort (or reporting) to vary as the firm is being monitored over time, d_j. Standard errors are clustered at the county level. The estimation for the respective workplace safety rates is partitioned by years of availability.

The results are reported in Table 1.4. The county level unemployment rate is inversely related with non-fatal workplace accidents, consistent with the SOII results. To facilitate comparison with the results generated by the SOII data, Table 1.4 includes the elasticity at means for the workplace safety measures. The ODI elasticities are between -0.067 to -0.099 compared to the SOII smallest estimated magnitude of -0.092 (Table 1.3, Column 4) and the largest magnitude of -0.103 (Table 1.2, Column 1) in both Tables 1.2 and 1.3.

As a robustness check to the pro-cyclicality of fatal occupational injuries I analyze a subset of the Mortality Detail Files from the National Center for Health Statistics (NCHS). The Mortality Detail File data contain the complete set of death certificates from all 50 states and the District of Columbia. Ruhm (2000) uses the Mortality Detail File data to analyze mortality rates across different age groups and cause of death. From 1978-1992 the place of accident for a subset of death categories is available.[22] Three of the places identified are reasonable approximations of workplace accidents: farm, mine or quarry, or industrial place and premise. I omit farm accidents and estimate a state-year number of employed and unemployed workers, average hours, age, education for CPS respondents who identify their industry as mining or manufacturing. There is one year of overlap between the CFOI and the Mortality Detail File in 1992 allowing for direct comparison between the two sets of data. There were 1,134 fatalities with “Mine or Quarry” or “Industrial Place and Premise” in the Mortality Detail File and 946 fatalities nationally in the mining or manufacturing industries (durable and nondurable goods) in the CFOI. The identification of occupational fatalities is more lax in the Mortality Detail File data and estimates should be viewed cautiously. I generate state-year manufacturing and mining unemployment rates (and employment/unemployment levels) from the CPS monthly micro data.

[22] The data analyzed are a subset of Ruhm’s (2000) category 6 “other accidents and adverse effects” (622). The data for 1992 are slightly less inclusive than 1978-1991 (omit “Misadventures during medical care, abnormal reactions, and late complications”). I include individual year fixed effects and replicate the analysis with1992 omitted, the results are qualitatively unchanged.

The upper left panel in Figure 1.4 shows the distribution of person-month observations in the monthly CPS used to generate these estimates. The mean number of person-month observations in the Mortality Detail File is 3,434 (compared with a mean of 14,465 to the data concurrent with the CFOI) with a low of 203 and a high of 16,433. The upper right panel of Figure 1.4 is the histogram of the estimated manufacturing and mining unemployment rates by state-year. The lower left panel compares the estimated industry-specific unemployment rates to the BLS LAUS state-year unemployment rates (with lowess estimation). The industry specific measures are slightly higher than the BLS state-year unemployment rates, though they are reasonably correlated suggesting that the industry-specific unemployment rates are reasonable proxies for the state-level business cycle. I generate an industry-specific fatality rate per 100,000 workers, shown in the lower right panel of Figure 1.4 with a mean (median) *worker-based* fatality rate of 11.68 (8.07), higher than the CFOI data which could be an artifact of the downward secular trend in workplace accidents, the nature of the specific industries identified in the Mortality Detail Files, or measurement error introduced by generating an industry-specific employment level by state and year.

I replicate the state and year fixed effect with state-specific trend line estimation strategy separately for the industry-specific fatality ratio and then with the fatality counts as dependent variables. Table 1.5 reports the estimated fatal accident elasticities at means.[23] Columns (1) and (2) estimate the impact of the industry-specific state and year unemployment rate on the fatality rate. There is a negative correlation after controlling for state, year and a secular state-specific trend line. Unlike the CFOI analysis, there is not statistically significant evidence of a U-shaped relationship (though the signs are consistent) between hours worked and fatalities (results omitted for space considerations). The worker based fatality elasticity at means are -0.25 (most comparable to the estimate -0.18 from Table 1.2) and -0.23 (most comparable to the -0.25 estimate from Table 1.3) respectively. The p-values are 0.106 and 0.115 respectively for Table 1.5 estimates. The lack of statistical significance[24] is likely driven in part by the imprecise identification of occupational fatalities in the Mortality Detail File data and the CPS generated unemployment rate (and corresponding employment levels). However, the estimated elasticities from the NCHS Mortality Detail data are consistent with the more precise CFOI estimates, lending credibility through replication.

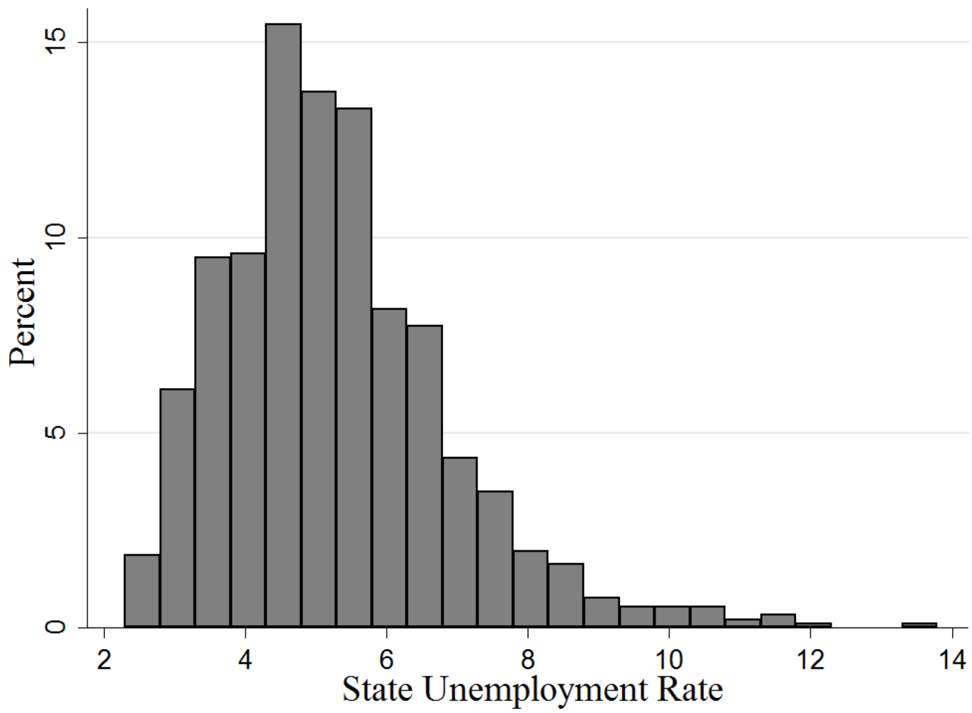
[23] There are four state years with zero values preventing a direct log transformation.
[24] The estimated elasticities were statistically significant at traditional levels when using Huber-White sandwich estimators.

1.7 Conclusion

Both fatal and non-fatal injuries are pro-cyclical in the United States. The pro-cyclicality of workplace fatalities indicates that the pro-cyclical nature of workplace accidents is not *merely* claims reporting moral hazard behavior. The pro-cyclicality of fatal injuries indicates a change in the actual workplace risk of accident or injury. Among the competing explanations for pro-cyclical injuries, the workforce composition effect (to the degree that it was adequately controlled for) was not significantly correlated with workplace safety in this study but attenuation bias and the nature of state-level fixed effects may be partially responsible for this non-finding. The extensive margin appears to be important with a 40 hour work week being the tipping point, where injuries increase after 40 hours of work per week. Even after including hours worked, the workplace unemployment fatality elasticity was statistically significant.

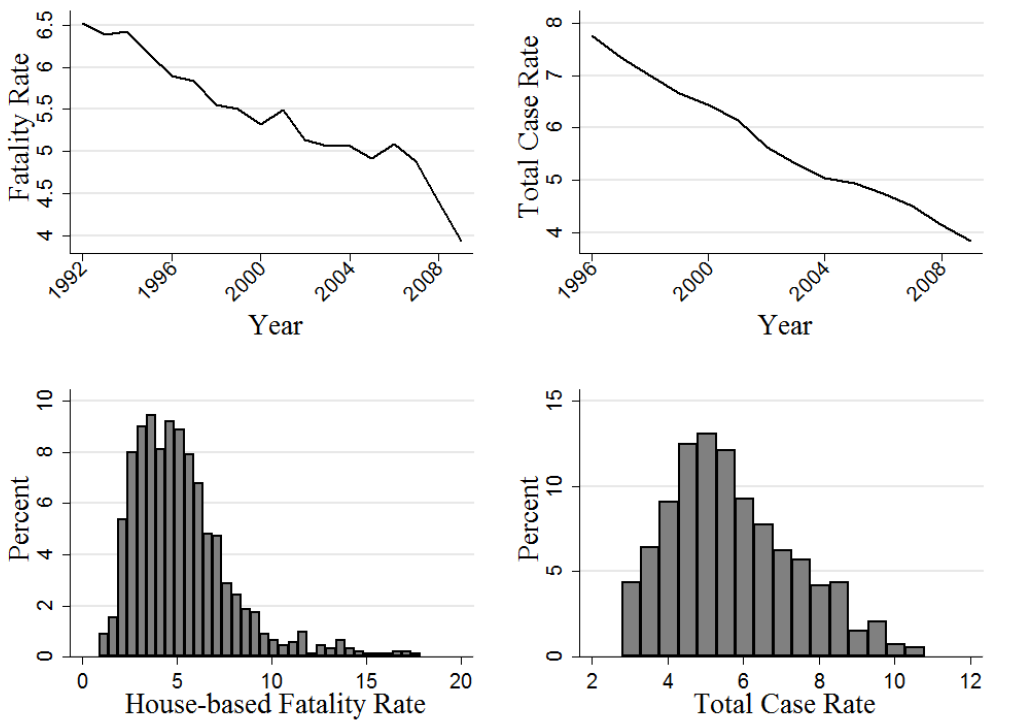
The establishment of a pro-cyclical workplace fatality rate has potentially broad implications for workplace safety. Identifying the relative importance of the various channels discussed to the pro-cyclicality of workplace fatalities will allow researchers to directly assess how each channel contributes to workplace safety, absent the conflicting economic incentives that are present for non-fatal occupational injuries.

Figure 1.1: State Unemployment Rate Distribution



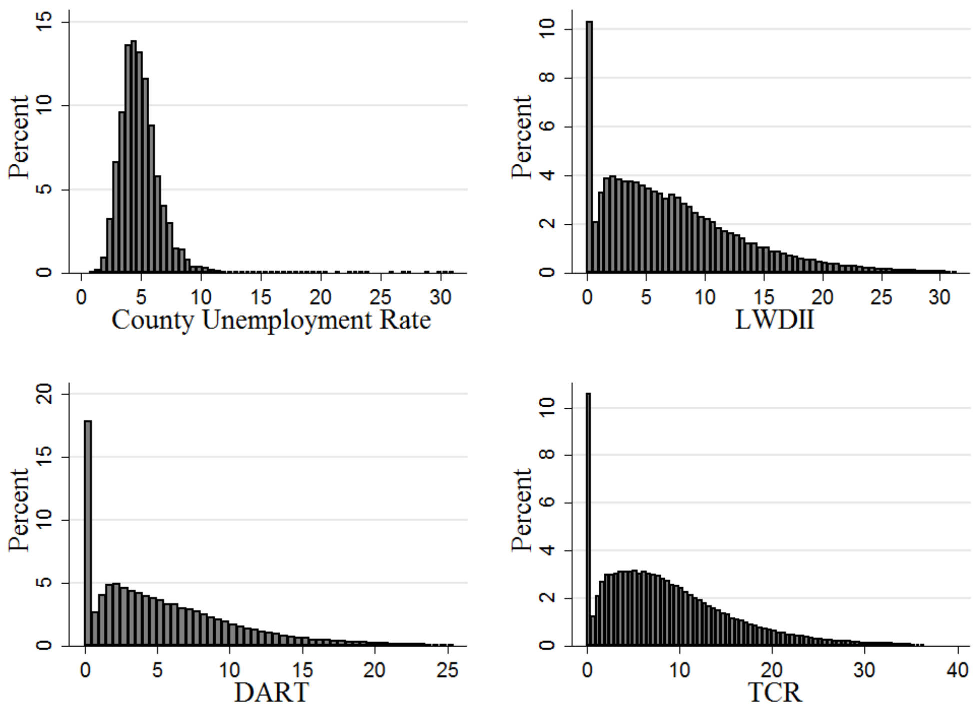
Source: BLS LAUS (1992-2009).

Figure 1.2: Workplace Safety Fatality Rate (CFOI) and Injury or Illness (SOII)



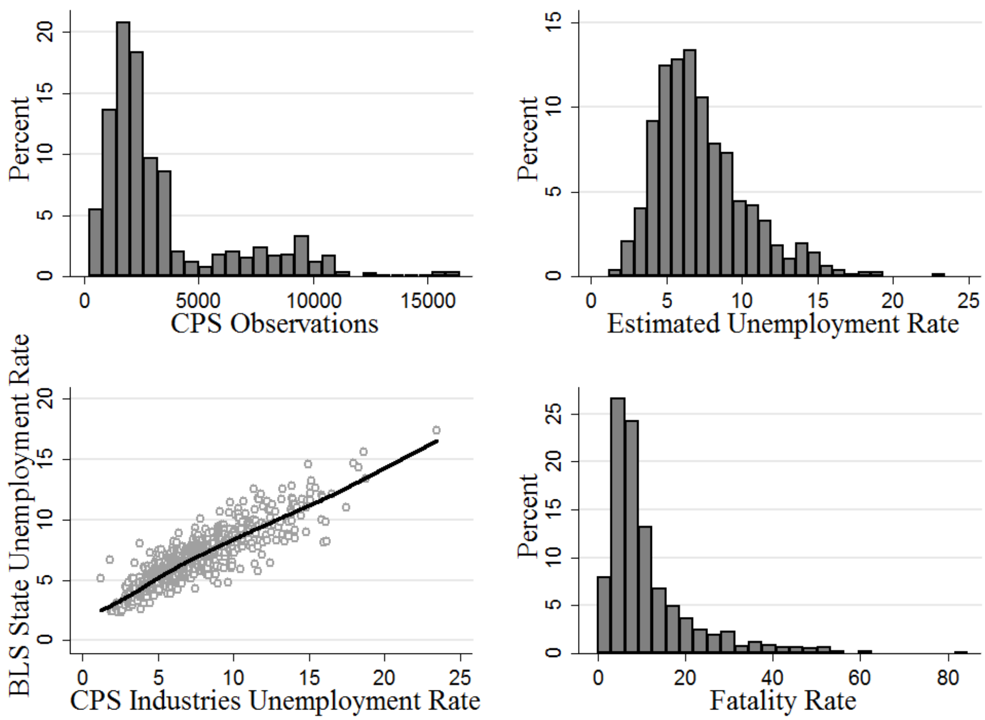
Notes: Fatality rate is per 100,000 equivalent years of work. Total Case Rate is per 100 equivalent years of work. Source: BLS CFOI (1992-2009), SOII (1996- 2009), CPS (1992-2009).

Figure 1.3: ODI Workplace Safety and County Unemployment Rates



Notes: All workplace safety rates are hours-based on a per 100 worker year equivalent basis.
Source: OSHA Data Initiative 1996-2008. BLS LAUS (1996-2008).

Figure 1.4: Mortality Detail File Variables



Source: NCHS Mortality Detail File (1978-1992). CPS Monthly Data 1978-1992.

Table 1.1: CFOI and SOII Descriptive Statistics

Variable	Total	CFOI Only	SOII	Difference	OLS
Fatalities	114.855	103.741	123.064	19.323***	
	(110.824)	(105.491)	(114.009)	(7.376)	
Hours-based	5.422	6.118	4.908	-1.210***	
Fatality Rate	(3.098)	(3.649)	(2.499)	(0.203)	
Total Case Rate			5.695		
			(1.660)		
Unemployment	5.235	5.506	5.036	-0.470***	
Rate	(1.611)	(1.669)	(1.537)	(0.106)	
Hours Worked	39.193	39.286	39.124	-0.162***	-0.150***
	(0.774)	(0.780)	(0.764)	(0.051)	(0.013)
Fraction Full	0.752	0.756	0.749	-0.007***	-0.007***
Time	(0.037)	(0.041)	(0.033)	(0.002)	(0.001)
Age	39.700	39.226	40.051	0.824***	0.033**
	(1.291)	(1.315)	(1.156)	(0.082)	(0.016)
<12 Years	0.113	0.114	0.113	-0.001	-0.001*
Education	(0.027)	(0.0266)	(0.0265)	(0.002)	(0.000)
12 Years	0.324	0.335	0.315	-0.020***	-0.000
Education	(0.047)	(0.055)	(0.038)	(0.003)	(0.001)
13-15 Years	0.292	0.287	0.295	0.008***	0.001
Education	(0.039)	(0.046)	(0.033)	(0.003)	(0.001)
16+ Years	0.272	0.264	0.277	0.013***	0.000
Education	(0.061)	(0.074)	(0.048)	(0.004)	(0.001)
Fraction Male	0.536	0.537	0.535	-0.002	-0.002***
	(0.014)	(0.016)	(0.013)	(0.001)	(0.000)
Fraction White	0.784	0.827	0.752	-0.074***	-0.000
	(0.155)	(0.151)	(0.151)	(0.010)	(0.001)
Fraction Union	0.119	0.118	0.120	0.002	-0.000
	(0.056)	(0.051)	(0.059)	(0.004)	(0.001)
Agriculture,	0.107	0.110	0.104	-0.006***	-0.002***
Mining,	(0.033)	(0.043)	(0.023)	(0.002)	(0.000)
Construction					
Labor Force					-10,605***
					(2668.16)
CPS	14,464.66	14,345.1	14,552.98	207.88	
Observations	(9797.78)	(9776.86)	(9821.53)	(654.50)	
Workers	2,635,053	2,163,958	2,983,020	819,062***	
	(2844543)	(2407236)	(3084878)	(188092.8)	
Observations	918	390	528		918

Notes: Means are reported with standard deviations in parentheses; standard errors in parentheses for SOII-CFOI difference. In the OLS column, each variable reported is the independent variable in a regression of the independent variable upon the state unemployment rate, state fixed effects, state trend lines, and year fixed effects. Standard errors clustered at the state level are reported in parentheses. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: BLS LAUS (1992-2009), Monthly CPS (1992-2009), BLS CFOI (1992-2009), SOII (1996-2009).

Table 1.2: Workforce Safety Outcomes and Unemployment (Logs)

Variable		
Panel A: Census of Fatal Occupational Injuries (CFOI)		
	Hours-based	Worker-based
ln(Unemployment Rate)	-0.157**	-0.181***
	(0.070)	(0.069)
Observations	918	918
R-squared	0.8879	0.8929
Panel B: Survey of Occupational Injuries and Illnesses (SOII)		
	Single Trend	Pre and Post Trend Line
ln(Unemployment Rate)	-0.103***	-0.097***
	(0.020)	(0.027)
Observations	528	528
R-squared	0.975	0.978

Notes: Estimates include state fixed effects, state trend lines, and year fixed effects. Standard errors are clustered at the state level. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: BLS LAUS (1992-2009), Monthly CPS (1992-2009), BLS CFOI (1992-2009), SOII (1996-2009).

Table 1.3: Log Transformation Estimation of CFOI State Year Fatality Rates with State Year Unemployment and Control Variables (Hours and Worker Based)

Variable	Fatalities (CFOI)		Non-Fatal (SOII)	
	Hours-based	Worker-based	Single Trend	Pre and Post Trend
ln(Unemployment Rate)	-0.149**	-0.250***	-0.100***	-0.092***
	(0.072)	(0.075)	(0.025)	(0.028)
Hours		-2.971**		
		(1.139)		
Hours Squared		0.037**		
		(0.015)		
Age	0.007	0.010	0.001	0.002
	(0.026)	(0.027)	(0.009)	(0.010)
12 Years Education	-0.118	-0.018	-0.351	-0.521
	(1.107)	(1.112)	(0.460)	(0.589)
13-15 Years Education	-0.128	-0.117	-0.052	-0.252
	(1.030)	(1.012)	(0.442)	(0.534)
16+ Education	0.262	0.474	-0.534	-0.682
	(1.137)	(1.170)	(0.515)	(0.630)
Fraction Male	1.676	2.087*	0.0064	0.004
	(1.143)	(1.159)	(0.425)	(0.505)
Fraction White	-0.617	-0.831*	-0.007	0.026
	(0.491)	(0.472)	(0.294)	(0.305)
Fraction Union	-0.306	-0.470	0.583	0.665*
	(1.027)	(0.992)	(0.357)	(0.390)
Agric., Mining, Constr.	-1.083	-0.897	0.293	0.173
	(0.917)	(0.942)	(0.582)	(0.605)
Observations	918	918	528	528
R-squared	0.889	0.896	0.975	0.979

Notes: Estimates include state fixed effects, state trend lines, and year fixed effects. Standard errors are clustered at the state level. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: BLS LAUS (1992-2009), Monthly CPS (1992-2009), BLS CFOI (1992-2009), SOII (1996-2009).

Table 1.4: ODI Non-Fatal Hours-based Occupational Injury Fixed Effect Estimation (Robustness Check)

Variable	LWDII	TCR	DART
County Unemployment Rate	-0.107***	-0.135***	-0.098***
	(0.026)	(0.033)	(0.022)
Elasticity at Means	-0.067	-0.083	-0.099
Observations	265,877	321,972	367,898
Clusters	2,824	2,779	2,786
R-squared	0.729	0.722	0.680

Notes: LWDII are available from 1996-2001, TCR and DART are available 2002-2008. Estimation includes individual firm, calendar year, and year in the ODI database fixed effects. Standard errors are clustered at the county level. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: OSHA Data Initiative (1996-2008). BLS LAUS (1996-2008).

Table 1.5: Mortality Detail Worker-based Fatality Elasticity at Means (Robustness Check)

	Fatality Ratio	
Variable	(1)	(2)
Unemployment Rate	-0.248	-0.230
	(0.154)	(0.146)
Observations	765	765
Controls	No	Yes

Notes: Delta-method standard errors are reported in parentheses. The p-value for (1) is 0.106 and (2) is 0.115. All estimation includes state and year fixed effects and individual state trend lines. Controls include hours, hours squared, age, educational attainments, gender and race by state-year. Source: NCHS Mortality Detail File (1978-1992). Monthly CPS (1978-1992).

Chapter 2

Mandated Intensive Care Staffing and Patient Outcomes: a Natural Experiment in Arizona

2.1 Introduction

There has been a relative increase in the demand for intensive care services within hospitals (see Halpern et al. (2006); Halpern and Pastores (2010); Green (2002); and Green (2005)) as health care services that were previously only provided on an inpatient basis can now be provided through outpatient hospital services. This increased relative demand for intensive care services *within* hospitals can delay critical care for at-risk patients (see Chalfin et al. (2007)). Health care professionals and organizations have advocated for increased nursing staff levels as a means of improving patient care, decreasing the length of stay, and increasing patient throughput (see Aiken (2001); Mason (2003); ASRN (2008); Brown (2010); and Rosenberg and Pfeiffer (2011)).

The connection between higher nurse staffing levels and improved patient outcomes is empirically tenuous, often dependent upon the methodology employed. Most analyses finding an improvement in patient outcomes are based on cross-sectional analysis which is sensitive to omitted variable bias.[1] Studies based on alternative identification strategies have found little or no impact for increased staffing levels.[2]

In October 2002, the Arizona Department of Health Services mandated that *no more* than three patients receiving intensive care services be assigned to a

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[1] Needleman et al. (2002) and Aiken et al. (2002) are among those studies most frequently cited that are based upon cross-sectional analysis. Blegen et al. (2011) is a recent publication using cross-sectional data to find a positive impact of increased nurse staffing levels.
[2] See Evans and Kim (2006). Many recent publications have focused on the exogenous change imposed in California when it mandated unit-specific nurse staffing levels in 2004. See Donaldson et al. (2005); Bolton et al. (2007); Spetz et al. (2009); and Cook et al. (2010).

nurse and that fewer than three patients be assigned per nurse for especially critically ill patients. Arizona had no specific nurse staffing mandate prior to October 2002, and the new mandate applied only to patients receiving intensive care services. Arizona's mandated minimum nurse-to-patient ratio provides a "natural experiment" to estimate the impact of increased nurse staffing level requirements on patient health care outcomes in the high stakes medical environment of intensive care services.

I contribute to the literature regarding nurse staffing levels' impact on patient outcomes in three ways: 1) the exploitation of the exogenous variation imposed by Arizona's regulation provides credible causal estimates of the impact of increased nurse staffing levels on patient outcomes—mitigating the potential impact of omitted variables bias inherent in cross-sectional analysis, 2) it provides a sample size large enough to observe changes in low probability events such as hospital mortality[3], and 3) the first analysis of Arizona's intensive care services regulation.

2.2 Arizona Regulation

Effective October 1, 2002 the Arizona Department of Health Services initiated Arizona's first mandated nurse-to-patient ratio. As part of the regulations involving the licensing of health care institutions, section R9-10-220 of the Arizona Administrative Register required that *intensive care* service nurses have a *maximum* of three patients per nurse and directs that a lower patient count per nurse should be reached depending on the severity of the patients. Hospitals are required to have a written protocol in place directing how to reduce the number of patients per nurse "to meet patient acuity" (R9-10-208). [4] The regulation also required that a patient be visited by a physician at least once every twenty-four hours.

The timeline for the adoption of Arizona's nurse staffing level regulation is presented in Figure 2.1. The Arizona Department of Health Services gave notice of a rulemaking docket opening on March 23, 2001. This signaled their intent to "make new rules updating requirements for general hospitals" because the "current rules are outdated, do not accurately reflect industry standards, and do not meet rulemaking requirements". It made no specific mention of a nurse staffing level mandate. The subsequent notice of proposed rulemaking on October 19, 2001contains the first mention of the 1:3 ratio. Regulation R9-10-220 went

[3] Numata et al. (2006) review the literature of the impact of nurse staffing levels on hospital mortality. The largest sample size of the articles reviewed was 118,940. By way of comparison, I employ 2,055,439 observations in this analysis.
[4] R9-10-208 and R9-10-220 can be found at http://www.azsos.gov/public_services/title_09/9-10.htm (accessed October 31, 2011).

into effect one year later on October 1, 2002. Arizona's Department of Health Services proposed a 1:2 nurse-to-patient ratio mandate on June 20, 2003. That proposal went into effect March 5, 2005 and remains in effect today.[5]

2.3 Previous Research

2.3.1 General Literature Review

The correlation between nurse staffing levels and patient outcomes has received increased scrutiny over the past decade as California phased in mandated nurse-to-patient ratios by unit type in acute-care hospitals.[6] Although cross-sectional analysis has found evidence of improved patient outcomes for higher nurse staffing levels (Aiken et al. (2002); Needleman et al. (2002); Cho et al. (2003); Kane et al. (2007); Stanton and Rutherford (2004); Dall et al. (2009); Blegen et al. (2011)), recent findings have found little evidence to support a causal relationship between higher nurse staffing levels and improved patient outcomes (see Donaldson et al. (2005); Evans and Kim (2006); Bolton et al. (2007); Spetz et al. (2009); and Cook et al. (2010)). Needleman, Buerhaus, and Pankratz (2011) find an increased risk of mortality with nurse staffing levels below an estimated "ideal" nurse-patient-ratio conditioned upon the patients' health status.

Lang et al. (2004) and Unruh (2008) provide reviews of the literature, through 2003 and 2006 respectively, addressing the impact of increased nurse staffing levels on patient, nurse satisfaction and financial outcomes. Most of the analyses reviewed are *cross-sectional* and generally report improved patient outcomes with increased staffing levels. Among the cross-sectional analyses, two by Aiken et al. (2002) and Needleman et al. (2002) are frequently cited. Aiken et al. (2002) use data from 168 Pennsylvania hospitals over 20 months with varying

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[5] Twenty five years earlier the California legislature mandated the same 1:2 minimum nurse-to-patient ratio in Intensive Care Units (ICUs) which was unchanged by California's Assembly Bill 394 passed in 1999 that required California's Department of Health Services impose nurse-to-patient ratios by staffing unit. The California ICU nurse-to-patient ratio was passed in the 1976-1977 California state legislative session. See Coffman, Seago, and Spetz (2002). There has been a legislative push in Arizona to follow California's lead still further by extending the presence of minimum nurse-to-patient ratios to other hospital units through what has been called the Arizona Hospital Patient Protection Act. Those efforts have been unsuccessful to date (see Rowley (2009) and Innes (2009)).

[6] California Assembly Bill 394, passed in 1999, required unspecified minimum nurse-to-patient ratios in acute-care hospitals and empowered the California Department of Health Services to determine those ratios. The proposed ratios were announced in January of 2002 and were implemented in 2004. See Coffman, Seago, and Spetz (2002) for a review of the background of Assembly Bill 394.

nurse-to-patient ratios to estimate the impact of a nurse's workload on the rate of failure to rescue (mortality associated with a diagnosis of sepsis, pneumonia, upper gastrointestinal bleeding, shock or cardiac arrest or deep venous thrombosis). They estimate that an additional patient per nurse led to a 7% increase in the odds of dying within 30 days of admission, increased the likelihood of nurse burnout by 23% and job dissatisfaction by 15%. Needleman et al. (2002) used data from 799 hospitals across 11 states in 1997 and find that as the percentage of care by RNs increased there was a reduction in adverse outcomes including pneumonia, cardiac arrest, and failure to rescue.

In an attempt to overcome the omitted variables bias inherent in any cross-sectional analysis, Evans and Kim (2006) use exogenous variation in staff levels created by unexpected surges in admissions on Fridays and Saturdays to examine the impact on health outcomes for patients admitted on Thursdays. The increased patient load had a statistically significant but relatively small impact on readmission rates and no impact on mortality.

2.3.2 California A.B. 394

California A.B. 394 decreased the number of patients per nurse as the law required (see Cook et al. (2010)) but there is little evidence that patient outcomes subsequently improved. Donaldson et al. (2005) use data from 268 units in 68 hospitals participating in the CalNOC (California Nursing Outcomes Coalition) project to examine the unit-level impact California's 2004 mandatory 1:6 nurse-patient ratio in medical and surgical units. Hospitals appeared to comply with the mandated minimum legislation by increasing nursing staff, but the authors noted an inability to measure true compliance which requires that the ratio be maintained at all times. There was no significant change in the rate of patient falls and pressure ulcers after the legislation was passed. Bolton et al. (2007) use the same data as Donaldson et al. (2005) to examine identical patient health care outcomes after a 1:5 nurse-to-patient ratio was imposed in 2005. They find that the hours of care provided to patients by a registered nurse increased from 2004 to 2006 and that the nurse-to-patient ratio declined. Although there were trends toward better patient outcomes, there were no statistically significant differences in patient falls and ulcers. Spetz et al. (2009) and Cook et al. (2010) have also failed to find statistical evidence of improved patient outcomes.

2.3.3 Intensive Care Services

Numata et al. (2006) provide an overview of the literature connecting nurse staffing levels to patient mortality in critical care settings through 2005. Hugonnet, Uckay, and Pittet (2007) and Hugonnet, Chevrolet, and Pittet (2007) use three years of data from the University of Geneva Hospitals' ICU and determine that a lower patient-to-nurse ratio led to a reduced patient risk of late-onset ventilator-associated pneumonia and lower rates of infection. Tarnow-Mordi et al. (2000) report that after adjusting for patient risk (through APACHE II scores), patients who were admitted into an ICU where the workload was high (measured by occupancy and nursing requirements per patient) experienced higher rates of mortality. Pronovost et al. (1999) observe that a decreasing nurse-to-patient ratio reduced the length of stay in the hospital and ICU for patients undergoing abdominal aortic surgery. Dimick et al. (2001) find that patients undergoing hepatic resection had a decreased chance of complications in ICUs with lower nurse-to-patient ratios.

2.4 Data Description

For this analysis I use data from the Healthcare Cost and Utilization Project's (HCUP) State Inpatient Databases (SID) which contain patient-level data for the universe of hospital inpatients stays by participating states. Individual states differ in what variables are provided and at what level of aggregation. The Arizona SID (hereafter SID) contains demographic data including age, race and zip code of residence for each patient. The SID also has clinical data (medical diagnoses (ICD-9s), treatment codes (CPT-4s)), and hospital administrative data (hospital identifiers[7], health insurance type, and charges that are attributed to departments within the hospital).

The timing of inpatient stays can be identified to the month and year of admission. The pre-intensive care ratio mandate period of analysis initially extends from January 1, 1998 to October 31, 2001—a nearly four year window. The Arizona Department of Health Services regulation requiring a 1:3 nurse-to-patient ratio first appeared in the Arizona Administrative Register on October 19, 2001. The post-intensive care ratio period of analysis begins October 1, 2002, the same day the ratio was to be enforced and ends August 31, 2006. Given that there are two changes to mandated intensive care nurse staffing levels prior to August 31, 2006, this initial lengthy period of pre and post analysis is best viewed as a test of the impact of both changes. Later I vary the length of the pre and post period as a robustness check and to avoid conflating the second change in the nurse staffing levels with the first. The exclusion of the inpatient stays between the initial announcement and the enactment of the regulation prevents the

[7] There is a break in the comparability of such identifiers at the beginning of 2003, but HCUP provides American Hospital Association (AHA) linkage files by year which allow for hospitals to be identified across the break in hospital identifiers.

regulation's impact on patient outcomes from being biased downward by hospitals increasing nursing staffing levels in the interim period.

The SID includes Uniform Billing (UB-92) codes from 1998 to 2006 which assign charges to revenue centers including Room and Board, Pharmacy, Operating Room and Intensive Care Units (ICUs). The UB-92 billing code in the SID includes the prefix 20x which maps into intensive care (General (200), Surgical (201), Medical (202), Pediatric (203), Psychiatric (204), Intermediate (206), Burn Care (207), Trauma (208) and Other Intensive Care (209)). The intensive care services mandate contained in R9-10-220 indicates that an "intensive care unit is staffed with a minimum of one registered nurse for every three patients and according to an acuity plan". The R9-10-220 regulation does not distinguish the subtype of intensive care unit and the regulations are dependent upon the designation of the patient as being an intensive care patient and not to a physical location.

Observations with missing values for total charges or the UB-92 ICU charge field were dropped from the analysis. Any inpatient stay with a positive value for the UB-92 ICU charge is identified as an ICU patient. Twenty-nine percent of observations were flagged as being an ICU patient over the course of the pre and post period. I restrict the analysis to patients age 18 and older to avoid conflating neonatal and pediatric intensive care services. Table 2.1 provides selected summary statistics by ICU status for 2,055,439 inpatient stays during the pre and post regulation periods. ICU patients are 12 years older on average, more likely to be male, and to be white, than their non-ICU counterparts. Moreover, ICU patients are more likely to be on Medicare and less likely to have private insurance than their non-ICU counterparts (likely an artifact of the disparity in the patients' ages).

I restrict the analysis to hospitals with inpatient records for all of the years of the analysis (leaving 48 hospitals) and that have both ICU and non-ICU observations in a given year for every year to limit the analysis to comparable infrastructure of health production leaving a balanced panel of 28 hospitals. Rural hospitals are explicitly prohibited in R9-10-220 from providing intensive care services. Further, changes in reporting practices by hospitals could lead to inconsistent reporting of the UB-92 charges and inconsistent ICU identification.

The Agency for Healthcare Research and Quality (AHRQ) and the National Quality Forum enumerate lists of patient outcomes that are thought to be sensitive to nurse staffing levels or which past research have identified as being sensitive.[8] The lists include in-hospital mortality, length of stay, pressure ulcers, hospital-acquired pneumonia (often associated with being on a ventilator), urinary

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[8] See http://www.ahrq.gov/research/nursestaffing/nursestaff.htm#Staffing for the AHRQ list and http://www.qualityforum.org/Home.aspx for the National Quality Forum list (accessed October 3, 2011).

tract infections (UTIs, often from catheterization), and hospital-acquired sepsis (a bacterial bloodstream infection).

In-hospital mortality and length of stay are available in the SID. All other dependent variables are generated from the diagnoses fields included on the inpatient abstracts. The SID includes eleven diagnosis fields through the year 2002 and only nine thereafter. To be consistent across the period of analysis, patient outcomes were coded only from the first nine diagnosis fields for all years. ICU patients have one and a half more complete diagnosis fields than non-ICU patients in the SID (see Table 2.1) consistent with a greater set of complications mandating intensive care. The first diagnosis field is the primary diagnosis, and is excluded from identifying nurse-sensitive patient outcomes because any such diagnosis must be a *subsequent* outcome to hospital admission and therefore cannot be the principal reason that the patient is hospitalized. Table A1 in Appendix A reports the inclusion and exclusion criteria employed in identifying the patient outcome variables as well as the source materials employed.

2.5 Methodology

To estimate the impact of the mandated nurse-to-patient ratio in Arizona ICUs, I employ a difference-in-differences (DID) estimation strategy to compare the nurse-sensitive dependent outcomes for ICU and non-ICU patients in the post-mandate period. The binary dependent variables are estimated using a linear probability model for computational ease. Equation 2.1 (corresponding to Model (1) in Tables 2.3-2.5) includes no demographic controls or diagnosis indicators:

$$y_i = \beta_1 ICU + \beta_2 Post + \beta_3 Post \times ICU + \varepsilon_i \tag{2.1}$$

Robust standard errors are clustered at the hospital level and allow for unobserved correlation for patients within the same hospital.

Equation 2.2 (Model (2) in Tables 2.3-2.5) expands Model (1) by including demographic controls including gender, race, ten-year age bins, admission month, the health insurance type as a proxy for socioeconomic status, whether the patient identified an Arizona zip code as their place of residence and a hospital fixed effect (h_j).

$$y_i = \beta_1 ICU + \beta_2 Post + \beta_3 Post \times ICU + \beta_4 X_i + h_j + \varepsilon_i \tag{2.2}$$

Equation 2.3 (Model (3) in Tables 2.3-2.5) additionally includes an indicator for each of the 7,327 unique ICD-9s (d_k) included in the primary diagnosis field of the patients.

$$y_i = \beta_1 ICU + \beta_2 Post + \beta_3 Post \times ICU + \beta_3 X_i + h_j + d_k + \varepsilon_i \qquad (2.3)$$

Under the identifying assumption that the variables of interest for the “treatment” and “control” group move in a parallel fashion in the pre period and would continue to do so in the absence of the “treatment”, the DID estimation strategy identifies the impact of Arizona’s change in intensive care services regulations. These include: 1) a physician visit every 24 hours, 2) a 1:3 (later 1:2) nurse-to-patient ratio, and 3) an acuity plan to deal with more at risk patients. The focus on outcomes that have been deemed to be nurse-sensitive is meant to shift the focus to the nurse patient ratio and acuity plan.

2.6 Results

2.6.1 Patient Outcomes

Table 2.2A and 2.2B provide a pre/post regulation mean comparison of the dependent variables by intensive care status. The reported means are largely consistent with previously published estimates (see Needleman (2002)). A simple comparison of means within intensive care patients in the fourth column from the left yields mixed results. Mortality and length of stay decline but the rate of pressure ulcers, pneumonia, UTI, and sepsis rise. For example, the rate of sepsis rises from 0.0321 (Pre, Column 2) to 0.0469 (Post, Column 3), an increase of 0.0148 (Difference, Column 4). With the exception of the length of stay, the intensive care and non-intensive care differences move in the same direction: for example, the mortality rate falls for each group in the post-regulation period as compared to the pre-regulation period.

The presence of a common qualitative change suggests that a simple comparison of differences in just the ICU mean outcomes might capture a secular trend in patient care outcomes instead of a “treatment” effect. This secular trend in patient outcomes could be due to a change in staff awareness and subsequent reporting behavior for nurse-sensitive outcomes subsequent to local or national initiatives to monitor patient care more closely. Although a direct ICU difference in mean comparison indicates lower mortality rates and length of stay for ICU patients, it also implies an *increase* in the rate of pressure ulcers, pneumonia, UTI and sepsis. All differences are statistically significant at the one percent level.

Figure 2.2 Panel A depicts the relationship between mortality by ICU status. The circles and diamonds represent monthly means for ICU and non-ICU patients respectively. The ICU means have fewer observations and therefore show more variation than the non-ICU average. The solid line (ICU) and the dashed line (non-ICU) represent local linear regressions estimates.[9] The vertical lines indicate October of 2001 and 2002, the delineation of the pre and post periods. There is a clear seasonal component to the inpatient for mortality for both the ICU *and* non-ICU patients, generally peaking during winter months. Figure 2.3 shows the age of the ICU and non-ICU patients over the period of analysis. The seasonal variation in patient age which peaks during the winter months for both ICU and non-ICU patients could be due to the seasonal migration of older adults who reside in northern states to winter in Arizona ("snowbirds"), or it could be that pneumonia and other illnesses that might differentially impact older adults. Therefore, Models (2) and (3) in Tables 2.3, 2.4, and 2.5 include an indicator variable of whether the patient had an Arizona zip code listed as their place of residence to account for snowbird seasonal variation (an imperfect measure of whether the patient is a snowbird since older adults who winter likely spend enough time to have a semi-permanent residence during their stay). Although there is seasonal variation in the age of the patient, the ICU (solid line) and non-ICU levels (dashed line) in Figure 2.3 are stable in the pre and post period. As noted previously, admission month indicator variables are included in the analysis.

Both Table 2.2 and Figures 2.2, 2.4 and 2.5 show that with the exception of pressure ulcers, ICU patients have worse outcomes than non-ICU patients throughout the sample period. The AHRQ identification of pressure ulcers indicates that only patients who have stayed more than four days in the hospital should be coded as having pressure ulcers related to their hospital stay.[10] The pressure ulcer prevalence rate moves roughly at the same rate in the pre period for both ICU and non-ICU patients.

The identifying assumption of the difference-in-differences estimation strategy is that the two groups moved in parallel prior to the "treatment". In all three figures (2.2, 2.4-2.5), the pre-period relationship (to the left of the first vertical line) between ICU and non-ICU patients is largely consistent with the identifying assumption.

Tables 2.3 through 2.5 report β_1 (ICU Identifier), β_2 (Post Identifier), and β_3 ($Post \times ICU$) from Equations 1 through 3 for in-hospital mortality (Table 2.3, Panel A), length of stay (Table 2.3, Panel B), hospital-acquired pneumonia (Table 2.4, Panel A), pressure ulcers (Table 2.4, Panel B), UTIs (Table 2.5, Panel A), and hospital-acquired sepsis (Table 2.5, Panel B). The coefficient of interest, β_3, measures whether the law effectively improved patient outcomes: $\beta_3 < 0$

[9] Created using Stata's lowess command using the default bandwidth of 0.8.
[10] The AHRQ Technical Specifications can be found: http://www.qualityindicators.ahrq.gov/Downloads/Software/SAS/V43/TechnicalSpecifications/PSI%2003%20Pressure%20Ulcer%20Rate.pdf (accessed October 4, 2011).

indicates an improvement given that the dependent variables are negative in nature in the post period for ICU patients relative to non-ICU patients. In Table 2.3, Panel A estimates of mortality, for example, we see the effect of the mandatory staffing rule has no statistically significant effect on mortality: in the full model on the right hand side column of Table 2.3, the staffing rule effect increases the mortality rate by a statistically insignificant 0.0007.

None of the estimates of the mandatory staffing rule indicate a significant improvement in patient outcomes. Again, the effect of the rule on mortality is very close to zero as indicated above. The $Post \times ICU$ coefficient is negative, suggesting some improvement, for length of stay and UTIs, but the response is statistically insignificant for both. Like the in-hospital mortality results, pressure ulcers see a statistically insignificant increase in the post-regulation period (0.0014 in Column 3, Panel B, of Table 2.4). Notably, the rate of hospital-acquired pneumonia and sepsis experience a statistically significant *increase* in the post-regulation period, 0.0075 and 0.0092, respectively. These coefficients represent a 17.9 and 28.7 percent *relative* increase to the pre-regulation baseline. That is, the only significant results of the mandatory staffing change, using the full model, indicate that intensive care patient outcomes were worse in the post-regulation period.

I repeat the analysis, restricting to specific populations of interest for mortality, length of stay, pressure ulcers, and pneumonia. Table 2.6 reports the Model 3 estimates after restricting the analysis to Medicaid patients (Panel A), Medicare patients (Panel B), and the Privately Insured (Panel C). Table 2.7 reports the estimates for females (Panel A), males (Panel B), and non-whites (Panel C). The results for these restricted groups are consistent with the full sample[11]: the staffing rule either has no statistically significant effect on patients' outcomes (mortality, length of stay, pressure ulcers), or if the staffing rule is statistically significant, it suggests patients are worse off after the implementation of the rule (pneumonia). That is, the only statistically significant change in the $Post \times ICU$ period is an *increase* in the rate of hospital-acquired pneumonia—consistent with the results for the entire sample.

2.6.2 Robustness Checks

To evaluate the sensitivity of the results to the period of analysis chosen I replicate Equation 2.3 (Model (3) in Tables 2.3-2.5) for all six dependent variables using a two, and then a one year, window of analysis (instead of four years). Table 2.8 and 2.9 contain two panels. Panel A presents the estimates from the two year pre/post window analysis, and Panel B reports the one year pre/post

[11] The results for UTI and sepsis are also consistent across subgroups but are omitted.

window of analysis. The sepsis and pneumonia $Post \times ICU$ coefficient remains positive and statistically significant in the two-year analysis but are no longer significant once the analysis is restricted to one-year examination periods around the regulation change. None of the other coefficients are statistically significant.

I re-estimate Equation 2.3 (Model (3) in Tables 2.3-2.5) for the 1998 to 2006 sample, checking whether the effect of the staffing mandate on patient outcomes are robust to the definition of an ICU patient using the ratio of intensive care to total charges. I compare patients with no intensive care billing charges to those with the fraction of total charges related to intensive care greater than or equal to twenty percent (Panel A), forty percent (Panel B), and sixty percent (Panel C) in Table 2.10 and 2.11 (those with intensive care billing between zero and the relevant minimum percent are excluded from the analysis). Only the $Post \times ICU$ coefficient is reported. The $Post \times ICU$ coefficient on pneumonia remains positive and significant, but the sepsis coefficient is no longer statistically significant. Once intensive care patients are defined as having greater than or equal to forty percent (Panel B) and sixty percent (Panel C) of their charges be intensive care related, none of the coefficients remains significant.

2.6.3 Nurse Staffing Levels

There are a number of possible institutional responses to R9-10-220's staffing level mandate including: 1) failure to comply (noncompliance), 2) compliance by increasing the number of nursing hours employed for intensive care patients (compliance), 3) compliance by reallocating nursing hours away from non-intensive care patients to intensive care patients (nurse reallocation), 4) compliance by increasing the number of nursing hours employed and then offset that increase in cost by decreasing the number of support personnel in the hospital (staff reallocation), and 5) a procedural change in how patients are identified as needing intensive care (recoding).

Arizona does not record unit level nurse staffing levels so it is not possible to measure the direct effect of R9-10-220 on nurse staffing levels for intensive care services by hospital.[12] However, Arizona's Department of Health Services does require that hospitals submit an annual Uniform Account Report (UAR)[13] which includes data on annual levels of nurse staffing, patient hours and licensed beds for the each reporting hospital. I use the UAR data to calculate the annual *state* nurse full time equivalent (FTE) per licensed bed—calculated as

[12] California is the only state to record unit level nurse staffing (see Jiang, Stocks, and Wong (2006)).
[13] Arizona UAR data can be found at http://azdhs.gov/plan/crr/cr/hospitals.htm (accessed October 31, 2011).

[(Nursing Hours/2080) /Licensed Beds].[14] Figure 2.6 shows the Registered Nurse FTEs per licensed bed (dashed line) and the total nursing FTEs (RNs plus Licensed Practical Nurses (LPNs)) per licensed bed (solid line) by year. Because of variation in reporting periods employed in hospitals[15], the impact of that law should not be visible until 2003. There is a clear increase in the nurse FTE-to-licensed bed ratio between 2002 and 2003. The increase is driven by the presence of more RNs which are traditionally more likely to be employed than LPNs to work with intensive care patients. Figure 2.6 suggests that Arizona's intensive care nurse patient legislation was binding to some degree upon hospitals in Arizona and that hospitals were not able to simply substitute within hospital nursing resources to meet the newly required staffing levels (nursing reallocation).[16]

As an alternative measure of whether the regulation increased intensive care nurse staffing levels I employ data from the National Sample Survey of Registered Nurses (NSSRN) in 2000 (pre-regulation) and 2004 (post-regulation). The NSSRN has a rich set of information including whether the RN worked in a hospital and which unit they primarily spent their time. I restrict the analysis to those RNs who reported working in hospitals with non-missing values for hospital unit, hours worked within the principal position, and time spent in direct patient care in Arizona, New Mexico and Texas. New Mexico and Texas represent border-states that did not experience mandated nurse staffing levels as Arizona and California did from 2000 to 2004.

Using a difference-in-difference (DID) approach, I estimate Equation 2.4 for Arizona nurses only:

$$\ln(Hours_i) = \beta_1 ICU + \beta_2 Post + \beta_3 Post \times ICU + \varepsilon_i \tag{2.4}$$

[14] See Jiang, Stocks, and Wong (2006) for a description of different measures of nurse staffing including FTE per bed.

[15] Some hospitals report from January 1 to December 31 (calendar year) while others report from July 1 to June 30 (Arizona's fiscal year) and others from October 1 to September 30 (the Federal government's fiscal year).

[16] The Arizona Department of Health Services' Division of Licensing Services is charged with inspecting Arizona hospitals and providing citations for failure to comply with state regulations including the ICU nurse-to-patient ratio (R9-10-220) and an acuity plan (R9-10-208). The Division of Licensing Services posts the last three years of surveys conducted for each hospital in Arizona online. All 505 surveys conducted for the 105 Arizona hospitals between 2007 and 2010 were examined. Only two citations for failure to comply with the intensive care nurse-to-patient ratio (0.4%) were found. There were 21 occurrences where hospitals failed to comply with the acuity plan which requires hospitals to lower the patients per nurse when the patients' condition indicates the hospital staff should do so (4.16%).

where *ICU* is an indicator variable equal to one for a hospital nurse that identifies the intensive care unit as the hospital unit where they spend their most patient care time and *Post* indicates that the RN's response occurs in 2004 after the regulation was put into effect. Given the natural log transformation of the hours worked, the interaction term ($Post \times ICU$) measures the percentage change in the hours worked by intensive care nurses in the post period.

The first column in Table 2.12 indicates that ICU nursing hours increased by 10.12 percent after the mandate was in effect, consistent with an increased demand for intensive care nurses in Arizona during the post-period. The ten percent increase is roughly equivalent to the increase from 1.2 to 1.3 FTE (an 8.33 percent increase) in Figure 2.6. The statistical power of the analysis is limited by only having 450 RNs in Arizona who worked at a hospital during the survey years (only 84 of which worked in the ICU). The coefficient is not significant and has a p-value of 0.195.

To evaluate whether a secular increase in intensive care services, independent of the regulation, might have led to this ten percent increase I employ a "difference-in-difference-in-differences" (DDD) approach in Equation 2.5, which also includes data from Texas and New Mexico (as "control" states):

$$\ln(Hours_i) = \beta_1 ICU + \beta_2 Post + \beta_3 Post \times ICU + \beta_4 Post \times AZ + \beta_5 ICU \times AZ + \beta_6 Post \times ICU \times AZ + \varepsilon_i \tag{2.5}$$

The analysis includes an additional indicator *AZ* equal to one for Arizona RNs, interacted with the other variables in Model 2.1. The coefficient β_6 provides a test of whether intensive care Arizona RNs increased their hours worked after the regulation was enacted: Column 2 with Texas and New Mexico as "control" states, and Column 3 with Texas only as a "control" state. The *Post* coefficient is positive and significant in the DDD model (8.96 and 8.59 percent). The $Post \times AZ$ coefficient, though positive, is not statistically significant indicating that the increased number of RN hours in the post period represents a secular trend across all of the states. The coefficients for the effect of the Arizona law on intensive care nursing hours for the DDD specification are 10.22 percent (p-value 0.255) and 10.9 percent (p-value 0.24) respectively—consistent with the DID estimation in Column 1. Though not statistically significant, these $Post \times ICU$ coefficients (in both the DID and DDD specifications) indicate that nursing hours provided by intensive care RNs rose by about 10 percent more than non-intensive care RNs after the implementation of the mandatory nurse staffing rule in Arizona.

The increase in overall nurse staffing levels in Figure 2.6 and the relative increase in hours worked by intensive care nurses in the NSSRN are suggestive that the regulations imposed on intensive care services in Arizona were binding to some degree. If such is the case, it suggests that the first (noncompliance) and third (nurse reallocation) hospital responses delineated previously were not fully realized. Regarding the fifth (recoding) potential institutional response, where the hospitals simply recode admissions as not requiring intensive care services (that would normally be ICU patients) in order to meet the mandatory staffing rule, the fraction of patients designated as intensive care increases from 29.0 in the pre period to 29.2 percent in the post period, suggesting that recoding was not a problem in the data. I estimate the probability of a patient being assigned to the intensive care using a linear probability model:

$$\Pr(ICU) = \beta_1 Post + \beta_2 X_i + h_j + d_k + \varepsilon_i \tag{2.6}$$

where *Post* is an indicator variable for an inpatient stay initiating on or after October 1, 2002. The estimated probability includes hospital (h_j) and principal diagnosis (d_k) fixed effects. Standard errors are clustered at the hospital level to allow for unobserved correlation for patients within the same hospital. Consequent with the changes in mean fraction of patients in ICUs, the β_1 coefficient for Arizona hospitals, using the linear probability model in Equation 2.6, is 0.0169 with a p-value of 0.417. There was a small *increase*, though not significant, in the number of ICU patients, suggesting no evidence of recoding in order to achieve the mandatory staffing rule.

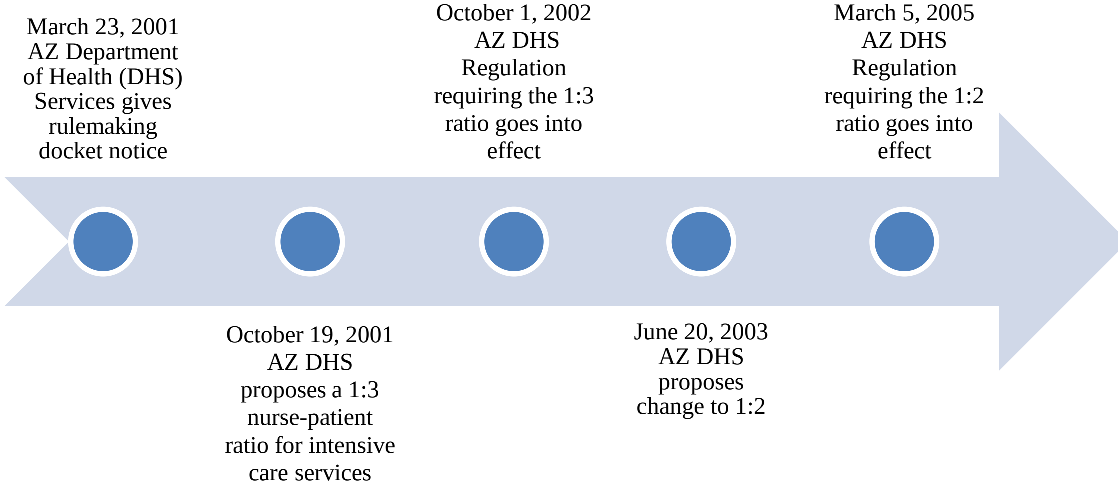
However, the second (compliance) and fourth (staff reallocation) responses are still consistent with the observed increase in staffing levels in Figure 2.6. To the degree that the contribution of increased nurse staffing levels is offset by fewer support personnel (staff reallocation) it could bias the impact of the new regulations toward zero. Fewer support personnel might require RNs to spend less time in patient care and more time completing tasks that other hospital support personnel might perform. The NSSRN asks RNs what percentage of their time for their principal position is dedicated to direct patient care.

I replicate the analysis used to evaluate the impact of the regulation on nursing hours (using Equations 2.4 and 2.5) with the natural log transformation of the percentage of time spent in direct patient care as the dependent variable. The results reported in Table 2.13 Columns 1 (DID, the $Post \times ICU$ coefficient) and Columns 2-3 (DDD, the $Post \times ICU \times AZ$ coefficient) indicate that ICU RNs spend 13-22 percent *more* of their time in patient care than non-ICU RNs. The DID coefficient is slightly negative (-0.0062) and the DDD coefficients are positive (0.0202, 0.0574) indicating an increased interaction with patients, although none of the coefficients are statistically significant. There does not appear to be evidence that the percentage of time dedicated to patient care declined. Hence, there is no evidence that the increase in the nursing-to-patient ratio in Figure 2.6 is anything other than compliance with the mandatory staffing rule.

2.7 Conclusion

The composition of hospital patients has shifted towards intensive care as conditions that traditionally were treated in the hospital in the past are shifted to outpatient treatment. The examination of nurse staffing levels is motivated by their impact on the cost of care and potential to improve patient outcomes. The exogenous imposition of regulated nurse staffing levels in Arizona provides a “natural experiment” to analyze whether patient outcomes improve with such a regulation. The available evidence suggests the regulation was binding on nurse staffing levels; so that the post-ICU coefficients represent estimates of the *marginal* improvement in health generated by an incremental increase in nurse staffing levels. There is no evidence of improved patient outcomes in the post-regulation ICU.

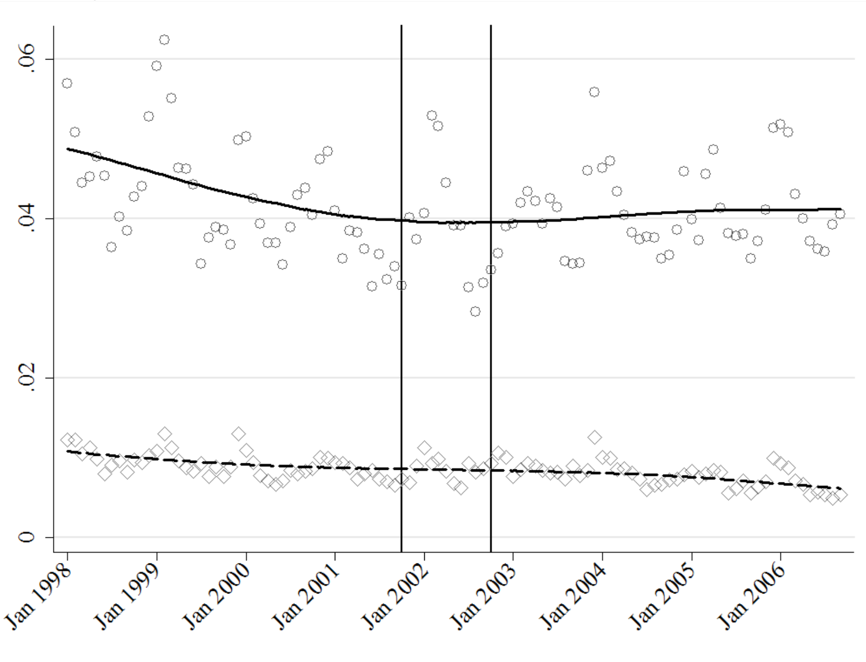
Figure 2.1: Arizona Regulation Timeline



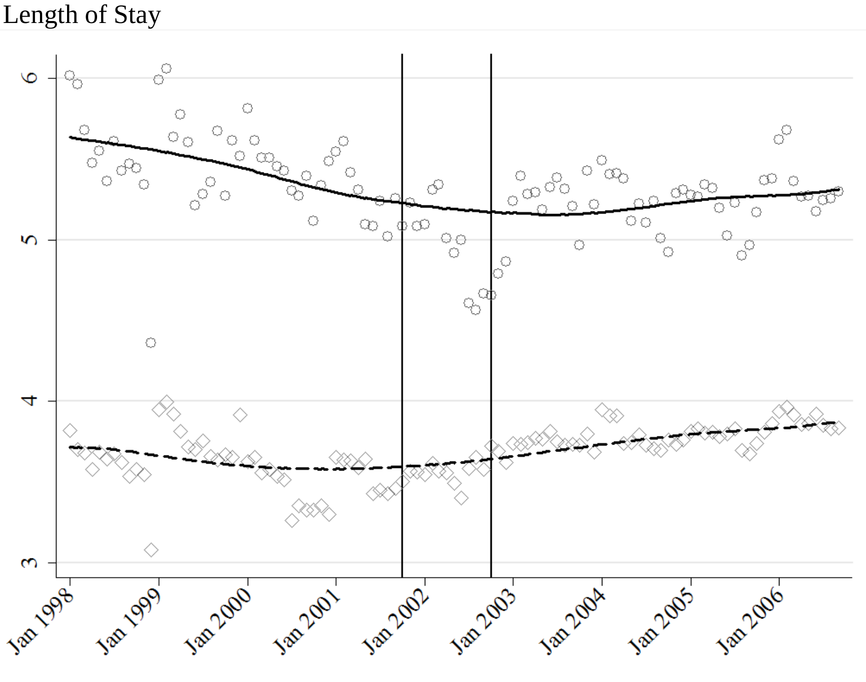
Notes: The Arizona Administrative Register (http://www.azsos.gov/aar/contents.htm) where all of these regulations can be found (accessed November 5, 2011).

Figure 2.2: Mortality and Length of Stay

Panel A: Mortality

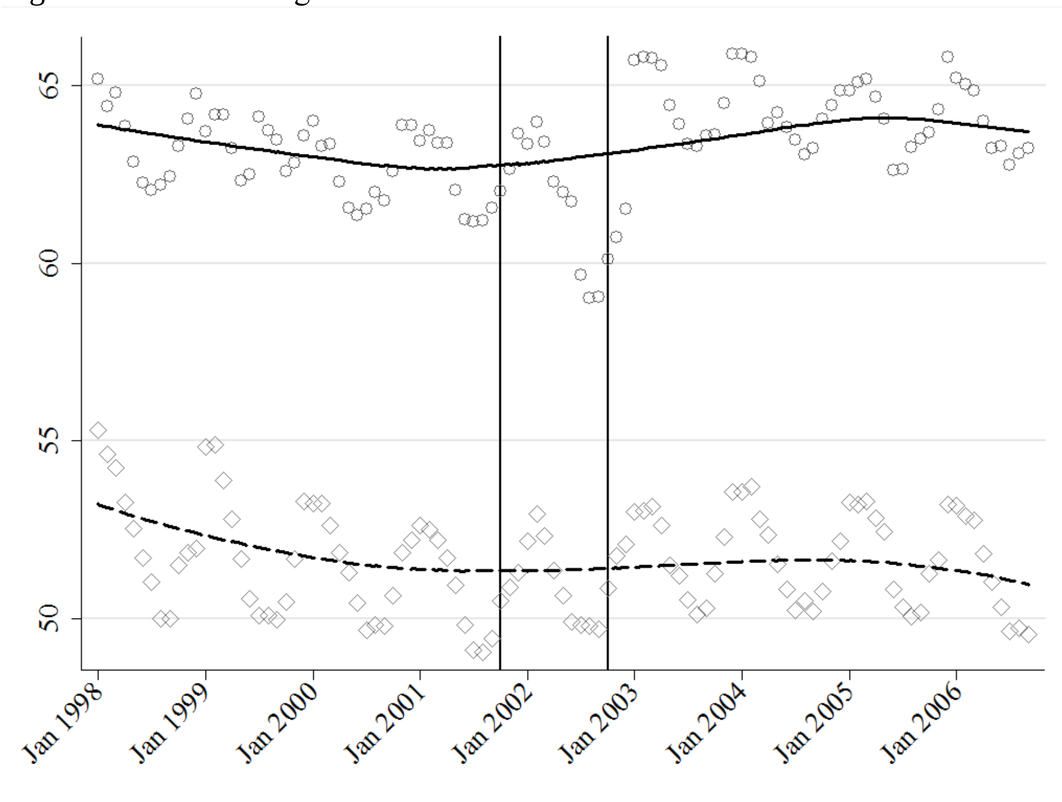


Panel B: Length of Stay



Notes: The solid line represents the ICU monthly means and the dashed line represents the non-ICU monthly means for 28 balanced panel hospitals. Source: AZ SID 1998-2006.

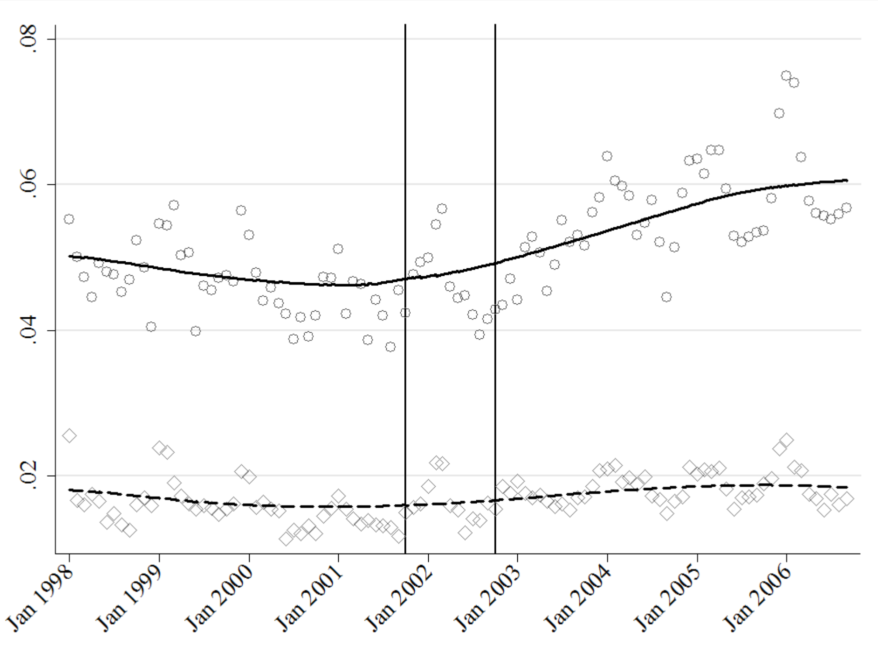
Figure 2.3: Patient's Age



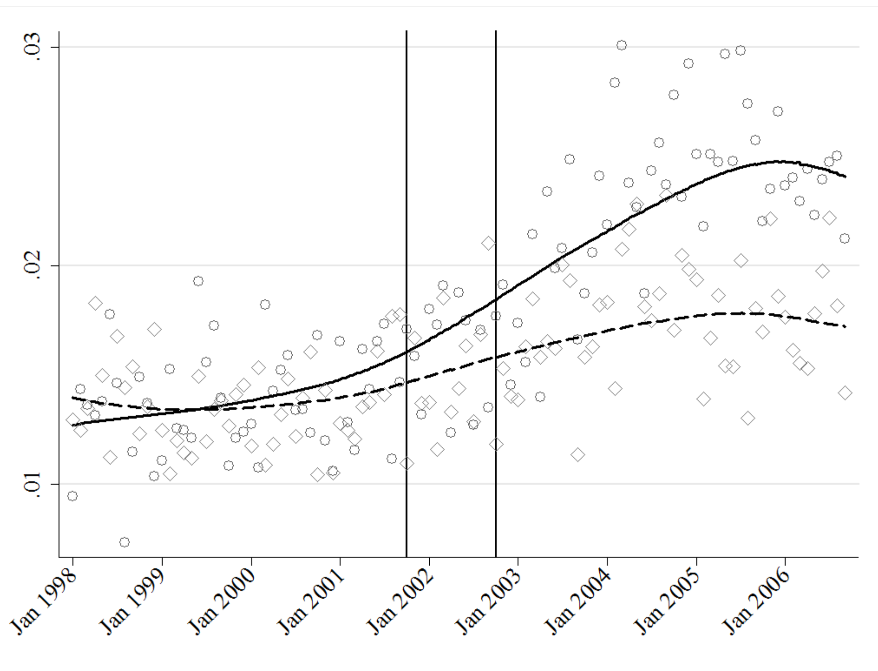
Notes: The solid line represents the ICU monthly means and the dashed line represents the non-ICU monthly means for 28 balanced panel hospitals. Source: AZ SID 1998-2006.

Figure 2.4: Hospital-acquired Pneumonia and Pressure Ulcers

Panel A: Hospital-acquired Pneumonia

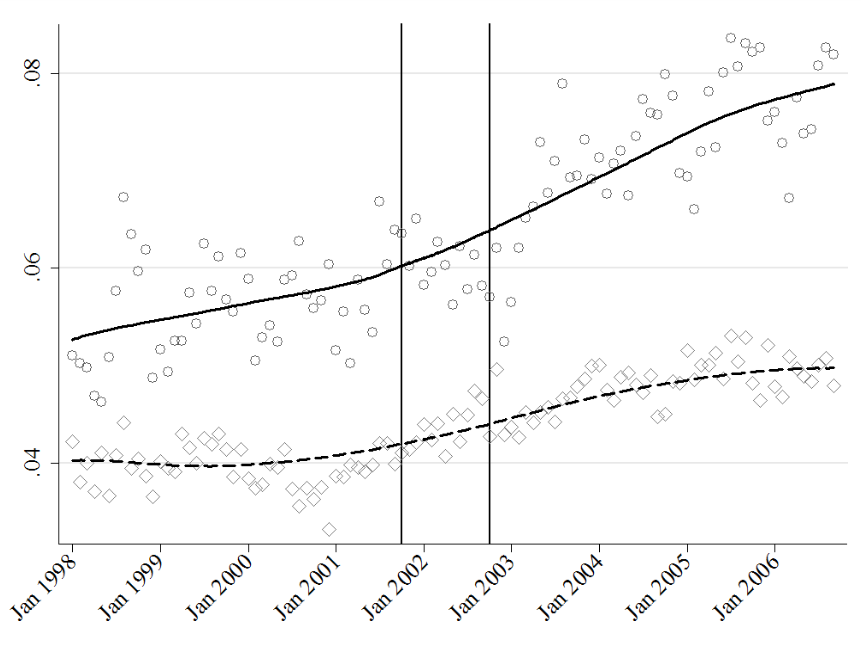


Panel B: Pressure Ulcers

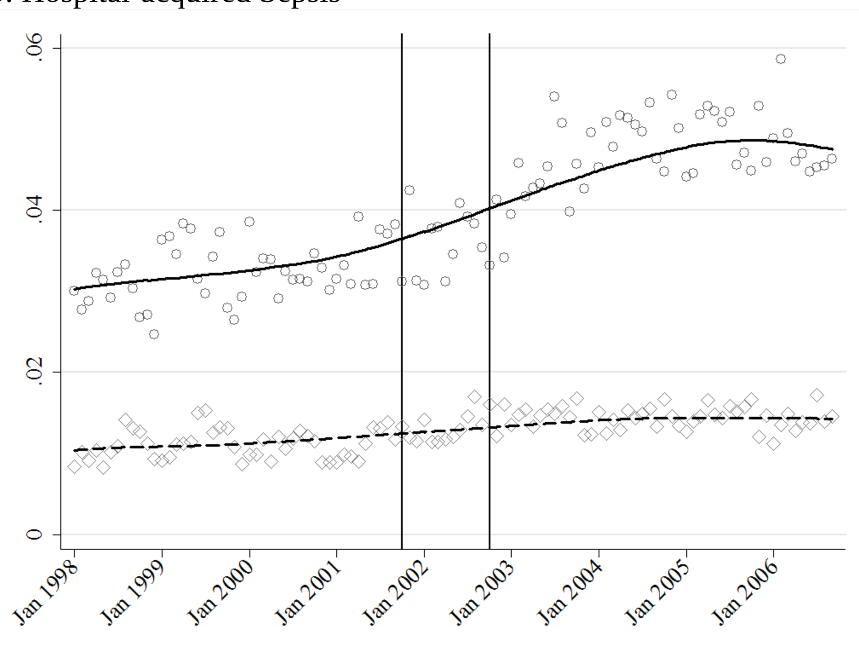


Notes: The solid line represents the ICU monthly means and the dashed line represents the non-ICU monthly means for 28 balanced panel hospitals. Source: AZ SID 1998-2006.

Figure 2.5: Urinary Tract Infection (UTI) and Hospital-acquired Sepsis
Panel A: Urinary Tract Infection (UTI)

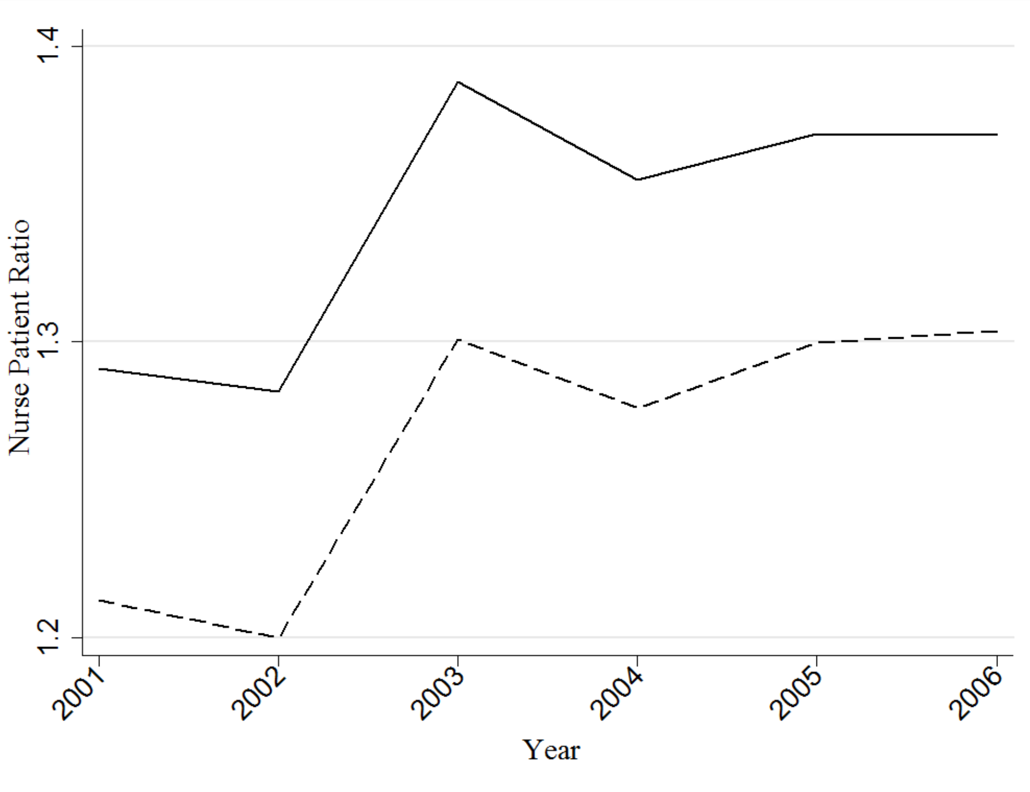


Panel B: Hospital-acquired Sepsis



Notes: The solid line represents the ICU monthly means and the dashed line represents the non-ICU monthly means for 28 balanced panel hospitals. Source: AZ SID 1998-2006.

Figure 2.6: Arizona Annual Full Time Equivalent (FTE) Nurses per Licensed Bed



Notes: Calculated as [(Nursing Hours/2080)/Licensed Beds]. Source: Arizona UAR reports.

Table 2.1: Summary Statistics Inpatient Stays

Variable	Total	Non-ICU	ICU	Difference
Age	55.15	51.72	63.52	11.80
	[21.82]	[22.34]	[17.93]	[0.03]
Female	0.613	0.668	0.480	-0.188
	[0.487]	[0.471]	[0.500]	[0.001]
White	0.703	0.689	0.738	0.049
	[0.457]	[0.463]	[0.440]	[0.001]
Arizona Residency	0.963	0.967	0.954	-0.013
	[0.188]	[0.178]	[0.210]	[0.0003]
Private Insurance	0.365	0.395	0.292	-0.103
	[0.481]	[0.489]	[0.455]	[0.001]
Medicare	0.392	0.338	0.528	0.190
	[0.488]	[0.473]	[0.500]	[0.001]
Total Diagnoses	5.48	5.05	6.60	1.55
	[2.66]	[2.62]	[2.47]	[0.003]
Total Charges	21,200.76	15,306.76	35,550.35	20,243.59
	[38,599.56]	[30,862.84]	[50,078.22]	[57.558]
Observations	2,055,439	1,456,990	598,449	

Notes: Means are reported with standard deviations in square brackets; standard errors in square brackets for ICU-(Non-ICU) difference. All differences are significant at the 1% level. Source: AZ SID 1998-2006.

Table 2.2A: Dependent Variable Summary Statistics and Pre-Post Comparisons (ICU)

Variable	Total	Pre	Post	Difference
Mortality	0.041	0.042	0.041	-0.0015
	[0.199]	[0.201]	[0.198]	[0.0005]
	(598,449)	(289,773)	(308,676)	
Length of Stay	5.300	5.410	5.1973	-0.213
	[6.643]	[6.850]	[6.441]	[0.017]
	(598,449)	(289,773)	(308,676)	
Pressure	0.0185	0.0137	0.0232	0.0095
Ulcers	[0.135]	[0.116]	[0.1506]	[0.0006]
	(228,172)	(112,898)	(115,274)	
Pneumonia	0.0514	0.0462	0.0560	0.0098
	[0.2208]	[0.2100]	[0.2300]	[0.0006]
	(582,228)	(277,127)	(305,101)	
UTI	0.0645	0.0561	0.0722	0.0161
	[0.2457]	[0.2301]	[0.2588]	[0.0006]
	(582,228)	(277,127)	(305,101)	
Sepsis	0.0397	0.0321	0.0469	0.0148
	[0.1953]	[0.1763]	[0.2114]	[0.0006]
	(369,161)	(179,219)	(189,942)	

Notes: Means are reported with standard deviations in square brackets for Pre (prior to 11/2001) and Post (after 9/2002) with observation count in brackets; standard errors in square brackets for Post-Pre difference. All differences are significant at the 1% level. Source: AZ SID 1998-2006.

Table 2.2B: Dependent Variable Summary Statistics and Pre-Post Comparisons (Non-ICU)

Variable	Total	Pre	Post	Difference
Mortality	0.008	0.009	0.008	-0.0013
	[0.091]	[0.095]	[0.088]	[0.0002]
	(1,456,990)	(708,644)	(748,346)	
Length of Stay	3.6782	3.5810	3.7702	0.1892
	[4.9697]	[4.8501]	[5.0787]	[0.0082]
	(1,456,990)	(708,644)	(748,346)	
Pressure Ulcers	0.0156	0.0134	0.0175	0.0041
	[0.1240]	[0.1152]	[0.1313]	[0.0004]
	(311,951)	(147,289)	(164,662)	
Pneumonia	0.0171	0.0157	0.0184	0.0027
	[0.1297]	[0.1243]	[0.1344]	[0.0002]
	(1,370,279)	(653,865)	(716,414)	
UTI	0.0439	0.0394	0.0480	0.0086
	[0.2048]	[0.1945]	[0.2137]	[0.0004]
	(1,370,279)	(653,865)	(716,414)	
Sepsis	0.0128	0.0110	0.0144	0.0034
	[0.1124]	[0.1043]	[0.1190]	[0.0003]
	(703,639)	(328,112)	(375,527)	

Notes: Means are reported with standard deviations in square brackets for Pre (prior to 11/2001) and Post (after 9/2002) with observation count in brackets; standard errors in square brackets for Post-Pre difference. All differences are significant at the 1% level. Source: AZ SID 1998-2006.

Table 2.3: Mortality and Length of Stay

Variables	(1)	(2)	(3)
Panel A: Mortality			
ICU Identifier	0.0331***	0.0344***	0.0216***
	(0.004700)	(0.005200)	(0.003700)
Post Identifier	-0.0013	-0.0023	-0.0018*
	(0.001400)	(0.001600)	(0.001000)
Post*ICU	-0.0001	0.0016	0.0007
	(0.005300)	(0.006000)	(0.004000)
Diagnosis Code Identifiers	No	No	Yes
Demographics	No	Yes	Yes
Hospital Fixed Effects	No	Yes	Yes
Observations	2,055,439	2,055,439	2,055,439
R-squared	0.0127	0.0151	0.1068
Panel B: Length of Stay			
ICU Identifier	1.8290***	1.8588***	2.0412***
	(0.321200)	(0.335300)	(0.246000)
Post Identifier	0.1893	0.2151	0.1573
	(0.247200)	(0.281700)	(0.171400)
Post*ICU	-0.402	-0.3671	-0.3164
	(0.397700)	(0.405700)	(0.264800)
Diagnosis Code Identifiers	No	No	Yes
Demographics	No	Yes	Yes
Hospital Fixed Effects	No	Yes	Yes
Observations	2,055,439	2,055,439	2,055,439
R-squared	0.0179	0.0268	0.2295

Notes: Linear probability estimates for a balanced panel of 28 hospitals. Robust standard errors are clustered at the hospital level. Demographic controls include gender, race, 10-year age bins, admission month, health insurance type, and whether the patient was an Arizona resident. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: AZ SID 1998-2006.

Table 2.4: Hospital-acquired Pneumonia and Pressure Ulcers

Variables	(1)	(2)	(3)
Panel A: Pneumonia			
ICU Identifier	0.0305***	0.0318***	0.0228***
	(0.002600)	(0.002800)	(0.001900)
Post Identifier	0.0027	0.0024	0.0015
	(0.001700)	(0.001700)	(0.001500)
Post*ICU	0.0071*	0.0079*	0.0075**
	(0.003800)	(0.004200)	(0.002900)
Diagnosis Code Identifiers	No	No	Yes
Demographics	No	Yes	Yes
Hospital Fixed Effects	No	Yes	Yes
Observations	1,952,507	1,952,507	1,952,507
R-squared	0.0096	0.0106	0.0595
Panel B: Pressure Ulcers			
ICU Identifier	0.0002	0.0007	0.0027**
	(0.001500)	(0.001600)	(0.001000)
Post Identifier	0.0041*	0.0028	0.0018
	(0.002000)	(0.002100)	(0.002000)
Post*ICU	0.0055**	0.0044**	0.0014
	(0.002000)	(0.001900)	(0.001300)
Diagnosis Code Identifiers	No	No	Yes
Demographics	No	Yes	Yes
Hospital Fixed Effects	No	Yes	Yes
Observations	540,123	540,123	540,123
R-squared	0.0008	0.0026	0.0321

Notes: Linear probability estimates for a balanced panel of 28 hospitals. Robust standard errors are clustered at the hospital level. Demographic controls include gender, race, 10-year age bins, admission month, health insurance type, and whether the patient was an Arizona resident. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: AZ SID 1998-2006.

Table 2.5: Urinary Tract Infection (UTI) and Hospital-acquired Sepsis

Variables	(1)	(2)	(3)
Panel A: UTI			
ICU Identifier	0.0167***	0.0177***	0.0072***
	(0.004600)	(0.005300)	(0.002200)
Post Identifier	0.0086**	0.0073**	0.0073**
	(0.004000)	(0.003400)	(0.003000)
Post*ICU	0.0075	0.0048	-0.0003
	(0.005800)	(0.006900)	(0.003500)
Diagnosis Code Identifiers	No	No	Yes
Demographics	No	Yes	Yes
Hospital Fixed Effects	No	Yes	Yes
Observations	1,952,507	1,952,507	1,952,507
R-squared	0.0026	0.0045	0.0909
Panel B: Sepsis			
ICU Identifier	0.0211***	0.0217***	0.0247***
	(0.003100)	(0.003500)	(0.002800)
Post Identifier	0.0034***	0.0042**	0.0026**
	(0.001200)	(0.001700)	(0.001200)
Post*ICU	0.0114**	0.0122**	0.0092**
	(0.004400)	(0.004500)	(0.003800)
Diagnosis Code Identifiers	No	No	Yes
Demographics	No	Yes	Yes
Hospital Fixed Effects	No	Yes	Yes
Observations	1,072,800	1,072,800	1,072,800
R-squared	0.0085	0.0106	0.1517

Notes: Linear probability estimates for a balanced panel of 28 hospitals. Robust standard errors are clustered at the hospital level. Demographic controls include gender, race, 10-year age bins, admission month, health insurance type, and whether the patient was an Arizona resident. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: AZ SID 1998-2006.

Table 2.6: Patient Outcomes by Payment Source

Variables	Mortality	Length of Stay	Pressure Ulcers	Pneumonia
Panel A: Medicaid				
ICU Identifier	0.0246***	2.6869***	0.0060***	0.0269***
	(0.0055)	(0.3456)	(0.0019)	(0.0040)
Post Identifier	-0.0009	0.4045*	0.0007	-0.0004
	(0.0009)	(0.2030)	(0.0036)	(0.0014)
Post*ICU	-0.0071	-0.6109	-0.0015	0.0070*
	(0.0051)	(0.3620)	(0.0025)	(0.0040)
Observations	350,707	350,707	68,994	333,857
R-squared	0.1595	0.2651	0.0755	0.097
Panel B: Medicare				
ICU Identifier	0.0249***	1.8158***	0.0022**	0.0244***
	(0.0046)	(0.1807)	(0.0010)	(0.0018)
Post Identifier	-0.0028	0.1744	0.0027	0.0046*
	(0.0022)	(0.2537)	(0.0027)	(0.0026)
Post*ICU	0.0045	-0.2387	0.0018	0.0074**
	(0.0049)	(0.2727)	(0.0016)	(0.0030)
Observations	805,733	805,733	289,562	787,764
R-squared	0.0965	0.2196	0.0354	0.0589
Panel C: Private				
ICU Identifier	0.0171***	2.1372***	0.0028**	0.0205***
	(0.003400)	(0.338800)	(0.001200)	(0.001700)
Post Identifier	-0.0011*	0.1034	0	0.0007
	(0.000600)	(0.128200)	(0.001200)	(0.001000)
Post*ICU	-0.0025	-0.261	0.0008	0.0054**
	(0.003700)	(0.279400)	(0.001400)	(0.002200)
Observations	749,619	749,619	150,758	693,940
R-squared	0.1072	0.2327	0.0462	0.0556

Notes: Linear probability estimates for a balanced panel of 28 hospitals. Robust standard errors are clustered at the hospital level. Estimates include hospital, diagnosis fixed effects and demographic controls (Model 3 from previous Tables). Demographic controls include gender, race, 10-year age bins, admission month, and whether the patient was an Arizona resident. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: AZ SID 1998-2006.

Table 2.7: Patient Outcomes by Demographic Group

Variables	Mortality	Length of Stay	Pressure Ulcers	Pneumonia
Panel A: Female				
ICU Identifier	0.0195***	2.0631***	0.0027*	0.0197***
	(0.0033)	(0.2976)	(0.0013)	(0.0019)
Post Identifier	-0.0015**	0.1801	0.0008	0.0014
	(0.0007)	(0.1367)	(0.0021)	(0.0013)
Post*ICU	0.0003	-0.4367	0.0019	0.0084***
	(0.0036)	(0.2697)	(0.0016)	(0.0028)
Observations	1,260,763	1,260,763	283,627	1,199,092
R-squared	0.1044	0.2402	0.0396	0.0606
Panel B: Male				
ICU Identifier	0.0241***	2.0183***	0.0029**	0.0262***
	(0.004300)	(0.230800)	(0.001100)	(0.002200)
Post Identifier	-0.0027	0.0947	0.0032	0.0014
	(0.001600)	(0.244100)	(0.001900)	(0.002200)
Post*ICU	0.0018	-0.1833	0.0006	0.0069**
	(0.004700)	(0.306500)	(0.001600)	(0.003100)
Observations	794,676	794,676	256,496	753,415
R-squared	0.1124	0.2187	0.0354	0.0618
Panel C: Minority				
ICU Identifier	0.0223***	2.2347***	0.0055***	0.0236***
	(0.005000)	(0.300900)	(0.001300)	(0.002600)
Post Identifier	-0.0015	0.1816	0.0015	0.0017
	(0.001200)	(0.135800)	(0.002400)	(0.001400)
Post*ICU	-0.0038	-0.346	0.0009	0.0079*
	(0.004600)	(0.298800)	(0.002200)	(0.004200)
Observations	610,361	610,361	137,910	576,178
R-squared	0.1319	0.249	0.0527	0.0718

Notes: Linear probability estimates for a balanced panel of 28 hospitals. Robust standard errors are clustered at the hospital level. Estimates include hospital, diagnosis fixed effects and demographic controls (Model 3 from previous Tables). Demographic controls include gender (for Panel C), race (for Panels A and B), 10-year age bins, admission month, health insurance type, and whether the patient was an Arizona resident. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: AZ SID 1998-2006.

Table 2.8: Patient Outcomes with Varying Lengths of Pre and Post Periods (Mortality, Length of Stay, Pressure Ulcers)

Variables	Mortality	Length of Stay	Pressure Ulcers
Panel A: Two Year Periods			
ICU Identifier	0.0203***	2.0605***	0.0032***
	(0.0037)	(0.2371)	(0.0011)
Post Identifier	-0.001	0.1765	0.0017
	(0.0009)	(0.1541)	(0.0018)
Post*ICU	0.0014	-0.2588	0.001
	(0.0036)	(0.2486)	(0.0014)
Observations	1,103,101	1,103,101	284,269
R-squared	0.1094	0.2254	0.0352
Panel B: One Year Periods			
ICU Identifier	0.0192***	1.9834***	0.0035**
	(0.0038)	(0.2140)	(0.0014)
Post Identifier	-0.001	0.1269	0.0008
	(0.0009)	(0.1095)	(0.0016)
Post*ICU	0.001	-0.1852	0.0003
	(0.0031)	(0.2123)	(0.0019)
Observations	572,051	572,051	146,926
R-squared	0.1144	0.229	0.0447

Notes: Linear probability estimates for a balanced panel of 28 hospitals. Robust standard errors are clustered at the hospital level. Estimates include hospital, diagnosis fixed effects and demographic controls (Model 3 from previous Tables). Demographic controls include gender, race, 10-year age bins, admission month, health insurance type, and whether the patient was an Arizona resident. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: AZ SID 2000-2004.

Table 2.9: Patient Outcomes with Varying Lengths of Pre and Post Periods (Pneumonia, UTI, Sepsis)

Variables	Pneumonia	UTI	Sepsis
Panel A: Two Year Periods			
ICU Identifier	0.0238***	0.0083***	0.0250***
	(0.0020)	(0.0028)	(0.0031)
Post Identifier	0.0022*	0.0059**	0.0028**
	(0.0012)	(0.0027)	(0.0011)
Post*ICU	0.0047*	-0.0007	0.0080**
	(0.0026)	(0.0038)	(0.0038)
Observations	1,040,905	1,040,905	564,753
R-squared	0.0565	0.0898	0.1554
Panel B: One Year Periods			
ICU Identifier	0.0240***	0.0080**	0.0260***
	(0.0021)	(0.0034)	(0.0028)
Post Identifier	0.0019	0.0042*	0.0026***
	(0.0011)	(0.0022)	(0.0008)
Post*ICU	0.0026	-0.0009	0.0043
	(0.0020)	(0.0042)	(0.0028)
Observations	540,981	540,981	291,859
R-squared	0.0578	0.0914	0.1671

Notes: Linear probability estimates for a balanced panel of 28 hospitals. Robust standard errors are clustered at the hospital level. Estimates include hospital, diagnosis fixed effects and demographic controls (Model 3 from previous Tables). Demographic controls include gender, race, 10-year age bins, admission month, health insurance type, and whether the patient was an Arizona resident. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: AZ SID 2000-2004.

Table 2.10: Patient Outcomes with Varying ICU Identification Criteria (Mortality, Length of Stay, Pressure Ulcers)

Variables	Mortality	Length of Stay	Pressure Ulcers
Panel A: ICU Charges Ratio>=0.2			
Post*ICU	-0.0008	-0.3223	0.0014
	(0.0054)	(0.3577)	(0.0025)
Observations	1,731,912	1,731,912	410,446
R-squared	0.0898	0.2357	0.0341
Panel B: ICU Charges Ratio>=0.4			
Post*ICU	0.0019	-0.3883	-0.0008
	(0.0045)	(0.7031)	(0.0023)
Observations	1,499,101	1,499,101	326,984
R-squared	0.0841	0.2446	0.0365
Panel C: ICU Charges Ratio>=0.6			
Post*ICU	0.0013	0.6909	-0.0006
	(0.0052)	(0.7935)	(0.0063)
Observations	1,460,998	1,460,998	313,013
R-squared	0.0862	0.2505	0.037

Notes: The ICU Charges Ratio is the total charges attributed to intensive care services by UB-92 divided by the total charges. Linear probability estimates for a balanced panel of 28 hospitals. Robust standard errors are clustered at the hospital level. Estimates include hospital, diagnosis fixed effects and demographic controls (Model 3 from previous Tables). Demographic controls include gender, race, 10-year age bins, admission month, health insurance type, and whether the patient was an Arizona resident. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: AZ SID 1998-2006.

Table 2.11: Patient Outcomes with Varying ICU Identification Criteria (Pneumonia, UTI, Sepsis)

Variables	Pneumonia	UTI	Sepsis
Panel A: ICU Charges Ratio>=0.2			
Post*ICU	0.0094**	0.002	0.0042
	(0.0044)	(0.0050)	(0.0057)
Observations	1,640,512	1,640,512	879,615
R-squared	0.0576	0.0956	0.1689
Panel B: ICU Charges Ratio>=0.4			
Post*ICU	0.0042	0.0016	0.002
	(0.0044)	(0.0038)	(0.0070)
Observations	1,411,055	1,411,055	731,974
R-squared	0.0457	0.0998	0.1889
Panel C: ICU Charges Ratio>=0.6			
Post*ICU	-0.0039	-0.0036	-0.0065
	(0.0051)	(0.0045)	(0.0046)
Observations	1,373,953	1,373,953	705,655
R-squared	0.046	0.1008	0.192

Notes: The ICU Charges Ratio is the total charges attributed to intensive care services by UB-92 divided by the total charges. Linear probability estimates for a balanced panel of 28 hospitals. Robust standard errors are clustered at the hospital level. Estimates include hospital, diagnosis fixed effects and demographic controls (Model 3 from previous Tables). Demographic controls include gender, race, 10-year age bins, admission month, health insurance type, and whether the patient was an Arizona resident. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: AZ SID 1998-2006.

Table 2.12: Natural Log Transformation of Hours Worked in Primary Position (NSSRN)

Variables	(1)	(2)	(3)
Post*ICU	0.1012	-0.001	-0.0078
	(0.0780)	(0.0446)	(0.0505)
Post*ICU*AZ		0.1022	0.109
		(0.0897)	(0.0928)
Post	0.1051***	0.0896***	0.0859***
	(0.0308)	(0.0202)	(0.0209)
ICU	-0.1094*	-0.0204	-0.0439*
	(0.0642)	(0.0245)	(0.0265)
AZ		-0.028	-0.0530**
		(0.0240)	(0.0237)
ICU*AZ		-0.089	-0.0655
		(0.0685)	(0.0693)
Post*AZ		0.0155	0.0192
		(0.0368)	(0.0372)
Observations	450	2,032	1,670
R-squared	0.0489	0.0228	0.0297

Notes: Includes data from Arizona, Texas, and New Mexico. Model (1) is a DID for Arizona only. Model (2) is a DDD with both TX and NM as control states and Model (3) includes only TX as a control state. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Huber-White robust standard errors are included in brackets below the coefficients. Source: NSSRN 2000, 2004.

Table 2.13: Natural Log Transformation of Percentage Time in Direct Patient Care (NSSRN)

Variables	(1)	(2)	(3)
Post*ICU	-0.0062	-0.0264	-0.0637
	(0.1363)	(0.0636)	(0.0745)
Post*ICU*AZ		0.0202	0.0574
		(0.1501)	(0.1551)
Post	-0.0409	-0.0263	-0.039
	(0.0646)	(0.0374)	(0.0450)
ICU	0.1334*	0.1771***	0.2192***
	(0.0757)	(0.0422)	(0.0437)
AZ		0.0277	0.0426
		(0.0529)	(0.0551)
ICU*AZ		-0.0437	-0.0858
		(0.0865)	(0.0873)
Post*AZ		-0.0145	-0.0018
		(0.0745)	(0.0786)
Observations	450	2,016	1,657
R-squared	0.0082	0.0108	0.0144

Notes: Includes data from Arizona, Texas, and New Mexico. Model (1) is a DID for Arizona only. Model (2) is a DDD with both TX and NM as control states and Model (3) includes only TX as a control state. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Huber-White robust standard errors are included in brackets below the coefficients. Source: NSSRN 2000, 2004.

Chapter 3

Compensating Wage Differentials in the Mining Industry

3.1 Introduction

Since Adam Smith, a basic neoclassical tenet of economics is that wages increase with unpleasant working conditions, other things equal.[1] Hence, for two otherwise equivalent jobs, the job with a higher perceived risk of death (or serious injury) must pay a higher wage in order to attract fully informed workers from the safer job into the riskier job. The difference in wages between the two jobs is commonly referred to as the compensating wage differential (for the incremental increase in risk). Prior research on compensating wage differentials generally suffers from three significant limitations. First, prior estimates were generated from cross section data with no ability to control for unobservable variables at a local labor market level. Second, most studies suffer from an aggregation bias that results when the risk from broadly defined occupations (or industries) from one data set are imputed to individuals in another data set. Third, the reporting of non-fatal injuries is subject to claims-reporting[2] bias (by both firm and workers) due to the insurance coverage of those claims.

The theory of compensating wage differentials not only applies to the supply of workers into industries and occupations with varying levels of risk, but it also includes the demand for workers under various production technologies

[1] Wealth of Nations (1776) Book I, Chapter X, Part 1.

[2] Claims reporting moral hazard generating this bias is defined as a change in the propensity to file a claim for a change in the (expected) benefit level holding risk level constant. See Butler and Worrall (1991) for a discussion of claims reporting and risk bearing moral hazard (a change in the risk bearing activities of workers). Bolduc et al. (2002) refer to them respectively as “ex-post” and “ex-ante” moral hazard. Empirical estimates for the value of a statistical life (VSL) are not subject to this potential source of bias.

that may be adopted.[3] Firms choose a riskier production technology when the resulting increased marginal revenue product more than offsets the compensating wage required to induce workers into that occupation. Faced with a marginal decline in workplace safety quality, firms could increase wages and/or safety technology to reach a new equilibrium profit maximizing level of inputs. That intra-marginal substitution decision could be impacted by local labor market monopsony power, diminishing returns to safety technology, the time required to implement new safety technology, and workers' risk preferences.

Both the risk aversion of workers[4] and cost constraints of the firm facing different safety technologies imply that wages will increase as occupational injury risk increases, other things equal (including the workers' human capital). This generates a positive wage/risk locus, a result formally developed in the hedonic models of risk-premium for wages. Along a market wage/risk locus, workers sort themselves to maximize their utility given economic and demographic conditions as well as, importantly, their own risk preferences. Less risk-averse workers will tend to be sorted into riskier workplaces, partly because they require smaller compensating wage differentials to work at riskier jobs than the more risk-averse workers. This would lead to a compensating wage differential estimate lower than what would be required to attract the median worker to assume the same level of occupational risk. Alternatively, high quality workers (whose quality may not be entirely observable to researchers) may be able to leverage their ability to gain access to safer jobs (a non-pecuniary benefit). This assortative matching could bias any estimated compensating wage differential toward zero.

This analysis contributes to the literature by employing changes in relative exposure to risk *within* the US mining industry to estimate changes in compensating wage differentials, using panel data allowing for fixed effects by county. An important advantage of studying the US mining sector is that the production technology is relatively homogeneous, and remained largely unchanged over the sample period.[5] Moreover, the longitudinal nature of the sample allows for the inclusion of fixed effects to obviate omitted variable bias.[6] The mining industry is also subject to constant inspections objectively documenting the level of occupational risk which mitigates any reporting bias and the random nature of an occupational injury. Mining operators are also required

[3] The classic paper on this topic is Rosen (1974); for a recent review of the literature, and formal discussion of these hedonic models allowing for socio-demographic differences (such as age), see Viscusi and Aldy (2003) or Aldy and Viscusi (2007).

[4] Risk neutral workers will also demand compensating wages for occupational risk if the injury results in uncompensated lost work time or pain and suffering.

[5] Past research exploiting variation in industry level risk could be identifying industry wage premiums (see Leigh (1995) and Dorman and Hagstrom (1998)).

[6] Risky jobs may be correlated with unpleasant work environments creating a biased estimate for a *risk* compensating differential.

by law to provide training enumerating site-specific occupational risks. The homogeneity of the industry, the time-varying nature of the sample, and the unique presence of occupational risk measures combine to make this data more amenable to an analysis of compensating risk differentials than many prior studies.

3.2 Previous Research

Previous research has focused on estimating wage/risk trade-offs, treating the risk of non-fatal or fatal injury as the treatment parameter. When comparing different jobs, theory predicts that there is a positive relationship between occupational risk and wages (Leeth and Ruser (2003); Aldy and Viscusi (2007); Viscusi and Aldy (2003); and Moore and Viscusi (1990)). Viscusi and Aldy (2003) review 30 papers estimating the value of a statistical life (VSL) for the US labor market. After surveying the results, they provide $7 million as the median estimate.

Viscusi and Aldy (2003) report that thirty-one studies of the US labor market and eight studies from outside the US have found statistically significant relationships between *non-fatal* injuries and wages. The implicit compensating wage varied, reflecting both differences in the measurement of risk and econometric methods employed. They found that most US studies report estimates of compensating wages for non-fatal injuries in the range of $20,000–$70,000 per injury. They also found that studies of other countries produced estimates broadly consistent with those found in the US and Canada, although international estimates tend to be lower. Overall, this literature indicates significant non-fatal risk premium for men in blue collar occupations. In addition, nine of ten studies reviewed by Viscusi and Aldy found higher wage-risk (non-fatal) premiums for union workers than for non-union workers.

Several papers have used industry-level fixed effects (see Vicusi (1978); Freeman and Medoff (1981); Marin and Psacharopoulos (1982); Dickens (1984); Leigh and Folsum (1984); Dillingham (1985); Cousineau et al. (1992); and Lott and Manning (2000)) to control for unobserved differences across industries. Three papers use individual worker fixed effects to estimate the VSL. Brown (1980) uses the NLSY Young Men's sample from 1966-71 and 1973 to estimate the compensating differential for the actuarial probability of death associated with the industry in which the individual was employed. There is a positive and significant relationship in specifications without the individual worker fixed effects but the presence of individual fixed effects reduces the compensating differential by eighty percent and it loses its statistical significance. Kniesner et al. (2010) use data from the 1993-2001 Panel Study of Income Dynamics (PSID) matched to 720 industry-occupation cell fatality risks to analyze the VSL generated from the Census of Fatal Occupational Injuries (CFOI). They estimate a VSL between $7 and $12 million. Lavetti (2010) uses exogenous variation in weather to instrument for occupational risk of a fatality for crab fishermen in the Alaskan Bering Sea between 2003 and 2009. Lavetti estimates the VSL to be $6.7 million and finds that the wage-risk tradeoff is concave in risk.

While a few studies have focused on safety outcomes in US mining—for example the effect of inspections on coal mine accidents (Kniesner and Leeth, 2004) and the effects of unions on mine safety (Morantz, 2010)—none have examined compensating wages in the US mining industry. Farber (1978) uses a bargaining model of wage determination in the US coal industry and finds unionized mine workers are relatively risk averse, suggesting their wages will be sensitive to the perceived riskiness of their environment. Fishback and Kantor (1995) find indirect historical evidence of compensating wages for coal unions by showing that coal workers' wages fell as workers' compensation insurance was introduced, state by state, from 1911 to 1939. They argue that since workers compensation provide ex-post payments for fatal and non-fatal accidents, the fact that wages fell significantly in their multivariate specifications after workers' compensation insurance was introduced indicates a reduction in the ex-ante compensating risk premiums[7] for workers.

3.3 Data

3.3.1 Quarterly Census of Employment and Wages (QCEW)

The QCEW provides a quarterly census of employment and total wages in the United States (generated from administrative data) which can be partitioned by six-digit NAICS industry codes and aggregated at successive geographic levels (county, state, and national). One limitation of the QCEW data is that information for geographic areas smaller than the state level is suppressed when there are a limited number of employers within an industry. The wage and employment data are generated from state and Federal unemployment insurance programs and covers 98 percent of all workers.

The QCEW contains the number of employees and total compensation paid to the employees by quarter. It does not contain information on the hours worked. The "average weekly wage" is published using the employment and compensation data. I collapse the quarterly observations to generate an annual measure of "average weekly wage" (in real 2010 dollars) by county for the years

[7] The risk premium identified is for both the fatal and non-fatal injury exposure. Their identification strategy does not allow for occupational risk premiums to be separately identified.

2003-2010[8] at the three-digit NAICS industry code 212 (Mining, except Oil and Gas). The difference in aggregation between the two-digit, three-digit or even four-digit industry codes represents a trade-off between reducing the probability of information being suppressed and introducing heterogeneity in the production process. To maintain relative homogeneity, I focus on the three-digit NAICS industry code.

3.3.2 Mine Safety and Health Administration (MSHA)

The Mine Safety and Health Administration (MSHA), a subsidiary of the US Department of Labor, oversees the enforcement of safety and health standards for operations that extract and process minerals including mandated annual inspections. MSHA is divided into two sections charged with oversight of both 1) coal and 2) metal and nonmetal mines respectively. Unlike OSHA, states are not allowed to opt out of MSHA oversight in favor of their own alternative. Thus, MSHA provides one federal standard across all mining operations within the US.

The MSHA Open Government Initiative has made publicly available all data on accidents, fatalities, employment (including hours worked), inspections, citations, and monetary penalties for individual mineral extraction operations (including mines, quarries etc.) starting from January 1, 2000 to the present. Matching MSHA data to the QCEW results in 124 counties (992 observations) with non-missing values for all variables of interest at the three-digit level of aggregation, the unbalanced panel consists of 451 counties with 2,046 observations. I employ the term “mining operations” to refer collectively to mineral extraction operations. The distribution of mining operations for the balanced panel by county shown in Figure 3.1 is skewed to the right, with 22.87 as the mean number of mining operations per county (on average 10 are coal mines). A few county-years have over 100 mining operations[9], while the median is 17 mining operations with a minimum of 2 mines.

The average weekly hours worked is calculated from the annual MSHA employment data by dividing the total annual hours worked by the employees and then multiplying by 50 weeks.[10] I use this information together with the QCEW

[8] The QCEW changes from the Standard Industrial Classification (SIC) to North American Industry Classification System (NAICS) after 2002. The MSHA data on safety and inspections begins in 2000. Therefore, the inclusion of twice-lagged occupational risk variables mandates that hourly wage data being on or after the year 2002. Given the change in industry classification system, I restrict the analysis to 2003-2010 in the QCEW.

[9] The highest number of mining operations in a county occur in Pike County, Kentucky which has trademarked itself as America’s Energy Capital™ (see http://americasenergycapital.org/ accessed November 19, 2011).

[10] The Bureau of Labor Statistics (BLS) maintains the assumption of a 50-week work year when calculating accident and fatality rates.

average weekly wage to generate an hourly real wage for each individual county, the distribution of which is presented in Figure 3.2 for the balanced panel, with corresponding mean hourly wage of $22.01 given in Table 3.1. Though there are a few high wage mines, with real wages of $40 or more per hour, the general distribution of real wages is approximately normal. To check the representativeness of the real hourly wage generated, I compare the MSHA/QCEW hourly wage against data from the American Community Survey (ACS). The ACS has 290 observations from the years 2005-2007[11] in the balanced panel counties at the three-digit industry aggregation level with values for hours worked, income, and the number of weeks worked in the previous year. The median real wage for this ACS sample is $22.55, very similar to the mean generated from the MSHA/QCEW sample.[12]

Following the protocol of the Bureau of Labor Statistics' Survey of Occupational Injury and Illness (SOII), I generate an accident rate per 100 workers equal to $\frac{A}{H} \cdot 200{,}000$ where A is the annual number of accidents recorded by the MSHA, and H is total annual hours worked. The 200,000 scalar is the equivalent time of 100 workers working 40 hours per week for 50 weeks per year. The distribution of accidents per 100 workers, shown in Figure 3.3, is skewed to the right, with most rates falling below three accidents per 100 equivalent full time workers. As can be seen in Table 3.1 in the rows labeled "accident rate", the balanced panel accident rate is 3.14 between 2003 and 2010. The comparable figures reported for the national mining industry by the SOII in 2003 and 2009 are 4.6 and 3.2, respectively, both within a standard deviation of the values given in Table 3.1.[13] The fatality rate per 100 worker for balanced panel is 0.01—compared to the 0.02 fatality rate for metal and nonmetal production and the 0.03 coal mining fatality rate from 2001-2005 posted by MSHA.[14] Thus, the risk of occupational injury in the balanced panel is a reasonable approximation to the industry as a whole.

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[11] Prior to 2005, the ACS does not identify county. After 2007, the number of weeks worked in the previous year is only reported as six intervals instead of a continuous measure.

[12] Given that the dependent variable is the hourly wage, the degree of wage rigidity inherent in the mining industry could potentially limit the responsiveness of the hourly wage to a change in occupational risk. Only 8.8 percent of employees in the mining industry as a whole (NAICS 21) are represented by a union (see http://www.bls.gov/news.release/union2.t03.htm (accessed November 19, 2011)).

[13] The 2009 value was found at http://www.bls.gov/iif/oshwc/osh/os/ostb2435.pdf (accessed September 30, 2011). The 2003 value was found at http://www.bls.gov/iif/oshwc/osh/os/ostb1355.pdf (accessed September 30, 2011).

[14] Found at: http://www.msha.gov/mshainfo/factsheets/mshafct2.htm (accessed September 30, 2011).

The occupational injury rates utilized in this analysis are internally generated. They represent county-specific accident rates and citations. Past research has used aggregated fatal and non-fatal occupational injury rates generated from national industry or occupation injuries. The industry level of aggregation induces measurement error since it fails to measure site-specific occupational risk that an individual worker faces.[15] This measurement error will bias compensating differential estimates toward zero. Within an industry, job risk will also vary by occupation. Focusing on county-level risk for one industry will minimize the measurement error introduced by aggregation.

Mining inspections by MSHA occur with greater frequency and regularity than OSHA inspections for non-mining industries (Kniesner and Leeth, 2004); MSHA conducts inspections *at least* twice a year for operations above ground and at least four times a year for operations below ground. In addition, MSHA inspectors have more autonomy than OSHA inspectors; they are authorized to enter the mining operation without a warrant and are prohibited from providing notice of an impending inspection. Any violations that are encountered must be cited and inspectors have the power to halt operations until the risk posed by a violation has been removed. In a post-inspection conference, inspectors and mine operators may discuss the citations and resolve the citation onsite. Regardless of the citation's final resolution, it is included in the MSHA data. Operators have the right to appeal any citation that is reported.[16] MSHA data includes the number of initial citations and the penalty initially proposed in association with the citation. The proposed penalty assigns a dollar value to the severity of the citations (measured in $1,000 units for this analysis).

MSHA Code of Federal Regulations 30 Part 46 mandates the training and retraining of miners, including site-specific risks.[17] Mining operations are required to have a written training protocol and document that each individual miner receives the appropriate training. MSHA requires that "False certification is punishable under section 110(a) and (f) of the Federal Mine Safety and Health Act" be printed on all training certification forms. New miners are required to receive at least four hours of training which includes "instruction on the recognition and avoidance of electrical hazards and other hazards present at the mine" prior to being permitted to do any work. They must receive an additional 20 hours within 90 days of beginning work. Experienced miners that are newly

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[15] Viscusi and Aldy (2003) point out that the use of industry level risk measures could create correlations for individual residuals within industries.

[16] Per the Department of Labor, http://www.dol.gov/compliance/guide/msha.htm (accessed September 30, 2011).

[17] See http://www.msha.gov/30cfr/46.0.htm (accessed November 19, 2011) for the regulation. MSHA provides an Instructor's Guide with Lesson Plans for 30 CFR Part 46 at http://www.msha.gov/training/part46/ig37.pdf (accessed November 19, 2011).

hired by the company must receive the same training although there is not a minimum amount of hours required. Miners must also receive safety training when assigned a new task. Miners who perform the same task in a different mine are required to serve “site-specific hazard awareness training”. All miners are required to receive eight hours of training annually that “must include instruction on *changes at the mine* that could adversely affect the miner's health or safety” [emphasis added]. Monforton and Windsor (2010) exploit a natural experiment in which the imposition of the training requirements were only later imposed on a subset of mining workers in 1999 to evaluate the training's impact on injury outcomes. They find that the training requirements decrease the rate of permanent disabling injuries. The MSHA requirements constitute a structural transmission of the occupational risk that miners face. Ashenfelter (2006) points out that the theory of compensating differentials assumes fully informed agents trade off increased risk for increased wages. Given the site-specific training and the Mining Act’s requirement that all injuries be made public, this assumption is more realistic in the mining industry than in other settings.

Viscusi and Aldy (2003) point out that “an ideal measure of on-the-job fatality and injury risk would reflect both the worker's perception of such risk and the firm's perception of the risk”. The regularity of inspections suggests that MSHA citations provide an important *objective* measure of mine safety that could be *perceived* by workers and firms, independent of the number of realized accidents. In contrast, *realized* mine safety in the form of accidents might represent a biased estimate of the underlying true occupational risk depending upon the stochastic nature of occupational injury. The rate of *reported* accidents is also subject to bias resulting from moral hazard behavior on the part of both workers and employers. For example, Boone, van Ours and Zweimuller (2011) find that workers are less likely to file a claim in economic downturns. Claims reporting and risk-bearing moral hazard under workers’ compensation or employer provided disability programs, both by the firm and the worker, may over or under report accidents.

Hence, citations are a useful barometer of potential workplace risk. The number of citations provides a single index of occupational risk whereas past research has focused separately on the risk of fatal and non-fatal injury.[18] Lagged citations are an ex-ante measure of occupational risk, reasonably available during any wage determination process. As Morantz (2008) notes, the Mining Act (upon which the MSHA is founded) “requires employers to report, and make publicly available, each and every mining accident, injury, and fatality in real time”.

[18] Viscusi and Aldy (2003) point out that the high correlation between fatal and non-fatal injury rates complicates their joint estimation. Failure to include one could bias the coefficient of the other upward.

While fatalities are free of claims reporting moral hazard bias by either the firm or the worker, fatalities are relatively rare events (there are 85 fatalities in 58 separate incidents in the balanced panel). As such, they may be too idiosyncratic (representing extreme realizations of any accident generating process and its error term) to provide consistently useful information on workplace risk at the mine level: the noise-to-signal ratio is likely large. The citations available on the MSHA data are not the final number of citations for each county, but the *initial* number identified by the inspectors (and hence, likely the initial number identified by the mine workers as well); corrections or appeals typically follow initial reports. Table 3.1 indicates that there are 457.43 initial citations per county per year in the balanced sample.

The MSHA records the total years of mining experience for miners involved in accidents but no other demographic information. Data from the ACS shows little variation in the education of miners: 166 have 12 years of education and only 32 have two years of college or more. With the homogeneous levels of education among miners, job tenure is likely to be of greater importance in determining wages relative to other professions. The mean total experience by year for workers experiencing an accident ranges between 10 and 12 years. By comparison, the mean ACS age for miners is 46.5. Hence, the MSHA total experience measure is lower than would be expected given the ACS age, if miners never changed occupations.[19]

Table 3.1 provides a test of the difference between those counties in the balanced and unbalanced panels. Those counties in the balanced panel have more mining operations, employees fatalities, and citations (because counties with identifiable small operations in the three digit-sample are excluded, yielding counties with more and larger mining operations). The real wage is not statistically significantly different between the balanced and unbalanced sample, and the unbalanced panel has a higher accident rate. Given the observable differences between the balanced and unbalanced panels, I focus on the balanced panel results.

3.4 Fixed Effects Estimation

3.4.1 Baseline Results

To estimate the effect of occupational risk on the real hourly wages I sequentially include county-level accident rates and number of citations (and their

[19] An alternative explanation could be that the mean age in the ACS data might overstate the age of workers most exposed to risk if less experienced workers are disproportionately assigned to more dangerous tasks.

respective lagged values). The log transformation of real wages (w_{it}) for county *i* at time *t* is regressed on X_{it-j} which includes a set of occupational risk measures along with their once and twice-lagged values, f_i are county fixed effects, and τ_t are year fixed effects.

$$\ln(w_{it}) = \sum_{j=0}^{2} \beta_{xt-j} X_{it-j} + \tau_t + f_i + \varepsilon_{it} \tag{3.1}$$

Standard errors are clustered at the county level (124 clusters).

Table 3.2 reports the results (β_{xt-j}) of the Equation 1 estimation. There is no statistically significant relationship between the accident rate (or its lagged values) and the log transformation of the real hourly wage. With both the accident rate and the number of citations included, the lagged number of citations and twice-lagged citations are correlated with the real wage. The positive coefficient on the first lag and the negative coefficient on the second lag suggest that the *change* in the number of citations from time period *t*-2 to *t*-1 is positively related to the wage. Because the dependent variable is the mean hourly wage which might be slower to change with occupational risk than the marginal wage, the coefficients are likely biased toward zero. The lack of a significant correlation for the accident rate could be the result of the inclusion of county fixed effects, which accounts for much of the relationship between accidents and wages in the data.

To evaluate the impact of citations with a broader set of occupational risk I add more controls: M_{it} is the total number of mining operations within the county (a change in mining operations will change the number of citations), C_{it} is a vector including the number of coal mines within the county (the number of coal mines are separately identified from other mining operations given the more frequent number of inspections mandated under MSHA's regulations), the number of miners in a county-year, as well as whether a fatality occurred in the county in the previous year, and the initial proposed penalty (monetary value) associated with the number of citations to Equation 3.1:

$$\ln(w_{it}) = \sum_{j=0}^{2} \beta_{xt-j} X_{it-j} + \beta_M M_{it} + \beta_C C_{it} + \tau_t + f_i + \varepsilon_{it} \tag{3.2}$$

Table 3.3 reports the estimation of Equation 3.2. Model (1) (reported in Column 1) evaluates whether the accident rate remains statistically insignificant with the greater set of controls (and absent the number of citations). The number of citations is added in Model (2). To control for the *severity* of the citations, Model

(3) includes the initial proposed penalty in dollar terms. The sign and magnitude of the coefficients on the first and second lagged values of citations remain stable (and consistent with Table 3.2) throughout the specifications.

Because observations are averaged local labor market outcomes (for miners in their respective county), bias may be introduced through a changing composition in labor force skill. Assuming workers face an exogenous probability of suffering an occupational accident, the total mining experience recorded by MSHA represents the industry-specific human capital and relevant measure of skill mix. Given the mean ACS age and the evidence that accident frequency falls with age (Butler, Hartwig, and Gardner, 1997), the smaller total experience measured in the MSHA, may be a biased measure of the industry specific human capital of the mean worker in the county-year. Under the less restrictive assumption that the level of bias is fixed within a county, and that any changes in bias over time are constant across counties each year, the returns to total experience will not be biased. Thus Model (4) includes total experience (*TE*), as seen in Equation 3.3:

$$\ln(w_{it}) = \sum_{j=0}^{2} \beta_{xt-j} X_{it-j} + \beta_M M_{it} + \beta_C C_{it} + \beta_{TE} TE_{it} + \tau_t + f_i + \varepsilon_{it} \tag{3.3}$$

The β_{TE} coefficient is positive and significant at the ten percent level in a one-tailed test. The lack of a significant correlation for most of the independent variables in Models 1-4 is, again, partially the result of the inclusion of county fixed effects. Given the inclusion of county fixed effects, the coefficients represent marginal changes in the real wage due to deviations from the county-level baseline occupational risk.

The relatively small coefficients for mining operations and citations should be interpreted within the relative magnitude of the variables of interest. Focusing on Model 4 results, the lagged number of employees has a negative impact on wages (supply side), significant at the ten percent level. Moving from the 25th to the 75th percentile of the number of employees represents an increase of 600 employees and a 4.44 percent decrease in wages. The lagged number of citations has a positive impact on wages, significant at the five percent level, even after controlling for the proposed monetary penalty (Models 3 and 4) and total experience (Model 4). The relatively small coefficient on the lagged citations represents a 3.51 percent increase in wages when moving from the 25th to the 75th percentile of citations, as this increase represents an increase of 444.5 citations

(444.5 *0.000079=0.0351). [20] The negative coefficient on the second lag of the citations (significant at the ten percent level) indicates that the change in citations from time *t*-2 to *t*-1, even more than the level of citations, impacts the wage. By the same measure, the coefficient on the lagged difference in citations yields a more modest 1.04 percent increase. The human capital measure used in the wage regressions, total experience, is statistically significant (in one tailed tests) and positive. The total experience coefficients implies that moving from the 25th to the 75th percentile of total experience (an increase of 7.85 years) represents a very modest 2.19 percent increase in wages.[21]

To test whether the results are driven by extreme values in the citation change variable, I interact the *change* in citations from *t*-2 to *t*-1 ($\Delta Citation_{t-1}$) with an indicator variable equal to one if the change in citations falls above the 75th percentile (57.5) of the change in citation distribution.

$$\ln(w_{it}) = \beta_1 \Delta Citation_{t-1} + \beta_2 \left(\Delta Citation_{t-1} \times 1(\Delta Citation_{t-1} \geq 57.5) \right) + f_i + \varepsilon_{it} \quad (3.4)$$

Thus β_2 represents a test of whether extreme positive citation "shocks" drive the observed relationship between citations and real wages. The results of Equation 3.4 are reported in Table 3.4. The significance and magnitude of β_1 is reduced by the inclusion of the citation interaction term (compare Columns 1 and 2 in Table 3.4) in models without year effects. With the inclusion of year fixed effects (Column 3) and the number of mining operations in a county (Column 4), the change in citation coefficient is similar in magnitude to the coefficient of the first lagged citation in Table 3.3. Although statistically insignificant, the sign of β_2 is unexpectedly negative with the full set of controls (Column 4).

3.4.2 Wage-Risk Linear Tradeoff

Past research has found a concave wage-risk tradeoff (see Viscusi (1981); Olson (1981); Dorsey and Walzer (1983); Leigh and Folsum (1984); and Lavetti (2010)). Equation 3.5 includes the squared change in citations from time *t*-2 to *t*-1 ($\Delta Citation_{t-1}$):

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[20] A replication of the analysis, using only those county-years where the hourly wage falls between \$10 and \$30, yields similar qualitative results-reported in Table B.1 of Appendix B. The first lagged citation coefficient remains significant and of similar magnitude.

[21] The results from the unbalanced panel are reported in Appendix B: Table B.2 and are consistent with the patterns seen in Table 3. Appendix B, Table B.3 reports the unbalanced panel results when restricting the analysis to real hour wages between \$10 and \$30.

$$\ln(w_{it}) = \beta_1 \Delta Citation_{t-1} + \beta_2 \left(\Delta Citation_{t-1}\right)^2 + f_i + \varepsilon_{it} \tag{3.5}$$

Table 3.5 reports the results with an increasing set of controls as indicated (year fixed effects, number of mining operations, and the full set of controls included in Table 3.3). The hypothesis that the compensating wage differential is linear ($\beta_2 = 0$) in occupational risk cannot be rejected.

3.4.3 Monopsony Power

To the degree that mining operations have labor market power and miners have limited ability to translate their acquired human capital to find alternative employment, monopsony power might limit the degree to which a change in occupational risk translates into higher compensation. Equation 3.6 includes the interaction term of citations with the number of mining operations by county-year.

$$\ln(w_{it}) = \beta_1 \Delta Citation_{t-1} + \beta_2 \left(\Delta Citation_{t-1} \times Mines\right) + \beta_3 Mines + f_i + \varepsilon_{it} \tag{3.6}$$

If there is monopsonistic power in mining, then the citation effect will likely be diminished where there are fewer mines, and the β_2 interaction will be positive. The β_2 interaction coefficient is zero and insignificant (reported in Table 3.6). The lack of an empirical result may be driven in part by the QCEW suppression of counties with few employers--those most likely to exhibit monopsonistic power.

3.5 Instrumental Variable (IV) Compensating Differential Estimate

As safety citations seem to be a relatively bias-free measure of mining risk, and a robust predictor of compensating wages, this suggests an instrumental variable (IV) strategy for consistently recovering the compensating wage for injury risk. That is, to avoid the bias in injury rates from claims reporting moral hazard, instrument injury risk using citations as an unbiased index of job safety. Such an IV strategy will also deal with the measurement error bias inherent in prior studies of workplace risk.

To estimate compensating wages for non-fatal accident risk, I employ the lagged safety variables as IVs for the current accident rate. To allow for clustering at the local labor market (county) level, I employ Generalized Method of

Moments (GMM) IV estimation.[22] Although the IV estimate will be consistent in the presence of within group correlation, the standard errors are not.[23]

The high correlation between county fixed effects and wages, and county fixed effects and accident rates, removes most of the partial correlation between wages and accidents in the fixed effects models of Tables 3.2 and 3.3. To examine the predictive power of the lagged safety measures (accident rate, initial citation count, and proposed penalties) upon the contemporaneous accident rate controlling for the current number of mines (M_{it}) and employees (E_{it}) without county fixed effects, I estimate the following model in Column 1 of Table 3.7:

$$Acc_{it} = \beta_1 Acc_{it-1} + \beta_2 Citations_{it-1} + \beta_3 Penalty_{it-1} + \beta_M M_{it} + \beta_E E_{it} + \tau_t + \varepsilon_{it} \quad (3.7)$$

and then add county fixed effects (f_i) in Column 2. All estimates include robust standard errors clustered at the county level.

Columns 1 and 2 represent the first-stage test of the association of the lagged safety measures (lagged accident rate, lagged citations, and lagged proposed penalties) on the current accident rates. In Column 1, without the fixed effects to remove all county specific commonalities, the lagged accident rate and citations are significantly related to the contemporaneous accident rate. Economies of scale in the provision of safety, explains the significant negative effect that employees have on the accident rate, holding mining operations constant.

Likewise, the more mining operations there are in a county for a given number of employees, the greater the competition among firms for miners and the more likely they will complete for labor with both pecuniary and non-pecuniary benefits, including safer working conditions. The number of coal mining operations is positively related to an increased accident rate. Lagged citations are positive, possibly indicating the effects of unmitigated risks on the likelihood of future accidents, even after controlling for current accidents.

As expected, the inclusion of fixed effects mitigates the signal that the lagged accident rate provides, as well the effect of mining operations and number of mining employees in the county. The lagged citation effect also is reduced and is no longer significant. Interestingly, the lagged proposed penalty becomes significant at the one percent level, even with the county fixed effects, and reduces the subsequent accident rate. Indeed, the coefficient on the penalty

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[22] I utilize the Stata ado file ivreg2 for the cross-section analysis and xtivreg2 for the fixed effects estimation clustering at the county level.

[23] See Baum, Schaffer and Stillman (2003, 2007). A White (1980) test rejects the null hypothesis of homoscedastic error term with a p-value of 0.0003 prohibiting the use of the more efficient linear 2SLS estimation.

variable more than quadruples in magnitude in the present of the county fixed effects.

The weak identification test statistics are presented in the fourth row from the bottom in Table 3.7 for the cross section model (Column 4) and for the fixed effects model (Column 6). The Kleibergen-Paap F statistic for the weak identification is 27.253 (4.956) for the cross section (fixed effects) model. Thus, the weak identification test is only passed for the cross-sectional model. The presence of three instruments allows for over-identification tests using the Hansen J statistic. Under the null hypothesis that the instruments are valid, the p-value (reported under the corresponding test statistic) for the overidentification test in the cross section (fixed effects) is 0.041 (0.155).

The IV-GMM cluster robust estimates are reported in Columns 4 and 6 of Table 3.7. The accident rate reports the number of accidents per the equivalent of 100 workers. Thus a one unit increase represents a one percent increase in exposure to the miners. With a coefficient of 0.0272 (statistically significant at the five percent level), at the mean real wage of $22.01, this 2.72 percent increase represents $1,197.34 over 2,000 hours (40 hours for 50 weeks in a year). Among one hundred workers, the total value of an accident in terms of a compensating wage is equal to $119,734 (100 multiplied by the one percent incremental increase in exposure to an occupational accident).

The mining compensating wage for injury risk is slightly higher that the range that Viscusi and Aldy (2003) report ($20,000-$70,000), though inflation brings this estimate within range of their top value. The higher compensating differential reported here for an accident may be explained by the relative severity of mining accidents. During 2003 to 2010, 58.54 percent of accidents reported to MSHA resulted in restricted or lost time. The median days lost was 22 and the median days restricted was 11.[24] In 2004, for example, the incidence rate for days away from work for non-fatal injuries for underground coal mines was 5.4 per 100 workers, compared to 1.4 overall in the private sector (Bureau of Labor Statistics, 2005).

Given the rejection of the null hypothesis for the instruments in the over-identification test, and consistent with suggestions by Murray (2006), I reiterate the IV-GMM analysis using alternate combinations of the instruments. Table 3.8 presents the results using all three lagged safety measures (Model 1), the lagged accident rate and citations (Model 2), the lagged accident rate and proposed penalties (Model 3), and the lagged citations and proposed penalties (Model 4). Given the strong association of the lagged accident rate to the contemporaneous accident rate in Column 1 of Table 3.7, it is not surprising that the first three IV-GMM estimates in Table 3.8 which include the accident rate pass the weak

[24] Author's calculations using MSHA accident data from 2003-2010.

instrument test. When the lagged accident rate is no longer included in Column 4, the instruments are only weakly correlated and the resulting coefficients are dramatically different than the alternative models. The combination of the lagged accident rate and the proposed penalties pass the overidentification test and generate a coefficient of 2.43 percent (significant only at the ten percent level). The coefficient yields a slightly more modest estimate of $106,528 compensating wage differential for a non-fatal occupational injury.

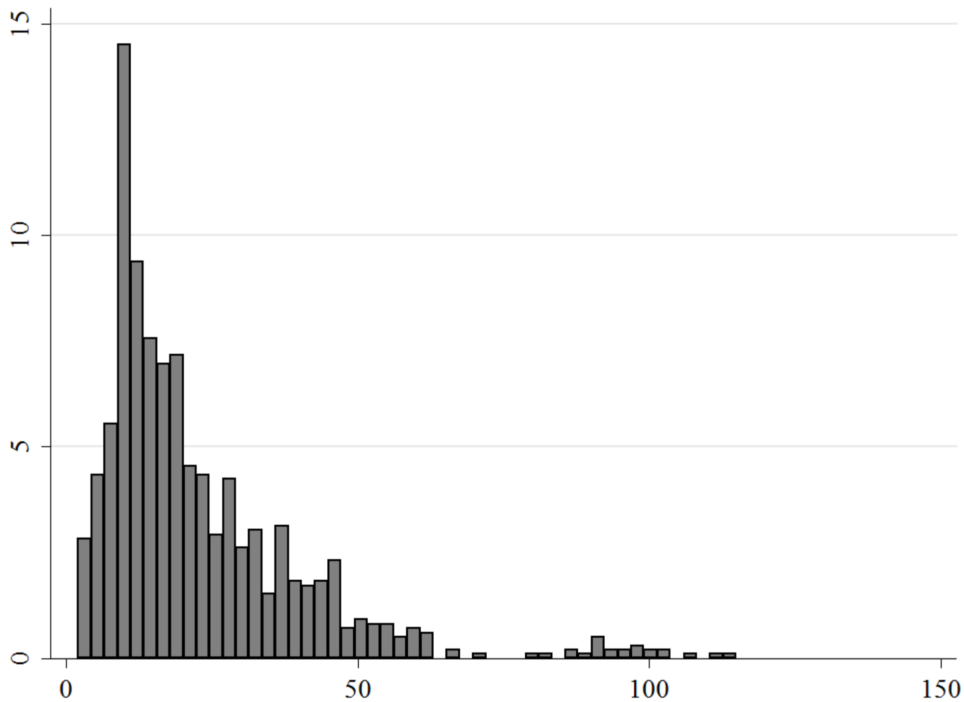
3.6 Conclusion

In this chapter, I examine compensating wages for miners employing a balanced panel of county-year mining labor markets generated from QCEW and MSHA data. I find evidence that lagged citations increase current wages, even with county fixed effects, providing evidence of compensating wages in the mining industry for increases in perceived risk.

To reconcile my estimates with the prior literature that has identified risk only through cross-sectional analysis, without the controls for localized labor markets employed here, I instrument accident rates with lagged accident rates, citations and penalties (controlling for employment, and number of mining operations), and find statistically significant effects of accidents on wages when I restrict the analysis to cross-sectional variation (without fixed effects), I find results similar to prior estimates in the literature. Fixed effects, however, removes the significance of the accident rate on the compensating wage, whether it is estimated by OLS or IV.

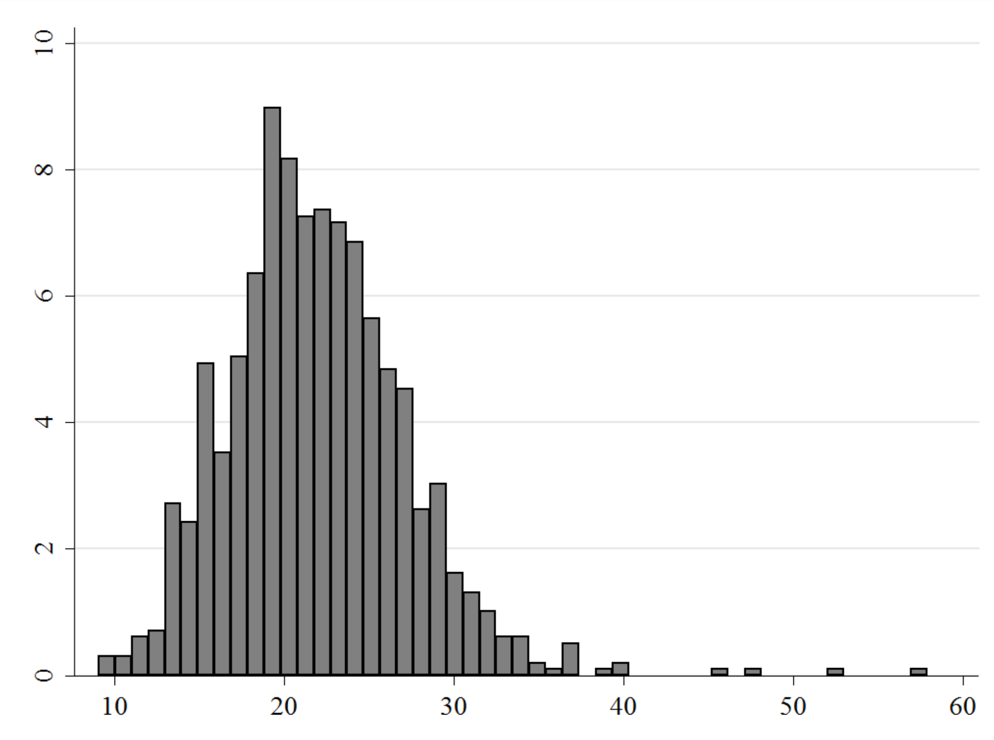
The analysis presented here also provides important information hitherto absent in the public policy analysis of mining safety: that safety citations directly affecting compensating wages, and that the value of a non-fatal injury in mining is large relative to compensating differentials found in studies of non-mining industry. It is difficult to know whether these results would significantly alter that negative conclusions of the cost-effectiveness of MSHA safety efforts found by Kniesner and Leeth (2004) in their dynamic simulations of MSHA inspections, but at least it could have helped inform the plausibility of some of their estimation models. That compensating wages associated with MSHA citations are so high suggests significant benefits to mining firms from increases in mining safety that reduces citations. Whether the social costs of the MSHA system that generates those citations are greater than the potential reduction in the compensating wages benefits, is beyond the scope of this paper.

Figure 3.1: Mining Operations in County Histogram



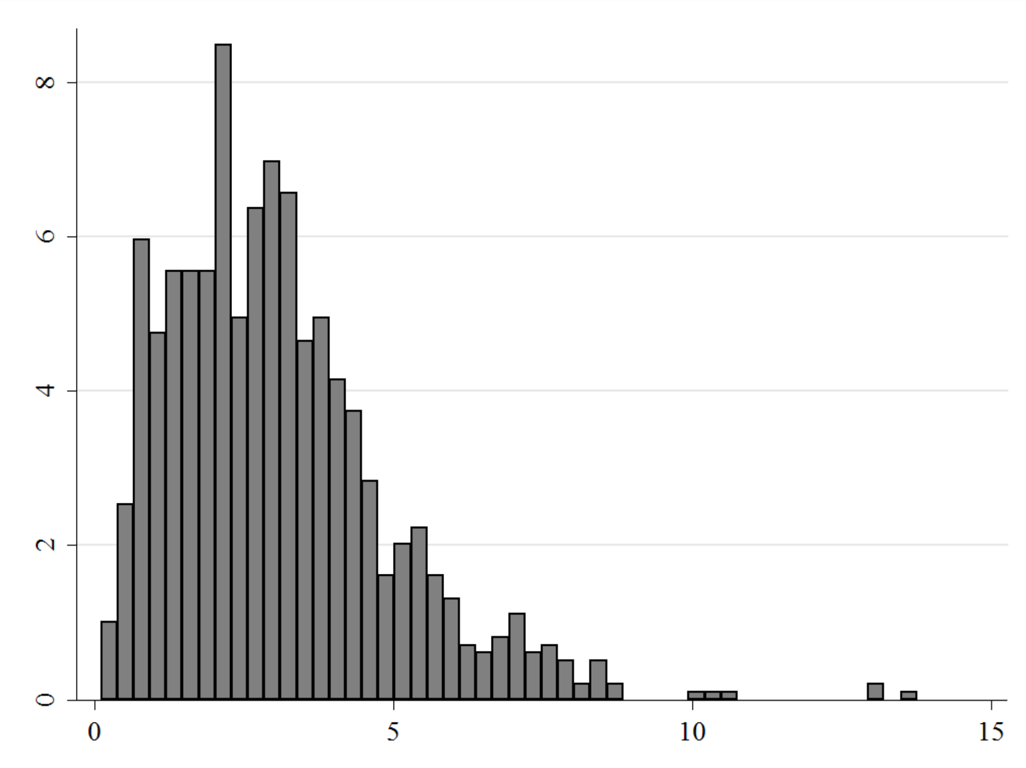
Notes: Percentages are reported on the vertical axis. Includes balanced panel observations for NAICS 212. Source: MSHA 2003-2010.

Figure 3.2: Real Hourly Wage Histogram



Notes: Percentages are reported on the vertical axis. Includes balanced panel observations for NAICS 212. Source: QCEW 2003-2010, MSHA 2003-2010.

Figure 3.3: Accident rate (per 100 workers) Histogram



Notes: Percentages are reported on the vertical axis. Includes balanced panel observations for NAICS 212. The Accident rate is the number of accidents per 100 equivalent workers. Accident rate= $\frac{A}{H} \cdot 200{,}000$ where A is the annual number of accidents recorded by the MSHA, and H is the annual hours worked. Source: MSHA 2003-2010.

Table 3.1: Summary Statistics by Balanced Panel Status

Variable	Total	Balanced	Unbalanced	Difference
Real Hourly Wage	21.88	22.01	21.77	0.24
	[7.13]	[5.28]	[8.52]	[0.32]
Employees	438.58	676.36	214.79	461.58***
	[565.52]	[695.98]	[248.69]	[22.85]
Mining Operations in	16.02	22.87	9.58	13.30***
County	[15.04]	[17.72]	[7.63]	[0.60]
Accident rate (per 100	3.21	3.14	3.28	-0.15
workers)	[3.17]	[2.28]	[3.83]	[0.14]
Fatality	0.059	0.086	0.033	0.053**
	[0.508]	[0.640]	[0.337]	[0.022]
Citations	271.12	457.43	95.77	361.66***
	[531.17]	[701.94]	[144.07]	[22.10]
Proposed Penalties	131.52	226.26	42.34	131.51***
($1,000)	[361.09]	[475.09]	[156.14]	[21.32]
Total Experience	10.45	11.08	9.85	1.22***
	[6.73]	[5.69]	[7.53]	[0.30]
Observations	2,046	992	1,054	
Counties	451	124	327	

Notes: Means are reported with standard deviations in parentheses; standard errors in parentheses for Balanced-Unbalanced difference. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: QCEW 2003-2010, MSHA 2003-2010.

Table 3.2: Ln (Real Hourly Wage) Regression

Variables	(1)	(2)	(3)	(4)	(5)	(6)
Accident rate	-0.002971	-0.003063	-0.003066	-0.002944	-0.002966	-0.002905
	(0.002896)	(0.002991)	(0.002968)	(0.002971)	(0.002968)	(0.002963)
Accident rate (first lag)		0.003934	0.004068	0.00423	0.004287	0.004377
		(0.004464)	(0.004237)	(0.004232)	(0.004214)	(0.004167)
Accident rate (second lag)			-0.001765	-0.001634	-0.001576	-0.001611
			(0.002694)	(0.002731)	(0.002712)	(0.002757)
Citations				0.00002	-0.000003	-0.00001
				(0.000020)	(0.000018)	(0.000019)
Citations (first lag)					0.000037*	0.000068***
					(0.000020)	(0.000024)
Citations (second lag)						-0.000048**
						(0.000021)
Observations	992	992	992	992	992	992
R-squared	0.748075	0.748765	0.748955	0.749218	0.749747	0.75055

Notes: Estimation based upon 992 balanced panel observations for NAICS equal to 212. All estimations include county and year fixed effects. Standard errors are clustered at the county level (124 clusters). Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: QCEW 2003-2010, MSHA 2001-2010.

Table 3.3: Ln (Real Hourly Wage) Regression, Full Controls

Variables	(1)	(2)	(3)	(4)
Citations		-0.00002	-0.00001	-0.000006
		(0.000022)	(0.000045)	(0.000046)
Citations (first lag)		0.000080***	0.000081***	0.000079***
		(0.000024)	(0.000027)	(0.000027)
Citations (second lag)		-0.000040*	-0.000043**	-0.000040*
		(0.000020)	(0.000021)	(0.000022)
Accident rate	-0.00304	-0.002889	-0.002932	-0.002774
	(0.002982)	(0.002990)	(0.002980)	(0.002985)
Accident rate (first lag)	0.004001	0.004191	0.004185	0.00458
	(0.004269)	(0.004211)	(0.004189)	(0.004407)
Accident rate (second lag)	-0.00201	-0.001867	-0.001869	-0.001903
	(0.002754)	(0.002781)	(0.002826)	(0.002865)
Fatality Indicator	-0.004164	-0.002922	-0.002515	-0.004329
	(0.014815)	(0.014799)	(0.015022)	(0.015360)
Fatality Indicator (first lag)	-0.021921	-0.02208	-0.021445	-0.023193
	(0.017030)	(0.017049)	(0.017008)	(0.016938)
Fatality Indicator (second lag)	-0.014312	-0.018513	-0.018021	-0.020534
	(0.016209)	(0.016422)	(0.016988)	(0.017077)
Proposed Penalty			-0.00001	-0.000009
			(0.000042)	(0.000043)
Proposed Penalty (first lag)			-0.000004	-0.000004
			(0.000025)	(0.000024)
Proposed Penalty (second lag)			0.000009	0.00001
			(0.000029)	(0.000029)
Mining Operations in County	0.004128	0.003998	0.003956	0.003995
	(0.003935)	(0.003926)	(0.003998)	(0.003901)
Coal Mining Operations	0.000467	0.000473	0.000274	0.000191
	(0.004217)	(0.004222)	(0.004208)	(0.004132)
Employees	0	0.000005	0.000005	0.000015
	(0.000047)	(0.000050)	(0.000050)	(0.000048)
Employees (first lag)	-0.000039	-0.000081**	-0.000082**	-0.000074*
	(0.000038)	(0.000040)	(0.000040)	(0.000039)
Employees (second lag)	-0.000061	-0.00003	-0.000028	-0.00004
	(0.000052)	(0.000057)	(0.000058)	(0.000058)
Total Experience				0.002789#
				(0.001838)
Observations	992	992	992	992
R-squared	0.75258	0.754332	0.754412	0.756823

Notes: Estimation based upon 992 balanced panel observations for NAICS equal to 212. All estimations include county and year fixed effects. Standard errors are clustered at the county level (124 clusters). Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels in two tailed tests. Tests of significance for total experience are made at the 10%(#), and 5%(##), levels in one tailed tests. Source: QCEW 2003-2010, MSHA 2001-2010.

Table 3.4: Ln (Real Hourly Wage) Regression, Fourth Quartile Interaction

Variables	(1)	(2)	(3)	(4)
$\Delta Citation_{t-1}$	0.000070***	0.000047	0.000077*	0.000083*
	(0.000016)	(0.000044)	(0.000044)	(0.000043)
$1(\Delta Citation_{t-1} \geq 57.5)$		0.00004	-0.000037	-0.000054
		(0.000072)	(0.000070)	(0.000069)
Year Fixed Effects	No	No	Yes	Yes
Mining and Coal Operations in County	No	No	No	Yes
Observations	992	992	992	992
R-squared	0.729837	0.729912	0.749173	0.750239

Notes: Estimation based upon 992 balanced panel observations for NAICS equal to 212. All estimations include county fixed effects. Standard errors are clustered at the county level (124 clusters). Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: QCEW 2003-2010, MSHA 2001-2010.

Table 3.5: Ln (Real Hourly Wage) Regression, Linearity Test

Variables	(1)	(2)	(3)	(4)	(5)
$\Delta Citation_{t-1}$	0.00007046***	0.00006134***	0.00005177***	0.00004913***	0.00005746**
	(0.00001611)	(0.00001811)	(0.00001925)	(0.00001870)	(0.00002366)
$\Delta Citation_{t-1}$ Squared		0.00000003*	0.00000001	0.00000001	0
		(0.00000002)	(0.00000002)	(0.00000002)	(0.00000002)
Year Fixed Effects	No	No	Yes	Yes	Yes
Mining and Coal Operations in County	No	No	No	Yes	Yes
Full Controls	No	No	No	No	Yes
Observations	992	992	992	992	992
R-squared	0.72983688	0.73006196	0.74912457	0.75011581	0.75651323

Notes: Full controls include all of the same variables as in Table 3.3. Estimation based upon 992 balanced panel observations for NAICS equal to 212. All estimations include county fixed effects. Standard errors are clustered at the county level (124 clusters). Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: QCEW 2003-2010, MSHA 2001-2010.

Table 3.6: Ln (Real Hourly Wage) Regression, Monopsony Power

Variables	(1)	(2)
$\Delta Citation_{t-1}$	0.000090**	0.000074**
	(0.000040)	(0.000035)
$\Delta Citation_{t-1} \times Mines$	0	0
	(0.000001)	(0.000001)
Mining Operations in County	0.000828	0.004168
	(0.002404)	(0.003868)
Observations	992	992
R-squared	0.730041	0.756568

Notes: Estimation based upon 992 balanced panel observations for NAICS equal to 212. All estimations include county fixed effects. Model (1) includes no other controls. Model (2) includes the full set of controls. Standard errors are clustered at the county level (124 clusters). Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: QCEW 2003-2010, MSHA 2001-2010.

Table 3.7: First stage and IV-GMM Estimates

	Accident rate (First Stage)		Ln (Real Hourly Wage)			
			Cross Section Estimates		Fixed Effect Estimates	
	OLS	OLS	OLS	IV	OLS	IV
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Accident rate			0.009499	0.027233**	-0.002953	0.007017
			(0.007169)	(0.012682)	(0.002898)	(0.044062)
Accident rate	0.467840***	0.015555				
(first lag)	(0.073362)	(0.077183)				
Citations	0.000513***	0.000405				
(first lag)	(0.000160)	(0.000253)				
Proposed Penalty	-0.000142	-0.000614***				
(first lag)	(0.000127)	(0.000184)				
Mining Operations	-0.019010***	-0.002476	0.000989	0.001625	0.003704	0.00372
in County	(0.006493)	(0.026919)	(0.001237)	(0.001231)	(0.003912)	(0.003567)
Coal Mining	0.011561*	-0.019237	-0.001154	-0.001934	0.000692	0.000811
Operations	(0.006061)	(0.037991)	(0.001182)	(0.001225)	(0.004245)	(0.004053)
Employees	-0.000245***	-0.000271	0.000099***	0.000104***	-0.000056	-0.000052
	(0.000077)	(0.000395)	(0.000020)	(0.000019)	(0.000047)	(0.000047)
County Fixed Effects	No	Yes	No	No	Yes	Yes
Weak Identification Test				27.253		4.956
Overidentification Test				6.408		3.729
				(0.041)		(0.155)
Observations	992	992	992	992	992	992
R-squared	0.32568	0.531072	0.110011	0.083733	0.74991	0.067397

Notes: Estimation based upon 992 balanced panel observations for NAICS equal to 212. All estimations include individual year fixed effects and county fixed effects as indicated. Standard errors are clustered at the county level (124 clusters). The Weak Identification Test statistic is the Kleibergen-Paap rk Wald F Statistic. The Overidentification Test is the Hansen J Statistic. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: QCEW 2003-2010, MSHA 2001-2010.

Table 3.8: Ln (Real Hourly Wage) IV-GMM (Alternating Instrument Estimates)

Variables	(1)	(2)	(3)	(4)
Accident rate	0.027233**	0.026869**	0.024282*	0.098804***
	(0.012682)	(0.012650)	(0.012422)	(0.027574)
Accident rate (first lag)	Yes	Yes	Yes	No
Citations (first lag)	Yes	Yes	No	Yes
Proposed Penalty (first lag)	Yes	No	Yes	Yes
Weak Identification Test	27.253	39.090	38.699	6.073
Overidentification Test	6.408	4.644	0.347	0.854
	(0.0406)	(0.031)	(0.556)	(0.355)
Observations	992	992	992	992
R-squared	0.083733	0.0848	0.091751	-0.556425

Notes: Estimation based upon 992 balanced panel observations for NAICS equal to 212. All estimations include the number of employees, mining operations (coal and non-coal), and year fixed effects. Standard errors are clustered at the county level (124 clusters). The Weak Identification Test statistic is the Kleibergen-Paap rk Wald F Statistic. The Overidentification Test is the Hansen J Statistic. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Column (4) reports the uncentered R2. Source: QCEW 2003-2010, MSHA 2001-2010.

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Appendix A

Appendix to Chapter 2

Table A.1: Health Care Outcomes Inclusion and Exclusion Criteria

Health Care Outcome	Inclusion Criteria	Exclusion Criteria
Pressure ulcers	ICD-9-CM: 707.0*, 707.23-707.25	Length of stay of less than 5 days; principal diagnosis of pressure ulcers; secondary diagnosis of pressure ulcers present on admission; MDC 9 (Skin, subcutaneous tissue, and breast); MDC 14 (Pregnancy, childbirth, and puerperium); any diagnosis of hemiplegia, paraplegia, or quadriplegia: (ICD-9-CM: 333.71, 334.1, 342.**, 343.*, 344.**, 438.2-438.53, 768.70, 768.72, 768.73); any diagnosis of spina bifida or anoxic brain damage: (ICD-9-CM: 348.1,741.**,768.5); transfer from a hospital, skilled nursing facility, intermediate care facility, or other health care facility (pointoforiginub04: 4, 5, 6); with missing: gender (female), age (age), discharge quarter (dqtr), year (year), principle or secondary diagnosis (dx1, dx2)
Hospital-acquired pneumonia	ICD-9-CM: 507.0, 997.3*, 514, 482.0*-482.2, 482.4*-482.9, 485, 486	Primary diagnosis—ICD-9-CM: 480.*-487*, 507.0, 514, 997.3*; secondary diagnosis—ICD-9-CM: 480.*, 481, 483.*, 484.*, 487.*; MDC 4; AIDS (ICD-9-CM: 042); immunocompromised states (ICD-9-CM: 279.**)
Hospital-acquired sepsis	ICD-9-CM: 038.**, 790.7	Length of stay of less than 3 days; primary diagnosis of sepsis; immunocompromised states (ICD-9-CM: 279.**); AIDS (ICD-9-CM: 042); DRG: 20, 68– 70, 79–81, 89–91, 126, 238, 242, 277–279, 320–322, 415–417, 423
Urinary tract infection	ICD-9-CM: 599.0, 996.64	Primary diagnosis of UTI; MDC 11-15; any diagnosis of ICD-9-CM: 646.60-646.64, 639.8

Notes:

(1) Diagnoses 10-25 are dropped for consistency across data set due to hospital procedure changes across years (dx10-dx25)

(2) Codes are based on AHRQ Patient Safety Indicators (PSI) (http://www.qualityindicators.ahrq.gov/modules/psi_overview.aspx) and Needleman, et al. Nurse-Staffing Levels and the Quality of Care in Hospitals. *N Eng J Med.* 2002;346(22):1715-1722.

(3) The presence of '*' or '**' is used to indicate the use of a 4^{th} or 5^{th} digit for clarity and ease of comparison in the ICD-9-CM reference book

Appendix B

Appendix to Chapter 3

Table B.1: Ln (Real Hourly Wage) Regression (Truncated), Full Controls, Balanced Panel

Variables	(1)	(2)	(3)	(4)
Citations		-0.000001	0.000015	0.000016
		(0.000019)	(0.000045)	(0.000045)
Citations (first lag)		0.000053***	0.000057***	0.000057**
		(0.000019)	(0.000022)	(0.000022)
Citations (second lag)		-0.000025	-0.000025	-0.000025
		(0.000018)	(0.000019)	(0.000020)
Accident rate	-0.004667	-0.004521	-0.004625	-0.004602
	(0.002960)	(0.002960)	(0.002952)	(0.002952)
Accident rate (first lag)	0.005233	0.005416	0.005353	0.005466
	(0.003597)	(0.003554)	(0.003541)	(0.003600)
Accident rate (second lag)	-0.00283	-0.002632	-0.002651	-0.002661
	(0.002601)	(0.002639)	(0.002677)	(0.002687)
Fatality Indicator	-0.017193	-0.015991	-0.015559	-0.016156
	(0.015440)	(0.015913)	(0.016644)	(0.016622)
Fatality Indicator (first lag)	-0.025152	-0.026564	-0.025758	-0.026265
	(0.017646)	(0.017470)	(0.017490)	(0.017417)
Fatality Indicator (second lag)	-0.012429	-0.016532	-0.015648	-0.016369
	(0.016899)	(0.017051)	(0.017833)	(0.017841)
Proposed Penalty			-0.000016	-0.000016
			(0.000042)	(0.000042)
Proposed Penalty (first lag)			-0.000006	-0.000006
			(0.000023)	(0.000023)
Proposed Penalty (second lag)			0.000007	0.000007
			(0.000027)	(0.000027)
Mining Operations in County	0.006092**	0.005972**	0.005872**	0.005894**
	(0.002708)	(0.002713)	(0.002748)	(0.002742)
Coal Mining Operations	-0.002094	-0.002219	-0.002669	-0.002697
	(0.003103)	(0.003065)	(0.003305)	(0.003304)
Employees	-0.000008	-0.000012	-0.00001	-0.000007
	(0.000043)	(0.000045)	(0.000043)	(0.000043)
Employees (first lag)	-0.000018	-0.000043	-0.000045	-0.000044
	(0.000042)	(0.000046)	(0.000046)	(0.000047)
Employees (second lag)	-0.000062	-0.000041	-0.000039	-0.000041
	(0.000048)	(0.000049)	(0.000051)	(0.000051)
Total Experience				0.000694
				(0.001138)
Observations	932	932	932	932
R-squared	0.766889	0.768262	0.768519	0.768706

Notes: Estimation based upon 932 balanced panel observations for NAICS equal to 212 where the real hourly wage is between \$10 and \$30. All estimations include county and year fixed effects. Standard errors are clustered at the county level. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: QCEW 2003-2010, MSHA 2001-2010.

Table B.2: Ln (Real Hourly Wage) Regression, Full Controls, Unbalanced Panel

Variables	(1)	(2)	(3)	(4)
Citations		-0.000069*	-0.000065	-0.000061
		(0.000036)	(0.000049)	(0.000049)
Citations (first lag)		0.000102***	0.000087**	0.000083**
		(0.000039)	(0.000039)	(0.000038)
Citations (second lag)		-0.000033	-0.000038	-0.000034
		(0.000026)	(0.000028)	(0.000028)
Accident rate	-0.000102	-0.000074	0.000024	0.000249
	(0.004922)	(0.004898)	(0.004878)	(0.004839)
Accident rate (first lag)	0.000925	0.000991	0.001056	0.000827
	(0.002178)	(0.002157)	(0.002151)	(0.002314)
Accident rate (second lag)	-0.003967*	-0.004023*	-0.003989*	-0.004174*
	(0.002230)	(0.002200)	(0.002234)	(0.002306)
Fatality Indicator	0.017324	0.018754	0.018513	0.015972
	(0.021701)	(0.021426)	(0.021443)	(0.021525)
Fatality Indicator (first lag)	-0.028174	-0.025553	-0.024933	-0.026115
	(0.017471)	(0.017750)	(0.017801)	(0.017942)
Fatality Indicator (second lag)	-0.01025	-0.012568	-0.013651	-0.01595
	(0.015275)	(0.015310)	(0.015581)	(0.015786)
Proposed Penalty			-0.000014	-0.000012
			(0.000037)	(0.000038)
Proposed Penalty (first lag)			0.000027	0.000026
			(0.000033)	(0.000033)
Proposed Penalty (second lag)			0.000003	0.000004
			(0.000032)	(0.000032)
Mining Operations in County	0.00471	0.004628	0.004668	0.004575
	(0.003486)	(0.003461)	(0.003515)	(0.003455)
Coal Mining Operations	-0.000818	-0.000187	-0.000191	-0.000126
	(0.004063)	(0.004023)	(0.004164)	(0.004124)
Employees	-0.000005	0.000026	0.000025	0.000036
	(0.000054)	(0.000055)	(0.000056)	(0.000055)
Employees (first lag)	-0.000044	-0.000098**	-0.000095**	-0.000089*
	(0.000044)	(0.000048)	(0.000046)	(0.000047)
Employees (second lag)	-0.000036	-0.000013	-0.000013	-0.000024
	(0.000051)	(0.000057)	(0.000058)	(0.000058)
Total Experience				0.002365**
				(0.001041)
Observations	2,046	2,046	2,046	2,046
R-squared	0.807767	0.808805	0.808939	0.810461

Notes: Estimation based upon 2,046 unbalanced panel observations for NAICS equal to 212. All estimations include county level and individual year fixed effects. Standard errors are clustered at the county level (451 clusters). Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: QCEW 2003-2010, MSHA 2001-2010.

Table B.3: Ln (Real Hourly Wage) Regression (Truncated), Full Controls, Unbalanced Panel

Variables	(1)	(2)	(3)	(4)
Citations		-0.00003	-0.000022	-0.000021
		(0.000025)	(0.000045)	(0.000046)
Citations (first lag)		0.000059**	0.000052**	0.000051**
		(0.000023)	(0.000026)	(0.000026)
Citations (second lag)		-0.000005	-0.000014	-0.000013
		(0.000021)	(0.000024)	(0.000024)
Accident rate	-0.004506*	-0.004485*	-0.004436*	-0.004359*
	(0.002308)	(0.002299)	(0.002290)	(0.002274)
Accident rate (first lag)	0.002155	0.002227	0.002297	0.002251
	(0.002460)	(0.002445)	(0.002439)	(0.002509)
Accident rate (second lag)	-0.001205	-0.001233	-0.001226	-0.00129
	(0.001801)	(0.001770)	(0.001810)	(0.001822)
Fatality Indicator	0.007332	0.008688	0.009238	0.008573
	(0.022515)	(0.022504)	(0.022792)	(0.022840)
Fatality Indicator (first lag)	-0.027926	-0.027208	-0.025885	-0.026175
	(0.018274)	(0.018346)	(0.018344)	(0.018345)
Fatality Indicator (second lag)	-0.008285	-0.010549	-0.010277	-0.010805
	(0.016150)	(0.016163)	(0.016698)	(0.016767)
Proposed Penalty			-0.000013	-0.000013
			(0.000037)	(0.000037)
Proposed Penalty (first lag)			0.000005	0.000005
			(0.000027)	(0.000027)
Proposed Penalty (second lag)			0.000018	0.000018
			(0.000025)	(0.000025)
Mining Operations in County	0.006161**	0.006028**	0.006061**	0.006047**
	(0.003025)	(0.003045)	(0.003061)	(0.003058)
Coal Mining Operations	-0.002872	-0.002577	-0.002613	-0.002614
	(0.003533)	(0.003536)	(0.003707)	(0.003707)
Employees	0.000009	0.000023	0.000022	0.000026
	(0.000052)	(0.000057)	(0.000056)	(0.000055)
Employees (first lag)	-0.000036	-0.000068	-0.000068	-0.000068
	(0.000048)	(0.000054)	(0.000053)	(0.000054)
Employees (second lag)	-0.000032	-0.000024	-0.000021	-0.000023
	(0.000047)	(0.000049)	(0.000052)	(0.000052)
Total Experience				0.000559
				(0.000775)
Observations	1,871	1,871	1,871	1,871
R-squared	0.796629	0.797295	0.797442	0.797574

Notes: Estimation based upon 1,871 unbalanced panel observations for NAICS equal to 212 where the real hourly wage is between $10 and $30. All estimations include county and year fixed effects. Standard errors are clustered at the county level. Asterisks denote significance at the 10%(*), 5%(**), and 1%(***) levels. Source: QCEW 2003-2010, MSHA 2001-2010.